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Evaluating the impact of investment incentives - the case of the Italian Law 488

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**EVALUATING THE IMPACT OF INVESTMENT INCENTIVES:
THE CASE OF THE ITALIAN LAW 488⁽⁰⁾**

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ABSTRACT

Since the second half of the '90s, investment incentives channeled through the Law 488 have represented the main policy instrument for reducing territorial disparities in Italy. From 1996 to 2003, the total amount of funds distributed to industrial firms has accounted for 16 billions of Euro involving 27,846 financed projects mainly in the southern regions. The Law 488 allows firms willing to invest in lagged areas to receive a public subsidy that covers a fraction of the investment outlays. The incentives are assigned through competitive auctions according to pre-determined specific criteria, such as the proportion of own funds invested in the project; the number of jobs involved and the proportion of assistance sought. This paper aims at evaluating the impact of Law 488 subsidies on firms' investment. We employ a *linked* dataset that matches for *all* the firms that have applied for the grants –both subsidized firms and firms with rejected applications– the features of the Law 488 intervention with financial account data that covers both pre-intervention and post-intervention periods. The focus is to evaluate whether the Law 488 made it possible investments that otherwise would not have been done. In doing so, we compare the investment performance of subsidized firms with that of the firms that applied for the grants but were not financed. We analyze the extent to which investments have been triggered by intertemporal substitution (firms could have anticipated investment projects originally planned for the post-intervention period to take advantage of the incentives). Moreover, we study the role of cross-sectional substitution (subsidized firms could have taken some of the investment opportunities that non-subsidized firms would have got in absence of the incentives). We find that financed firms have substantially increased their investments when compared with the pool of rejected application firms. We also find evidence of intertemporal substitution: financed firms slow down significantly their investment activity in the years following the program. Finally, the impact of the L488 is more pronounced when the size of the market where the firms compete is small or when the firms are close as for their industrial distance, so to suggest that financed firms displace their non-financed competitors.

JEL: D24, H25, H71, R0.

Keywords: Investment incentives, program evaluation, territorial disparities.

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1. Introduction

Financial assistance to manufacturing industry channeled through the Law 488 (L488) has for many years been the main policy instrument for reducing territorial disparities in Italy. Significant amounts of public money have been spent to stimulate investment. From 1996 to 2003, the total amount of funds distributed to industrial firms has accounted for 16 billions of Euro, involving 27,846 financed projects mainly in the southern regions. The L488 allows firms willing to invest in lagged areas to receive a public subsidy that covers a fraction of the investment outlays. The incentives are assigned through competitive auctions according to pre-determined specific criteria, such as the proportion of own funds invested in the project; the number of jobs involved and the proportion of assistance sought.¹

The extent to which investment incentives have economic payoff has been at the forefront on economic research for decades (see, for instance, Hall and Jorgenson (1967) or King (1977)). Moreover, the role of incentives in reducing territorial disparities is a central topic in regional science (Faini and Schiantarelli (1987), Harris and Trainor (2005), and Gabe and Karybill (2002)). Although this literature is voluminous, there is no shared agreement on the effectiveness of investment subsidies. Evaluating the effects of government sponsored projects, one has to face the question of what would have taken place without the subsidies. That is, evaluating a subsidy program is an exercise in counterfactual analysis. Since neither the subsidized firms nor those not applying can be considered random draws, the challenge is to construct a valid control group. Moreover, to evaluate whether the L488 made it possible investments that otherwise would not have been done two more issues ought to be tackled. First, one has to analyze the extent to which additional investments have been triggered by time-substitution (Abel (1982), Adda and Cooper (2000), and Auerbach and Hines (1988)). To take advantage of the incentives, firms could have anticipated investment projects originally planned for the post-intervention period. Second, one has to study the role of cross sectional substitution (Klette et al (1999) and Lee (1996)). Subsidized firms could have taken some of the investment opportunities that non-subsidized firms would have got in absence of the incentives.

By adopting a difference-in-differences framework, this paper takes advantage of the auction mechanism that is used to allocate the incentives under the L488. We compare the group of financed firms with the group of firms that applied for the incentives but were not financed since they score low in the L488 ranking. As suggested by Brown et al (1995), the main virtue of rejected application group is that it is very similar to the treatment group in terms of its characteristics. While the rejected application firms are hardly a random group of firms, they may get as close as a control group as is possible. We check further the reliability of the comparison group in two respects. First, we implement an intuitive version of the regression discontinuity design (Campbell

¹ Although this paper focuses on a particular type of financial assistance in a given country, its scope is much wider: investment incentives programs very similar to the L488 are now being implemented in many EU countries. See Braunerhjelm et al (2000) and Yuill et al (1999).

(1969)) and contrast financed firms just above the financing threshold in the L488 ranking with non-financed firms just below that threshold. Second, we construct a comparison group that mirrors the time-series pattern of the treated group before the program took place. This group comprises of firms for which the deviation respect to the investment growth rate of the treated firms is minimized. A central focus of the paper is to evaluate to what extent the impact of the L488 is biased by time- and cross sectional-substitution. We deal with the former by using a long-time series of post-intervention observations and the latter by restricting the estimates to firms that compete in geographically bounded markets or otherwise close to each other as for their industrial distance.

When compared with the pool of firms that requested the L488 grants without being financed, we find that financed firms have substantially increased their investments. The increase in investment takes place in the second year of the treatment. However, we also find evidence of intertemporal substitution: in the years following the program financed firms slow down significantly their investment activity compare to the rejected application group. Finally, the impact of the L488 is more pronounced when the size of the market where the firms compete is small or when the firms are close as for their industrial distance. In our view, this suggests that financed firms might have displaced their non-financed competitors. Overall, these results cast some doubts on the efficacy of the L488.

The paper is structured as follows. We start in Section 2 with a detailed description of the L488. Section 3 describes the data and the methodology. The empirical findings are the focus of Section 4. Finally, Section 5 offers some concluding comments.

2. A Description of the L488

This section explains the main features of the L488 (see: *Gazzetta Ufficiale della Repubblica Italiana no. 299, 21 December 1992*). More details can be found in IPI (2002) and Bronzini et al (2005).

Assistance under the L488 takes the form of a project-related capital grants. There is no entitlement to assistance: applications are ranked on the basis of five pre-determined specific criteria and award offers are only made if funding is available. Incentives are restricted to areas designed as Objective 1, 2 or 5b² for the purpose of EU Structural Funds together with some areas that do not qualify for Structural Fund support but which have been approved by the European Commission under Article 92(3)c. Assisted area coverage amount to 48.9 percent of the national population.³ Eligible for assistance are manufacturing and extractive firms.^{4,5} The investment

² Objective 1 refers to the regions suffering general underdevelopment, as reflected in having GDP per capita less than 75% of the EU average. Objective 2 is related to regions suffering a concentration of declining industries, as 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.

³ Objective 1 corresponds to seven regions in the South of Italy, Abruzzi having lost its Objective 1 status at the end of 1996. The Objective 2 and 5b areas, are all located in the Centre-North of the country and Abruzzi, as are the areas approved under Article 92(3)c that are not eligible for Structural Funding.

⁴ In addition, selected producer services are also eligible. They are however not included in the evaluation analysis in

projects covered by the L488 are the following: setting-up, extension (defined as 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 renewals), reconversion (adaptation of existing production facilities in order to manufacture different products), reactivation (takeover of unused production facilities by persons that had previously been involved in the management of the firm) and relocation (eligible only in cases where a transfer of the production facility is required by the national or local authorities).

The L488 is featured by maximum award rates, which depend on both the region where the investment is localized and the size of the firm. The maximum award rates differ from the actual award rates offered since, as shown below, the selection mechanism favors the firms that request lower rates. 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.⁶

Award offers are made on the basis of competitive auctions. Application are ranked by eligible area on the basis of the following five criteria:⁷ (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. The five criteria carry equal weight: the values related to each criteria are normalized to produce a single score that determines the place of the project in the regional ranking. Assistance is awarded in order of merit to the extent that the budget allocated to the area allows. If the application is successful, the rate of award offered is the rate requested in the application.

The L488 auctions are run on a yearly basis. Four L488 auctions were concluded before 2001, which represents the last year for which we have the availability of balance sheet data (see Section 3). Administration of assistance is the hand of the Italian Ministry for Industry. The timing of the assistance is precisely defined (see Figure 1). Application are submitted to a specific deadline. Within four months from the deadline, the Ministry for Industry publishes the rankings. Then, the law requires that in two-month time awarded firms should receive the first annual installment.⁸ Overall sums are paid out in three equal installments (two if the project is completed within 24 months). The second and third installments are paid the same date in subsequent years.

To the extent that our evaluation exercise is concerned, two important aspect of the L488 scheme have to be kept in mind. First, the L488 does not require that the investment project is

the text.

⁵ Starting from 2001, the L488 scheme has been extended through separate auctions to the tourism and transport sectors.

⁶ Additional endowments are available for SMEs in Objective 1 and Objective 2 and 5b outside Article 92(3)c.

⁷ The criteria 4 and 5 were introduced starting from the 3rd auction that took place in 1998 (see below).

⁸ As for the timing of the first installment, there have however been delays. For the first three L488 auctions we consider, the delays were the following: 0 month for the 1st auction; 1 month for the 2nd auction (deadline: 12/96; first installment: 7/97); 1 month for the 3rd auction (deadline: 3/98; first installment 10/98). For the 4th auction that have been substantial delays. For instance, even though the 1st installment was supposed to be received by May 1999, many firms did not receive it until late 2001.

actually started by the time of the first installment. However, the second and third installments are contingent on two-thirds and the entire investments being realized. Therefore, while in the year of the first installment the financial accounts of the subsidized firm might fail to detect the investment activity triggered by the program, the impact of the L488 should be apparent in the financial year of the second (and, to a lesser extent, the third) installment. More importantly, the estimation results we present below are based on the assumption that there are no other governmental programs that are correlated with the allocation of L488 funding. For instance, if the rejected application firms receive other types of financial assistance outside the L488 scheme, then our results will be downward biased. A feature of the L488 regulation minimizes the scope of this bias: financing under this program cannot be combined with other source of public financing. In particular, it is required that firms applying for the L488 money have to give up to other public subsidies. Applying firms are explicitly warned that renouncing to other sources of public money can be particularly costly because under the L488 there is no entitlement to assistance. Therefore, an applying firm has to give up to other financial assistance without guarantee that it will actually receive the L488 grant.⁹

3. Data and Empirical Strategy

We use the official L488 Dataset of the Ministry for Industry. This data set records firms that have applied for the incentives, both financed and non-financed firms. The L488 data set provides us with information that is valuable for the evaluation exercise, such as the firm ranking at the regional level and the timing of the installments. We also make use of the CERVED Dataset, a financial statement data set that contains information on Italian corporations. The use of this data set is due to fact that the L488 data set lacks information on investment, which is the target of the L488 and our outcome variable, as well as additional covariates and firm features. There are additional advantages in using these data. First, the CERVED data cover a major proportion of the Italian corporations. Second, the data set extends from 1993 to 2001, allowing us to study the impact of the program over a period that includes pre-intervention as well as post-intervention years. There are however also drawbacks in using the CERVED data. First, this data set is skewed toward larger firms since it collects balance sheets only from corporations. Second, there are frequent misprints as regard to the firm identifiers (*fiscal* and *chambre of commerce* codes) that we use to link CERVED data to the L488 Dataset.

The time pattern of the linked data set is described in Figure 2. Four L488 auctions took place over the period 1994-2001. For these auctions the treatment started (with the 1st installment) and finished (with the 3rd installment) within the temporal window provided by the CERVED data. We focus below on the 2nd and 3rd auctions. These auctions are ideal for our purposes since they occurred roughly at the midpoint of the CERVED time-window, thus providing us with pre- and

⁹ The ban on combining L488 money with other incentive programs does not apply for the investment tax credit envisaged under the so-called *Tremonti* Law. However, this program applies in an automatic manner to all industrial firms. Therefore, we do not expect that it affects the evaluation of the L488.

post-intervention observations. The 1st auction has been excluded because it included a transitory clause that allowed L488 non-eligible firms to be financed as well. To be sure, these firms received (before the parliament approval the L488 in 1992) pledges of assistance outside the L488 scheme. However, due to public finance problems disbursements were postponed until mid nineties when it was decided that they would have been covered with the 1st auction of the L488 allocations. We have also excluded the 4th auction. The reason is that disbursement under this auction were highly irregular. For instance, even though the 1st installment was supposed to be received by May 1999, many firms (40% in the L488 Dataset) did not receive it until 2000. Since CERVED data end anyway in 2001, the exclusion of the 4th auction is of little harm.

As for the 3rd and 4th auctions, the L488 Dataset includes respectively 3,358 and 3,731 corporations. We study the impact of the program by contrasting the subsidized firms with the firms that have applied for the incentives but that have not been offered the award since they scored low in the auction ranking. A problem with this strategy is that firms can apply for subsidies under different auctions. Since firms can be receiving L488 money under more than one auction, we only keep firms that have received the grant once. Similarly, for each auction we exclude from the pool of rejected application firms the firms that have nonetheless won the award under any auction in the 1993-2001 period. By implementing those exclusions, we are left with 2,433 and 2,881 firms respectively for the 3rd and 4th auction.

Subsequently, we link the L488 data set with the CERVED data set and reconstruct *uninterrupted* balance-sheets from 1994 to 2001 (1995-2001) for 1,008 (1,329) firms that participated at the 2nd (3rd) auction.¹⁰ This sample is labeled *Full Ranking Balanced Sample* and represents our main sample. Note that we lost 1993 data to construct our dependent variable, which is defined as investments over pre-dated capital stock. Note also that for each auction we use only two yearly pre-treatment observations. While using a two-years window as the pre-intervention period is common practice (see: Gruber (1994)), this is also a sensible choice with our data. As a matter of fact, the coverage of the CERVED data set increases over time and reconstructing uninterrupted balance-sheet data starting in the initial CERVED years of data availability would have resulted in an unduly restriction of the number of observations. For instance, if we include the 1994 year in the estimation period for the first auction, we are left with less than 400 firms out of 1,008 firms. In any case, as we checked, limiting the estimation period to two before-intervention year is only of a limited relevance for our results.

To tackle sample attrition issues, we also construct an *unbalanced* panel. This panel differ from the former as for a L488 firm to be included it only requires to have at least one pre-intervention (1995 or 1996 for the 2nd auction and 1996 or 1997 for the 3rd auction) balance-sheet data and one post-intervention data. The unbalanced panel includes 1,089 firms and 1,746 firms respectively for the two auctions.

¹⁰ In the linking procedure, firm identifier misprints reduced the samples to 1,196 and 1,498 firms, respectively for the two auctions. To end up with the sample size described in the text, we selected only firms with non-negative values for capital stock, assets, and sales, and trimmed the (firm \times year) sample at the 5 and 95 percentiles.

Notwithstanding the heavy data trimming that has been required for constructing the two samples, our data cover a substantial fraction of the overall financing. The grants received by the firms included in our full ranking balanced panel represent 394 millions of Euro (21% of the total L488 financing) and 417 millions of Euro (27% of the total), respectively for the 2nd and 3rd auction. Table 1 depicts the main descriptive statistics for the variables used in the paper, distinguished by auctions. Our main dependent variable is given by investment over capital, where investment is calculated as the time difference between the stocks of physical capital measured in two successive years plus depreciation. Note that the average over-time and across applying firms investments over capital amount to 36% and 37.6% in the two auctions. The fact that this share is unusually high, is due to the accounting practice followed by the CERVED that evaluates physical capital *net* of the depreciation (see also Section 4.2). Bronzini et al (2005) provide a comparison between the pool of firms that have requested the L488 financing, which are described in Table 1, and the remaining firms in the CERVED universe. Compared to the firms that have *not applied* for the grant, applying firms are larger and more profitable. They are also featured by an higher share of own capital, better access to bank and other financial institutions credit, and higher proportion of physical capital.

We seek to establish the role of investment subsidies by comparing investments between subsidized firms and non-subsidized firms. Since neither the subsidized firms nor those not applying can be considered random draws, the challenge is to construct a valid control group. Our empirical strategy takes advantage of the auction mechanism that is used to allocate the incentives under the L488. We compare the group of financed firms (*treated*) with the group of firms that applied for the incentives but were not financed since they scored low in the L488 ranking (*untreated*). Thus, our main comparison group comprises of firms with rejected applications. In the full ranking balanced and unbalanced panels, this group includes all non-financed firms, irrespective of their L488 ranking.

The main virtue of 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 as a control group as is possible. The peculiar scheme envisaged by the L488 to allocate the subsidies provides additional arguments. Many scholars (see: Scalera and Zazzero (2000) and Del Monte and Giannola (1997)) have argued that the L488 auction mechanism is very ineffective in discriminating among applying firms. In particular, since 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 firm participating in the auction, it is suggested that the actual allocation of subsidies among the pool of applying firms might have followed a quasi-random assignment. Clearly, if incentives were randomly assigned among the firms participating in the auction, than the untreated firms could be considered as statistically equivalent to the treated firms in all respects except treatment status. In other words, this amounts to believe that the untreated group provides the correct missing counterfactual. To investigate this argument we present in the Appendix, Panel A mean and median differences about the main observables 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 auction and 1996 for the third auction (in any case, using 1996 and 1997 instead would have made no difference). The evidence in favor of the random allocation hypothesis is mixed. We find that treated firms are larger, more profitable, and display higher cash flow.

The mixed evidence presented above compounded with the fact that treatment and control groups could also differ by some unobservable characteristics could clearly invalidate our identification hypothesis. This is why we provide estimates of the impact of treatment also for additional control groups. We substantiate our results in two ways. First, we contrast only firms that are at the middle stage of the ranking. We implement an intuitive version of the regression discontinuity design (Campbell (1969)) and contrast financed firms just above the financing threshold in the L488 ranking with non-financed firms just below that threshold. The idea here is that whatever the actual degree of randomness in the assignment mechanism, it is more likely that the correct counterfactual is provided by the untreated firms that have L488 scores that are similar 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 took place. In particular, systematic differences in levels are not the main concern because they can be controlled for using diff-in-diffs methodologies. However, failure of the parallel trend assumption will invalidate our estimates. Therefore, we use as counterfactual a group that comprises of firms for which the deviation respect to the investment growth rate of the treated firms is minimized. Panel B and Panel C in the Appendix suggest that the two additional control groups might be suitable for our purposes: differences in observables between treatment and control are now much reduced and most of the times are not significant.

Estimating the impact of the L488 amounts to gauge the extent to which the investments triggered by the incentives are *additional*. Would the grant element make it possible investments that otherwise would not have been undertaken? Even if provided with the correct counterfactual, it is difficult to evaluate additionality.

A first problem is given by *time substitution*. Because of the availability of subsidies, firms could have anticipated investment projects originally planned for the post-intervention period. As shown by Abel (1982), a temporary investment subsidy gives firms a strong incentive to invest while the incentive is in effect. This effect has been extensively studied in the literature on the effects of incentives for investments and purchasing of durable goods (see, for instance, Auerbach and Hines (1988) and Adda and Cooper (2000)). In short, a potential effect of the L488 could have been that of favoring a stronger investment activity during the period in which the incentive scheme was in place, at the cost of deterring future investments. We deal with this problem by using a long-time series of post-intervention observations. In particular, for the 2nd auction that took place in 1997 we are able to study the investment behavior up to 2001, which is two years after the end of the program.

Time substitution is not the only obstacle that might prevent one to assess the impact of the investment incentives. To be sure, there could be indirect effects of the L488. *Cross sectional*

substitution implies that subsidized firms take some of the investment opportunities that non subsidized firm would have got in absence of the L488 (see Harris and Trainor (2005) and Lee (1996)). This might also occur because of general equilibrium effects. The L488 may change the price of capital in a region as a whole if it affects a substantial number of firms. For instance, Goolsbee (1998) shows that investment incentives have little impact because much of the benefit does not go to investing firms but rather to capital suppliers through higher prices. Cross sectional substitution is particularly relevant when the size of the market where the firms compete is small or when the firms are close as for their industrial distance (see: Rosenthal and Strange (2004)). In this vein, one would expect that cross sectional substitution is more intensive for firms located in the same area or competing in the same sector. This is exactly the intuition we exploit in our empirical approach. To assess the role of the indirect effects for our results we compare treated and untreated firms either within the same area or sector.

We will be running simple regressions of the following form:

$$(1) \quad y_{it} = \alpha L488_i + \sum_t \beta_t YEAR_t + \sum_t \gamma_t (L488_i * POST_t) + Z_{it} \delta + \varepsilon_{it}$$

where y_{it} is the outcome variable, a measure of capital accumulation for firm i in year t , $L488$ is a dummy variable indicating whether the firm has received the L488 grant, $YEAR$ denotes time dummies, $POST$ is series of dummies for each of the years after the introduction of the policy, and Z_{it} is a vector of covariates. Our coefficients of interest are the γ_t : the impact of the L488 on the treated evaluated overtime.

4. Results

4.1 Baseline Results

We start by comparing I/K , the ratio of investment over physical assets, for treated and untreated firms belonging to the full ranking balanced panel. We use the CERVED gross investment flow over the capital stock at the beginning of the period. The results are reported in Table 2, which describes separately the 2nd and the 3rd auctions. For each auction, Panel A reports simple differences while Panel B describes the estimates our coefficients of interest γ_t .

As for the 2nd auction, simple differences highlight that investments over capital have been consistently higher for the subsidized firms over the whole period. However, the sign of the differences becomes negative in the last year of data availability. In particular, treated firms are featured by an higher investment activity both in mean and median even before the program. To measure the excess investment for the treated that is attributable to the L488, we turn to diff-in-diffs estimation. We find that in 1998, which corresponds to the second year of the treatment (that is, the financial year of the second installment) there is a statistical significant *average* effect of the program.¹¹ During that year, simple differences indicate that the investment ratio of treated firms

¹¹ The standard errors reported in tables will differ only marginally if we allow the clustering of the residual at the sector level. We also run the cluster correction at the sector by region and size by region levels, again, with minor

outperform that of the untreated firms by 35 percentage points. Diff-in-diffs estimates of the mean impact suggest that roughly half of this increase can be thought as the causal effect of the L488. Median diff-in-diffs results are however less favorable. When evaluated at the median the impact in 1998 is not different from zero. The discrepancy is due to the fact that at the high level of the I/K ratio treated firms have done much better than their untreated counterparts.

Our results also indicate evidence of time substitution. In 2001, that is two years after the end of the financial assistance granted under the 2nd auction, the investment of the treated firms is lower than that of the untreated group by a significant degree (46% according to the diff-in-diffs estimates). The circumstance that in 2001 financed firms are featured by a lower accumulation is confirmed by the diff-in-diffs results evaluated at the median. Here, a note of caution is warranted. Diff-in-diffs estimates are more reliable when one compares outcomes just before and just after the policy change because the identifying assumption is much more likely to hold over a short time-window. With a long time-window, many other things are likely to happen and confound the policy change effect. Having said that, one can measure the cumulative effect over time, which accounts for both the increase in investment in the second year of treatment and its decline afterwards. We calculate (not reported) that at the end of 2001 the cumulative effect of the L488 was positive but insignificant.

The effectiveness of the program is more evident in the 3rd auction, for which simple differences pre-intervention are not significant and there is a significant causal effect of the program in the second year of the treatment both in mean (roughly equal to 20%) and median (the causal effect of the program is equal to 0.078 and the median for the untreated is equal in 1999 to 0.191). Notice that the results of the 2nd and 3rd auctions can not be directly compared, because the allocation of grants are based on slightly different rules: in the 3rd auction were introduced the two additional criteria, respectively the regional priorities and the environmental impact. As for the time substitution, our estimates indicates that in 2001 (one year after the end of the treatment) the difference between subsidized and rejected application groups is negative. They are however not statistically significant. Unfortunately, CERVED data ends in 2001. Thus we might be not able to fully disentangle substitution effects, which in the 2nd auction occur two years after the end of the treatment. As for the cumulative effect (not reported), we find that at the end of 2001 financed firms outperform rejected application firms by a factor 1.6 (the diff-in-diffs coefficient display also high significance).

4.2 Robustness

We adhere to standard practice by using as dependent variable the gross investment flow over the capital stock at the beginning of the period (see: Cummins et al (1994) and Lamont (1997)). However, there are two potential sources of bias with this variable. First, as described in paragraph 2, the investment projects covered by the L488 include, in addition to setting-ups and extensions, also modernizations, restructurings and reconversions. To the extent that the L488 finances

modernization, restructuring and reconversion projects (see, also, Driehuis and van den Noord (1988)) treated firms might have accelerated the renovation of their capital stock by selling old assets and buying new ones. Asset sales however will bias upward the estimated impact of the program. Second, another concern is due to the fact that in the CERVED data set physical capital is evaluated *net* of the depreciation. Since in Italy capital amortization is allowed on the basis of a yearly fixed fraction, older firms are likely to be featured by a smaller net capital stock. To the extent that the L488 tends to favor larger and established firms (see: the Appendix and Bronzini et al (2005)) this will also lead to a positive bias. We deal with this problem by using two alternative dependent variables.¹² Panel B of Table 3 shows the diff-in-diffs analogues of Table 2 where investment over (pre-dated) capital is substituted by investment over sales (measured at the beginning of the period). Panel C of Table 3 reports the results obtained when we use the ratio between investments and (pre-dated) assets as dependent variable. Overall, the results confirm our previous findings. We find evidence of a positive effect of the L488 in the second year of the treatment in both auctions (with a higher significance of the estimates in the 3rd auction); as for the 2nd auction we still find evidence of a strong substitution effect in 2001. More importantly, we do not find evidence that the results on I/K could have been upwardly biased. By using the alternative dependent variables the impact of the program appears more pronounced.

We are worried that our balanced panel could be affected by survivorship bias. In particular, there could be a differential loss of balance sheets availability for treated and untreated firms (see: Pakes and Ericson (1998)). Suppose that two marginal firms apply for the grants and only one of the two gets the subsidy. A possible scenario is that the subsidized firm continues its operations while the non-subsidized firm liquidates. In these circumstances, the estimates from the balanced panel could be negatively biased because the marginal non-subsidized firms that likely display the lowest accumulation rates are no longer included in the comparison sample. To tackle this issue we use the *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 pre-intervention and one post-intervention balance-sheet data. To the extent that liquidation of non-financed firms increases in the year after the program, one would expect higher γ_t for these years. Table 4 Panel B describes the results. For the unbalanced panel we find higher coefficients in initial years of the program. This is true for both auctions. Even though the increases are moderate, these results suggest that at least for some marginal firms, the incentive have increased the probability of remaining in activity.

The interpretation of our results relies on the identification assumption that there are no omitted time-varying firm effects correlated with the program. The identification assumption will be violated if, for instance, as a result of the L488 financing treated firms will become more productive. Another source of violation is represented by the availability of external source of

¹² We only report a sub-sample of the robustness checks performed as to verify the sensitivity of our results to different dependent variable. In particular, we also use the following alternative dependent variables: investment over sales at the *end* of the period, investment over assets at the *end* of the period, and *net* investment over capital. Results were remarkably similar to those reported in the text.

finance. As shown by Banerjee and Duflo (2004), if the degree of credit rationing, or the interest rate, decreases as a result of the availability of the subsidy, then our estimates will mistakenly attribute the variation in investment allowed by the higher availability of non-L488 external financing to the effects of the program. Therefore, we check the robustness of our estimates to the inclusion a number of covariates at the firm level (Table 4 Panel C). We include sales, ROA, a measure of leverage (own capital over debt), a measure of the interest rate (interest costs over debt) and a measure of internal funds (cash flow over assets). The results suggest that the role of time-varying omitted variables is modest.

To further corroborate our findings we estimate the impact of the program when the full ranking balanced panel is splitted along some interesting dimension. First, we split the sample by the location of the investment project. As explained in paragraph 2, the financing is not restricted to the lagged areas of the south of Italy. However, one might think that the efficacy of the grants might varies across territories. For instance, to the extent that lagged areas are characterized by higher marginal productivity of capital, a give amount of grant might trigger more investment in the southern regions than their northern counterparts. Our results in Table 5 Panel B support this view. Second, the L488 can have very different effects on big, cash-rich firms than on small cash-poor firms. Table 5 Panel C checks the role of firm size for our results. Small and large firms are respectively defined as firms below and above the median of the firm's sales. We find that the effect for small firms seems to be higher. However, all the main regularities are still there. Finally, as explained in paragraph 2, the L488 design is such that the amount of the grants differ among firms. Among the subsidized firms of the full ranking balanced panel the grant coverage ratio (defined as the grant in percentage of the investment) range from 1% to 80%. By splitting the sample at the median grant coverage ratio, Table 5 Panel D verify the importance of the rate of financing obtained for our estimates. We find that firms characterized by a grant coverage ratio below the median do not exhibit an increase in their investment activity in the second year of the treatment. The effect is concentrated among the firms that received more generous financing.¹³

4.3 Alternative comparisons

In the case of a randomized experiment, treatment and control groups are identical for large sample. Even in the case of a non-randomized experiment like the L488 investment incentives, the closer are the treatment and control groups the more convincing is the diff-in-diffs approach. In what follows, we implement this idea by contrasting treated at untreated groups that are supposed to be more similar that their counterparts in the full ranking sample. We follow two distinct routes.

First, recall that the L488 scheme envisages that at the regional level all the asking firms are ranked in a decreasing order given by the normalized single score. Then, funding is allocated starting by the top of the ranking and going down until the budget allocate to the region allows. Therefore, for each region there is a threshold level in the ranking. Our approach is to contrast

¹³ Because of the lack of independence, sample-split experiments are clearly only illustrative. Southern firms are smaller and receive higher grants.

firms that are at the middle stage of the ranking. In particular, we compare treated (untreated) firms just above (below) the regional financing threshold in the ranking. The idea here is that whatever the actual assignment mechanism is, it is more likely that the correct counterfactual is provided by the untreated firms that have L488 scores that are similar to the treated ones. This represents an intuitive version of the regression discontinuity design (see: Campbell (1969) and, for convincing applications, Angrist and Lavy (1999) and van der Klaauw (1996)). For each region we first select only the firms that are within the $\pm 30\%$ percentile of the firm distribution around the threshold. Next, we take an even more conservative stance and select only firms that are within the $\pm 10\%$ 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. The results from these experiments are reported in Table 6. Again, we find that a positive effect of the L488 financing is detected in the second year of the treatment and the rise in investment comes at the expenses of future accumulation.

Second, 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 for using diff-in-diffs methodologies. However, failure of the “parallel trend” identifying assumption will bias diff-in-diffs estimates. Notice also that ideally one would like to verify that the parallel trend assumption holds over a long period (see Blundell et al (2004)). We construct this comparison as follows. First, we focus on the 3rd auction, for which we have more pre-intervention observations. Second, for this comparison we make also use of two extra years of observations (1994 and 1995), which were previously discarded because their inclusion unduly reduces the number of firms in the sample. Next, we construct a balanced panel by the method described in paragraph 3. We end up with 924 firms (compared with the 1,329 firms of the baseline for the 3rd auction) for which we have the availability of their I/K ratios from 1994 to 1997 (in the baseline we use only 1996 and 1997). We calculated annual rates of growth of I/K for treated firms. Then, we select among the untreated group only the firms that for each single year display an annual I/K rate of growth within the interval $(1 \pm g) * m_t$, where m_t represent the I/K annual rate of growth for the treated. The results described in Table 7 are based on two comparison groups. For the first group (*Wide Bands*) we set $g=2$ (for the median g is set equal to 12). For the second group (*Narrow Bands*) we assume that $g=1.5$ ($g=10$ for the median). The two groups includes respectively 473 (328) and 368 (309) firms in the regressions for the mean (median). We performed additional robustness checks and verified that by altering the values of g the results were only marginally affected. Panel A of Table 7 presents a new benchmark for the full ranking balanced panel, which differs from the previous baseline because of the inclusion of 1994 and 1995 data. The diff-in-diffs results from this new benchmark are similar to the baseline. More importantly, the violation of the parallel trend assumption seems not to be what drives our results. Both the experiments in Panel B (*Wide Bands*) and Panel C (*Narrow Bands*) confirm that there is a positive effect of the L488 in 1999 jointly with a potential role of intertemporal substitution.

4.4 Indirect effects

Next, we turn on the indirect effects of the L488. The L488 amounts to a straightforward transfer from the government to the subsidized firms. These firms might displace existing non-subsidized firms on the assumption that the size of the local market is fixed and cannot support any additional production (see, also, Harris and Trainor (2005) and Lee (1996)). Cross sectional substitution might also occur because of general equilibrium effects. The L488 may change the price of capital in a region or sector as a whole if it affects a substantial number of firms. For instance, Goolsbee (1998) shows that investment incentives have little impact because much of the benefit does not go to investing firms but rather to capital suppliers through higher prices. Cross sectional substitution should be particularly relevant when the size of the market where the firms compete is small or when the firms are close as for their industrial distance (see: Rosenthal and Strange (2004)). Therefore, one would expect that the positive bias deriving from cross sectional substitution will be more intensive for firms located in the same area or competing in the same sector. Exploiting this intuition, we compare treated and untreated firms either within the same area or sector. We focus on the two largest regions and sectors, for which the availability of information is maximized. Thus, our test will represent a conservative estimate of the indirect effects, which are more widespread in small regions or sectors. Panel B of Table 8 reports the within-region estimate for Campania and Puglia. We find that the impact in the second year of the treatment is higher for both auctions compared to the baseline. We take these results as evidence in favor of the existence of indirect effects. The interpretation in terms of policy is however complicated by the circumstances that time-substitution effect is also more pronounced (at least in the 2nd auction). Table 8 Panel C reports the within-sector results. Admittedly, this experiment is less informative since the sectoral classification of our data is not detailed enough to capture firms competing in the same market. For instance, traditional productions, which represent the bulk of the Italian manufacturing, are grouped within a single sector. We find evidence of cross-sectional substitution at the sector level limited to the low value added productions in the 3rd auction.

4.5 Where do the money end up?

Our results indicate that in order to take advantage of the subsidies firms have anticipated investments originally planned for future periods. An implication of this result is that the L488 might have had additional impacts beyond the time-effect on investments. For instance, since the subsidy represents a transfer of money to firms and overtime there is little additionality, it could have resulted in higher profits and cash flows. This is not the only possibility. The incentive could have given rise to allocative inefficiencies by encouraging a non-optimal mix of factors. In this regard, the L488 scheme is quite ambiguous. On the one hand, firms are encouraged through lower relative capital cost to over-invest in capital. On the other hand, the L488 criterion regarding the number of jobs involved in the project will tend to offset the bias toward more capital intensive techniques. A related concern (see Alesina et al (2001)) is that subsidies can foster the creation of a culture of rent-seeking, and this, in turn, jeopardize future efficiency. To make a first cut to these issues we

present in Table 9 results where we apply the regression framework described above and use labor costs, profits, cash flow, and debts (all normalized by firm's sales) as dependent variables. We fail to find any significant effect on labor costs. This suggests that the allocative inefficiency generated by the scheme is a concern of second-order. More interesting, we find that in 2001 the firms treated under the 2nd auction display higher profits and cash flows compounded with lower debt. This finding is consistent with the time-substitution effect of the subsidies. Financed firms use the public money to undertake investment that in absence of the money would have been undertaken years later. Therefore, it is when the investment was originally planned that financial resources are freed for alternative uses.

5. Conclusions

Since 1996 massive financial assistance has been channeled to manufacturing firms through the L488. Up to 2003, the total amount of funds distributed to industrial firms has accounted for 16 billions of Euro. The L488 allows firms willing to invest in lagged areas to receive a public subsidy that covers a fraction of the investment outlays. The incentives are assigned through competitive auctions according to pre-determined specific criteria, such as the proportion of own funds invested in the project; the number of jobs involved and the proportion of assistance sought.

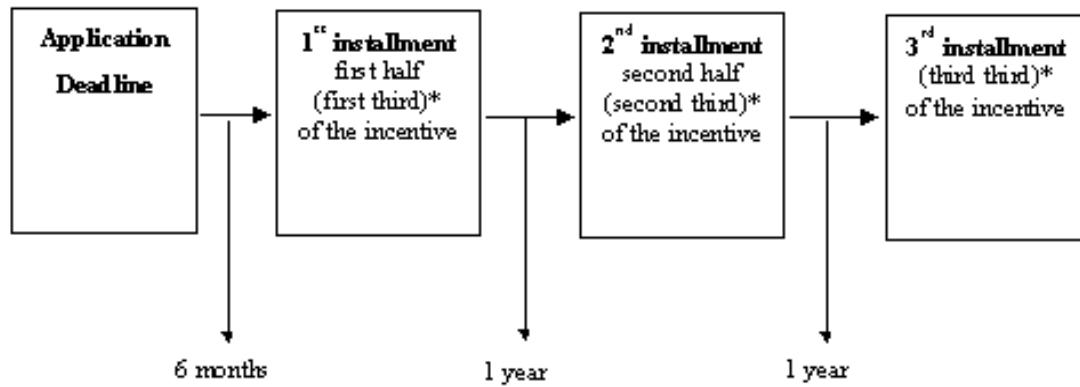
This paper provides a first attempt to evaluate the impact of the L488 incentives. The firms in our sample received Euro 394 and Euro 417 million in investment subsidies respectively for the two 2nd and 3rd L488 auction. This money was spent to increase investments. Evaluated over the value of net physical assets at the end of the previous financial year and compared to the group of rejected application firms the L488 subsidies caused extra investments equal on average to 17% and 20%, respectively for the 3rd and 4th auction. The increase in investment materialized in the second year of the treatment (1998 and 1999, respectively for the two auctions).

However, to be able to ascertain whether the receipt of financial assistance from public funds actually makes a difference to firm investment performance requires to settle two issues. If as effect of the L488 subsidized firms anticipate investment project originally planned for future periods, then the increase in investment cannot be considered additional since it is offset by a future reduction. If as effect of the L488 subsidized firms displace non-subsidized firms, then the increase in investment cannot be considered additional because it crowds out other investment. In this regard, this paper has shown that the L488 money generated less additional investment than their direct impact in the second year of the treatment will suggest. The increase in investment triggered by the incentives is counterbalanced by a decline in accumulation experienced by the subsidized firms later in time. Moreover, we find evidence that the impact of the L488 is more pronounced when the size of the market where the firms compete is small or when the firms are close as for their industrial distance. This suggests that financed firms might have displaced their non-financed competitors. Overall, these results cast some doubts on the efficacy of the L488.

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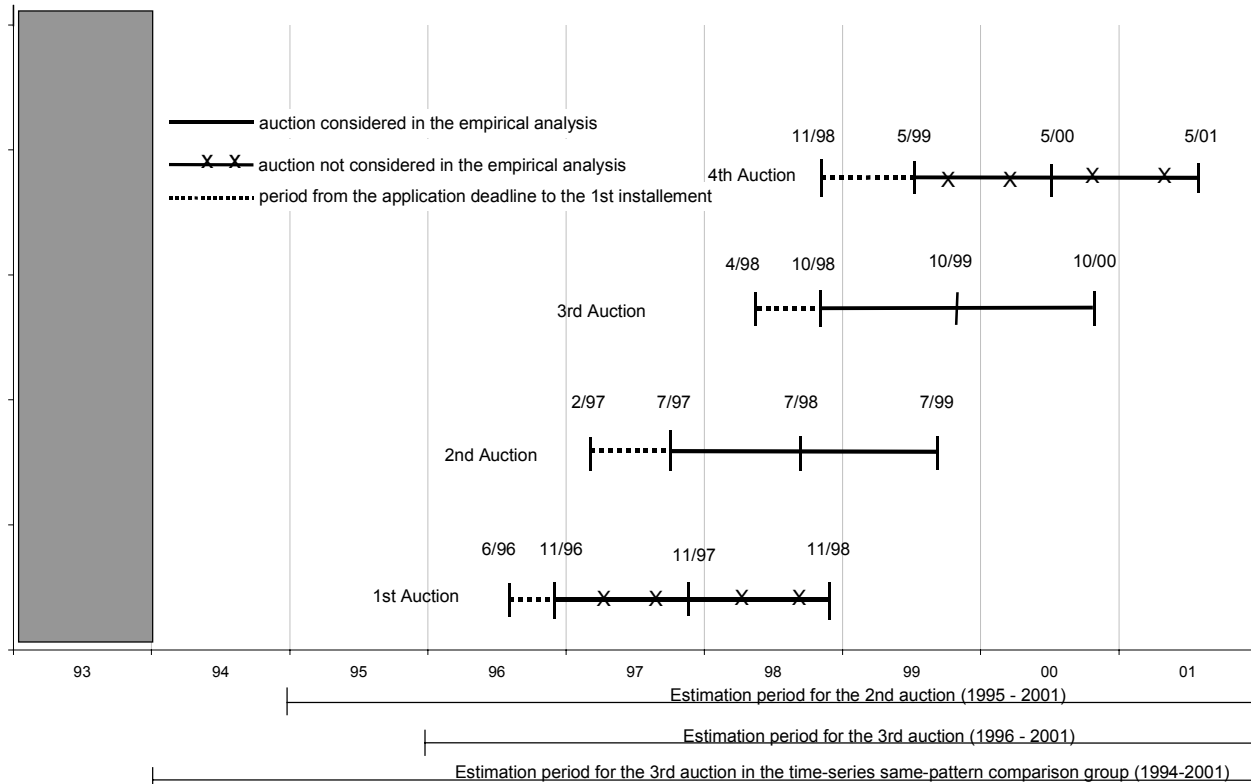
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Figure 1. Timing of the assistance provided by the L488



* Only for projects to be completed in more than two years.

Figure 2. Time pattern of the *linked* dataset



Note: Figure 1 denotes the envisaged timing of the auctions. In some cases, actual disbursements were delayed. In particular, there was a one-month delay in both the 2nd and the 3rd auctions. Moreover, as explained in the text, as for the 4th auction that have been substantial and very erratic delays.

Table 1. Descriptive Statistics

<i>A. 2nd Auction</i>					
	Observation	Mean	Std. Dev	min	Max
Investment/Capital	6,344	0.359	0.422	-0.011	2.276
Investment/Sales	6,344	0.361	10.19	-0.096	745
Investment/Assets	6,344	0.096	0.130	-0.007	1.345
Sales	6,344	24,143	132,205	4	4,242,256
Interest Costs/Debt	6,344	0.044	0.040	0	1.571
Own Capital/Debt	6,344	0.797	3.268	-0.854	167.108
ROA	6,344	0.021	0.092	-3.967	0.596
Cash flow/assets	6,344	0.086	0.096	-3.715	0.682
Profits/sales	6,344	0.010	0.592	-35.777	22.226
Labor cost/sales	6,344	0.205	0.163	0	4.793
Cash flow/sales	6,344	0.096	0.571	-33.444	24.023
Debt/sales	6,344	1.069	7.516	0.021	569.888
<i>B. 3rd Auction</i>					
Investment/Capital	7,177	0.376	0.424	-0.004	2.314
Investment/Sales	7,177	0.155	1.636	-0.008	114.039
Investment/Assets	7,177	0.091	0.120	-0.003	1.625
Sales	7,177	27,126	185,624	2	4,464,834
Interest Costs/Debt	7,177	0.044	0.176	0	11.845
Own Capital/Debt	7,177	0.669	1.514	-0.831	60.975
ROA	7,177	0.022	0.073	-0.939	0.521
Cash flow/assets	7,177	0.089	0.080	-0.890	0.592
Profits/sales	7,177	0.004	0.619	-41.578	2.872
Labor cost/sales	7,177	0.197	0.175	0	6.372
Cash flow/sales	7,177	0.085	0.440	-32.961	3.370
Debt/sales	7,177	0.951	12.416	0	1,038

Notes: Descriptive statistics refer to 1995-2001 and 1996-2001, respectively for the 2nd and 3rd auctions.

Table 2. Full-Ranking Balanced Panel. Dependent Variable: I_t/K_{t-1} . Simple differences and *Diff-in-Diffs* Estimates

Year Treatment	2 nd Auction							3 rd Auction					
	1995	1996	1997	1998	1999	2000	2001	1996	1997	1998	1999	2000	2001
			X	X	X					X	X	X	
<i>A. Simple Differences</i>													
Mean	0.049*	0.049*	0.062**	0.112***	0.011	0.048*	-0.081***	0.031	0.039	0.046*	0.100***	0.067**	0.008
	(0.029)	(0.029)	(0.029)	(0.028)	(0.028)	(0.029)	(0.029)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)
Median	0.051**	0.050**	0.081***	0.059***	0.039*	0.059***	-0.026	-0.005	0.034	0.035	0.094***	0.057**	0.004
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.024)	(0.024)	(0.025)	(0.025)	(0.024)	(0.024)
<i>B. Diff-in-Diffs (Baseline Estimates)</i>													
Mean	-	-	0.013	0.063*	-0.037	-0.001	-0.130***	-	-	0.011	0.064**	0.031	-0.027
			(0.035)	(0.035)	(0.035)	(0.035)	(0.036)			(0.031)	(0.032)	(0.031)	(0.031)
Median	-	-	0.031	0.009	-0.010	0.008	-0.077***	-	-	0.018	0.078***	0.041	-0.011
			(0.027)	(0.026)	(0.026)	(0.027)	(0.027)			(0.029)	(0.029)	(0.029)	(0.029)

Notes: *** (**) [*] denotes significance at the 1% (5%) [10%] level. The sample includes 6,344 observations for the 2nd Auction and 7,177 observations for the 3rd Auction.

Table 3. Full-Ranking Balanced Panel. Alternative Measure of Investment. *Diff-in-Diffs* Estimates

Year Treatment	2 nd Auction					3 rd Auction			
	1997 X	1998 X	1999 X	2000	2001	1998 X	1999 X	2000 X	2001
<i>A. Baseline (Dependent Variable I_t/K_{t-1})</i>									
Mean	0.013 (0.035)	0.063* (0.035)	-0.037 (0.035)	-0.001 (0.035)	-0.130*** (0.036)	0.011 (0.031)	0.064** (0.032)	0.031 (0.031)	-0.027 (0.031)
Median	0.031 (0.027)	0.009 (0.026)	-0.010 (0.026)	0.008 (0.027)	-0.077*** (0.027)	0.018 (0.029)	0.078*** (0.029)	0.041 (0.029)	-0.011 (0.029)
<i>B Dependent Variable I_t/S_{t-1}</i>									
Mean	0.008 (0.010)	0.001 (0.010)	0.003 (0.010)	0.008 (0.010)	-0.025** (0.010)	-0.002 (0.006)	0.021*** (0.007)	0.025*** (0.007)	0.007 (0.007)
Median	0.015** (0.007)	0.015** (0.007)	0.005 (0.007)	0.011* (0.007)	-0.016** (0.007)	0.000 (0.005)	0.023*** (0.005)	0.023*** (0.005)	0.004 (0.005)
<i>C. Dependent Variable I_t/A_{t-1}</i>									
Mean	0.002 (0.007)	0.015* (0.007)	-0.001 (0.007)	0.007 (0.007)	-0.026*** (0.007)	-0.003 (0.006)	0.024*** (0.006)	0.020*** (0.006)	0.008 (0.006)
Median	0.012* (0.006)	0.014** (0.006)	0.006 (0.006)	0.002 (0.006)	-0.017** (0.006)	0.005 (0.005)	0.027*** (0.005)	0.015*** (0.005)	0.004 (0.005)

Notes: *** (**) [*] denotes significance at the 1% (5%) [10%] level. The sample includes 6,344 observations for the 2nd Auction and 7,177 observations for the 3rd Auction.

Table 4. Full-Ranking *Unbalanced* Panel and Estimates with Additional Covariates. Dependent Variable: I_t/K_{t-1} . *Diff-in-Diffs* Estimates

Year Treatment	2 nd Auction					3 rd Auction			
	1997 X	1998 X	1999 X	2000	2001	1998 X	1999 X	2000 X	2001
<i>A. Baseline (Balanced Panel with no Additional Covariates)</i>									
Mean	0.013 (0.035)	0.063* (0.035)	-0.037 (0.035)	-0.001 (0.035)	-0.130*** (0.036)	0.011 (0.031)	0.064** (0.032)	0.031 (0.031)	-0.027 (0.031)
Median	0.031 (0.027)	0.009 (0.026)	-0.010 (0.026)	0.008 (0.027)	-0.077*** (0.027)	0.018 (0.029)	0.078*** (0.029)	0.041 (0.029)	-0.011 (0.029)
<i>B. Unbalanced Panel</i>									
Mean	0.026 (0.036)	0.076** (0.035)	-0.022 (0.035)	0.005 (0.036)	-0.138*** (0.036)	0.059* (0.031)	0.076** (0.031)	0.025 (0.031)	-0.025 (0.032)
Median	0.045 (0.028)	0.034 (0.028)	-0.002 (0.028)	0.024 (0.028)	-0.070** (0.028)	0.054** (0.026)	0.087*** (0.027)	0.035 (0.027)	0.001 (0.027)
<i>C. Estimates with Additional Covariates</i>									
Mean	0.015 (0.035)	0.061* (0.035)	-0.037 (0.035)	0.000 (0.035)	-0.132*** (0.036)	0.012 (0.032)	0.065** (0.032)	0.037 (0.032)	-0.025 (0.032)
Median	0.036 (0.027)	0.029 (0.027)	-0.004 (0.027)	0.026 (0.027)	-0.083*** (0.027)	0.008 (0.026)	0.086*** (0.026)	0.055** (0.026)	-0.008 (0.026)

Notes: *** (**) [*] denotes significance at the 1% (5%) [10%] level. The sample in Panel A includes 6,344 observations for the 2nd Auction and 7,177 observations for the 3rd Auction. The sample in Panel B includes 6,818 observations for the 2nd Auction and 8,834 observations for the 3rd Auction. The sample in Panel C includes 6,344 observations for the 2nd Auction and 7,177 observations for the 3rd Auction.

Table 5. Full-Ranking Balanced Panel. Sample Splits. Dependent Variable: I_t/K_{t-1} . Diff-in-Diffs Estimates

Year Treatment	2 nd Auction					3 rd Auction			
	1997 X	1998 X	1999 X	2000	2001	1998 X	1999 X	2000 X	2001
<i>A. Full Sample (Baseline)</i>									
Mean	0.013 (0.035)	0.063* (0.035)	-0.037 (0.035)	-0.001 (0.035)	-0.130*** (0.036)	0.011 (0.031)	0.064** (0.032)	0.031 (0.031)	-0.027 (0.031)
Median	0.031 (0.027)	0.009 (0.026)	-0.010 (0.026)	0.008 (0.027)	-0.077*** (0.027)	0.018 (0.029)	0.078*** (0.029)	0.041 (0.029)	-0.011 (0.029)
<i>B1. Center North</i>									
Mean	0.016 (0.050)	0.008 (0.050)	-0.023 (0.050)	0.024 (0.050)	-0.125** (0.051)	-0.038 (0.043)	0.017 (0.042)	0.016 (0.043)	-0.016 (0.043)
Median	0.018 (0.039)	-0.022 (0.039)	-0.026 (0.039)	0.008 (0.039)	-0.101** (0.039)	0.004 (0.034)	0.052 (0.034)	0.050 (0.034)	-0.001 (0.034)
<i>B2. South</i>									
Mean	0.095* (0.052)	0.146*** (0.051)	-0.007 (0.051)	0.001 (0.051)	-0.157*** (0.052)	0.086* (0.048)	0.126*** (0.048)	0.063 (0.048)	-0.027 (0.048)
Median	0.080** (0.040)	0.129*** (0.039)	0.017 (0.039)	0.017 (0.040)	-0.092** (0.040)	0.061 (0.038)	0.112*** (0.038)	0.044 (0.038)	-0.020 (0.038)
<i>C1. Small Firms</i>									
Mean	0.010 (0.056)	0.093* (0.056)	-0.043 (0.057)	-0.044 (0.058)	-0.163*** (0.060)	-0.007 (0.054)	0.126** (0.055)	0.085 (0.056)	-0.005 (0.057)
Median	0.078* (0.040)	0.104** (0.040)	0.011 (0.040)	-0.011 (0.041)	-0.107** (0.042)	0.057 (0.038)	0.073* (0.039)	0.099** (0.040)	-0.012 (0.040)
<i>C2. Large Firms</i>									
Mean	0.024 (0.046)	0.046 (0.045)	-0.039 (0.044)	0.019 (0.045)	-0.116*** (0.045)	0.022 (0.038)	0.029 (0.038)	-0.005 (0.037)	-0.036 (0.037)
Median	0.014 (0.034)	-0.032 (0.033)	-0.028 (0.033)	-0.002 (0.033)	-0.089*** (0.033)	-0.017 (0.040)	0.063 (0.040)	-0.011 (0.040)	-0.017 (0.039)

Table 5 (cont). Full-Ranking Balanced Panel. Sample Splits. Dependent Variable: I_t/K_{t-1} . Diff-in-Diffs Estimates

Year Treatment	2 nd Auction					3 rd Auction			
	1997 X	1998 X	1999 X	2000	2001	1998 X	1999 X	2000 X	2001
<i>D1. Low Grant Coverage Ratio</i>									
Mean	-0.051 (0.039)	-0.018 (0.039)	-0.060 (0.039)	0.008 (0.040)	-0.137** (0.040)	-0.014 (0.040)	-0.010 (0.039)	0.032 (0.039)	-0.041 (0.039)
Median	-0.025 (0.030)	-0.031 (0.030)	-0.053* (0.030)	-0.005 (0.030)	-0.086** (0.030)	-0.024 (0.037)	0.045 (0.037)	0.034 (0.037)	-0.012 (0.036)
<i>D2. High Coverage Ratio</i>									
Mean	0.082* (0.042)	0.146*** (0.041)	-0.015 (0.041)	-0.009 (0.042)	-0.123** (0.042)	0.037 (0.042)	0.147** (0.042)	0.030 (0.042)	-0.012 (0.042)
Median	0.085** (0.035)	0.116*** (0.034)	0.033 (0.034)	0.026 (0.035)	-0.072** (0.036)	0.062 (0.038)	0.137*** (0.038)	0.049 (0.038)	0.008 (0.038)

Notes: *** (**) [*] denotes significance at the 1% (5%) [10%] level. The sample in Panel A sample includes 6,344 observations for the 2nd Auction and 7,177 observations for the 3rd Auction. The sample in Panel B1 includes 3,590 observations for the 2nd Auction and 2,935 observations for the 3rd Auction. The sample in Panel C2 includes 3,409 observations for the 2nd Auction and 3,585 observations for the 3rd Auction. The sample in Panel C1 includes 2,617 observations for the 2nd Auction and 2,951 observations for the 3rd Auction. The sample in Panel D2 includes 3,727 observations for the 2nd Auction and 4,226 observations for the 3rd Auction. The sample in Panel D1 includes 4,324 observations for the 2nd Auction and 6,070 observations for the 3rd Auction. The sample in Panel D2 includes 4,251 observations for the 2nd Auction and 5,990 observations for the 3rd Auction. Center North includes the following regions: Valle d'Aosta, Piemonte, Lombardia, Liguria, Trentino Alto Adige, Veneto, Friuli Venezia Giulia, Emilia Romagna, Marche, Umbria, Toscana, and Lazio. South includes the following regions: Abruzzo, Molise, Campania, Puglia, Basilicata, Calabria, Sicilia, and Sardegna. Small (large) firms are those below (above) the median sales. Low (high) grant coverage ratio firms are those below (above) the median grant coverage ratio.

Table 6. Firms in the Neighborhood of the L488 Cutoff. Balanced Panel. Dependent Variable: I_t/K_{t-1} . *Diff-in-Diffs* Estimates

Year Treatment	2 nd Auction					3 rd Auction			
	1997 X	1998 X	1999 X	2000	2001	1998 X	1999 X	2000 X	2001
<i>A. Full Ranking (Baseline)</i>									
Mean	0.013 (0.035)	0.063* (0.035)	-0.037 (0.035)	-0.001 (0.035)	-0.130*** (0.036)	0.011 (0.031)	0.064** (0.032)	0.031 (0.031)	-0.027 (0.031)
Median	0.031 (0.027)	0.009 (0.026)	-0.010 (0.026)	0.008 (0.027)	-0.077*** (0.027)	0.018 (0.029)	0.078*** (0.029)	0.041 (0.029)	-0.011 (0.029)
<i>B. 30% Cutoff Neighborhood</i>									
Mean	0.015 (0.066)	0.119* (0.065)	-0.060 (0.065)	-0.025 (0.066)	-0.238*** (0.066)	0.026 (0.048)	0.050 (0.048)	0.043 (0.048)	-0.070 (0.048)
Median	0.013 (0.056)	0.107* (0.055)	-0.009 (0.054)	-0.034 (0.055)	-0.120** (0.056)	-0.002 (0.037)	0.093** (0.037)	0.027 (0.037)	-0.036 (0.037)
<i>C. 10% Cutoff Neighborhood</i>									
Mean	-0.029 (0.079)	0.104 (0.079)	-0.070 (0.078)	-0.093 (0.080)	-0.223*** (0.080)	-0.011 (0.072)	-0.021 (0.072)	0.023 (0.072)	-0.057 (0.072)
Median	0.092 (0.062)	0.135** (0.061)	0.083 (0.061)	0.105* (0.062)	-0.041 (0.062)	-0.060 (0.060)	-0.017 (0.060)	-0.088 (0.062)	-0.104* (0.061)

Notes: *** (**) [*] denotes significance at the 1% (5%) [10%] level. The sample in Panel A includes 6,344 observations for the 2nd Auction and 7,177 observations for the 3rd Auction. The sample in Panel B includes 1,914 observations for the 2nd Auction and 2,901 observations for the 3rd Auction. The sample in Panel C includes 1,186 observations for the 2nd Auction and 1,264 observations for the 3rd Auction.

Table 7. Firms with Same Pattern of I_t/K_{t-1} Growth Rates. 1994 - 2001 Balanced Panel.
Dependent Variable: I_t/K_{t-1} . *Diff-in-Diffs* Estimates

Year Treatment	3 rd Auction			
	1998 X	1999 X	2000 X	2001
<i>A. 1994 2001 Full Ranking</i>				
Mean	-0.043 (0.062)	0.198*** (0.062)	0.045 (0.062)	-0.059 (0.062)
Median	-0.021 (0.029)	0.069** (0.030)	0.003 (0.030)	-0.047 (0.029)
<i>B. Wide Bands</i>				
Mean	-0.056 (0.073)	0.144** (0.073)	-0.095 (0.073)	-0.164** (0.073)
Median	-0.124** (0.061)	0.012 (0.061)	-0.051 (0.060)	-0.058 (0.060)
<i>C. Narrow Bands</i>				
Mean	-0.082 (0.092)	0.213** (0.092)	0.004 (0.092)	-0.136 (0.092)
Median	-0.092 (0.067)	0.015 (0.067)	-0.096 (0.067)	-0.074 (0.067)

Notes: *** (**) [*] denotes significance at the 1% (5%) [10%] level. The sample in Panel A includes 5,436 observations. The sample in Panel B includes 2,808 observations for the mean and 1,945 observations for the median. The sample in Section C includes 2,184 observations for the mean and 1,831 observations for the median.

Table 8. Selected Regions and Sectors. Balanced Panel. Dependent Variable: I_t/K_{t-1} . *Diff-in-Diffs* Estimates

Year Treatment	2 nd Auction					3 rd Auction			
	1997 X	1998 X	1999 X	2000	2001	1998 X	1999 X	2000 X	2001
<i>A. All Region Sample (Baseline)</i>									
Mean	0.013 (0.035)	0.063* (0.035)	-0.037 (0.035)	-0.001 (0.035)	-0.130*** (0.036)	0.011 (0.031)	0.064** (0.032)	0.031 (0.031)	-0.027 (0.031)
Median	0.031 (0.027)	0.009 (0.026)	-0.010 (0.026)	0.008 (0.027)	-0.077*** (0.027)	0.018 (0.029)	0.078*** (0.029)	0.041 (0.029)	-0.011 (0.029)
<i>B1. Campania</i>									
Mean	-0.063 (0.091)	0.115 (0.089)	0.011 (0.088)	-0.096 (0.089)	-0.288*** (0.090)	0.085 (0.076)	0.112 (0.079)	0.072 (0.077)	0.007 (0.077)
Median	0.105* (0.059)	0.157*** (0.057)	0.042 (0.057)	-0.041 (0.057)	-0.116** (0.058)	0.034 (0.056)	0.092 (0.057)	0.022 (0.056)	-0.033 (0.056)
<i>B2. Puglia</i>									
Mean	-0.118 (0.106)	0.123 (0.105)	-0.102 (0.104)	-0.050 (0.109)	-0.201* (0.109)	0.126 (0.092)	0.116 (0.092)	0.123 (0.094)	-0.088 (0.095)
Median	0.044 (0.086)	0.197** (0.085)	-0.023 (0.084)	-0.095 (0.088)	-0.132 (0.088)	0.135 (0.104)	0.141 (0.103)	0.165 (0.106)	0.002 (0.107)
<i>C1. Textiles, Textile Products, Leather and Footwear, and Wood</i>									
Mean	-0.057 (0.092)	0.022 (0.090)	0.014 (0.090)	0.021 (0.092)	-0.094 (0.095)	0.102 (0.088)	0.140 (0.088)	0.116 (0.088)	0.044 (0.089)
Median	0.188** (0.078)	0.025 (0.077)	0.106 (0.077)	0.154** (0.078)	0.027 (0.081)	0.088 (0.076)	0.167** (0.076)	0.183** (0.076)	0.043 (0.077)
<i>C2. Basic Metals and Metal Products</i>									
Mean	0.066 (0.087)	0.059 (0.087)	-0.061 (0.087)	0.112 (0.087)	-0.193** (0.088)	-0.095 (0.074)	-0.128* (0.074)	-0.076 (0.075)	-0.150** (0.075)
Median	0.131* (0.071)	0.097 (0.070)	0.011 (0.070)	0.061 (0.071)	-0.033 (0.072)	-0.027 (0.055)	-0.080 (0.055)	0.002 (0.055)	-0.071 (0.055)

Notes: *** (**) [*] denotes significance at the 1% (5%) [10%] level. The sample in Panel A includes 6,344 observations for the 2nd Auction and 7,177 observations for the 3rd Auction. The sample in Panel B1 includes 1,095 observations for the 2nd Auction and 1,182 observations for the 3rd Auction. The sample in Panel B2 includes 808 observations for the 2nd Auction and 875 observations for the 3rd Auction. The sample in Panel C1 includes 1,093 observations for the 2nd Auction and 1,169 observations for the 3rd Auction. The sample in Panel C2 includes 1,115 observations for the 2nd Auction and 1,243 observations for the 3rd Auction.

Table 9. Full-Ranking Balanced Panel. Dependent Variables: Labor costs/sales, profits/sales, cash flow/sales, and debts/sales. *Diff-in-Diffs* Estimates

Year Treatment	2 nd Auction					3 rd Auction			
	1997 X	1998 X	1999 X	2000	2001	1998 X	1999 X	2000 X	2001
<i>A. Labor cost/sales</i>									
Mean	0.011 (0.013)	-0.001 (0.013)	0.008 (0.013)	-0.002 (0.013)	0.010 (0.014)	0.023* (0.013)	0.017 (0.013)	0.016 (0.013)	0.012 (0.013)
Median	-0.012 (0.011)	-0.009 (0.011)	0.002 (0.011)	-0.002 (0.012)	0.000 (0.011)	0.045 (0.011)	0.009 (0.011)	0.012 (0.011)	0.002 (0.011)
<i>B. Profits/sales</i>									
Mean	0.048 (0.050)	0.035 (0.050)	0.043 (0.049)	0.019 (0.050)	0.137*** (0.050)	0.006 (0.047)	0.014 (0.047)	0.008 (0.047)	0.019 (0.047)
Median	-0.001 (0.002)	-0.002 (0.002)	-0.003 (0.002)	-0.007 (0.002)	-0.007 (0.002)	0.002 (0.001)	0.004** (0.002)	0.001 (0.002)	0.002 (0.002)
<i>C. Cash flow/sales</i>									
Mean	0.060 (0.048)	0.036 (0.048)	0.043 (0.048)	0.027 (0.048)	0.134*** (0.049)	0.004 (0.033)	0.011 (0.033)	0.010 (0.033)	0.013 (0.033)
Median	0.002 (0.007)	0.006 (0.007)	-0.001 (0.007)	0.002 (0.007)	-0.001 (0.007)	0.004 (0.006)	0.008 (0.006)	0.009 (0.006)	0.012* (0.006)
<i>D. Debts/sales</i>									
Mean	0.144 (0.641)	0.167 (0.635)	0.030 (0.634)	-0.106 (0.642)	-1.972*** (0.646)	-1.144 (0.941)	-1.352 (0.945)	-1.323 (0.942)	-1.436 (0.942)
Median	-0.009 (0.041)	-0.021 (0.041)	-0.029 (0.041)	0.019 (0.041)	-0.066 (0.042)	-0.0018 (0.029)	-0.007 (0.030)	-0.002 (0.029)	-0.004 (0.029)

Notes: *** (**) [*] denotes significance at the 1% (5%) [10%] level. The sample includes 6.344 observations for the 2nd Auction and 7.177 observations for the 3rd Auction.

APPENDIX
Mean and median differences between treated and non-treated firms (various samples)

	2 nd Auction		3 rd Auction	
	Mean diff	Median diff	Mean diff	Median diff
<i>A. Full Ranking Balance Panel</i>				
Sales (thousand Euro)	17,339** (7,067)	1,141*** (310)	41,867*** (11,373)	392 (324)
Own Capital/Debt	0.026 (0.102)	0.034 (0.032)	0.035 (0.063)	0.065** (0.031)
Interest Costs/Debt	0.000 (0.003)	-0.002 (0.004)	-0.004* (0.003)	-0.003 (0.003)
Cash flow/Assets	0.017*** (0.006)	0.017** (0.007)	0.010** (0.005)	0.011** (0.005)
ROA	0.013** (0.006)	0.009*** (0.003)	0.011** (0.005)	0.007** (0.002)
<i>B. 30% Cutoff Neighborhood</i>				
Sales (thousand Euro)	8,681 (11,310)	961** (372)	15,604 (9,813)	110 (468)
Own Capital/Debt	-0.036 (0.087)	0.014 (0.051)	0.042 (0.116)	0.084 (0.068)
Interest Costs/Debt	-0.002 (0.006)	-0.006 (0.007)	-0.001 (0.004)	0.000 (0.004)
Cash flow/Assets	0.016 (0.010)	0.009 (0.009)	0.021*** (0.007)	0.021*** (0.007)
ROA	0.011 (0.009)	0.003 (0.004)	0.016** (0.007)	0.010** (0.004)
<i>C. Firms with Same Pattern of I_t/K_{t-1} Growth Rates. 1994 2001 Balanced Panel.</i>				
Sales (thousand Euro)	-	-	48,787* (26,353)	-1,728* (983.60)
Own Capital/Debt	-	-	0.044 (0.079)	0.040 (0.109)
Interest Costs/Debt	-	-	-0.005 (0.004)	-0.004 (0.005)
Cash flow/Assets	-	-	0.006 (0.008)	0.007 (0.013)
ROA	-	-	0.008 (0.008)	0.007 (0.009)

Notes: *** (**) [*] denotes significance at the 1% (5%) [10%] level. Mean and median differences are calculated with reference to the first year of the pre-intervention period (1995 for the 2nd Auction and 1996 for 3rd Auction). The sample in Panel A includes 883 observations for 2nd auction and 1,195 for 3rd Auction. The sample in Panel B includes 274 observations for 2nd Auction and 483 for 3rd Auction. The sample in Panel C includes 473 observations for mean and 328 for median.