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EVIDENCE OF CREDIT CONSTRAINTS FROM AN INVESTMENT INCENTIVES PROGRAM⁽⁰⁾

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Abstract

To determine whether firms are credit constrained we analyze how they react to the availability of capital grants: constrained firms will use grants to expand investment, while unconstrained firms will use them as a substitute for other borrowing. This idea is applied to a sample of Italian firms that requested the financial assistance provided by an investment incentives program. By comparing the investment and debt performances of subsidized firms with that of firms whose applications were rejected, we show that there is only weak evidence of limits to borrowing.

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1. Introduction

Although economists regard credit constraints (see for instance De Meza and Webb, 1987; Keeton, 1979; and Stiglitz and Weiss, 1981) as a key real-world phenomenon,¹ it has been quite difficult to provide compelling evidence on the existence of limits to firms' borrowing.

A firm is credit constrained if it cannot borrow as much as it would like at the existing market rate. How does one go about finding support for this contention? The strategy widely adopted in the literature (Fazzari, Hubbard and Petersen, 1988) is to study the effects of close substitutes for credit. If there were no credit constraints, greater access to credit substitutes would be irrelevant for investment choices. Unfortunately, there is a crucial limitation to the use of this strategy. As Kaplan and Zingales (1997) show, access to credit substitutes is likely to be correlated with other characteristics of the firms, which affect their investment decisions. To solve this problem, recent papers have exploited the availability of substitutes for credit that can be reasonably considered exogenous; that is, they contain no information on the prospects of the firm. Natural candidates for this role are shocks to substitutes for credit driven by policy variations, such as those deriving from changes in eligibility for concessional loan programs (see Banerjee and Duflo, 2004; and Zia, 2006).² However, using policy variations to proxy for exogenous variations is not an easy task. To identify the effect of the availability of substitutes for credit one should compare the performance of a firm exposed to the policy change with that of an unexposed firm, which should be very similar to the former prior to the policy variation.³ Therefore, for estimation purposes the policy change should be as random (and inequitable!) as possible, treating similar firms dissimilarly. Clearly, this is very difficult to come across, as changes in eligibility usually apply to a whole group of recipients (such as a sector or a size group) and do not discriminate among them.

This paper deals with the above shortcoming by following a novel identification strategy. It proposes a test based on the behavior of the firms that requested a capital grant under an investment incentives program carried out by the Italian government (Law 488/92).⁴ We take advantage of the mechanism used to allocate the incentives. Grants are awarded through competitive calls for projects according to predetermined criteria, such as the proportion of firms' equity invested in the project, the number of jobs involved, and the proportion of assistance sought, which result in a single firm ranking. By adopting a difference-in-

¹ For instance, Banerjee and Newman (1993) and Galor and Zeira (1993) show the role of credit constraints in economic development, while Bernanke and Gertler (1989) and Kiyotaki and Moore (1997) highlight their impact on the business cycle.

² Predecessors are Rosenzweig and Wolpin (1993), who take advantage of weather-induced variations in liquidity for Indian farmers, and Lamont (1997), who uses the availability of cash flow driven by oil-price shocks to look at non-oil investment of oil companies.

³ This represents the real-world counterpart of the ideal experiment, which is comparing the performance of a subsidized firm with that of the same firm had it not been financed.

⁴ Italy offers an ideal setting for testing the role of credit constraints. Certain sources of finance are mostly unavailable to firms, as the stock and bond markets are poorly developed. Moreover, the industrial structure is tilted towards small firms, which are traditionally considered more subject to financial constraints (see Bianco, 1997; and Guiso, 2006).

differences framework, we compare the performance of subsidized firms with that of firms whose applications were rejected, that is firms that applied for the grants but were not financed as they scored low in the ranking. To the extent that the selection bias is similar for subsidized and rejected firms, our strategy allows it to be differentiated out.

As working hypothesis, we start by assuming that the two groups can be considered random draws. Then we refine our empirical strategy in three respects. First, we implement an intuitive version of the regression discontinuity design (Campbell, 1969) and contrast financed firms just above the financing threshold in the ranking with non-financed firms just below that threshold. The idea here is that whatever the actual degree of randomness in the award mechanism, it is more likely that the correct counterfactual will be provided by the untreated firms that have scores similar to the treated ones. Second, to assess the size of the potential bias due to the failure of the parallel trend hypothesis (Blundell et al., 2004), we construct a comparison group that mirrors the time-series pattern of the treated group before the program took place. This group comprises firms whose deviation with respect to the growth rates of the outcome variables of the treated firms is minimized. Finally, to make sure that what we observe is truly driven by the availability of the grants (and not by the selection due to the award scheme) we run a placebo experiment. This experiment uses only firms that are ranked below the financing threshold, and thus are not financed. It compares rejected-application firms with a high ranking with rejected-application firms with a low ranking. Should the outcome-variable dynamics be the result of sample selection, we would find placebo results that mirror those obtained by comparing subsidized and rejected-application firms.

To gauge the role of credit constraints, we look at firm investment and debt reactions to the availability of substitutes for credit. *Credit constrained* firms will use grants to expand investment (without grants it would not have been undertaken), while *unconstrained* firms will use them as a substitute for other borrowing (since investment is already at the equilibrium level, the firm wants to substitute a costly source of finance with an inexpensive one). This simple logic, however, is subject to two qualifications, which can be assessed empirically. First, our test requires the ongoing interest rate not to decline as a result of the availability of the grants. Second, it requires the schedule of marginal productivity of capital not to shift consequently upon the receipt of public money.

Turning to our results, we find only weak evidence of borrowing limits. Our analysis refers to the period 1995-2002 and focuses on the second and third calls for projects under Law 488. In the case of the second call, we find evidence that financed firms reduced their debts, while they did not significantly increase their investment. By examining the yearly dynamics of investment and debt, we uncover that financed firms brought forward investment projects originally planned for the post-intervention period to take advantage of the incentives. In the years following the program, the investment activity of the financed firms slowed down significantly; at that time, they reduced their liabilities as well. Overall, these findings provide strong

evidence against the hypothesis of credit constraints. For the third call, the evidence is more in favor of the existence of credit constraints, as financed firms increased their investment and there is no evidence of debt reduction. However, we also find that as a result of the program the productivity of the subsidized firms deteriorated. This suggests that these firms undertook infra-marginal investment projects that were only made profitable by the availability of the grants, and thus estimated patterns of investment and debt cannot be interpreted as evidence of the existence of credit constraints.

The paper is structured as follows. We start in Section 2 with a description of Law 488. Section 3 describes the economic rationale of our test. Section 4 presents the data and describes the empirical design. The empirical findings are the focus of Section 5. Finally, Section 6 offers some concluding remarks.

2. The policy experiment

To identify the role of credit constraints we exploit the finance available as a result of an investment incentives program carried out by the Italian government: Law 488/92. This section explains the main features of the policy experiment (see *Gazzetta Ufficiale della Repubblica Italiana no. 299, 21 Dicembre 1992*). More details can be found in IPI (2002) and Bronzini et al. (2005).

Assistance under Law 488 takes the form of project-related capital grants. There is no entitlement to assistance: applications are ranked on the basis of specific criteria (see below) and award offers are only made if funding is available. Incentives are restricted to areas designated Objective 1, 2 or 5b⁵ for the purpose of EU Structural Funds, together with some areas that do not qualify for Structural Fund support but have been approved by the European Commission under Article 92(3)c. Generally speaking, they correspond to the south of Italy and to selected areas of central and northern Italy.⁶ Only manufacturing and mining and quarrying firms are eligible for assistance.⁷ The law covers a large range of projects.⁸

⁵ Objective 1 refers to the regions suffering general underdevelopment, i.e. having GDP per capita less than 75 percent of the EU average. Objective 2 regions have a concentration of declining industries, reflected in higher average unemployment, higher dependency on industrial employment and observable job losses in specific industries. Objective 5b includes predominantly peripheral rural regions, as reflected in a high share of agricultural employment and low level of agricultural income.

⁶ Objective 1 corresponds to seven regions in the south of Italy, Abruzzo having lost its Objective 1 status at the end of 1996. The Objective 2 and 5b areas are all located in the centre and north of the country and in Abruzzo, as are the areas approved under Article 92(3)c that are not eligible for Structural Funding. Assisted area coverage amounts to 48.9 percent of the national population.

⁷ In addition, selected producer services are also eligible. However, they are not included in the evaluation analysis in the text.

⁸ The investment projects covered are the following: setting-up, extension (defined as a project that increases the capacity of the firm to produce its existing products or enable new products), modernization (investment in innovation that increases productivity and/or improves working conditions or the environment), restructuring (reorganization and technological renewal), reconversion (adaptation of existing production facilities in order to manufacture different products), re-activation (take-over of unused production facilities by persons previously involved in the management of the firm) and relocation (eligible only in cases where a transfer of the production facility is required by national or local authorities).

Law 488 provides for maximum award rates, which depend on both the region where the investment is located and the size of the firm.⁹ The maximum award rates differ from the actual award rates offered because, as shown below, the selection mechanism favors firms that request lower rates.

Award offers are made on the basis of competitive calls for projects. Applications are ranked by eligible area on the basis of the following five criteria: (1) the proportion of own funds invested in the project in relation to total investment; (2) the number of jobs involved in the project in relation to the total investment; (3) the value of assistance sought as a proportion of the maximum award rate applicable to the project; (4) a score related to the priorities of the region in relation to location, project type and sector; (5) a score related to the environmental impact of the project.¹⁰ The five criteria carry equal weight: the values related to each criteria are normalized to produce a single score that determines the project's place in the regional ranking. Assistance is awarded in order of merit to the extent allowed by the budget allocated to the area. If the application is successful, the rate of award offered is the rate requested in the application.

Law 488 calls are issued on a yearly basis. Four Law 488 calls were concluded before 2002, the last year for which financial-statement data are available (see Section 4). Assistance is administered by the Italian Ministry of Industry. The timing of the assistance is precisely defined. Applications are submitted within a specific deadline. Within four months of the deadline, the Ministry of Industry publishes the rankings. The law requires that firms awarded assistance receive the first annual installment within two months.¹¹ The amounts awarded are paid out in three equal installments (two if the project is completed within 24 months). The second and third installments are paid on the same date in subsequent years. One important aspect of Law 488 must be kept in mind: the scheme requires financed firms to undertake some investment activity to validate the receipt of the money. In particular, the second and third installments are contingent on two-thirds and the whole of the investments being realized.

For the purpose of our evaluation exercise, the estimation results we present below are based on the assumption that there are no other governmental programs correlated with the allocation of Law 488 funding. For instance, if the untreated firms receive other types of financial assistance outside the Law 488 scheme, then the comparison with the treated group will underestimate the effect of the program. A feature of Law 488 minimizes the scope of this bias: financing under the program cannot be combined with other sources of public funding. In particular, firms applying for Law 488 money have to give up other public subsidies.

⁹ Maximum rates for SMEs (large firms) range from 50 (50) percent in Objective 1 areas to 20 (10) percent in Article 92(3)c areas outside Objective 2 and 5b. Additional endowments are available for SMEs in Objective 1 and Objective 2 and 5b outside Article 92(3)c.

¹⁰ Criteria 4 and 5 were introduced starting from the third call for projects that was held in 1998 (see below).

¹¹ As for the timing of the first installment, there have nonetheless been delays. In particular, there was a one-month delay in both the second and third calls. Moreover, as explained in the text, for the fourth call there were substantial and very erratic delays.

Applicant firms are explicitly warned that renouncing other sources of public money can be particularly costly because Law 488 does not give entitlement to assistance. This means that an applicant must give up other financial assistance without any guarantee that it will actually receive the Law 488 grant.

3. The rationale of the empirical test

In this section, we present a simple methodology that allows us to determine whether firms are credit constrained, based on how they react to the availability of capital grants. Constrained firms will use grants to expand investment, while unconstrained firms will use them as a substitute for other borrowing. Consider a firm with the standard production function $Y=F(K)$, where Y is output, K is capital, and $F(\cdot)$ has the usual shape, increasing and concave. Denote the interest rate on external (market or bank) borrowing by r_b . We say that a firm is credit constrained if it wants to borrow more at the ongoing interest rate. What happens when the firm in question gains a grant that covers a fraction of the investment outlays?

Consider first the unconstrained firm. A possible scenario is depicted in Figure 1. The horizontal axis in the figure measures K , while the vertical axis represents output. The downward sloping curve in the figure denotes the marginal product of capital $F'(K)$. We assume that at the rate r_b the firm has unconstrained access to external credit. As a result, it borrows until the point where the marginal product of capital is equal to the interest rate, with a capital outlay that in equilibrium is equal to K_b . Now consider what happens when the firm receives the subsidy S . Total investment is unchanged at K_b . The effect of Law 488 will be to substitute loans by grants. The firm will continue to borrow from external sources, but the borrowed amount is reduced (it is equal to $K_b - S$). This leads us to the following proposition. *If the firm is not credit constrained (i.e. it can borrow as much as it wants at the ongoing interest rate), the availability of grants should always lead to a fall in its external borrowing. Moreover, it will have no effect on total investment.*

We contrast this case with the scenario in Figure 2, where the assumption is that the firm is credit constrained. In the initial situation the firm borrows the maximum possible amount from external sources (K_b). Subsequently, the subsidy S is made available to the firm. This has no effect on external borrowing (since the investment is still less than the firm would like at rate r_b , and therefore total capital expenditure expands to $K_b + S$). This case can be summarized as follows. *If the firm is credit constrained, the availability of investment incentives will lead to an increase in its total investment without any change in market borrowing.*

We now discuss two important qualifications to the simple methodology.

First, the identification strategy outlined above depends crucially on the interest rate on external financing not declining for the firms financed. If, as a result of the availability of the subsidy, the interest rates fall, we

will not be able to observe different outcomes for constrained and unconstrained firms. Unfortunately, the possibility of an inverse relationship between subsidy and interest rates cannot easily be disregarded. Such a relationship can emerge if there is an upward supply curve for bank credit. Moreover, it can also arise if the cost of credit depends on the amounts collateralized, to the extent that the subsidy can be used as collateral. In both cases, an unconstrained firm, which uses the grant to substitute external borrowing, will face a lower interest rate and thus invest more.

Second, our identification strategy assumes that the downward sloping curve of the marginal product of capital will not shift as a result of Law 488 financing. Again, this possibility is not warranted. As explained in Section 2, to improve the chance of getting the grant, applicant firms can choose to undertake projects with higher labor intensity. In short, they can move towards infra-marginal projects, made profitable only because they will be financed by aid. If this is the case, then it will be not appropriate to interpret greater investment activity coupled with unchanged debt as evidence of financing constraints. Refusing to finance an infra-marginal project is not an indication of rationing, since profit-maximizing intermediaries would prefer marginal projects to infra-marginal ones.

Beyond these two qualifications, it should be noted that time-substitution of investment (see Abel, 1982; Auerbach and Hines, 1988; and Adda and Cooper, 2000) can obscure our identification strategy, as a temporary investment subsidy gives firms a strong incentive to invest while it is in effect. Indeed, firms may engage in offsetting behavior when presented with even carefully constructed incentives to alter their investment behavior: medium or long-term effects of finance availability on investment can be different from the short-term effect. Consider a firm that receives the Law 488 grant. Owing to the design of the scheme (see Section 2), the firm has to undertake some investment activity to validate the receipt of the money. This will occur regardless of whether the firm is credit-constrained or not. However, only credit-constrained firms will use the money to undertake additional investment. As for the unconstrained firms, the investment driven by the grant will come at the expense of future investment. In short, after a first increase under the program, a constrained firm is unlikely to experience a subsequent drop in investment and reduction in its external borrowing. By contrast, an unconstrained firm will anticipate investment outlays and substitute grants for other borrowings at the time the investment would have been made had the Law 488 incentives not been in place. We deal with this issue by using a long time series of post-intervention observations.^{12,13}

¹² As explained by Hamermesh (2000), to evaluate the impact of a policy change it is necessary to focus on a sufficiently long time-window. This will allow the agents to make all the adjustments triggered by the policy change itself, so that the evaluation can reflect the long-run impacts of the change. However, in the credit constraint literature, Banerjee and Duflo (2004), Lamont (1997), and Zinman (2002) use exogenous variations in finance availability and compare real activity *just* before and *just* after the change. These papers generally find that firms are indeed financing-constrained, as the impact of exogenous financing is not limited to a liability-side consolidation.

¹³ Note that in expectation of the introduction of Law 488 firms may also have postponed investment projects originally planned for the period before the start of the program. It should be noted that this would not imply a bias for our results.

4. The data and the empirical design

We use the official Law 488 Dataset of the Ministry of Industry. This dataset records all the firms that have applied for the incentive, both financed and non-financed. It provides us with information that is valuable for the evaluation exercise, such as the firm ranking and the timing of the installments. We also make use of the CERVED Dataset, a financial statement dataset that contains information on Italian corporations. The reason for using this dataset is that the Law 488 dataset lacks information on investment and debt, which are our main outcome variables, as well as additional covariates and firm features. There are additional advantages in using these data. First, the CERVED data cover a large proportion of Italian corporations. Second, the dataset extends from 1994 to 2002, allowing us to study the impact of the program over a period that includes pre-intervention as well as post-intervention years. However, there is also a drawback in using the CERVED data: there are frequent misprints regarding the firm identifiers that we use to link CERVED data to the Law 488 Dataset (see Appendix A for details of the loss of financial statements due to misprints).

The time pattern of the linked dataset is described in Figure 3. Four Law 488 calls for projects were held in the period 1994-2002. For these calls the treatment started (with the first installment) and finished (with the third installment) within the time-window provided by the CERVED data. We focus below on the second and third calls. These calls are ideal for our purposes as they occurred roughly at the midpoint of the CERVED time-window, thus providing us with pre- and post-intervention observations. The first call has been excluded because it contained a transitory clause allowing firms not eligible under Law 488 to be financed as well.¹⁴ We have also excluded the fourth call because disbursements were highly irregular.¹⁵

By linking the Law 488 dataset with the CERVED dataset and implementing the restrictions described in Appendix A, we reconstruct *uninterrupted* financial statements from 1994 to 2002 (1995-2002) for 515 (842) firms that participated in the second (third) call. We adhere to common practice and use a two-year window as pre-intervention period. We also select a subsample of small firms (respectively, 257 and 421 for the two calls), defined as those below the median sales. Small firms are traditionally considered more likely to be rationed as they have weaker and more opaque balance sheets. Table 1 shows the main descriptive statistics for the variables used in the paper, distinguished by call for projects. Our main dependent variables are investment and debt as a percentage of (initial period) capital stock.

Our empirical strategy takes advantage of the call mechanism used to allocate the incentives under Law 488. In the baseline (Figure 4.1), we compare the group of financed firms (*treated*) with the group of firms that

Subsidized and rejected-application firms will both act in expectation of the subsidy and therefore the effect will be differentiated away.

¹⁴ These firms received (before the parliamentary approval of Law 488 in 1992) pledges of assistance outside the Law 488 scheme. However, due to public finance problems disbursements were postponed until the mid-1990s when it was decided that they would have been covered by the first call for Law 488 allocations.

applied for the incentives but were not financed because they scored low in the Law 488 ranking (*untreated*). Thus, our comparison group comprises firms with rejected applications. The main virtue of the rejected-application group is that it is very similar to the treatment group in terms of its characteristics: it includes eligible firms that were interested in receiving the grant. As suggested by Brown et al. (1995), the rejected-application firms are hardly a random group of firms, but they may get as close to a control group as is possible.

Clearly, if incentives were randomly assigned among the firms participating in the call, then the firms with rejected applications could be considered statistically equivalent to the financed firms in all respects except treatment status. Although some scholars have argued that the mechanism envisaged is very poor at discriminating among applicant firms, and therefore the degree of randomness in the assignment may well be high,¹⁶ we try to substantiate our findings on empirical grounds. We strengthen our research design by using three additional experiments.

First, we compare only firms that are in the middle of the ranking (Figure 4.2). We implement an intuitive version of the regression discontinuity design and contrast financed firms just above the financing threshold in the Law 488 ranking with non-financed firms just below that threshold (*cutoff neighborhood* sample). The idea here is that whatever the actual degree of randomness in the award mechanism, it is more likely that the correct counterfactual will be provided by the untreated firms that have similar Law 488 scores to the treated ones.

Second, we construct an ad-hoc comparison group that mirrors the time-series pattern of the treated group before the program was launched (Figure 4.3). In particular, systematic differences in levels are not the main concern because they can be controlled for using diff-in-diff methods. However, failure of the parallel trend assumption would invalidate our estimates. Therefore, we use as counterfactual a group comprising firms whose deviation with respect to the growth rates of the outcome variables of the treated firms is minimized.

Third, we run a placebo experiment to make sure that what we observe in the outcome variables is truly driven by the availability of the grants, rather than the selection resulting from the award mechanism. This experiment uses only firms that are ranked below the financing threshold, and therefore are not financed. It compares rejected-application firms with high ranking with rejected-application firms with a low ranking (Figure 4.4). If the outcome-variable dynamics were the result of sample selection, we would find placebo results mirroring those we obtain from comparing subsidized and rejected-application firms.

¹⁵ For instance, although the first call installment was supposed to be received by May 1999, many firms did not receive it until 2000 (the actual timing of receipt is not recorded in the Law 488 dataset).

¹⁶ Scalera and Zazzero (2000) and Del Monte and Giannola (1997) have observed that some of the variables – such as, the share of own capital and the expected employment increase – on which the ranking is based are not under the direct control of firms participating in the call. As a result, the actual allocation of subsidies among the pool of applicant firms may have followed a quasi-random assignment.

We will be running simple regressions of the following form:

$$(1) \quad y_{it} = \alpha \text{Law 488}_i + \beta \text{Post}_t + \gamma (\text{Law 488}_i * \text{Post}_t) + \varepsilon_{it}$$

where y_{it} is an outcome variable, such as investment or debt flow as a percentage of the (initial period) capital stock, for firm i in year t . Law 488_i is a dummy variable indicating whether the firm has received the Law 488 grant, and POST is a dummy equal to 1 for the post-intervention period. The coefficient we are interested in is the γ : the impact of Law 488 on the treated. We will also estimate the impact of the program for each single year of the post-intervention period. In this case, we run the following regression:

$$(2) \quad y_{it} = \alpha \text{Law 488}_i + \sum_t \beta_t \text{Year}_t + \sum_t \gamma_t (\text{Law 488}_i * \text{Yearpost}_t) + \varepsilon_{it}$$

where Year_t denotes time dummies, and Yearpost_t is a series of dummies for each of the years after the introduction of the program. In this case we again focus on the coefficients of the interaction terms. Since the impact is evaluated over time, we will observe several γ_t coefficients corresponding to each year of the post-treatment period.

5. Results

5.1 Baseline

In the baseline experiment we simply contrast the group of financed firms with the group of firms that applied for the incentives but were not financed. The similarity between the two groups can be assessed by looking at the firms' characteristics before the start of the program. In Appendix B, we present mean differences about the main variables of the two groups. The differences are calculated with reference to the first year of the pre-intervention period, which is 1995 for the second call and 1996 for the third call.¹⁷ As for the second call no difference is found to be statistically significant, either for the baseline sample or for the small-firms subsample. Some significant differences, however, arise in the third call, where treated firms display a greater share of own capital over debt and higher profitability; in the small-firms subsample treated firms also appear significantly smaller, as evaluated by sales, than their untreated counterparts.

In Table 2 we present first evidence on the impact of the grants on investment and borrowing. We use the difference-in-differences approach of equations (1) and (2), where the focus is on the changes in physical capital and debt¹⁸ before and after the policy experiment, using the rejected-application group as a control.

¹⁷ In any case, using 1996 and 1997 instead would have made no difference.

¹⁸ Throughout the paper we report the results for the outcome variables defined as investment and debt flow over capital of the initial year of the pre-treatment period, which is 1995 and 1996 respectively for the two calls. As we checked, using investment and debt flow divided by sales and assets, instead of capital, leads to very similar results. Note also

The first column presents results we obtain from collapsing the time series information into a single pre- and post- intervention period,¹⁹ while the remaining columns show year-by-year interaction coefficients. For each auction, we present results for both the full sample and the small-firms subsample. In order to keep the table manageable, we present only the coefficient of the interactions.

In the case of the second call (Panel A and B), we find that over the entire post-intervention period the accumulation of physical capital for treated firms turns out to be not significantly different from that of the untreated firms (the interaction coefficient displays a negative sign but it is not significant at the usual level). On the other hand, the flow of debt displays a statistically significant reduction. For the full sample the change in the debt stock of financed firms during the post-intervention period is about 30 percent below the corresponding change for rejected firms (during the period 1997-2002 the average debt change for unsubsidized firms is 1.96). As to the year-by-year impacts, we find that the accumulation of physical capital for treated firms outperforms that of the untreated group at the time of the first and second installments (1997 and 1998). The interaction coefficients, however, are statistically significant for the full sample only in 1997.²⁰ We also find evidence of time substitution. In 2000, that is one year after the end of the financial assistance granted under the second call, the investment of the treated firms is significantly lower than that of the untreated group. Consistent with the argument of Section 3, the decrease in the stock of debt becomes larger at roughly the same time. Similar results are found for the small-firms sample. Therefore, we can conclude that for the second call our results provide strong evidence against the hypothesis of financial constraint, as unconstrained firms use the grants to substitute for costly sources of finance.²¹

The existence of credit constraints seems to find empirical support in the third call (Panel C and D). We find a positive and statistically significant effect on investment. On the other hand, we fail to find any reduction in the debt pattern of the treated group, as the interaction coefficient displays a positive sign but is not significant. Turning to the yearly effects, we still note evidence of time substitution. The boost in investment under the program (1998, 1999, and 2000) comes at the expense of a subsequent reduction (2001 and 2002). However, the initial increase is larger than the successive decrease, so that the net effect is positive. As explained in Section 3, this evidence would allow us to conclude in favor of the existence of borrowing limits if the qualifications on interest rate and investment efficiency were satisfied. However, as shown in the next section, this is not the case.

that investment is measured net of depreciation as Law 488 subsidizes net investment; however, using gross accumulation rates modifies the results only marginally.

¹⁹As shown by Bertrand et al. (2004), this takes care of the potential inconsistency of the standard errors due to serial correlation.

²⁰ The standard errors reported in the tables allow clustering of the residuals at the regional level. We also run the cluster correction at the sector by region levels, with only minor modifications with respect to what we report in tables.

²¹ The conclusion about the absence of credit constraints depends critically on the availability of a long time-window. For instance, had the time-window available been limited to two years after the policy experiment, as is common in the

The interpretation of our results relies on the identification assumption that there are no omitted time-varying firm effects correlated with the program. We check the robustness of our estimates with the inclusion of a number of covariates at the firm level. We include sales, ROA, a measure of leverage (equity capital as a percentage of debt) and a measure of internal funds (cash flows as a percentage of assets). The results (not presented for the sake of brevity) suggest that the role of omitted time-varying variables is modest. The estimates we obtain are almost indistinguishable from those of the baseline.

Finally, note that one problem with our sample of uninterrupted balance sheets is the survivorship bias. In particular, there could be a differential loss of balance-sheet availability for treated and untreated firms (see Pakes and Ericson, 1998). Suppose that two marginal firms apply for grants and only one gets the subsidy. A possible scenario is that the subsidized firm continues its operations while the unsubsidized one goes out of business. In these circumstances, the estimates from the balanced panel could be biased because the marginal unsubsidized firms, which are more likely to be financially constrained, are no longer included in the comparison sample. To tackle this issue we also reconstruct an *unbalanced panel*. For this sample we do not require the availability of the financial accounts over the entire period. The unbalanced panel includes the firms that have at least one set of pre-intervention and one of post-intervention balance-sheet data. Results from this sample (available upon request) are broadly in line with those presented below, suggesting that attrition is not an issue for our estimates.

5.2 Interest rate and efficiency

To interpret the evidence on investment and debt patterns as a test of credit constraints, two hypotheses must hold. First, the ongoing interest rate should not decline as a result of the availability of the grants. Second, the schedule of marginal productivity of capital should not shift as a consequence of the receipt of public money. We study whether this is indeed the case by estimating analogues of equations (1) and (2), where the outcome variable is taken to be the interest rate (defined as interest costs over total debt) and a measure of efficiency (defined as gross operating margin over value added, an indicator that does not reflect the financial savings due to receipt of the grant).

Results are presented in Table 3. In the case of second call, there seems to be strong evidence that the two hypotheses are satisfied. As a consequence of the program, for financed firms (relative to rejected-application firms) neither the interest rate declines nor efficiency worsens. These results support our identification strategy. On the other hand, for the third call results are less favorable. First, we find that the interaction coefficients for the interest rate are mostly negative (although not significant). Second, and crucially, we find that firm efficiency deteriorates. For the full sample, in the collapsed experiment the interaction term enters with a negative sign and high significance; in the yearly experiment, the interaction

literature, we would have observed an increase in investment coupled with an unchanged debt. Therefore, we would

coefficients always display negative signs (the 1998 coefficient also shows high statistical significance). These effects are confirmed for the small-firms subsample, in which, however, they are measured less precisely. A worsening of efficiency supports the idea that financed firms have moved towards infra-marginal projects, made profitable only because they are financed by public money. Given this upshot, in the rest of the paper we focus mostly on the results from the second call, for which the empirical evidence can be meaningfully interpreted as a test of financial rationing.

5.3 *Neighborhood of the financial cutoff*

In a randomized experiment, treatment and control groups are identical for a large sample. Even in the case of a non-randomized experiment, the closer are the treatment and the control groups the more convincing are the results. In this section, we implement this idea by comparing treated and untreated groups that are supposed to be more similar than their counterparts in the baseline experiment (see Figure 4.1). Recall that the Law 488 scheme envisages that all applicant firms be ranked at the regional level in a decreasing order given by the normalized single score. Funding is allocated starting from the top of the ranking and going down as far as the budget allocated to the region allows. Therefore, for each region there is a threshold level in the ranking. Our approach in this section is to contrast firms that are close to the middle of the ranking. In particular, we compare treated (untreated) firms just above (below) the regional financing threshold in the ranking (see Figure 4.2). By comparing firms displaying similar Law 488 scores but differently treated within the program, we are implementing an intuitive version of the regression discontinuity design (Campbell, 1969). Appendix B shows the differences in observables between the two groups in this experiment.

For each region we first select only the firms that are within the ± 10 th percentile of the firm distribution around the threshold. The choice of the cutoff neighborhood is clearly arbitrary. However, results differ only a little if we adopt different bounds (we tried the following intervals: ± 15 percent, ± 20 percent, and ± 30 percent). The evidence from this experiment is reported in Table 4. Results are in line with the previous findings. For the second call we find a significant negative impact of Law 488 financing on debt flows and a negative but insignificant effect on investment. Evaluated over time, when the program expires the treated firms reduce their investment and borrowing. At the same time, for the third call both investment and debt flow rise (they are, however, measured less precisely than in the baseline).

5.4 *Parallel trend*

We construct a comparison group that mirrors the time-series pattern of the treated group before the program took place. Systematic differences in levels are not the main concern because they can be controlled by using the diff-in-diff method. However, failure of the “parallel trend” identifying assumption will bias diff-in-diff estimates (Blundell et al., 2004). We calculate the average annual rates of growth of investment and debt

have erroneously concluded that firms were financing-constrained!

over capital for treated firms during the pre-intervention years. We then select among the untreated group only the firms that for each single year display an annual rate of growth of investment and debt within the interval $(1 \pm g) * m_t$, where m_t is the average annual rate of growth in the outcome variables for the treated (see Figure 4.3). The results described in Table 5 are based on the assumption that $g=1.5$. We perform additional robustness checks and verify that by altering the values of g the results are only marginally affected.²² As shown in the table, the violation of the parallel trend assumption does not seem to be what drives our results, as the evidence from this experiment mirrors the findings described so far. For the second call the only modifications refer to the statistical significance of the coefficient for debt, which decreases in the time-collapsed specification, and the timing of intertemporal substitution (in 2002 we find a significant investment and debt slump).

5.5 Placebo

The final robustness test is a placebo experiment (Meyer, 1995) to check whether the observed investment and debt dynamics are truly the effects of the availability of grants. Our concern is that as an effect of the award mechanism the money might be channeled to firms that cannot be considered statistically equivalent to the rejected-application group in all respects except treatment status. For instance, if the money goes to the most efficient firms, then it should come as no surprise that we find only weak evidence of credit constraints.

We proceed as follows. For each regional ranking, we take only rejected applicants and split this group in two at its median score. Then, we compare rejected-application firms with high ranking with rejected-application firms with low ranking. If the observed investment and debt dynamics were the result of sample selection, rather than Law 488 financing, we would find placebo results that mirror those we obtain from comparing subsidized and rejected-application firms. The evidence from the placebo experiment is reported in Table 6. It is encouraging evidence, as we find no effect whatsoever of Law 488 financing on the outcomes. We conclude that it is very unlikely our evidence on credit constraints is biased by the selection due to the award mechanism.

6. Concluding remarks

Understanding whether firms need further financial resources beyond those provided by private lenders is a critical task. In Italy, a long-standing discussion has focused on the role of the financial system. For example, it has been argued (see, for instance, Angelini and Generale, 2005) that a lack of funding might be one of the reasons behind the reduced size and technological backwardness of Italian firms.

To check empirically whether Italian firms are financially rationed, in this paper we propose a test based on firm investment and debt reactions to the availability of substitutes for credit granted under an investment

²² We tried $g=1.2$, $g=1.8$, and $g=2$.

incentives program. Our empirical strategy takes advantage of the mechanism used to allocate the aid. We are able to contrast the performance of subsidized firms with that of firms with rejected applications. Our results are corroborated by a number of robustness checks designed to show that the selection bias has a limited role. Overall, we find that the evidence of the existence of credit constraints is weak.

Important questions remain, however, and will be tackled by future research.

Our results show that while for the second call under Law 488 there is no support for credit constraints, firms financed under the third call have undertaken infra-marginal investment projects, made profitable only by the availability of the grants. How is it that a similar program produces such different results? We offer two possible explanations. First, a possible scenario is that the introduction of Law 488 required a learning phase. In contrast with the more productive and better informed firms that participated in the initial calls, less efficient firms only gradually uncovered the possibilities offered by the program. Second, it should be underscored that the results from the two calls cannot be precisely compared, as the award mechanism of the third call was based on two additional criteria. In particular, starting with this call a score was introduced relating to the priorities of the region in relation to location, project type and sector (along with a score relating to the environmental impact of the project). Since regional priorities are decided by local government, it could well be that the introduction of this criterion benefited less productive firms, such as those from declining industries that are the most shielded by local politicians.

Figure 1. The unconstrained case

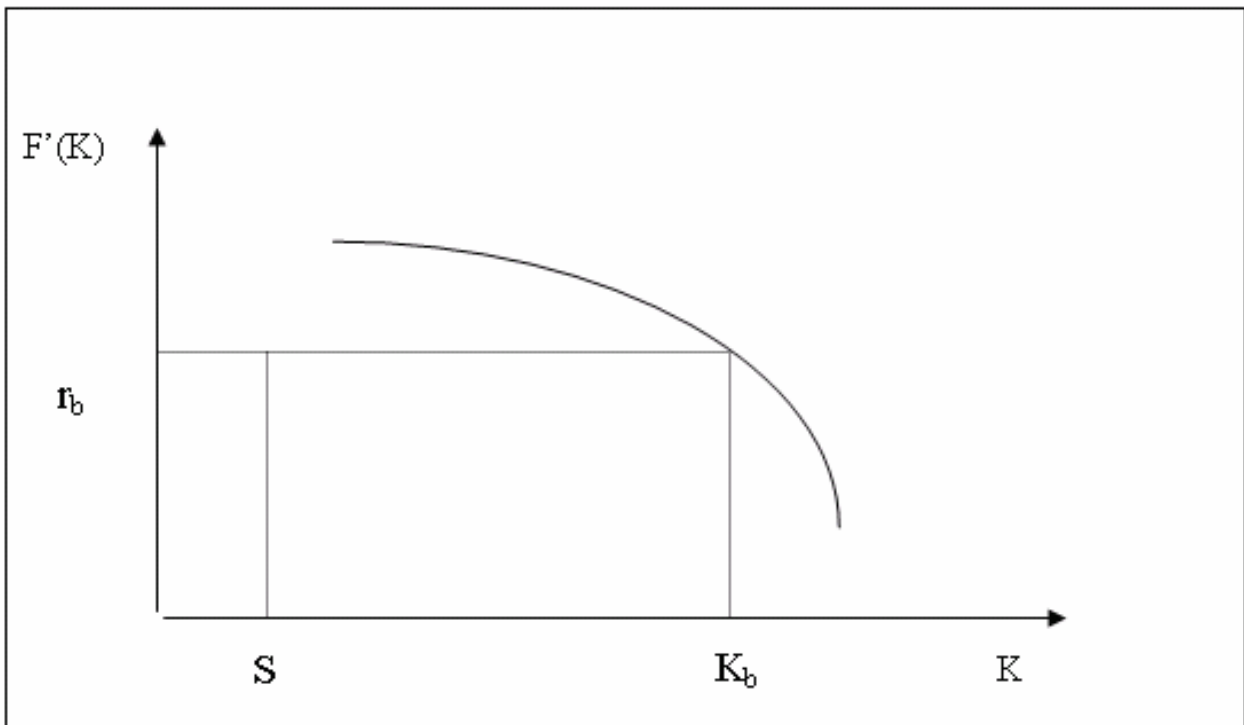


Figure 2. The constrained case

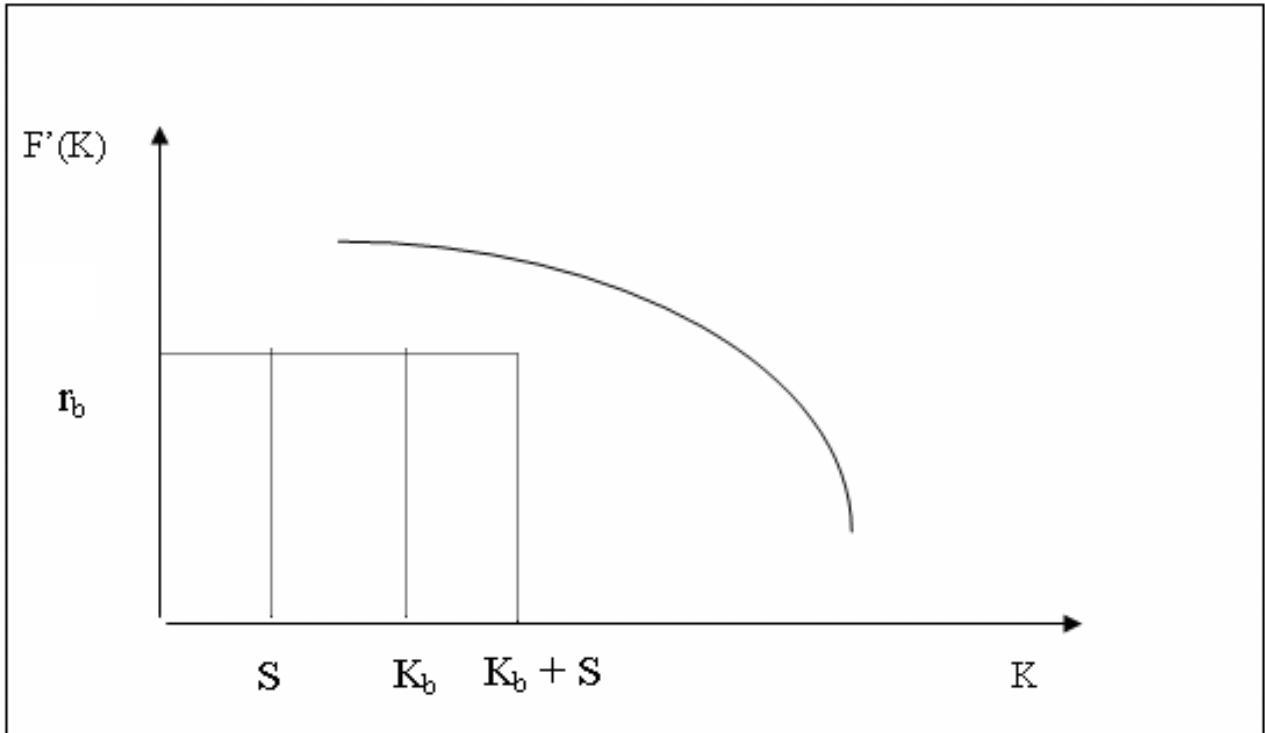
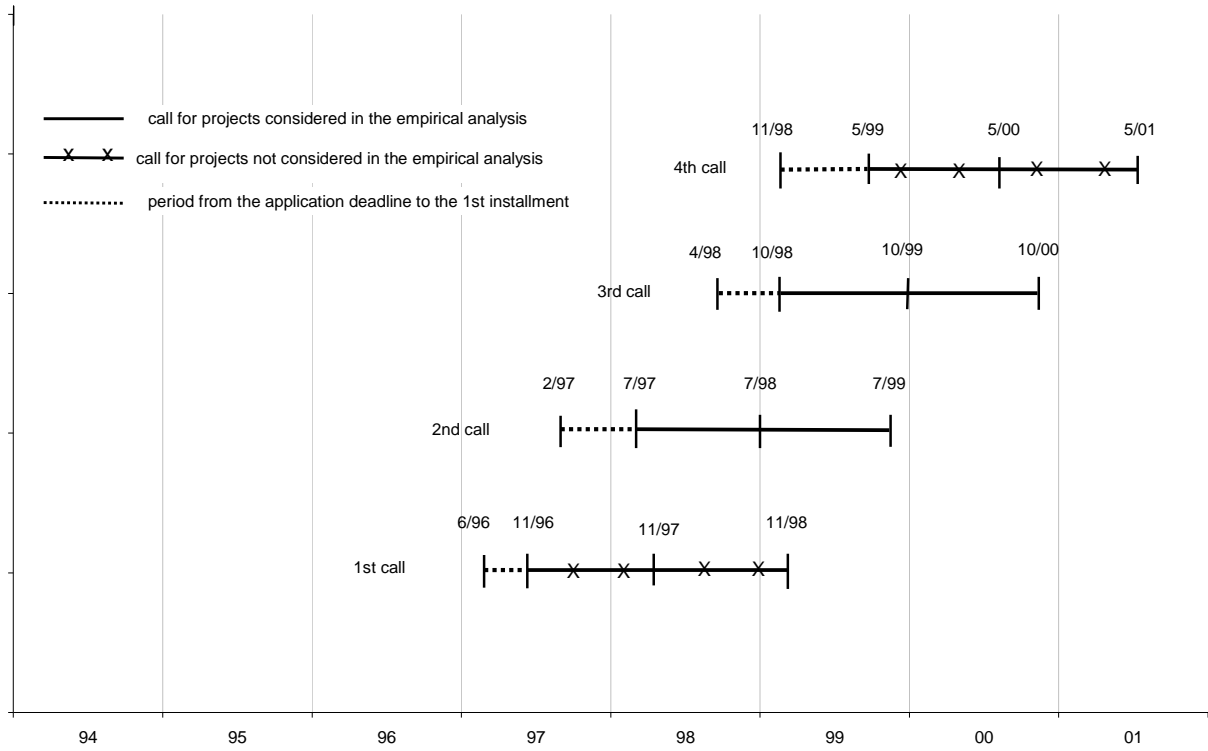


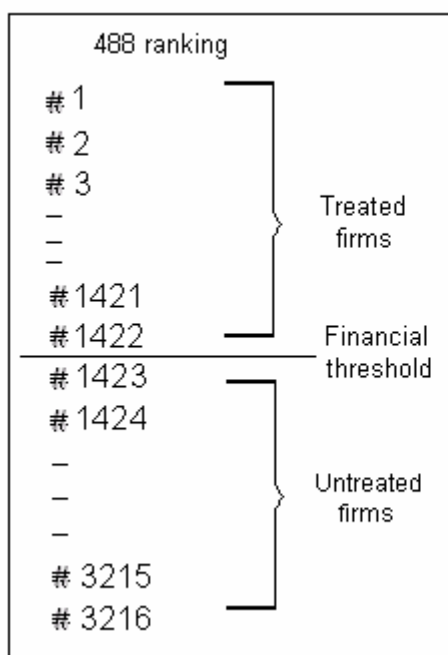
Figure 3. Time pattern of the data



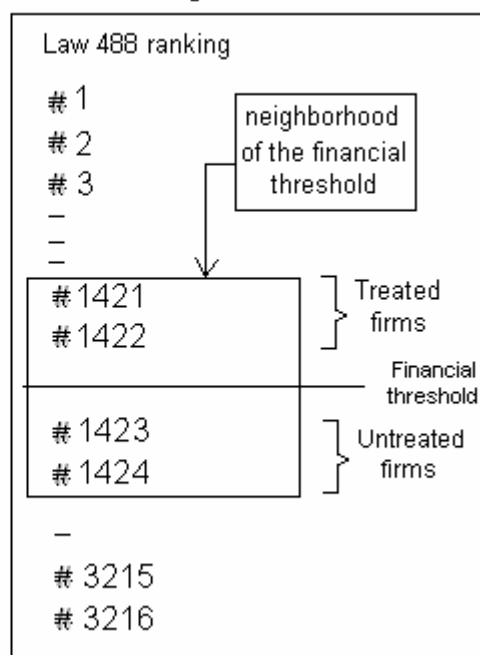
Notes. The figure denotes the envisaged timing of the calls for projects. As explained in the text, in some cases actual disbursements were delayed.

Figure 4. The empirical design

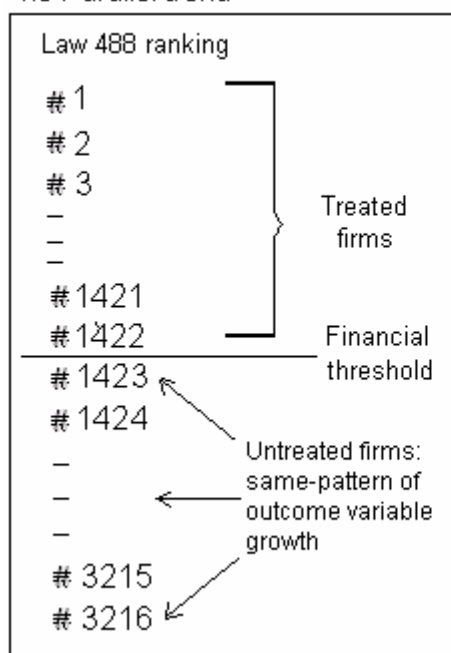
4.1 Baseline



4.2 Cutoff neighborhood



4.3 Parallel trend



4.4 Placebo

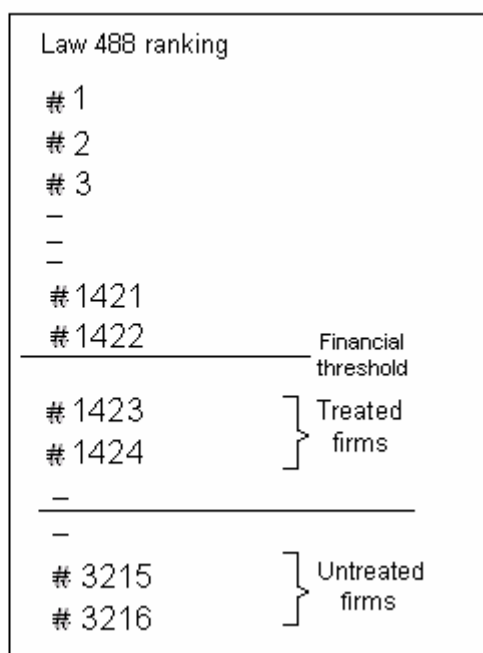


Table 1. Descriptive statistics: Baseline

Year:	1997	1998	1999	2000	2001	2002	No. of firms
<i>A. 2nd call – Full sample</i>							
Investment	1.421 (4.766)	1.051 (3.319)	1.705 (13.358)	2.451 (20.905)	0.933 (9.334)	-0.046 (2.384)	515
Debt	1.983 (9.080)	1.461 (5.469)	1.650 (11.092)	2.747 (22.511)	0.845 (10.883)	-0.070 (6.504)	515
Interest rate	0.043 (0.035)	0.041 (0.028)	0.034 (0.025)	0.041 (0.081)	0.038 (0.028)	0.037 (0.038)	515
Efficiency	0.287 (1.754)	0.379 (0.519)	0.343 (0.787)	0.732 (8.727)	0.346 (0.822)	0.259 (1.603)	515
<i>B. 2nd call – Small firms</i>							
Investment	1.935 (6.060)	1.354 (4.145)	2.893 (18.232)	4.509 (29.388)	1.624 (12.981)	-0.227 (2.704)	257
Debt	3.147 (11.357)	1.899 (7.091)	2.571 (14.475)	4.387 (31.299)	0.463 (12.748)	-0.524 (7.010)	257
Interest rate	0.049 (0.037)	0.043 (0.031)	0.035 (0.028)	0.043 (0.103)	0.038 (0.026)	0.041 (0.048)	257
Efficiency	0.269 (2.212)	0.354 (0.606)	0.272 (0.908)	1.088 (12.35)	0.264 (0.918)	0.152 (2.149)	257
<i>C. 3rd call – Full sample</i>							
Investment	1.089 (10.462)	1.704 (21.488)	1.955 (21.164)	1.788 (25.574)	0.534 (7.134)	0.353 (8.660)	842
Debt	2.706 (23.330)	4.175 (63.323)	4.251 (66.245)	1.427 (8.380)	1.828 (13.975)	0.678 (13.340)	842
Interest rate	0.045 (0.032)	0.039 (0.026)	0.032 (0.022)	0.036 (0.058)	0.035 (0.024)	0.034 (0.049)	842
Efficiency	0.383 (0.349)	0.378 (0.367)	0.388 (0.407)	0.361 (0.398)	0.336 (0.512)	0.261 (1.074)	842
<i>D. 3rd call – Small firms</i>							
Investment	1.820 (14.669)	3.069 (30.326)	3.623 (29.837)	3.250 (36.113)	0.883 (3.223)	0.517 (12.215)	421
Debt	4.092 (32.547)	7.437 (89.340)	7.437 (93.382)	1.951 (11.224)	3.223 (19.276)	0.613 (17.957)	421
Interest rate	0.046 (0.035)	0.040 (0.029)	0.032 (0.025)	0.037 (0.078)	0.035 (0.025)	0.034 (0.040)	421
Efficiency	0.389 (0.395)	0.369 (0.480)	0.391 (0.440)	0.381 (0.371)	0.342 (0.401)	0.166 (1.473)	421

Notes. For each variable and year the table reports mean and standard deviation (in parenthesis). Investment and debt are divided by the capital (see text).

Table 2. Investment and debt: Baseline

	(1)	(2)					
		Single year effects					
		<i>2nd call installments:</i>					
		<i>1st</i>	<i>2nd</i>	<i>3rd</i>			
<i>3rd call installments:</i>							
		<i>1st</i>	<i>2nd</i>	<i>3rd</i>			
		1997	1998	1999	2000	2001	2002
<i>A. 2nd call – Full sample</i>							
Investment	-0.479 (0.455)	0.594* (0.341)	0.890 (0.538)	-0.884 (0.954)	-2.354 (1.584)	-1.272 (0.854)	0.150 (0.431)
Debt	-0.608** (0.283)	-0.564 (0.451)	0.012 (0.820)	-0.495 (0.589)	-2.022 (1.606)	-1.033 (0.638)	0.453 (1.010)
<i>B. 2nd call – Small firms</i>							
Investment	-0.692 (0.802)	0.787 (0.628)	1.462 (0.855)	-1.520 (1.573)	-3.720 (2.739)	-1.371 (1.439)	0.211 (0.878)
Debt	-0.971*** (0.296)	-0.731 (0.519)	0.379 (1.129)	-0.871 (0.752)	-3.652 (3.016)	-2.026* (1.094)	1.074 (1.508)
<i>C. 3rd call – Full sample</i>							
Investment	1.768* (0.866)	-	2.602 (2.095)	3.118 (2.047)	4.399 (2.811)	-0.408 (1.263)	-0.871 (1.433)
Debt	3.130 (1.832)	-	8.190 (6.699)	7.321 (6.973)	-0.008 (1.022)	1.782 (1.825)	-1.634 (2.653)
<i>D. 3rd call – Small firms</i>							
Investment	3.340** (1.536)	-	5.202 (3.978)	5.885 (3.875)	8.467 (5.361)	-0.923 (2.552)	-1.930 (2.928)
Debt	6.049 (3.585)	-	16.017 (13.053)	14.647 (13.764)	-0.135 (2.021)	2.925 (3.432)	-3.209 (5.013)

Notes. Column (1) provides regression results from specification (1) in the text. It displays the coefficient of the interaction between Law 488 and POST. In addition to the coefficients displayed, the regression includes the dummy Post and the dummy Law 488. Column (2) provides regression results from specification (2) in the text. It displays the coefficients of the interactions between Law 488 and Yearpost. In addition to the coefficients displayed, the regression includes year dummies and the dummy Law 488. No. of observations as follows: Panel A, 4,120; Panel B, 2,056; Panel C, 5,894; Panel D, 2,947. *** (**) [*] denotes significance at the 1% (5%) [10%] level. Estimation period is 1995 and 2002 for Panels A and B and 1996 and 2002 for Panels C and D. The White robust standard errors reported in parentheses are corrected for the potential clustering of the residual at the regional level.

Table 3. Interest rate and efficiency : Baseline

	(1) Data collapsed into one single pre- and post-period	(2) Single year effects					
		<i>2nd call installments:</i>		<i>3rd call installments:</i>			
		<i>1st</i>	<i>2nd</i>	<i>1st</i>	<i>2nd</i>	<i>3rd</i>	
		1997	1998	1999	2000	2001	2002
<i>A. 2nd call – Full sample</i>							
Interest rate	0.001 (0.002)	0.001 (0.001)	0.003 (0.002)	0.000 (0.003)	0.004 (0.008)	0.001 (0.003)	0.002 (0.003)
Efficiency	0.018 (0.082)	0.188 (0.143)	0.071 (0.136)	0.090 (0.152)	0.456 (0.510)	0.152 (0.126)	0.063 (0.187)
<i>B. 2nd call – Small firms</i>							
Interest rate	0.004 (0.004)	0.001 (0.002)	0.001 (0.003)	0.003 (0.004)	0.013 (0.015)	0.003 (0.003)	0.004 (0.005)
Efficiency	0.035 (0.141)	0.266 (0.275)	0.100 (0.209)	0.053 (0.225)	1.001 (1.084)	0.215 (0.229)	0.158 (0.366)
<i>C. 3rd call – Full sample</i>							
Interest rate	-0.010 (0.008)	-	-0.009 (0.008)	-0.008 (0.008)	-0.011 (0.008)	-0.010 (0.008)	-0.011 (0.009)
Efficiency	-0.052** (0.024)	-	-0.074*** (0.019)	-0.010 (0.033)	-0.029 (0.023)	-0.023 (0.028)	-0.126 (0.119)
<i>D. 3rd call – Small firms</i>							
Interest rate	-0.019 (0.015)	-	-0.019 (0.015)	-0.017 (0.016)	-0.023 (0.016)	-0.018 (0.016)	-0.018 (0.017)
Efficiency	-0.058 (0.047)	-	-0.114 (0.032)	0.017 (0.066)	-0.046 (0.034)	0.004 (0.047)	-0.151 (0.209)

Notes. Column (1) provides regression results from specification (1) in the text. It displays the coefficient of the interaction between Law 488 and POST. In addition to the coefficients displayed, the regression includes the dummy Post and the dummy Law 488. Column (2) provides regression results from specification (2) in the text. It displays the coefficients of the interactions between Law 488 and Yearpost. In addition to the coefficients displayed, the regression includes year dummies and the dummy Law 488. No. of observations as follows: Panel A, 4,120; Panel B, 2,056; Panel C, 5,894; Panel D, 2,947. *** (**) [*] denotes significance at the 1% (5%) [10%] level. Estimation period is 1995 and 2002 for Panels A and B and 1996 and 2002 for Panels C and D. The White robust standard errors reported in parentheses are corrected for the potential clustering of the residual at the regional level.

Table 4. Investment and debt: Cutoff-neighborhood sample

	(1) Data collapsed into one single pre- and post-period	(2) Single year effects					
		<i>2nd call installments:</i>					
		<i>1st</i>	<i>2nd</i>	<i>3rd</i>			
		<i>3rd call installments:</i>					
		<i>1st</i>	<i>2nd</i>	<i>3rd</i>			
		1997	1998	1999	2000	2001	2002
<i>A. 2nd call – Full sample</i>							
Investment	-1.898 (1.101)	2.863 (1.905)	1.876 (1.159)	-4.609* (2.411)	-9.053* (5.140)	-3.636 (2.406)	1.619 (1.301)
Debt	-1.853* (0.906)	-1.707 (1.191)	1.833 (3.035)	0.420 (0.706)	-10.755 (7.152)	-3.566* (2.027)	2.653 (2.568)
<i>B. 2nd call – Small firms</i>							
Investment	-3.562 (2.232)	5.213 (3.583)	3.185 (2.024)	-8.002* (4.475)	-17.364* (9.838)	-7.051 (4.119)	2.644 (2.371)
Debt	-2.884* (1.403)	2.931 (1.896)	4.744 (5.231)	1.041 (1.839)	-17.700 (13.106)	-7.395* (4.182)	4.935 (4.565)
<i>C. 3rd call – Full sample</i>							
Investment	3.727 (2.281)	-	5.877 (5.646)	5.742 (5.428)	10.355 (7.291)	-1.111 (3.693)	-2.229 (4.235)
Debt	5.345 (5.209)	-	18.441 (19.129)	18.177 (20.037)	-3.063 (2.816)	-1.245 (3.651)	-5.582 (7.167)
<i>D. 3rd call – Small firms</i>							
Investment	7.426 (4.365)	-	11.815 (11.241)	11.341 (10.837)	20.672 (14.188)	-2.201 (7.461)	-4.494 (8.576)
Debt	11.203 (10.298)	-	37.204 (38.093)	37.244 (40.019)	-4.997 (5.620)	-1.938 (7.276)	-11.497 (14.258)

Notes. Column (1) provides regression results from specification (1) in the text. It displays the coefficient of the interaction between Law 488 and POST. In addition to the coefficients displayed, the regression includes the dummy Post and the dummy Law 488. Column (2) provides regression results from specification (2) in the text. It displays the coefficients of the interactions between Law 488 and Yearpost. In addition to the coefficients displayed, the regression includes year dummies and the dummy Law 488. No. of observations as follows: Panel A, 832; Panel B, 424; Panel C, 1,267; Panel D, 728. *** (***) [*] denotes significance at the 1% (5%) [10%] level. Estimation period is 1995 and 2002 for Panels A and B and 1996 and 2002 for Panels C and D. The White robust standard errors reported in parentheses are corrected for the potential clustering of the residual at the regional level.

Table 5. Investment and debt: Parallel trend sample

	(1) Data collapsed into one single pre- and post-period	(2) Single year effects					
		<i>2nd call installments:</i>					
		<i>1st</i>	<i>2nd</i>	<i>3rd</i>			
		<i>3rd call installments:</i>		<i>1st</i>	<i>2nd</i>	<i>3rd</i>	
		1997	1998	1999	2000	2001	2002
<i>A. 2nd call – Full sample</i>							
Investment	0.274 (0.211)	0.475 (0.353)	0.438 (0.356)	0.724 (0.423)	0.961 (0.569)	-0.391 (0.369)	-0.562** (0.212)
Debt	-0.418 (0.260)	-0.087 (0.539)	-0.411 (0.322)	-0.197 (0.582)	0.198 (0.659)	-1.003** (0.443)	-1.009** (0.406)
<i>B. 2nd call – Small firms</i>							
Investment	0.662 (0.428)	0.589 (0.569)	0.799 (0.737)	1.433* (0.699)	2.170* (1.171)	-0.088 (0.791)	-0.931** (0.336)
Debt	-0.471 (0.506)	0.359 (0.718)	-0.307 (0.518)	0.146 (0.944)	0.473 (1.451)	-2.187** (0.873)	-1.309** (0.495)
<i>C. 3rd call – Full sample</i>							
Investment	1.799** (0.841)	-	2.676 (2.052)	3.436 (1.993)	4.271 (2.795)	-0.453 (1.310)	-0.931 (1.472)
Debt	2.861 (1.849)	-	8.061 (6.696)	7.453 (6.979)	-0.428 (1.007)	1.419 (1.762)	-2.202 (2.534)
<i>D. 3rd call – Small firms</i>							
Investment	3.481** (1.522)	-	5.453 (3.936)	6.602 (3.825)	8.248 (5.379)	-0.909 (2.586)	-1.986 (2.968)
Debt	5.608 (3.554)	-	15.724 (13.020)	14.857 (13.723)	-0.817 (2.009)	2.686 (3.290)	-4.408 (5.003)

Notes. Column (1) provides regression results from specification (1) in the text. It displays the coefficient of the interaction between Law 488 and POST. In addition to the coefficients displayed, the regression includes the dummy Post and the dummy Law 488. Column (2) provides regression results from specification (2) in the text. It displays the coefficients of the interactions between Law 488 and Yearpost. In addition to the coefficients displayed, the regression includes year dummies and the dummy Law 488. No. of observations as follows: Panel A, 3,752; Panel B, 1,888; Panel C, 5,544; Panel D, 2,723. *** (**) [*] denotes significance at the 1% (5%) [10%] level. Estimation period is 1995 and 2002 for Panels A and B and 1996 and 2002 for Panels C and D. The White robust standard errors reported in parentheses are corrected for the potential clustering of the residual at the regional level.

Table 6. Investment and debt: Placebo sample

	(1)	(2)					
		Single year effects					
		<i>2nd call installments:</i>					
		<i>1st</i>	<i>2nd</i>	<i>3rd</i>			
	<i>3rd call installments:</i>		<i>1st</i>	<i>2nd</i>	<i>3rd</i>		
	1997	1998	1999	2000	2001	2002	
<i>A. 2nd call – Full sample</i>							
Investment	1.182 (0.741)	-1.776** (0.832)	-0.948** (0.389)	2.522 (1.607)	6.091 (3.541)	1.904 (1.254)	-0.699* (0.359)
Debt	0.732 (0.833)	-0.194 (0.879)	-0.156 (1.367)	-0.386 (0.857)	7.073 (5.221)	0.589 (1.058)	-1.128 (0.777)
<i>B. 2nd call – Small firms</i>							
Investment	2.509 (1.581)	-2.820* (1.339)	-1.503* (0.700)	5.607 (3.570)	11.992 (7.710)	3.253* (1.762)	-1.470 (1.199)
Debt	1.854 (1.409)	1.599 (1.785)	-2.532 (2.124)	0.557 (1.573)	13.883 (9.417)	1.086 (2.402)	-3.468 (2.414)
<i>C. 3rd call – Full sample</i>							
Investment	-0.062 (0.206)	-	-0.113 (0.392)	0.363 (0.561)	-0.238 (0.152)	0.269 (0.270)	-0.595** (0.264)
Debt	0.732* (0.416)	-	0.141 (0.487)	2.469* (1.338)	1.059* (0.545)	-0.112 (0.394)	0.104 (0.783)
<i>D. 3rd call – Small firms</i>							
Investment	0.051 (0.393)	-	-0.063 (0.782)	0.844 (1.074)	-0.307 (0.278)	0.638 (0.489)	-0.855 (0.551)
Debt	0.921 (0.714)	-	0.220 (0.744)	2.900 (2.566)	1.287 (0.765)	0.035 (0.671)	0.161 (1.167)

Notes. Column (1) provides regression results from specification (1) in the text. It displays the coefficient of the interaction between Law 488 and POST. In addition to the coefficients displayed, the regression includes the dummy Post and the dummy Law 488. Column (2) provides regression results from specification (2) in the text. It displays the coefficients of the interactions between Law 488 and Yearpost. In addition to the coefficients displayed, the regression includes year dummies and the dummy Law 488. No. of observations as follows: Panel A, 1,528; Panel B, 856; Panel C, 4,207; Panel D, 2,086. *** (**) [*] denotes significance at the 1% (5%) [10%] level. Estimation period is 1995 and 2002 for Panels A and B and 1996 and 2002 for Panels C and D. The White robust standard errors reported in parentheses are corrected for the potential clustering of the residual at the regional level.

Appendix A. Data description

The Law 488 Dataset includes 3,358 corporations for the second call for projects and 3,731 for the third. As explained in the text, we study the impact of the program by contrasting the subsidized firms with the firms that applied for the incentives but were not offered the award as they scored low in the ranking. A problem with this strategy is that firms can apply for subsidies at different calls. Since firms can be receiving Law 488 money under more than one call, we only keep firms that received the grant once. Similarly, for each call we exclude from the pool of rejected applications firms that nonetheless won the award under any call during the period examined. By implementing these exclusions, we are left with 2,433 firms for the second call and 2,881 for the third call.

Subsequently, we link the Law 488 dataset with the CERVED dataset to reconstruct an uninterrupted financial-statement sample. In the linking procedure, firm identifier (tax and chamber of commerce codes) misprints and the unavailability of balance-sheet data over the entire period reduce the sample to 1,036 and 1,334 firms, respectively, for the two calls. Moreover, we select only firms with non-negative values for capital stock, assets, and sales, and trim the (firm \times year) sample at the 5th and 95th percentiles of the distribution of investment and debt flows. As a result, we are able to reconstruct uninterrupted financial statements from 1994 to 2002 (1995-2002) for 515 (842) firms that participated in the second (third) call (the shares of firms financed are 63 and 29 percent in the two calls). This sample is labeled *Full sample* of the baseline model. Note that we lose 1993 data to construct our dependent variables, which are defined as investment and debt flow, calculated as the time difference between the stocks of physical capital and debt stock respectively, measured in two successive years, as a percentage of (initial period) capital stock.

For each call we use only two yearly pre-treatment observations. Therefore, the estimation period is 1995-2002 and 1996-2002, respectively, for the two calls. While using a two-year window as the pre-intervention period is common practice, this is also a sensible choice with our data. As a matter of fact, the coverage of the CERVED dataset increases over time and reconstructing uninterrupted financial-statement data starting in the initial CERVED years of data availability would have resulted in an undue restriction on the number of observations (for instance, if we include 1994 in the estimation period for the second call, we are left with less than 400 firms).

Appendix B. Mean differences between treated and untreated firms (various samples)

	Baseline	Cutoff neighborhood	Parallel trend	Placebo
<i>A. 2nd call – Full sample</i>				
Sales	189.417 (339.591)	-1760.048* (905.48)	58.372 (78.226)	1198.464** (537.538)
Own Capital / Total debt	0.049 (0.100)	0.145 (0.097)	-0.018 (0.164)	-0.182 (0.122)
Cash flow / Assets	0.010 (0.007)	0.024 (0.017)	0.002 (0.010)	0.004 (0.012)
ROA	0.007 (0.006)	0.008 (0.015)	0.001 (0.001)	0.001 (0.010)
<i>B. 2nd call – Small firms</i>				
Sales	42.790 (75.527)	241.309 (154.973)	58.372 (78.226)	-20.241 (85.603)
Own Capital / Total debt	0.068 (0.154)	0.132 (0.122)	-0.018 (0.164)	-0.289 (0.177)
Cash flow / Assets	0.007 (0.010)	0.041 (0.023)	0.002 (0.010)	-0.004 (0.018)
ROA	0.004 (0.008)	0.021 (0.020)	0.001 (0.001)	-0.011 (0.014)
<i>C. 3rd call – Full sample</i>				
Sales	-154.063 (223.051)	328.395 (433.278)	-209.913 (226.724)	-288.935 (237.822)
Own Capital / Total debt	0.170** (0.080)	0.028 (0.111)	0.159* (0.082)	0.026 (0.061)
Cash flow / Assets	0.009 (0.006)	0.035** (0.013)	0.008 (0.006)	0.007 (0.006)
ROA	0.011** (0.005)	0.034*** (0.012)	0.011** (0.005)	0.005 (0.005)
<i>D. 3rd call – Small firms</i>				
Sales	-136.420*** (47.651)	-140.725 (91.956)	-146.251*** (47.828)	-37.232 (51.935)
Own Capital / Total debt	0.235 (0.146)	-0.126 (0.162)	0.221 (0.152)	0.098 (0.104)
Cash flow / Assets	0.013 (0.010)	0.040** (0.019)	0.012 (0.010)	-0.007 (0.010)
ROA	0.019** (0.008)	0.047** (0.018)	0.020** (0.008)	-0.003 (0.008)

Notes. *** (**) [*] denotes significance at the 1% (5%) [10%] level. Mean differences are calculated with reference to the first year of the pre-intervention year (1995 for the 2nd call and 1996 and for the 3rd call).

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