Evaluating credit guarantees for SMEs: evidence from Italy *

Alessio D'Ignazio †and Carlo Menon ‡

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Abstract

We evaluate the effectiveness of a partial credit guarantee policy program using unique microdata from a large set of Italian firms. Our results show that the policy was effective to the extent that it resulted into an improved financial condition for the beneficiary firms. While the total amount of bank debt was unaffected, firms showed a significant increase in the long-term component. Furthermore, targeted firms benefited from a substantial decrease in interest rates. There are, instead, no effects on the real outcomes, although this result might be partly due to data limitation and the short temporal horizon.

JEL classification: G2, H2, O16

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†Research Department, Bank of Italy, Via Nazionale 91, 00184 Roma, Italy. Email address: alessio.dignazio@bancaditalia.it.

‡OECD, Science, Technology and Industry Directorate, and Bank of Italy. Email address: carlo.menon@oecd.org.

1 Introduction

Guarantee schemes are widespread in both developed and developing countries, as they are seen as an effective instrument to improve the access to financial assets for entrepreneurial activities. They are often funded by public institutions, and their implementation is frequently listed among the policy recommendations of international organizations (OECD (2011), European Commission (2011a)). The popularity of guarantee schemes is due to the multiplicative effects embedded in such policies, on their capacity to move private capitals, and on the possibility to recover a large share of the public fund at the end of the program.

Despite their popularity, economic theory is not conclusive on the net effect of credit guarantee schemes on firms finance. The actual effects of such programs is ultimately an empirical question, but there is very little evidence available so far. In this paper, we try to fill in this gap, by estimating the causal effect of a credit guarantee scheme implemented in Italy in 2008. As compared to existing empirical literature, which is mainly based on difference-in-differences estimator or on propensity score matching, this paper has the advantage of relying on more advanced estimation methods, requiring weaker assumptions and therefore providing more ground for the consistency of the estimated treatment effects.

The identification of net effects of the policy is indeed challenging, since treated and untreated firms may be intrinsically different, and this difference may be unobservable to us. Ideally, the program effectiveness would result from the difference in average outcome of the same group of firms with and without treatment at the same time, respectively. Such a counterfactual scenario is obviously unfeasible, but we reach a consistent estimate of treatment effects via instrumental variable estimation. The exogenous source of treatment is derived from some peculiar features of the scheme, and the validity of the IV procedure is made more plausible by the inclusion of a wide set of fixed effects and additional controls. Furthermore, the estimates survive to two demanding falsification tests.

We find that the program had no impact on the volume of loans, while increasing the amount of long-term debt and slightly decreasing the interest rate paid by firms. All other firm-level variables were unaffected; in particular, we were not able to find significant effect on the firm balance sheets, suggesting that the improved financial structure did not have a direct "real" effect, at least in the short term.

Our work is even more relevant, since it focuses on the "credit crunch" period, when credit constraints for firms were particularly stringent. This also leaves away some potential endogeneity of the policy, since the crunch was surely unexpected when the policy was designed, in the 2006.

2 SMEs lending and Credit guarantees schemes

The slowing down of economic growth since the turn of the century, the new challenges of globalisation, the fear of de-industrialisation and the recent financial crisis renewed the interest for industrial policies in Europe. Industrial policies are often based on a mix of subsidies, which may take the following forms: grants, defined as monetary payments that take the form of a lump sum, which are proportional either to the amount of the investment or to the costs borne by the firm for a given project; tax incentives, taking either the form of tax exemptions/reductions or that of tax credit; subsidized loans and public loans, mostly aimed at reducing the interest rate paid by firms (although the incentive might also take the form of repayments' postponement or that of more favourables evaluation of the merit of credit); and guarantees, in the case in which the public authority takes in the (partial) insolvency risk of a borrower, allowing otherwise constrained firms to access credit, and risky-but-creditworthy firms to get financing at a lower costs.

Furthermore, the rationale for the introduction of firm subsidies lies behind the general consensus on both the importance of the role of SMEs in the economies and on the difficulties that they face in accessing credit (European Commission, 2011b). The latter depends on the higher costs of small-scale lending, the lack of collateral, the reduced reliability of (often non audited) financial statements, asymmetric information, the limited liability structure of most SMEs (Beck, Klapper and Mendoza, 2008). Credit guarantees aim to offset such situations, improving both the access to credit and the terms of loans. On the other hand, the main argument against firm subsidies builds on their potential distorsive effects (de Meza (2002), Vogel and Adams (1997)) and on the fact that such policies tend to replace the markets rather than fixing the underlying problems causing credit restrictions to SMEs. In the case of credit guarantees the distorsive effect is considered smaller than the other forms of aids, while the most serious critiques consist in their potential triggering of moral hazard, both from the firms and from the banks, although this aspect is still debated. Furthermore, an additional advantage of credit guarantee schemes is the low initial cost, and the fact that large losses are incurred only in case of many bankrupcties or bank failures (Beck et al., 2008).

Broadly speaking, we can distinguish among two main typologies of credit guarantees schemes: (1) Mutual Guarantee Associations (e.g., Confidi in Italy): private societies formed by potential borrowers with limited access to bank loans that share their debt risk. However, they suffer from an adverse selection problem, as firms that consider joining the association know that the schemes attract the more risky firms. (2) Public Guarantee Schemes, funded by regional or national authorities: run either by an administrative unit of the government (e.g. development agencies, ministries, the central bank or publicly-owned banks) or by a legally separate credit guarantee organization. Resources usually take the form of periodic appropriations, i.e. continued subsidies, or of a fixed fund. Public guarantee schemes still represent the majority in low-income countries, while mutual guarantees are more largely used in high-income countries (Beck et al., 2008).

On a theoretical ground, the effect of the introduction of a credit guarantee scheme (CGS, hereinafter) is ambiguous. In the case of firms unable to meet the collateral requirement asked by the bank, CGS can result in more credit granted to the firm. Moreover, by reducing the informational asymmetries between a firm and a bank, the presence of a guarantee can lead to lower interest rates paid by the borrower, hence reducing moral hazard and adverse selection problems. Meyer and Nagarajan (1996) have argued that credit guarantees can lead to a learning process, where the banks discover that borrowers benefiting from the guarantee are not as risky and unprofitable as initially expected and become willing to provide loan to them in future without the guarantee. On a similar view are Riding, Madill and Haines (2007). On the other hand, CGS might as well lead to a riskier behavior from both the entrepreneur and the bank. In particular, if the CGS enforces entrepreneur's limited liability (i.e., if banks can only rely on CGS), then the firm might be willing to adopt riskier strategies than those adopted under normal circumstances (Lelarge, Sraer and Thesmar, 2008). From a bank point of view, if the share of loan covered by the guarantee is too large, the incentive to undertake a tough screening might get smaller (Benavente, Galetovic and Sanhueza, 2006). Another potential distortion is the fact that banks might be induced to be too quick in writing off loans backed by the guarantee and hence exerting little effort to collect the outstanding share of the loan.[1]

The empirical evidence about the effectiveness of CGS is both scarce and mixed. Hancock, Peek and Wilcox (2007) focus on the impact of credit guarantees provided in the US by the Small Business Administration, finding that disbursement of guarantees led to an increase in both firms' output and employment. Lelarge et al. (2008) studied the guarantee program Sofaris (known also as Ose Garantie), carried out in France. They find that credit additionality holds in the intensive margin, while there are no effects on the extensive margin. In addition, such program seems to have induced more risk taking from the benefiting firms. Kang and Heshmati (2008) studied the impact of two CGS implemented in Korea, focusing on firm sales, productivity,

[1] Vogel and Adams (1997) points at two sources of substitution that would diminish the effect of the guarantees and that would result into an overestimation of the additionality if the analysis is carried out at the bank level. The first one is intra-portfolio substitution: lenders may redefine the purpose of existing loans to qualify borrowers for loan guarantee, or they might employ 'column-shifting', moving distressed loans into the guaranteed portfolio. A second one is inter-lender substitution: enterprises serviced by other banks are captured by those banks operating under a guarantee scheme. Analysis at the firm level, however, is less tricky.

and employment. They find only weak evidence in favor of credit guarantees, whose effectiveness was lowered, among other things, by the fact that they were mainly employed to support financially unconstrained firms. Zecchini and Ventura (2009) study the effectiveness of a publicly funded guarantee scheme for SMEs implemented in Italy. They find that the guarantee resulted into a greater amount of bank loans to the firms; this effect however is rather small in size.[2] The public guarantee also lowered the costs borne by the firms. However, they noticed that the scheme did not necessarily target the most financially disadvantaged firms: there was no screening to assess whether a loan proposed by a bank to the Fund would have been granted even in the absence of a guarantee. Hence, the pattern of guarantees could reflect essentially bank lending decisions, more than SMEs' potential needs. Columba, Gambacorta and Mistrulli (2009) investigate the impact of the mutual guarantee institutions in Italy (Confidi) and show that small firms affiliated to the 'confidi' pay lower interest rates charged on loan contracts which are not backed by mutual guarantees with respect to similar firms. Their finding is consistent with the view that mutual guarantee instututions are better at screening and monitoring borrowers than banks are. In a more recent paper, Mistrulli and Vacca (2011) find that mutual guarantee institutions played a role in avoiding a break-up in credit flows to affiliated firms during the crisis. Moreover, affiliated firms also faced lower interest rates with respect to similar firms. On the other hand, the deterioration in credit quality over the crisis has been more intense for customers with guarantees from *Confidi*.

3 The policy

In this paper we focus on a partial credit guarantee regional program devised in 2006 in Italy to improve SMEs access to credit. The program, which started operating in 2008, benefits from a regional fund, managed by the regional administrative body.

Each firm loan involves an agreement between the Region and a private covenant bank. Before conceding the loan, both the Region and the covenant bank separately proceed to a credit screening. Loans backed by the guarantee typically have a 5 years amortization schedule, which follows a grace period whose length is variable. The protection individually offered by the Region on each loan covers up to 80 per cent of the losses in case of default of the firm or cancellation of the loan; such occurrences are referred as 'Credit event'.

[2] Other guarantee schemes: Sowalfin (Belgium); Tempte SA (Greece); Besluit Borgstelling Midden en Klein Bedrijf (Netherlands); AGROGARANTE; GARVAL; LISGARANTE; MCGF; NORGARANTE (Portugal); ISBA; SGR; Sociedad de Garantia Reciproca; Transaval S.G.R; (Spain); ALMI Foretagspartner AB (Sweden); CSC Centrale Suisse de Cautionnement (Switzerland).

So far, the program consisted of several waves: we focus on the first one, which began in 2008, since it leaves us with a long enough post-intervention window.[3]

Eligible firms are all SMEs having a total turnover larger than 1M euro in 2007 and lower than 43M, or lower than 50M and less than 250 employees (the upper bound is set by the definition of SME advanced by the European Commission in the Recommendation 2003/361/EC). The turnover's lower bound, however, is 500k euro for the firms belonging to a set of sectors considered 'prioritary'. Beneficiary firms must also be eligible for the application of the National Fund by Law 662/96 (i.e., SMEs according to the definition in European Commission Recommendation 2003/361, which are not in economic nor financial distress, belonging to any sector but those considered "sensitive", such as agricolture, transport, shipbuilding, motor vehicle, and so on).

3.1 Aims of the policy and expected outputs

The effectiveness of the partial credit guarantee scheme can be assessed along different viewpoints. Firstly, did this measure lead to an increase in the amount of credit granted to the beneficiary firms? To the extent that financially constrained firms can obtain guarantees at a cost lower than the market one, they could obtain more credit. Secondly, did the CGS result in lower interest rates? Thirdly, did the CGS improve the financial structure of the beneficiary firms? Even if there is no credit additionality, replacing short term debt with long term debt can improve the firm financial structure, eventually lowering their probability of default. Fourth, did the CGS lead to an increase in the level of output, investments, employment? These are indirect effects in firm performance which could originate from any of the direct mechanisms listed above, namely a larger amount of credit or a less onerous debt structure.

From the policy maker's point of view, the policy is overall successful if it increases the aggregate lending; the additional amount should be a multiple of the guarantee funds, since the program is expected to foster a larger deployment of bank's capital. The aggregate effect, however, could be hampered by substitution effects between banks, or by the lack of direct effects at firm level.

Although the actual beneficiaries of the program are both banks and firms, in the following of the paper we focus mainly on firms, since those were the target of the policy maker, and we have a clear a-priori on the expected outcome.

[3]Firms treated in following waves of the program are excluded from the sample.

4 Data

Our empirical exercises benefits from a unique and very rich database, a panel of about 9,000 firms located in the region where the policy was implemented and in the three neighbouring regions, which we use to further populate the control group. The dataset is built taking from three different sources.

We use the official dataset maintained by the regional administrative body, involving the full list of beneficiary firms. For each firm the dataset reports a wide set of information, such as: the date of the guarantee approval, the name of the covenant bank, the amount of guaranteed loans and that of the guarantee, the date of the first disbursement, the riskiness of the firm and several balance sheet variables referring to the year preceding the first disbursement.

The guarantee scheme that we focus on involved about 200 firms. We merge this dataset with two additional sources of information: the Central credit register (CCR) maintained at the Bank of Italy and the Cerved dataset on Italian corporations.

The CCR[4] provides bank-firm level information on a large set of financial variables. We are interested, in particular, to the amount of bank credit, the interest rate and the level of bad loans. Since our identification strategy relies on bank-firm relationship characteristics, we collect loans data at the bank-firm level. We gather annual end-of-the-year data, from 2003 to 2010.

We also gather firm balance sheet information from the Cerved archive, containing informations on all Italian corporation. We collect information covering the period 2003-2009 (2010 balance sheet data were still not available). We use such information both to improve our identification strategy and to investigate the impact of the guarantee scheme on real outcomes. All variables are measured at the end of the year.

We focus on eight dependent variables:

- Long-term loans: total stock of long-term loans owned by the firm (irrespectively on the lending bank)
- Total loans: total stock of bank loans, including both short-term loans and long-term loans.
- Interest rate: nominal interest rate paid by the firm on medium and long term bank loans.
- Bad loans: bad loans include all the outstanding bank credit to a borrower considered insolvent. The variable is binary and is equal to

[4]According to the Italian Banking Regulation 'for each borrower, financial intermediaries supervised by the Bank of Italy have to report to the CCR, on a monthly basis, the amount of each loan, either granted or disbursed by banks, for all loans exceeding a given threshold' (the threshold was € 75,000 until 31 December 2008).

one whenever at least one lending relationship involving the given firm is listed as 'bad' by the bank.

- Turnover: amount of sales.
- Total assets: includes assets (such as cash, stocks, money due), fixed assets (such as buildings and machinery) and intangible assets (such as patents).
- Trade debts: debts towards suppliers providing goods and services necessary to the production cycle and tangible-intangible assets. All variables are measured at the end of the year.
- Investments: yearly growth rate of fixed assets.

On the right hand side of the regression equation we include the following controls, always with one year time lag:

- Rating: z-score measuring the probability that a firm becomes insolvent. It ranges from 1 (excellent creditworthiness) to 9 (almost-certain insolvency).
- Number of lending banks: total number of banks holding lending relationships with the firm.
- Age of the firm: number of years since the funding year of the firm.
- Firm leverage: financial debts over the sum of financial debts and equity.

Among the controls, we often include also variables on the LHS of other specifications. Tables 1 and 2 report the descriptive statistics of the firms in the sample. To limit selection, we kept in the sample only firms which do not enter or exit along the period 2005-2009.

5 Empirical strategy and identification

The aim of the paper is to assess the impact of the credit guarantee scheme on a number of firm-level variables: total loans, credit costs, total debt, probability to default, and annual turnover.

Formally, we estimate the treatment effect with the following regression model:

$$y_{itmr} = \alpha + \beta T_{it} + X_{it}\gamma + \delta_i + \mu_{mt} + \rho_{rt} + \epsilon_{it} \tag{1}$$

where y is the potential output of interest, T is a treatment dummy, X is a set of firm-specific time-variant controls, and δ, τ, μ, and ρ are firm, year, bank and region fixed effects, respectively; i indexes firms, t years, m lending banks, and r regions. Bank and region fixed effects are allowed to change over time. If treatment were randomly assigned conditional on observables, the parameter β would be the consistent estimate of the average treatment effect on the treated (ATE). However, the treatment dummy is likely to be correlated with the error term, since firms are heterogeneous and some of their unobserved idiosyncratic characteristics may be correlated with both the outcome variable and the treatment dummy. This means that treated and untreated firms are systematically different, independently on the policy. We control for time-invariant heterogeneity by the means of individual fixed effects, exploiting the variability across time of the relevant variables. Still, to the extent that unobserved idiosyncratic factors are time-variant, our estimates are hardly consistent.

In particular, there are two main challenges to a consistent estimation of the treatment effect. First, the covenant bank may have been selected by local policy makers because of its special attitude toward local SMEs, or because of specific trends (e.g., shrinking in employment) of its portfolio of funded firms; in turn, these factors may affect the program outcome. Second, firms selected by the bank - or self-selected - into the program may be intrinsically different from the average firm, e.g., they can be more credit-constrained. This is also likely to have an independent impact on the outcome variables.

We overcome these two challenges and reach identification exploiting two peculiar aspects of the Italian credit markets.

The first key factor in our identification strategy is a M&A operation affecting the covenant bank. The first wave of the guarantee scheme was planned in 2006 and involved only one covenant bank, namely a regional bank - which we henceforth call 'bank A' - with a relevant share of loans in the region, and limited penetration outside the regional boundaries. A few months before the program was actually implemented, at the beginning of 2008, bank A was acquired by one of the leading Italian (and European) banking group, which we define 'bank B'.

Therefore, while it is likely that the original choice of the covenant bank A when the policy had been designed could be dependent on unobservable bank characteristics, bank B was involved in the program only because of the M&A operation, which was unexpected by regional policy makers. This rules out the possibility of a bias originating by a non random selection of the covenant bank.

This alone, however, does not completely exclude that the treatment and control groups are systematically different, either because treated firms are a specific selection of all firms funded by bank B, or because very "motivated" firms may decide to change funding bank in order to enroll into the program.

Related to this concern, the second characteristic of the Italian financial market contributing to our identification strategy is the stickiness of the firm-bank relationships. Italian firms tend to swap bank very rarely, because of high switching costs and of the importance of the reputation acquired in the past for obtaining new credit. It follows that firms that were financed by bank B before the policy was planned were highly likely to still be customer of bank B when the policy has been implemented. As shown in the next section, at firm level the lagged creditor bank is a very good predictor of the current creditor bank.

The interactions of these two peculiar characteristics of the Italian credit markets provide a powerful source of exogenous treatment: the firms that were funded by bank B were more likely to enroll into the program for reasons which are independent from the policy, and unforeseen by the regional policy maker. Intuitively, the group of firms which were funded by bank B before the policy was planned became "randomly" very likely to enroll the program. We therefore build an instrumental variable based on these conditions.

More precisely, the assumption we make to validate the exclusion restriction of our empirical strategy is that, conditional on firm characteristics and on firm, time, time-bank, and time-region fixed effects, the treatment outcome is independent on the firms being customer of Bank B some years before. In the following of the paper, we will further support the validity of this assumption with a battery of falsification tests.

This implies that we can estimate an exogenous treatment propensity for firms based on a dummy which is equal to one if the firm was lent by bank B before the PCG scheme had been designed, expecting that firms lent by bank B in year T-n have a higher propensity of being lent by bank B in year T and therefore to be in the treatment group.

Furthermore, bank B, being one of the major Italian and European banking group, holds a large number of lending contracts in neighbouring regions, which allows to control for a time-variant bank fixed effect; this is a particularly powerful control, since Italian banks show little variability in their lending strategy across geographical areas (Bank of Italy, 2011).

In theory, we could simply use a dummy equal to one if the firm is lent by bank B at time T-n as an instrument. However, given that the endogenous variable is binary, we opt for the "procedure 18.1" suggested by (Wooldridge, 2002)[5], which has been proven to produce consistent estimators and asymptotically valid standard errors and test statistics, while increasing efficiency. The exogenous treatment propensity is thus estimated in the following way:

$$Pr\left(T_{iT}\right) = \alpha + \phi_1 BankB_{t-3} + E_{it-3}\phi_2 + X_{i0}\phi_3 + \varepsilon_{iT} \tag{2}$$

where the probability of treatment in the year T in which the program was implemented (the 2008) is a function of having a lending relationship

[5]See in particular section 6.1.1 and 18.4.1

with Bank B three years earlier (thus in 2005, before the program was designed or announced) but no lending relationship with the covenant bank A, of being eligible three years earlier, and on a set of firm-specific variable at time 0, i.e., the year in which the firm enter the sample (2004 or 2005). The model is estimated with a Probit regression only for the treatment year T.

The estimated propensity score subsequently becomes the instrumental variable in a two-stages least square estimation of model 1.

We also perform a robustness test using the simpler binary instrument (equal to one if the firm had a lending relationship with Bank B three years earlier, and if eligibility conditions were met the year before treatment). As expected, the latter results are similar to the ones based on the exogenous treatment propensity, but less precise. Results are not reported for brevity, but they are available from the author upon request.

With respect to previous empirical investigations on the effects of CGS based on diff-in-diff estimator (Lelarge et al., 2008) or propensity score matching (Kang and Heshmati, 2008), the IV strategy we propose has the advantage of being robust to unobserved trend correlated with treatment, and to selection on unobservables.

6 Results

6.1 Aggregate effect

The first piece of analysis we perform is on the aggregate effect. This preliminary step is simply descriptive, since causality is not fully addressed. We estimate the following equation:

$$Loans_{irt} = \alpha + \beta \cdot T_{irt} + \gamma_{ir} + \rho_{rt} + \mu_{it} + \varepsilon_{irt} \tag{3}$$

in which the total loans of bank i in region r at year t are a function of a treatment dummy equal to one if the bank i is the covenant bank, plus bank-region, region-year and bank-year fixed effects. The unit of observation is the region-bank pair. A positive treatment coefficient would imply that the covenant bank in the treatment year in the region is giving more loans than the same bank it the same year in other regions, conditional on the mean of annual loans of the same bank and on the mean of all other banks in the same year in the same region (this can be defined a difference-in-difference approach). Estimates (table 3) are rather unprecise and not significantly different from zero. Therefore, we do not observe any aggregate effect of the policy, although the exercise can be flawed by a bias due to omitted firm dynamics, e.g., an increase in the riskiness of the portfolio of firms of the covenant bank in the region. These results alone, therefore, are not conclusive on the effect of the policy.

6.2 Firm level analysis

When we move to firm level analysis, we find that the policy was effective to the extent that it resulted into an improved financial condition for the beneficiary firms. While those results confirm that the total amount of bank debt was unaffected, treated firms showed a significant increase in the long-term component. Moreover,targeted firms benefited from a substantial decrease - of around 40 points - in interest rates. On the other hand, the program did not affect the risk of moral hazard: the probability to default for a treated firm remained the same of that of an otherwise identical untreated company. We do not observe, instead, any effect on the real outcomes: the policy had no significant impact on the turnover and investments.

In the following, we present these results in detail, commenting on both OLS and IVE results. In every column reporting IVE results, the last row shows the Kleibergen-Paap Wald F-statistic of the corresponding first stage regression, which is always well above the weak instrument threshold calculated by (Stock and Yogo, 2005).

- Long term loans (table 4): the policy targeted directly long term loans, and we indeed find a positive effect. OLS estimates show that those increased on average of 36% in the first year, 32% over two years, and 30% over three years. IVE coefficients are very similar: when the selection is controlled for, the effect is equal to 48% in the first year, 37% over two years, and 33% over three years.

- Total loans (table 5): OLS estimates suggest that the policy increased the volume of total loans too, since coefficients are significantly positive, and range from 17% (first year) to 12% (over three years). However, IVE coefficients are much smaller and not statically different from zero. In particular, the coefficient over three years is equal to -0.07 with a standard error of 0.09. Therefore, once we control for endogenous selection in the treatment sample, substitution across time and type of debts cancels out the positive impact of the program on the volume of loans.

- Interest rates (table 6): contrary to loans, the effect on interest rates is increasing over time, as it ranges from 37 base points in the first years to 48 base points over three years. IVE estimates are somewhat bigger than OLS in absolute value. The difference between OLS and IVE results suggest that treated firms would have paid a higher interest rate than untreated ones, conditional on the controls (which include the rating and the leverage in the year before). This may imply that treated firms have a more costly debt structure, or demand larger quantities of debt, as suggested also by the upward bias of OLS estimates of the effect on total loans.

- Bad loans (table 7): OLS estimates do not show any significant effect on the probability of loans to turn 'bad'; this result is confirmed by IVE, although over three years the probability of default becomes marginally significant.
- Turnover (table 8): we do not find any effect on firm turnover, neither with OLS nor IVE.
- Trade debts (table 9): OLS results suggest that the public guarantee did not lead to a small but significant reduction in trade debts; this is substantially confirmed by IVE.
- Investments (table 10): The OLS estimates indicate a positive effect of the policy on firm investments, which is significant on the first year only, suggesting intertemporal substitution (see (Bronzini and de Blasio, 2006)). When we estimate our model through IVE, the coefficient are still positive, especially for the first year, but they are not significant at 10% confidence rate. [6]

It is worth stressing that the absence of effects on real outcomes should not be interpreted as conclusive: firstly, balance sheet data are notably less precise and reliable than other sources; secondly, the maximum time horizon considered (two years) might be too short, as the additional funding may produce positive effects with a longer time lag.

7 Difference in differences model

In this section we provide some robustness and assess the impact of the policy by means of a matching - difference in differences model. The treatment group consists of all firms which first benefited from the guarantee in 2008 from "bank A". We exploit the fact that 70% of those firms were previously borrowing from "bank A" to restrict our focus on this subset (95 firms), matching it with a similar set of firms ("untreated") also borrowing from "bank A" before 2008. We perform a nearest neighbour matching, exploiting firms location, sector, pre-treatment dynamics of both long term debt and total debt, pre-treatment amount of borrowed funds (both long term and short term).

We then estimate the following model:

$$y_{it} = \beta_0 + \beta_1 dguarantee_{it} + \beta_2 post_t + \delta dguarantee_{it} \cdot post_t + \epsilon_{i,t} \tag{4}$$

[6] This result might also be affected by the optional revaluation of fixed assets undertaken by firms, according to the law decree 29 november 2008, n. 185.

where y_i is our outcome variable, "dsubsidy" is a dummy indicating whether or not the firm received the guarantee, and "post" is a dummy equal to 1 from 2008 onwards.

The estimates of the DID model (table 25) are in line with our previous findings. On average, the estimates show that there is no impact on total loans, while firms increase the amount of long term debt, suggesting that, in absence of treatment, firms over-rely on short term credit. We also find that treated firms face a statistically significant reduction of the cost of debt, with an estimated drop 24 base points. On the other hand, in accordance with previous results, there seem to be no effects on real outcomes.

8 Falsification test

We design two different falsification tests to reinforce the validity of our identification strategy. The first test simulates the policy in a region adjacent to the one under analysis, by creating a placebo treatment based on the same criteria we used to build our instrumental variable. We create a treatment dummy which is equal to one in year 2008 if firms were funded by the covenant bank B in 2005 and they were eligible in 2007. In all other respects, the regressions are identical to reported before. If such a placebo treatment ended up to be significant, it would mean that the way in which we build our instrumental variables brings in a bias. If otherwise the placebo treatment is insignificant, this would provide supporting evidence for our exclusion restrictions. Results (see tables 18, 19, 20, 21, 22, 23, 24) show that this is indeed the case: the placebo treatment is never significant, and point estimates are generally close to zero.

The second falsification exercise aims at testing the validity of the exclusion restrictions of the 2SLS estimation. Usually exclusion restrictions cannot be tested directly, but Altonji, Elder and Taber (2005) proposed a simple and clever test which works for the case of binary instrument. The test consists in regressing (by OLS) the output variables on the instrumental variables and other controls, while limiting the sample to the group of untreated eligible firms. The intuition here is that, under standard exclusion restrictions, the instrument should not have any direct effect on the output variables; the only effect of the instrument on the dependent variable is indirect, and pass through the treatment. This in turn implies that if we restrict the sample to the group of untreated firms, the instrument should not have any effect, conditional on the other set of included controls. We therefore estimate the model of equation 1 only on the group of untreated eligible firms. Again, results (tables 11, 12, 13, 14, 15, 16, 17) support the validity of the exclusion restrictions, since the instrument is never significant across all different regressions. In other words, this implies that being lent by bank B in year 2005 does not have any independent effect on the amount of lending or on real outcomes in the treatment period (2008-10), once other controls are partialled-out.

9 Limits: LATE and external validity

The recent econometric literature on instrumental variable estimation has pointed out that in some circumstances IV estimates of treatment effects may be consistent only for some groups of observation, leading to the estimation of a local average treatment effect (LATE), rather than the average across the full sample (ATE). Furthermore, the validity of the IV estimation relies on the so-called monotonicity assumption on the functional form of the first stage equation (Angrist and Pischke, 2008). In our setting, the treatment of LATE should be generalized to the case in which covariates are included in the regression, and exclusion restrictions are more plausible after conditioning on covariates. Under conventional LATE assumptions, 2SLS would produce a weighted average of covariate-specic LATEs, which are less intuitive to treat than traditional LATE in univariate 2SLS, but are also (more) likely to approximate the real causal relationship of interest ((Angrist and Pischke, 2008), paragraph 4.5.2). This seems to be confirmed by the fact that our 2SLS results are reassuringly similar to those obtained via a simpler DID estimator. This leads us to conclude that our estimates should not be very far from the real ATE.

Another limit to the generality of our findings is due to the geographically limited scope of the program. Targeting only one region, the sample of involved firms can be quite specific, and their reaction to treatment may be influenced by some local idiosyncratic firm characteristics.

10 Conclusion

Guarantee schemes are widespread in both developed and developing countries, as they are seen as an effective instrument to improve the access to financial assets for entrepreneurial activities, especially SMEs. They are often funded by public institutions, and their implementation is frequently listed among the policy recommendations of international organizations. The popularity of guarantee schemes is due to the multiplicative effects embedded in such policies, on their capacity to move private capitals, and on the possibility to recover a large share of the public fund at the end of the program.

Despite their popularity, economic theory is not conclusive on the net effect of CGS on firms finance. In this paper we evaluate the effectiveness of a partial credit guarantee policy program, using unique micro-data from a large set of Italian firms. By means of instrumental variable estimations based on specific features of the program, we are able to identify the treatment effect of the policy on a number of potential outcome variables - such as the total loans of each firms, the cost of credit, the debt structure, the firm turnover, and the probability to default - in the two-three years following treatment.

We find that the program had no significant impact on the total volume of loans. On the other hand, the policy leads to a statistically significant increase in the volume of long term loans. Furthermore, the introduction of the policy also resulted in lower interest rates paid by beneficiary firms. There is little evidence of moral hazard by firms, with the probability of default being low and only marginally significant over the three years of treatment. All other firm-level variables, including turnover and investment, were unaffected, although this result might be partly due to data limitation and the short temporal horizon.

IV results are confirmed by those obtained through an alternative identification strategy, involving a difference in differences estimation over a sample of homogeneous firms. Moreover, results survive through a a battery of highly demanding falsification tests.

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Table 1: Full sample - descriptive stats

year		**TOT debt**	**LT debt**	**bad loans dummy**	**interest rate**	**rating**	**sales**	**total assets**	**tang. assets**	**trade debt**	**leverage**
2005	mean	2017483	1153270	0.001716	4.077897	5.317068	5636.209	5379.539	1219.591	1438.912	0.657177
	median	848916	368194	0	3.891414	5	2625	2504	298	627	0.724115
	sd	3610677	2755714	0.041394	0.892598	1.894301	7429.28	9084.347	3977.733	2210.699	0.53088
	n	9323	9323	9323	6853	9304	9323	9323	9323	9323	9323
2006	mean	2233398	1287920	0.001287	4.912514	5.340785	6142.733	5821.326	1274.449	1553.157	0.664284
	median	933457	423526	0	4.864275	5	2846	2734	317	681	0.734681
	sd	3907975	2969855	0.035856	0.7264598	1.613317	8008.391	9464.796	4098.305	2369.999	0.845746
	n	9323	9323	9323	7045	9305	9323	9323	9323	9323	9323
2007	mean	2474517	1430665	0.001931	5.75192	5.389741	6499.584	6220.895	1353.015	1619.426	0.670156
	median	1016307	467537	0	5.81112	6	2948	2882	334	681	0.740891
	sd	4317271	3227082	0.0439	0.6715213	1.65384	8535.339	10279.67	4552.73	2488.082	0.84925
	n	9323	9323	9323	6731	9319	9323	9323	9323	9323	9323
2008	mean	2654928	1487478	0.002467	5.936113	5.311903	6503.84	7086.276	2078.629	1587.268	0.642748
	median	1061993	466755	0	6.066141	5	2931	3126	414	663	0.691882
	sd	4844138	3645612	0.04961	0.6951806	1.746788	8903.357	12222.59	6816.809	2523.64	0.618363
	n	9323	9323	9323	6213	9317	9323	9323	9323	9323	9323
2009	mean	2638953	1524788	0.008581	3.305491	5.278309	5761.565	7047.788	2095.489	1509.548	0.610987
	median	1056028	476669	0	2.975893	5	2506	3086	416	623	0.687394
	sd	5059570	3907204	0.09224	1.201312	1.820031	8260.962	12495.1	6941.42	2523.412	6.130604
	n	9323	9323	9323	6846	9299	9323	9323	9323	9323	9323
2010	mean	2730040	1616844	0.01751	3.269896	.	.	.	.	.	.
	median	1043787	492677	0	2.973338	.	.	.	.	.	.
	sd	5388697	4215494	0.131167	1.126024	.	.	.	.	.	.
	n	9195	9195	9195	6926	.	.	.	.	.	.

Table 2: Treated firms - descriptive stats

year		**TOT debt**	**LT debt**	**bad loans dummy**	**interest rate**	**rating**	**sales**	**total assets**	**tang. assets**	**trade debt**	**leverage**
2005	mean	2907457	1258042	0	4.408983	5.453125	9937.089	8231.74	1219.896	2856.031	0.660692
	median	2174204	647700	0	4.194082	5.5	7322	6601	542.5	2071.5	0.715218
	sd	2723123	1592766	0	0.9561308	1.413432	8566.589	6676.939	1877.819	2614.569	0.226909
	n	192	192	192	138	192	192	192	192	192	192
2006	mean	3542335	1577347	0	5.232166	5.442708	11015.06	9292.844	1347.141	3100.344	0.673063
	median	2617488	934331.5	0	5.162282	5	8284.5	7313.5	605	2188	0.741307
	sd	3152365	1857218	0	0.7414533	1.394398	9473.342	7581.168	2039.1	2956.594	0.229934
	n	192	192	192	154	192	192	192	192	192	192
2007	mean	4163611	1911259	0	6.045898	5.661458	11492.97	10103.32	1407.74	3245.76	0.696942
	median	3185489	1003190	0	6.068176	6	8546.5	7573.5	588	2328	0.754492
	sd	3719291	2238755	0	0.5310293	1.430767	9811.794	8377.706	2216.179	3150.274	0.216975
	n	192	192	192	144	192	192	192	192	192	192
2008	mean	5008740	2483693	0.0052083	5.871013	5.651042	11622.33	11714.79	2283.724	3371.635	0.696828
	median	3659496	1314705	0	5.984955	6	8271	8269.5	850.5	2310.5	0.731303
	sd	4588200	2918917	0.0721688	0.6175785	1.41351	10474.81	10316.62	3449.86	3623.116	0.21567
	n	192	192	192	167	192	192	192	192	192	192
2009	mean	4736023	2352933	0.015625	3.044598	5.744792	10669.34	11594.16	2300.229	3217.391	0.590344
	median	3187235	1267656	0	2.789686	6	7886	8044	818	2250.5	0.74081
	sd	4558656	2901230	0.1243438	0.9689904	1.615525	9767.317	10225.4	3606.989	3064.998	1.709888
	n	192	192	192	177	192	192	192	192	192	192
2010	mean	4807139	2453984	0.026455	3.134085	.	.	.	.	.	.
	median	3151785	1290529	0	2.92014	.	.	.	.	.	.
	sd	4905508	3219701	0.1609104	0.8595614	.	.	.	.	.	.
	n	189	189	189	182	.	.	.	.	.	.

Table 3: Aggregate effect - total loans

	(1)	(2)	(3)
Dep. variable		Total loans	
GCS wave 1	-0.539		-0.629
	(0.548)		(0.634)
Guaranteed loans		-0.0149	
		(0.0187)	
CGS wave 2			-0.607
			(0.709)
Constant	18.35***	18.35***	18.35***
	(0.0913)	(0.0919)	(0.0909)
Region X Year F.E.		YES	
Bank X Year F.E.		YES	
Observations	196	196	196
R^2	0.544	0.539	0.550

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

Table 4: Long term loans

	OLS	IV	OLS	IV	OLS	IV
Dep. variable			Long term loans			
Treated 1 year	0.363***	0.477***				
	(0.051)	(0.155)				
Treated 2 years			0.320***	0.371***		
			(0.053)	(0.130)		
Treated 3 years					0.297***	0.333**
					(0.055)	(0.130)
Turnover (t-1)	0.016	0.016	0.047**	0.047***	0.045***	0.045***
	(0.023)	(0.022)	(0.019)	(0.017)	(0.017)	(0.014)
Total assets (t-1)	0.359***	0.359***	0.367***	0.366***	0.434***	0.434***
	(0.038)	(0.038)	(0.024)	(0.024)	(0.025)	(0.021)
Low rating (t-1)	0.017	0.017	-0.071***	-0.070***	-0.078***	-0.078***
	(0.033)	(0.032)	(0.027)	(0.025)	(0.024)	(0.021)
Medium rating (t-1)	-0.01	-0.009	-0.030**	-0.029**	-0.031***	-0.031***
	(0.016)	(0.015)	(0.013)	(0.012)	(0.012)	(0.011)
Nr of banks (t-1)	0.088***	0.087***	0.134***	0.134***	0.149***	0.148***
	(0.023)	(0.022)	(0.019)	(0.018)	(0.017)	(0.016)
Firm age	0.114	0.112	0.207	0.206	0.395**	0.394**
	(0.183)	(0.171)	(0.161)	(0.150)	(0.188)	(0.174)
Constant	9.960***		9.368***		8.059***	
	(0.612)		(0.521)		(0.597)	
Observations	14977	14942	19980	19958	24917	24901
R^2	0.056	0.033	0.068	0.05	0.085	0.066
Number of idimp	5020	4985	5028	5006	5029	5013
widstat	e(widstat)	90.47	e(widstat)	200.1	e(widstat)	227.7

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
All regressions include Region*Year and Bank*Year fixed effects.

Table 5: Total loans

	OLS	IV	OLS	IV	OLS	IV
Dep. variable			Total loans			
Treated 1 year	0.173***	-0.012				
	(0.033)	(0.110)				
Treated 2 years			0.141***	-0.037		
			(0.035)	(0.095)		
Treated 3 years					0.123***	-0.066
					(0.037)	(0.092)
Turnover (t-1)	0.031*	0.031*	0.073***	0.073***	0.079***	0.079***
	(0.018)	(0.017)	(0.016)	(0.014)	(0.014)	(0.012)
Total assets (t-1)	0.301***	0.301***	0.304***	0.305***	0.372***	0.372***
	(0.029)	(0.029)	(0.019)	(0.019)	(0.019)	(0.017)
Low rating (t-1)	0.02	0.02	-0.073***	-0.075***	-0.102***	-0.104***
	(0.024)	(0.024)	(0.019)	(0.019)	(0.017)	(0.016)
Medium rating (t-1)	-0.007	-0.008	-0.033***	-0.033***	-0.042***	-0.043***
	(0.010)	(0.010)	(0.008)	(0.008)	(0.008)	(0.007)
Nr of banks (t-1)	0.107***	0.107***	0.159***	0.159***	0.180***	0.180***
	(0.018)	(0.018)	(0.015)	(0.014)	(0.014)	(0.013)
Firm age	-0.044	-0.04	0.058	0.059	0.108*	0.113*
	(0.091)	(0.089)	(0.082)	(0.083)	(0.065)	(0.060)
Constant	11.247***		10.570***		9.642***	
	(0.349)		(0.294)		(0.245)	
Observations	14977	14942	19980	19958	24917	24901
R^2	0.116	0.044	0.133	0.075	0.159	0.105
Number of idimp	5020	4985	5028	5006	5029	5013
widstat	e(widstat)	90.47	e(widstat)	200.1	e(widstat)	227.7

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
All regressions include Region*Year and Bank*Year fixed effects.

Table 6: Interest rate

	OLS	IV	OLS	IV
Dep. variable	Interest rate			
Treated 2 years	-0.367***	-0.473*		
	(0.080)	(0.258)		
Treated 3 years			-0.424***	-0.582**
			(0.080)	(0.236)
Turnover (t-1)	-0.079**	-0.079**	-0.029	-0.029
	(0.035)	(0.036)	(0.033)	(0.029)
Total assets (t-1)	-0.07	-0.07	-0.102**	-0.102***
	(0.051)	(0.049)	(0.050)	(0.037)
Low rating (t-1)	-0.181***	-0.183***	-0.227***	-0.229***
	(0.060)	(0.058)	(0.057)	(0.049)
Medium rating (t-1)	-0.091***	-0.092***	-0.106***	-0.107***
	(0.028)	(0.027)	(0.028)	(0.024)
Nr of banks (t-1)	-0.041	-0.041	-0.058	-0.058*
	(0.041)	(0.041)	(0.038)	(0.034)
Firm age	0.097	0.098	-0.226	-0.225
	(0.125)	(0.222)	(0.323)	(0.300)
ROA (t-1)	0.363	0.367	0.033	0.035
	(0.250)	(0.230)	(0.250)	(0.216)
Constant	4.917***		5.604***	
	-0.556		-1.048	
Observations	8757	8608	10981	10871
R^2	0.719	0.007	0.724	0.008
Number of idimp	2533	2384	2574	2464
widstat	e(widstat)	125	e(widstat)	153.4

Robust standard errors in parentheses

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

All regressions include Region*Year and Bank*Year fixed effects.

Table 7: Bad loans

	OLS	IV	OLS	IV	OLS	IV
Dep. variable			Bad Loan dummy			
Treated 1 year	0.01	0.02				
	(0.008)	(0.016)				
Treated 2 years			0.01	0.026		
			(0.006)	(0.019)		
Treated 3 years					0.011	0.044*
					(0.008)	(0.024)
Turnover (t-1)	-0.002	-0.002	-0.009***	-0.009***	-0.012***	-0.012***
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)
Total assets (t-1)	-0.004	-0.004	-0.002	-0.002	-0.009**	-0.009**
	(0.006)	(0.006)	(0.003)	(0.003)	(0.004)	(0.004)
Low rating (t-1)	0.002	0.002	-0.006**	-0.006**	-0.014***	-0.014***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Medium rating (t-1)	0.003	0.003	-0.005**	-0.005**	-0.011***	-0.010***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
Leverage (t-1)	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Nr of banks (t-1)	-0.002	-0.002	0	0	-0.004	-0.004
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Firm age	-0.001	-0.001	-0.001	-0.001	-0.002	-0.002
	(0.001)	(0.001)	(0.002)	(0.002)	(0.004)	(0.004)
Constant	0.058		0.116***		0.224***	
	(0.049)		(0.035)		(0.041)	
Observations	9956	9934	14940	14930	19868	19851
R^2	0.006	0.002	0.013	0.003	0.026	0.006
Number of idimp	4989	4967	4995	4985	5007	4990
widstat	e(widstat)	68.54	e(widstat)	90.83	e(widstat)	87.31

Robust standard errors in parentheses

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

All regressions include Region*Year and Bank*Year fixed effects.

Table 8: Turnover

	OLS	IV	OLS	IV
Dep. variable	Firm turnover			
Treated 1 year	-0.005	0.074		
	(0.035)	(0.085)		
Treated 2 years			-0.024	-0.075
			(0.041)	(0.078)
Low rating (t-1)	-0.022	-0.022	0.019	0.019
	(0.018)	(0.019)	(0.017)	(0.016)
Medium rating (t-1)	-0.015	-0.015	0.013	0.013
	(0.010)	(0.010)	(0.010)	(0.009)
Nr of banks (t-1)	0.092***	0.091***	0.111***	0.111***
	(0.014)	(0.013)	(0.014)	(0.013)
Firm age	0.008	0.008	0.04	0.04
	(0.076)	(0.070)	(0.068)	(0.053)
Constant	7.735***		7.515***	
	(0.229)		(0.207)	
Observations	14664	14576	19515	19452
R^2	0.024	0.006	0.099	0.007
Number of idimp	4973	4885	4991	4928
widstat	e(widstat)	112.2	e(widstat)	251.2

Robust standard errors in parentheses

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

All regressions include Region*Year and Bank*Year fixed effects.

Table 9: Trade debts

	OLS	IV	OLS	IV
Dep. variable	Trade debts			
Treated 1 year	-0.013	-0.02		
	(0.038)	(0.137)		
Treated 2 years			-0.019	-0.084
			(0.048)	(0.123)
Low rating (t-1)	-0.023	-0.023	-0.039*	-0.039*
	(0.026)	(0.026)	(0.022)	(0.021)
Medium rating (t-1)	-0.026*	-0.026*	-0.016	-0.016
	(0.014)	(0.013)	(0.012)	(0.011)
Nr of banks (t-1)	0.061***	0.061***	0.091***	0.091***
	(0.021)	(0.020)	(0.017)	(0.016)
Firm age	-0.008	-0.008	0.013	0.014
	(0.058)	(0.076)	(0.059)	(0.072)
Constant	6.453***		6.274***	
	(0.182)		(0.181)	
Observations	12848	12782	17105	17104
R^2	0.01	0.002	0.03	0.004
Number of idimp	4441	4375	4442	4441
widstat	e(widstat)	82.79	e(widstat)	160.6

Robust standard errors in parentheses

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

All regressions include Region*Year and Bank*Year fixed effects.

Table 10: Investments

	OLS	IV	OLS	IV
Dep. variable		Firm investments		
Treated 1 year	0.093**	0.174		
	(0.037)	-0.113		
Treated 2 years			0.034	0.049
			-0.026	-0.074
Low rating (t-1)	0.042**	0.042**	0.008	0.008
	-0.019	-0.019	-0.012	-0.012
Medium rating (t-1)	0.015	0.015	0.003	0.003
	-0.009	-0.009	-0.006	-0.007
Nr of banks (t-1)	-0.022*	-0.023*	-0.017**	-0.017*
	-0.013	-0.013	-0.009	-0.009
Firm age	0.092	0.09	0.058	0.058
	-0.072	(0.078)	-0.063	-0.066
Constant	-0.221		-0.138	
	-0.217		-0.19	
Observations	12371	12364	16598	16597
R^2	0.064	0.002	0.081	0.001
Number of idimp	4438	4431	4439	4438
widstat	e(widstat)	64.91	e(widstat)	133.9

Robust standard errors in parentheses

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

All regressions include Region*Year and Bank*Year fixed effects.

Table 11: Long term loans - falsification test

	OLS	OLS	OLS
Dep. variable		Long term loans	
IV 1 year	0.167		
	(0.173)		
IV 2 year		0.07	
		(0.170)	
IV 3 year			0.059
			(0.183)
Turnover (t-1)	0.021	0.052***	0.049***
	(0.023)	(0.019)	(0.017)
Total assets (t-1)	0.361***	0.369***	0.436***
	(0.038)	(0.025)	(0.025)
Low rating (t-1)	0.019	-0.075***	-0.083***
	(0.033)	(0.027)	(0.024)
Medium rating (t-1)	-0.015	-0.033***	-0.035***
	(0.016)	(0.013)	(0.012)
Nr of banks (t-1)	0.090***	0.137***	0.149***
	(0.023)	(0.019)	(0.018)
Firm age	0.123	0.21	0.390**
	(0.184)	(0.161)	(0.191)
Constant	9.828***	9.247***	8.040***
	(0.617)	(0.523)	(0.602)
Observations	14566	19434	24237
R^2	0.049	0.064	0.083
Number of idimp	4883	4891	4892

Robust standard errors in parentheses

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

All regressions include Region*Year and Bank*Year fixed effects.

Table 12: Total loans - falsification test

	OLS	OLS	OLS
Dep. variable		Total loans	
IV 1 year	-0.089		
	(0.118)		
IV 2 year		-0.092	
		(0.115)	
IV 3 year			-0.099
			(0.122)
Turnover (t-1)	0.035**	0.076***	0.081***
	(0.018)	(0.016)	(0.014)
Total assets (t-1)	0.298***	0.302***	0.369***
	(0.029)	(0.019)	(0.020)
Low rating (t-1)	0.019	-0.077***	-0.109***
	(0.024)	(0.020)	(0.017)
Medium rating (t-1)	-0.008	-0.033***	-0.044***
	(0.010)	(0.009)	(0.008)
Nr of banks (t-1)	0.110***	0.163***	0.182***
	(0.018)	(0.015)	(0.014)
Firm age	-0.048	0.055	0.115*
	(0.090)	(0.083)	(0.066)
Constant	11.200***	10.551***	9.624***
	(0.348)	(0.295)	(0.246)
Observations	14566	19434	24237
R^2	0.111	0.131	0.158
Number of idimp	4883	4891	4892

Robust standard errors in parentheses

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

All regressions include Region*Year and Bank*Year fixed effects.

Table 13: Interest rate - falsification test

	OLS	OLS
Dep. variable	Interest rate	
IV 2 year	-0.365	
	(0.338)	
IV 3 year		-0.598
		(0.401)
Turnover (t-1)	-0.079*	-0.029
	(0.043)	(0.042)
Total assets (t-1)	-0.083	-0.124**
	(0.063)	(0.063)
Low rating (t-1)	-0.141**	-0.212***
	(0.071)	(0.069)
Medium rating (t-1)	-0.069**	-0.100***
	(0.034)	(0.033)
Nr of banks (t-1)	-0.055	-0.066
	(0.046)	(0.044)
Firm age	-0.001	0.075
	(0.068)	(0.103)
ROA (t-1)	0.035	-0.06
	(0.304)	(0.293)
Constant	5.997***	4.946***
	(0.559)	(0.616)
Observations	6066	7582
R^2	0.734	0.738
Number of idimp	1518	1518

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
All regressions include Region*Year and Bank*Year fixed effects.

Table 14: Bad loans - falsification test

	OLS	OLS	OLS
Dep. variable		Bad Loan dummy	
IV 1 year	0.008		
	(0.014)		
IV 2 year		0.01	
		(0.016)	
IV 3 year			0.024
			(0.019)
Turnover (t-1)	-0.002	-0.010***	-0.013***
	(0.003)	(0.004)	(0.003)
Total assets (t-1)	-0.004	-0.002	-0.009**
	(0.006)	(0.003)	(0.004)
Low rating (t-1)	0.002	-0.005**	-0.014***
	(0.002)	(0.003)	(0.003)
Medium rating (t-1)	0.003	-0.004*	-0.010***
	(0.002)	(0.002)	(0.003)
Leverage (t-1)	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Nr of banks (t-1)	-0.001	0.001	-0.004
	(0.001)	(0.003)	(0.003)
Firm age	0	-0.001	-0.001
	(0.001)	(0.002)	(0.004)
Constant	0.053	0.113***	0.221***
	(0.050)	(0.035)	(0.041)
Observations	9683	14532	19326
R^2	0.006	0.012	0.026
Number of idimp	4852	4858	4870

Robust standard errors in parentheses

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

All regressions include Region*Year and Bank*Year fixed effects.

Table 15: Turnover - falsification test

	OLS	OLS
Dep. variable	Turnover	
IV 1 year	0.027	
	(0.112)	
IV 2 year		-0.158
		(0.127)
Low rating (t-1)	-0.024	0.017
	(0.019)	(0.017)
Medium rating (t-1)	-0.017*	0.01
	(0.010)	(0.010)
Nr of banks (t-1)	0.093***	0.111***
	(0.014)	(0.014)
Firm age	0.011	0.04
	(0.076)	(0.069)
Constant	7.697***	7.513***
	(0.230)	(0.209)
Observations	14263	18982
R^2	0.025	0.099
Number of idimp	4836	4854

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
All regressions include Region*Year and Bank*Year fixed effects.

Table 16: Investments - falsification test

	OLS	OLS
Dep. variable		Investments
IV 1 year	0.071	
	(0.121)	
IV 2 year		0.008
		(0.077)
Low rating (t-1)	0.039**	0.005
	(0.020)	(0.012)
Medium rating (t-1)	0.012	0.001
	(0.009)	(0.007)
Nr of banks (t-1)	-0.022	-0.017*
	(0.014)	(0.009)
Firm age	0.097	0.058
	(0.072)	(0.063)
Constant	-0.224	-0.131
	(0.218)	(0.190)
Observations	12040	16152
R^2	0.063	0.08
Number of idimp	4318	4319

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
All regressions include Region*Year and Bank*Year fixed effects.

Table 17: Trade debts - falsification test

	OLS	OLS
Dep. variable	Trade debts	
IV 1 year	-0.033	
	(0.181)	
IV 2 year		-0.113
		(0.175)
Low rating (t-1)	-0.023	-0.036*
	(0.026)	(0.021)
Medium rating (t-1)	-0.024*	-0.015
	(0.014)	(0.012)
Nr of banks (t-1)	0.061***	0.092***
	(0.021)	(0.017)
Firm age	-0.007	0.012
	(0.059)	(0.060)
Constant	6.423***	6.173***
	(0.182)	(0.184)
Observations	12530	16685
R^2	0.01	0.03
Number of idimp	4330	4331

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
All regressions include Region*Year and Bank*Year fixed effects.

Table 18: Total loans - falsification test II (adjiacent region)

	OLS	OLS	OLS
Dep. variable		Total loans	
IV 1 year	-0.028		
	(0.018)		
IV 2 year		-0.026	
		(0.017)	
IV 3 year			-0.030
			(0.019)
Turnover (t-1)	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)
Total assets (t-1)	0.216***	0.260***	0.341***
	(0.024)	(0.019)	(0.019)
Low rating (t-1)	0.027	-0.057***	-0.088***
	(0.027)	(0.021)	(0.019)
Medium rating (t-1)	-0.012	-0.028***	-0.034***
	(0.011)	(0.009)	(0.009)
Nr of banks (t-1)	0.050***	0.097***	0.121***
	(0.017)	(0.016)	(0.014)
Firm age	-0.201***	-0.019	-0.011
	(0.021)	(0.025)	(0.042)
Constant	12.715***	11.793***	10.979***
	(0.300)	(0.261)	(0.253)
Observations	10887	13837	16926
R^2	0.071	0.090	0.119
Number of idimp	3919	3919	3938

Robust standard errors in parentheses

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

All regressions include Region*Year and Bank*Year fixed effects.

Table 19: Long term loans - falsification test II (adjiacent region)

	OLS	OLS	OLS
Dep. variable		Long term loans	
IV 1 year	-0.005		
	(0.024)		
IV 2 year		-0.009	
		(0.023)	
IV 3 year			-0.010
			(0.024)
Turnover (t-1)	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)
Total assets (t-1)	0.182***	0.247***	0.332***
	(0.029)	(0.022)	(0.022)
Low rating (t-1)	0.053*	-0.026	-0.047**
	(0.032)	(0.025)	(0.022)
Medium rating (t-1)	-0.008	-0.013	-0.019*
	(0.014)	(0.012)	(0.011)
Nr of banks (t-1)	0.043**	0.079***	0.087***
	(0.020)	(0.018)	(0.016)
Firm age	-0.181***	-0.007	0.050
	(0.067)	(0.084)	(0.081)
Constant	12.496***	11.468***	10.544***
	(0.370)	(0.362)	(0.333)
Observations	10887	13837	16926
R^2	0.030	0.042	0.060
Number of idimp	3919	3919	3938

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
All regressions include Region*Year and Bank*Year fixed effects.

Table 20: Interest rate - falsification test II (adjiacent region)

	OLS	OLS	OLS
Dep. variable		Interest rate	
IV 1 year	-0.017		
	(0.031)		
IV 2 year		-0.047	
		(0.045)	
IV 3 year			-0.045
			(0.047)
Turnover (t-1)	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Total assets (t-1)	-0.012	-0.133**	-0.148***
	(0.046)	(0.056)	(0.048)
Low rating (t-1)	-0.079*	-0.125*	-0.203***
	(0.045)	(0.065)	(0.062)
Medium rating (t-1)	-0.023	-0.031	-0.078***
	(0.021)	(0.029)	(0.026)
Nr of banks (t-1)	0.009	-0.048	-0.011
	(0.030)	(0.038)	(0.037)
Firm age	-0.174***	0.119	0.031
	(0.051)	(0.148)	(0.187)
Leverage (t-1)	-0.037**	-0.048	0.031
	(0.016)	(0.039)	(0.019)
Constant	5.156***	6.490***	4.418***
	(0.364)	(0.584)	(0.672)
Observations	7758	9070	10967
R^2	0.745	0.723	0.740
Number of idimp	3315	3358	3481

Robust standard errors in parentheses

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

All regressions include Region*Year and Bank*Year fixed effects.

Table 21: Bad loans - falsification test II (adjiacent region)

	(1)	(2)	(3)
Dep. variable		Bad loan dummy	
IV 1 year	0.005		
	(0.005)		
IV 2 year		0.003	
		(0.005)	
IV 3 year			0.003
			(0.005)
Turnover (t-1)	0.000	-0.002	-0.002
	(0.002)	(0.002)	(0.002)
Total assets (t-1)	-0.004	0.002	0.002
	(0.003)	(0.005)	(0.005)
Low rating (t-1)	-0.004	0.001	0.001
	(0.003)	(0.005)	(0.005)
Medium rating (t-1)	-0.003	-0.002	-0.002
	(0.003)	(0.002)	(0.002)
Leverage (t-1)	-0.004	0.000	0.000
	(0.005)	(0.003)	(0.003)
Nr of banks (t-1)	-0.002	0.001	0.001
	(0.004)	(0.003)	(0.003)
Firm age	0.000	-0.001	-0.001
	(0.001)	(0.001)	(0.001)
Constant	0.047*	0.009	0.009
	(0.024)	(0.034)	(0.034)
Observations	7756	9068	9068
R^2	0.008	0.007	0.007
Number of idimp	3315	3358	3358

Robust standard errors in parentheses

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

All regressions include Region*Year and Bank*Year fixed effects.

Table 22: Turnover - falsification test II (adjiacent region)

Dep. variable	(1) turnover	(2) turnover
IV 1 year	-0.014	
	(0.021)	
IV 2 year		0.000
		(0.022)
Low rating (t-1)	-0.011	0.004
	(0.034)	(0.035)
Medium rating (t-1)	-0.003	0.004
	(0.019)	(0.019)
Nr of banks (t-1)	0.087***	0.119***
	(0.028)	(0.027)
Firm age	-0.046	0.009
	(0.054)	(0.064)
Constant	7.623***	7.211***
	(0.306)	(0.395)
Observations	7602	8888
R^2	0.023	0.069
Number of idimp	3272	3316

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
All regressions include Region*Year and Bank*Year fixed effects.

Table 23: Commercial debts - falsification test II (adjiacent region)

	(1)	(2)
Dep. variable	Commercial debts over assets	Commercial debts over assets
IV 1 year	0.001	
	(0.005)	
IV 2 year		0.003
		(0.005)
Low rating (t-1)	0.004	-0.008
	(0.007)	(0.006)
Medium rating (t-1)	0.002	-0.002
	(0.004)	(0.003)
Nr of banks (t-1)	0.006	-0.000
	(0.006)	(0.005)
Firm age	-0.021***	-0.012*
	(0.008)	(0.006)
Constant	0.327***	0.258***
	(0.046)	(0.042)
Observations	6888	8033
R^2	0.096	0.118
Number of idimp	3053	3106

Robust standard errors in parentheses

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

All regressions include Region*Year and Bank*Year fixed effects.

Table 24: Investments - falsification test II (adjiacent region)

	(1)	(2)
Dep. variable	Investments	Investments
IV 1 year	-0.029	
	(0.044)	
IV 2 year		-0.019
		(0.035)
Low rating (t-1)	0.114**	0.041
	(0.054)	(0.041)
Medium rating (t-1)	0.013	-0.004
	(0.027)	(0.022)
Nr of banks (t-1)	-0.048	-0.025
	(0.035)	(0.032)
Firm age	-0.742	-0.715
	(0.604)	(0.553)
Constant	3.270*	4.521**
	(1.927)	(2.006)
Observations	7408	8648
R^2	0.061	0.066
Number of idimp	3194	3237

Robust standard errors in parentheses

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

All regressions include Region*Year and Bank*Year fixed effects.

Table 25: matching-DID estimates

Dep. variable	(1) Tot. loans	(2) Short t. loans	(3) Int. rate	(4) Bad loans	(5) Turnover	(6) Tot. assets
treated	0.072	-0.049	0.018	-0.001	0.124	-0.089
	(0.104)	(0.142)	(0.076)	(0.003)	(0.105)	(0.205)
post	0.067**	-0.039	-1.273***	0.010*	-0.054**	0.314***
	(0.033)	(0.055)	(0.079)	(0.006)	(0.024)	(0.076)
treated*post	0.080	0.291***	-0.240**	0.012	0.006	0.154
	(0.057)	(0.086)	(0.111)	(0.012)	(0.045)	(0.113)
Constant	15.098***	14.024***	5.209***	-0.007**	9.040***	7.693***
	(0.162)	(0.243)	(0.242)	(0.003)	(0.185)	(0.604)
Observations	1894	1894	1651	1894	1518	1511
R^2	0.123	0.114	0.215	0.038	0.200	0.198

Robust standard errors in parentheses

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

All regressions include Region*Year and Bank*Year fixed effects.