

**EVALUATING THE IMPACT OF INVESTMENT INCENTIVES:  
THE CASE OF ITALY'S LAW 488/1992<sup>(0)</sup>**

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*Abstract*

Italy's Law 488/1992 allows firms willing to invest in lagging areas to receive a public subsidy. By comparing subsidized firms with firms with rejected applications, this paper evaluates whether the program made investments possible that otherwise would not have been made. We find evidence that financed firms brought forward investment projects originally planned for the post-intervention period to take advantage of the incentives. We also find some support that subsidized firms may have taken some of the investment opportunities that unsubsidized firms would have exploited in the absence of incentives.

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## 1. Introduction

Financial assistance to manufacturing industry channeled through Law 488/1992 (Law 488) 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 funds distributed to industrial firms amounted to €16 billion and involved 27,846 projects, mainly in the southern regions. Law 488 allows firms willing to invest in lagging areas to receive a public subsidy that covers a fraction of the investment outlays. The incentives are awarded through competitive auctions according to pre-determined criteria, such as the proportion of firms' equity invested in the project; the number of jobs involved and the proportion of assistance sought.<sup>1</sup>

The extent to which investment incentives have an economic payoff has been at the forefront of economic research for decades (see, for instance, Hall and Jorgenson [22] and King [24]). Moreover, the role of incentives in reducing territorial disparities is a central topic in regional science (Faini and Schiantarelli [16], Harris and Trainor [23], and Gabe and Karybill [17]; see, also, Glaeser [18]). Although this literature is voluminous, there is no agreement on the effectiveness of investment incentives. Evaluating the effects of government sponsored projects, one has to face the question of what would have happened without the subsidies. That is, evaluating an incentive program is a counterfactual exercise. Since neither the subsidized firms nor the unsubsidized firms can be considered random draws, the challenge is to construct a valid control group. Moreover, to evaluate whether Law 488 made investments possible that otherwise would not have been made, two more issues need to be tackled. First, one has to analyze the extent to which additional investments have been triggered by time-substitution (Abel [1], Adda and Cooper [2], and Auerbach and Hines [5]). To take advantage of the incentives, firms could have brought forward investment projects originally planned for the post-intervention period. Second, one has to study the role of cross-sectional substitution (Klette et al [25] and Lee [28]). Subsidized firms may have taken some of the investment opportunities that unsubsidized firms would have exploited in the 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 Law 488. We compare a group of financed firms with a group of firms that applied for the incentives but were not financed since they scored low in the Law 488 ranking. As suggested by Brown et al [10], the main virtue of the rejected application group is that it is very similar to the treatment group in terms of its

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<sup>1</sup> 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 Law 488 are now being implemented in many EU countries. See Braunerhjelm et al [8] and Yuill et al [33].

characteristics. While the rejected firms are hardly a random group, they may be as close to a control group as is possible. We further check the reliability of the comparison group in two respects. First, we implement an intuitive version of the regression discontinuity design (Campbell [11]) and contrast financed firms just above the financing threshold in the Law 488 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 firms for the which the deviation with respect to the average investment growth rate of the treated firms is minimized. A central focus of the paper is to evaluate to what extent the impact of Law 488 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 in their industrial distance.

When compared with the pool of firms that requested Law 488 grants without being financed, we find that financed firms initially increased their investments. The increase takes place in the second year of the treatment. However, we also find evidence of intertemporal substitution. In the years following the program, the investment activity of the financed firms slows down significantly compared to that of the rejected firms. Finally, the impact of Law 488 is more pronounced when the size of the market where the firms compete is small or when the firms are close in their industrial distance. In our view, this suggests that financed firms might have crowded out their non-financed competitors. Overall, these results cast some doubts on the efficacy of Law 488.

The paper is structured as follows. We start in Section 2 with a detailed description of Law 488. 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 Law 488**

This section explains the main features of Law 488 (see: *Gazzetta Ufficiale della Repubblica Italiana no. 299, 21 December 1992*). More details can be found in IPI [26] and Bronzini et al [9].

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 five pre-determined criteria and award offers are only made if funding is

available. Incentives are restricted to areas designated as Objective 1, 2 or 5b<sup>2</sup> 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 amounts to 48.9 percent of the national population.<sup>3</sup> Eligibility for assistance is restricted to manufacturing and extractive firms.<sup>4,5</sup> The investment projects covered by Law 488 are the following: setting-up, extension (defined as projects that increases the capacity of the firm to produce its existing products or introduce 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 who 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 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. The maximum award rates differ from the actual award rates offered since, as shown below, the selection mechanism favors 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.<sup>6</sup>

Award offers are made on the basis of competitive auctions. Applications are ranked by eligible area on the basis of the following five criteria:<sup>7</sup> (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

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<sup>2</sup> Objective 1 refers to the regions suffering from general underdevelopment, as reflected in GDP per capita that is less than 75% of the EU average. Objective 2 is related to regions suffering from 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 a low level of agricultural income.

<sup>3</sup> 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.

<sup>4</sup> In addition, selected producer services are also eligible. They are not included, however, in the evaluation analysis reported here.

<sup>5</sup> Since 2001 the Law 488 scheme has been extended through separate auctions to the tourism and transport sectors.

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

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

allocated to the area allows. If the application is successful, the rate of award offered is the rate requested in the application.

Law 488 auctions are conducted on a yearly basis. Four Law 488 auctions were concluded before 2001, which represents the last year for which financial-statement data are available (see Section 3). Assistance is administered by the Italian Ministry for Industry. The timing of the assistance is precisely defined (see Figure 1). Applications are submitted within a specific deadline. Within four months of the deadline, the Ministry for Industry publishes the rankings. The law requires that firms awarded assistance receive the first annual installment within two months.<sup>8</sup> 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.

For the purpose of our evaluation exercise, two important aspects of the Law 488 scheme have to be kept in mind. First, Law 488 does not require that the investment project is 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 statements of the subsidized firm may not reveal the investment activity triggered by the program, the impact of Law 488 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 correlated with the allocation of Law 488 funding. For instance, if the non-treated 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 the Law 488 regulation minimizes the scope of this bias: financing under this program cannot be combined with other sources of public financing. In particular, firms applying for Law 488 money have to give up other public subsidies. 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. Therefore, an applicant firm has to give up other financial assistance without any guarantee that it will actually receive the Law 488 grant.<sup>9</sup>

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<sup>8</sup> As for the timing of the first installment, there have nonetheless been delays. In particular, there was a one-month delay in both the 2<sup>nd</sup> and the 3<sup>rd</sup> auctions. Moreover, as explained in the text, for the 4<sup>th</sup> auction there were substantial and very erratic delays.

<sup>9</sup> The ban on combining Law 488 money with other incentive programs does not apply to 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 it to influence the evaluation of Law 488.

### 3. Data and Empirical Strategy

We use the official Law 488 Dataset of the Ministry for Industry. This data set records all the firms that have applied for the incentives, both financed and non-financed. It 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 Law 488 data set lacks information on investment, which is the target of the law 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 large proportion of 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. However, there are also drawbacks in using the CERVED data. First, this data set is skewed toward larger firms since it collects financial statements only from corporations. Second, there are frequent misprints as regard the firm identifiers that we use to link CERVED data to the Law 488 Dataset (see Appendix 1).

The time pattern of the linked data set is described in Figure 2. Four Law 488 auctions were held in the period 1993-2001. For these auctions the treatment started (with the 1<sup>st</sup> installment) and finished (with the 3<sup>rd</sup> installment) within the time-window provided by the CERVED data. We focus below on the 2<sup>nd</sup> and 3<sup>rd</sup> 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 post-intervention observations. The 1<sup>st</sup> auction has been excluded because it included a transitory clause that allowed firms not eligible under Law 488 to be financed as well.<sup>10</sup> We have also excluded the 4<sup>th</sup> auction. The reason is that disbursements under this auction were highly irregular.<sup>11</sup> However, since the CERVED data end in 2001, the exclusion of the 4<sup>th</sup> auction is not a great loss.

By linking the Law 488 data set with the CERVED data set, we reconstruct *uninterrupted* financial statements from 1994 to 2001 (1995-2001) for 1,008 (1,329) firms that participated in the 2<sup>nd</sup> (3<sup>rd</sup>) auction. This sample is labeled *Full Ranking Balanced Sample* and represents our main sample. Appendix 1 describes the sample construction in detail. Our data cover a substantial proportion of the overall financing. The grants received by the firms included in our full ranking

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<sup>10</sup> 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 nineties when it was decided that they would have been covered with the 1<sup>st</sup> auction of the Law 488 allocations.

<sup>11</sup> For instance, even though the 1<sup>st</sup> installment was supposed to be received by May 1999, many firms (40% in the Law 488 Dataset) did not receive it until 2000.

balanced panel represent €394 million (21% of the total Law 488 financing) and €417 million (27% of the total), respectively for the 2<sup>nd</sup> and 3<sup>rd</sup> auction. Table 1 shows the main descriptive statistics for the variables used in the paper, distinguished by auction. Our main dependent variable is investment as a percentage of capital.

We seek to establish the role of investment subsidies by comparing investments between subsidized firms and unsubsidized firms.<sup>12</sup> Since the two groups cannot 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 Law 488. 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 Law 488 ranking (*untreated*). Thus, our main comparison group comprises firms with rejected applications. In the full ranking balanced and unbalanced panels, this group includes all the non-financed firms, irrespective of their Law 488 ranking.

If incentives were awarded randomly among the firms participating in the auction, 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 the belief that the untreated group provides the correct missing counterfactual. Unfortunately, financed and rejected applicant firms can hardly be considered random groups. However, as suggested by Brown et al [10], the rejected application pool may be as close to a control group as is possible: it is very similar to the treatment group in terms of its characteristics and includes eligible firms that were interested in receiving the grant.<sup>13</sup> To investigate this argument, in Appendix 2 Panel A we present 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 2<sup>nd</sup> auction and 1996 for the 3<sup>rd</sup> auction.<sup>14</sup> 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 with regard to some unobservable characteristics, could clearly invalidate our identification hypothesis. This is why we also

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<sup>12</sup> A problem with this strategy is that firms can apply for subsidies under different auctions. Since firms can be receiving Law 488 money under more than one auction, we only keep firms that have received the grant once. Similarly, for each auction we exclude from the rejected application pool, firms that have nonetheless won the award under any other auction during the CERVED time-window.

<sup>13</sup> The peculiar scheme envisaged by Law 488 to allocate the subsidies provides additional arguments. Many scholars (see: Cannari and Chiri [12], Del Monte and Giannola [14], and Scalera and Zazzaro [31]) have argued that the Law 488 auction mechanism is very ineffective in discriminating among applicant 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 applicant firms might have been quasi-random.

<sup>14</sup> In any case, using 1996 and 1997 instead would have made no difference.

provide estimates of the impact of treatment for additional control groups. We substantiate our results in two ways. First, we compare only firms that are in the middle of the ranking. We implement an intuitive version of the regression discontinuity design (Campbell [11]) and compare financed firms just above the financing threshold in the Law 488 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 is provided by the untreated firms that have Law 488 scores that are similar to the treated ones.<sup>15</sup> Second, we construct an ad-hoc comparison group that mirrors the time-series pattern of the treated group before the program was launched. 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 would invalidate our estimates. Therefore, we use as counterfactual a group that comprises firms for which the deviation respect to the average investment growth rate of the treated firms is minimized. Panel B and Panel C in Appendix 2 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 time not significant.

Estimating the impact of Law 488 amounts to gauging the extent to which the investments triggered by the incentives are *additional*. Would the grant element make investments possible that otherwise would not have been undertaken? Even if provided with the correct counterfactual, it is difficult to evaluate additionality. A first problem is *time substitution*. Because of the availability of subsidies, firms may have brought forward projects originally planned for the post-intervention period. As shown by Abel [1], 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 [5] and Adda and Cooper [2]). In short, a potential effect of Law 488 could have been to boost investment during the period in which the incentive scheme was in place, at the cost of reducing investment subsequently. We deal with this problem by using a long-time series of post-intervention observations. In particular, for the 2<sup>nd</sup> auction, which was held in 1997, we are able to study investment behavior up to 2001, which is two years after the end of the program. 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. Subsidized and rejected applicant firms will both act in expectation of the subsidy and therefore the effect is differentiated away.

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<sup>15</sup> A similar empirical design is implemented in Greenstone and Moretti [20].



Time substitution is not the only obstacle to assessing the impact of the investment incentives. To be sure, there could be indirect effects of Law 488. *Cross sectional substitution* implies that subsidized firms take some of the investment opportunities that unsubsidized firms would have exploited in absence of Law 488 (see Harris and Trainor [23] and Lee [28]). This might also occur because of general equilibrium effects. Law 488 may change the price of capital in a region as a whole if it affects a substantial number of firms. For instance, Goolsbee [19] shows that investment incentives have little impact because much of the benefit does not go so much to investing firms as to suppliers of capital through higher prices. Cross sectional substitution is particularly important when the market in which the firms compete is small and when the firms are close in their industrial distance (see: Rosenthal and Strange [30]). 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 within the same area and within the same sector.

We will be running simple regressions of the following form:

$$(1) \quad y_{it} = \alpha \text{Law 488}_i + \sum_t \beta_t \text{YEAR}_t + \sum_t \gamma_t (\text{Law 488}_i * \text{POST}_t) + Z_{it} \delta + \varepsilon_{it}$$

where  $y_{it}$  is the outcome variable, investment of firm  $i$  in year  $t$ ,  $\text{Law 488}_i$  is a dummy variable indicating whether the firm has received the Law 488 grant,  $\text{YEAR}_t$  denotes time dummies,  $\text{POST}_t$  is a series of dummies for each of the years after the introduction of the program, and  $Z_{it}$  is a vector of covariates. The coefficients we are interested in are the  $\gamma_t$ : the impact of the Law 488 on the treated evaluated overtime.

## 4. Results

### 4.1 Baseline Results

We start by comparing investment for treated and untreated firms belonging to the full ranking balanced panel. The results are reported in Table 2, which describes the 2<sup>nd</sup> and the 3<sup>rd</sup> auctions separately. For each auction, Panel A reports simple differences while Panel B describes the estimates of the coefficients we are interested in  $\gamma_t$ .<sup>16</sup>

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<sup>16</sup> For each auction we use only two yearly pre-treatment observations (see: Gruber [21]). Therefore, the estimation period is 1995-2001 and 1996-2001, respectively for the two auctions.

As for the 2<sup>nd</sup> auction, simple differences highlight that investment was 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 marked by higher mean and median investment even before the program. To measure the excess investment for the treated firms attributable to Law 488, 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 statistically significant *average* effect of the program.<sup>17</sup> During that year, simple differences indicate that the investment of treated firms outperformed that of the untreated firms by 0.11, that is 35 percentage points of the investment of the rejected applicant firms. Diff-in-diffs estimates of the mean impact suggest that roughly half of this increase can be considered as the causal effect of Law 488. To provide some robustness with respect to influential observations, we also report the results of a quantile (median) regression (see, Lamont [27]). Median diff-in-diffs results are 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 investment treated firms did 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 2<sup>nd</sup> auction, the investment of the treated firms is lower than that of the untreated group to a significant extent (0.13, or 46% of the average investment of the untreated firms according to the diff-in-diffs estimates). The circumstance that financed firms show a lower accumulation in 2001 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-span. With a long time-span, 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 in the tables) that at the end of 2001 the cumulative effect of Law 488 was statistically not different from zero.

The effectiveness of the program is more evident in the 3<sup>rd</sup> auction, for which simple pre-intervention differences are not significant and there is a significant causal effect of the program in the second year of the treatment both for the mean (the causal effect of the program is equal to 0.06, roughly equal to 20% of the investment of the untreated) and for the median (0.08, 42% of the investment of the control group). Notice that the results of the 2<sup>nd</sup> and 3<sup>rd</sup> auctions cannot be directly compared, because the grants were allocated on the basis of slightly different rules: in the 3<sup>rd</sup> auction two

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<sup>17</sup> The standard errors reported in the tables will differ only marginally if we allow clustering of the residual at sector level. We also run the cluster correction at sector level by region and size by region levels, again, with minor modifications.

additional criteria were introduced, respectively regional priorities and environmental impact. As for the time substitution, our estimates indicate that in 2001 (one year after the end of the treatment) the difference between the subsidized and rejected application groups is negative. It is, however, not statistically significant. Unfortunately, the CERVED data end in 2001, so that it may not be possible to fully disentangle substitution effects, which in the 2<sup>nd</sup> auction occur two years after the end of the treatment. As for the cumulative effect (not reported in the tables), we find that at the end of 2001 financed firms outperformed rejected application firms by roughly 10% (the diff-in-diffs coefficient is also highly significant).

#### 4.2 Robustness

We adhere to standard practice by using gross investment as a percentage of the capital stock at the beginning of the period as the dependent variable (see: Cummins et al [13] and Lamont [27]). However, there are two potential sources of bias with this variable. First, as described in Section 2, the investment projects covered by Law 488 include, in addition to start-ups and extensions, modernizations, restructurings and reconversions. To the extent that Law 488 finances modernization, restructuring and reconversion projects (see, Driehuis and van den Noord [15]) treated firms might have accelerated the renovation of their capital stock by selling old assets and buying new ones. As far as asset sales occur in the financial year preceding the financial year in which the investment is recorded, this will give an upward bias to the estimated impact of the program. Second, another concern is that physical capital in the CERVED data set is stated *net* of depreciation. Since in Italy capital amortization is allowed on a straight-line basis, older firms are likely to show a smaller net capital stock. To the extent that Law 488 tends to favor larger and established firms (see: Appendix 2) this will also lead to a positive bias. We deal with this problem by using two alternative dependent variables.<sup>18</sup> Panel B of Table 3 shows the diff-in-diffs analogues of Table 2 where investment as a percentage of capital is substituted by investment as a percentage of sales (measured at the beginning of the period). Panel C of Table 3 reports the results obtained when we use investment as a percentage of (pre-dated) assets as the dependent variable. Overall, the results confirm our previous findings. We find evidence of a positive effect of Law 488 in the second year of the treatment in both auctions (with a higher significance of the estimates in the 3<sup>rd</sup> auction); as for the 2<sup>nd</sup> auction, we still find evidence of a strong substitution effect in 2001. More importantly, we do not find evidence that our results were upwardly biased: the impact of the program appears more pronounced with the alternative dependent variables.

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<sup>18</sup> We only report a sub-sample of the robustness checks performed to verify the sensitivity of our results to different dependent variables. In particular, we also use the following alternative dependent variables: investment as a percentage of sales at the *end* of the period, investment as a percentage of assets at the *end* of the period, and *net* investment as a percentage of capital. The results were remarkably similar to those reported in the text.

We are worried that our balanced panel could be affected by survivorship bias. In particular, there could be a differential loss of financial-statement availability for treated and untreated firms (see: Pakes and Ericson [29]). 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 firm goes out of business. In these circumstances, the estimates from the balanced panel could be negatively biased because the marginal unsubsidized firms, which are likely to display the lowest accumulation rates, are no longer included in the comparison sample. To tackle this issue we use a different sample, which is labeled *unbalanced panel*. For this sample we do not require the availability of the financial accounts over the entire period. The unbalanced panel includes firms that have as a minimum one pre-intervention and one post-intervention set of financial-statement data. Thus, to the extent that non-financed firms go out of business in the financial years following that of the first installment,<sup>19</sup> the unbalanced panel would include such firms. Table 4 Panel B describes the results. For the unbalanced panel we find higher coefficients in the early 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 incentives have increased the probability of staying in business.

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, treated firms become more productive as a result of Law 488 financing. Another source of violation is the availability of an external source of finance. As shown by Banerjee and Duflo [6], 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-Law 488 external financing to the effects of the program. Therefore, we check the robustness of our estimates to the inclusion of a number of covariates at the firm level (Table 4 Panel C). We include sales, ROA, a measure of leverage (equity capital as a percentage of debt), a measure of the interest rate (interest costs as a percentage of debt) and a measure of internal funds (cash flow as a percentage of assets). The results suggest that the role of omitted time-varying variables is modest.

To further corroborate our findings we estimate the impact of the program when the full ranking balanced panel is split along some interesting dimension. First, we split the sample by the location of the investment project. As explained in Section 2, the financing is not restricted to the lagging areas of the South of Italy. However, one might expect the efficacy of the grants might to vary across territories. For instance, to the extent that lagging areas are characterized by higher marginal productivity of capital, a given amount of grant might trigger more investment in the southern regions

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<sup>19</sup> If a firm goes out of business in the financial year of the first installment, it cannot be included in the sample because

than in their northern counterparts. Our results in Table 5 Panel B support this view. Second, Law 488 can have very different effects on big, cash-rich firms compared with small cash-strapped 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 firm 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 Section 2, Law 488 is designed in such a way that the amount of the grants differs among firms. Among the subsidized firms of the full ranking balanced panel the grant coverage ratio (defined as the grant as a percentage of the investment) ranges from 1% to 80%. By splitting the sample at the median grant coverage ratio, Table 5 Panel D verifies 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.<sup>20</sup>

#### *4.3 Alternative comparisons*

In the case of a randomized experiment, treatment and control groups are identical for large samples. Even in the case of a non-randomized experiment like the Law 488 investment incentives, the closer the treatment and control groups, the more convincing is the diff-in-diffs approach. In what follows, we implement this idea by comparing treated and untreated groups that are supposed to be more similar than their counterparts in the full ranking sample. We follow two distinct routes.

First, recall that the Law 488 scheme envisages that all the applicant firms are ranked at regional level in a decreasing order given by the normalized single score. Funding is then allocated starting at 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 is to compare 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. The idea here is that, regardless of the actual award mechanism, the correct counterfactual is more likely to be provided by the untreated firms that have Law 488 scores that are similar to the treated ones.<sup>21</sup> This represents an intuitive version of the regression discontinuity design (see: Campbell [11] and, for convincing applications, Angrist and Pischke [4] and van der Klaauw [32]). 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, the results differ

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of the lack of post-intervention data.

<sup>20</sup> Because of the lack of independence, sample-split experiments are clearly only illustrative. Southern firms are smaller and receive higher grants.

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 Law 488 financing is detected in the second year of the treatment and the rise in investment comes at the expense 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 by using diff-in-diffs methodologies. However, failure of the “parallel trend” identifying assumption will bias diff-in-diffs estimates. It should also be noted that ideally one would like to verify that the parallel trend assumption held over a long period (see Blundell et al [7]). We construct this comparison as follows. First, we focus on the 3<sup>rd</sup> auction, for which we have more pre-intervention observations. Second, for this comparison we also make use of two extra years of observations (1994 and 1995), which were previously discarded because their inclusion unduly reduced the number of firms in the sample. Next, we construct a balanced panel using the method described in Section 3 and Appendix 1. We end up with 924 firms (compared with the 1,329 firms of the baseline for the 3<sup>rd</sup> auction) for which we have investment data from 1994 to 1997 (in the baseline we use only 1996 and 1997).<sup>22</sup> We calculated the average annual rates of growth of investment for treated firms. Then, we selected among the untreated group only the firms that for each single year displayed an annual rate of growth of investment within the interval  $(1 \pm g) * m_t$ , where  $m_t$  is the average 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 include 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 differ 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 those of the baseline. More importantly, the violation of the parallel trend assumption does not seem 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 Law 488 in 1999, coupled with a potential role of intertemporal substitution.<sup>23</sup>

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<sup>21</sup> As shown in Appendix 2 Panel B, this is a reasonable assumption.

<sup>22</sup> See also Figure 2 and Appendix 2.

<sup>23</sup> As suggested by a *referee*, we also identified firms with similar prior investment patterns by using one-on-one matching (each treated firm is matched with the untreated firm with the closest growth rate of investment during the pre-intervention period). The results were similar to those reported in the text.

#### *4.4 Indirect effects*

Next, we turn to cross-sectional substitution, that is the indirect effects of Law 488. More investment by treated firms may depress the investment of untreated firms, thereby biasing the estimates. As suggested by Harris and Trainor [23] and Lee [28], subsidized firms may crowd out unsubsidized firms on the assumption that the size of the local market is fixed and cannot support any additional production. Crowding-out effects in the product market will be greater for firms that sell to a local customer base rather than those selling to another country or another region of Italy. However, indirect effects do not need to be limited to product markets. As explained by Goolsbee [19], the effect of investment incentives may be to drive up the price of capital in a region or sector as a whole if it affects a substantial number of firms. Cross-sectional substitution should be particularly significant when the size of the market where the firms compete is small or when the firms are close in their industrial distance (see: Rosenthal and Strange [30]). Therefore, one would expect that the positive bias deriving from cross-sectional substitution will be greater for firms located in the same area or competing in the same sector. Exploiting this intuition, we compare treated and untreated firms that are in the same area and those that are in the same sector. We focus on the two largest regions and sectors, for which most information is available. Thus, our test is a conservative estimate of the indirect effects, which are more widespread in small regions and 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. 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 products, which represent the bulk of Italian manufacturing, are grouped within a single sector. We find evidence of cross-sectional substitution at the sector level limited to the manufacture of low value added products in the 3<sup>rd</sup> auction.

#### *4.5 Where does the money end up?*

Our results indicate that in order to take advantage of the subsidies, firms have brought forward investments originally planned for future periods. An implication of this results is that Law 488 may have had other impacts beyond the time-effect on investments. For instance, since the subsidy represents a transfer of money to firms and over time there is little additionality, it may have resulted in higher profits and cash flows. This is not the only possibility. The incentive may have caused allocative inefficiencies by encouraging a non-optimal mix of factors. In this regard, the Law 488 scheme is somewhat ambiguous. On the one hand, firms are encouraged through lower relative capital cost to over-invest in capital. On the other hand, the Law 488 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 [3]) is that subsidies may foster

the creation of a culture of rent-seeking, and this, in turn, jeopardizes future efficiency. To make a first cut to these issues in Table 9 we present results where we apply the regression framework described above and use labor costs, profits, cash flow, and debts (all normalized by firms' 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 interestingly, we find that in 2001 the firms treated under the 2<sup>nd</sup> auction display higher profits and cash flows and 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 the absence of the money, they would have undertaken in later years. 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 Law 488. Up to 2003, the total amount of funds distributed to industrial firms amounted to €16 billion. Law 488 allows firms willing to invest in lagging areas to receive a public subsidy that covers a fraction of the investment outlays. The incentives are awarded through competitive auctions according to pre-determined criteria, such as the proportion of firms' equity invested in the project; the number of jobs involved and the proportion of assistance sought.

This paper is a first attempt to evaluate the impact of the Law 488 incentives. The firms in our sample received €394 million and €417 million in investment subsidies respectively for the 2<sup>nd</sup> and 3<sup>rd</sup> Law 488 auctions. This money was spent to increase investment. Evaluated over the value of net physical assets at the end of the previous financial year and compared to the group of rejected applicant firms the Law 488 subsidies caused extra investment equal on average to 17% and 20%, respectively for the 3<sup>rd</sup> and 4<sup>th</sup> auctions. 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 it is necessary to resolve two issues. If as an effect of Law 488, subsidized firms bring forward investment projects originally planned for future periods, then the increase in investment cannot be considered additional since it is offset by a future reduction. If as an effect of Law 488, subsidized firms crowd out unsubsidized firms, then the increase in investment cannot be considered additional because it replaces other investment. In this regard, this paper has shown that the Law 488 money generated less additional investment than its direct impact in the second year of the treatment suggests. The increase in investment triggered by the incentives was counterbalanced by a decline in accumulation by the subsidized firms later



in time. Moreover, we find evidence that the impact of Law 488 is more pronounced when the size of the market where the firms compete is small or when the firms are close in their industrial distance. This suggests that financed firms may have crowded out their non-financed competitors. Overall, these results cast some doubts on the efficacy of Law 488.

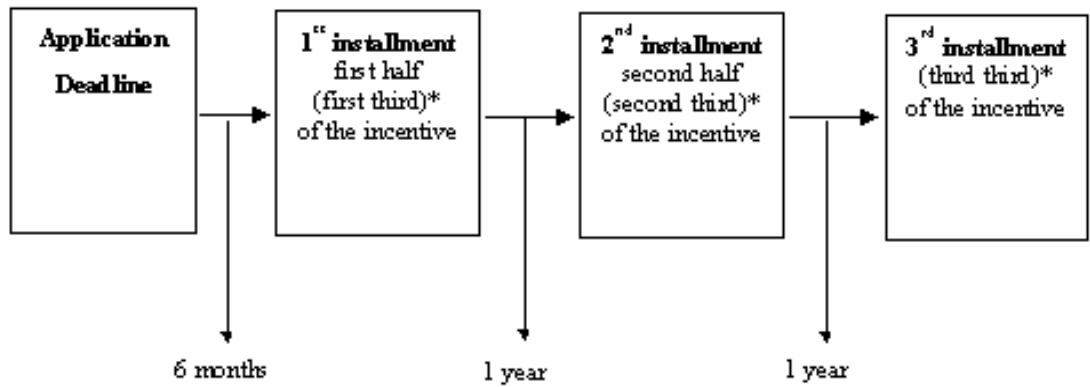
Needless to say, this evidence is hardly the last word on the effectiveness of the investment incentives. Two remarks are in order. First, the companies for which we have data tend to be large companies, and the effects of the availability of Law 488 subsidies are likely to be very different for big, cash-rich firms and for small, cash-strapped firms. Even in our sample of large companies, we find that the effect is concentrated among the relatively smaller firms. Second, we find that the results from the 3<sup>rd</sup> auction are more positive than those of the 2<sup>nd</sup> auction. A possible reason for this finding refers to the change in the Law 488 scheme, by which starting from the 3<sup>rd</sup> auction two additional criteria – one of which referring to the needs of the local economy and decided upon on a local basis – were introduced. It could be that the additional criteria have resulted in an improvement in the ownership of the program and by this means to its increased effectiveness.

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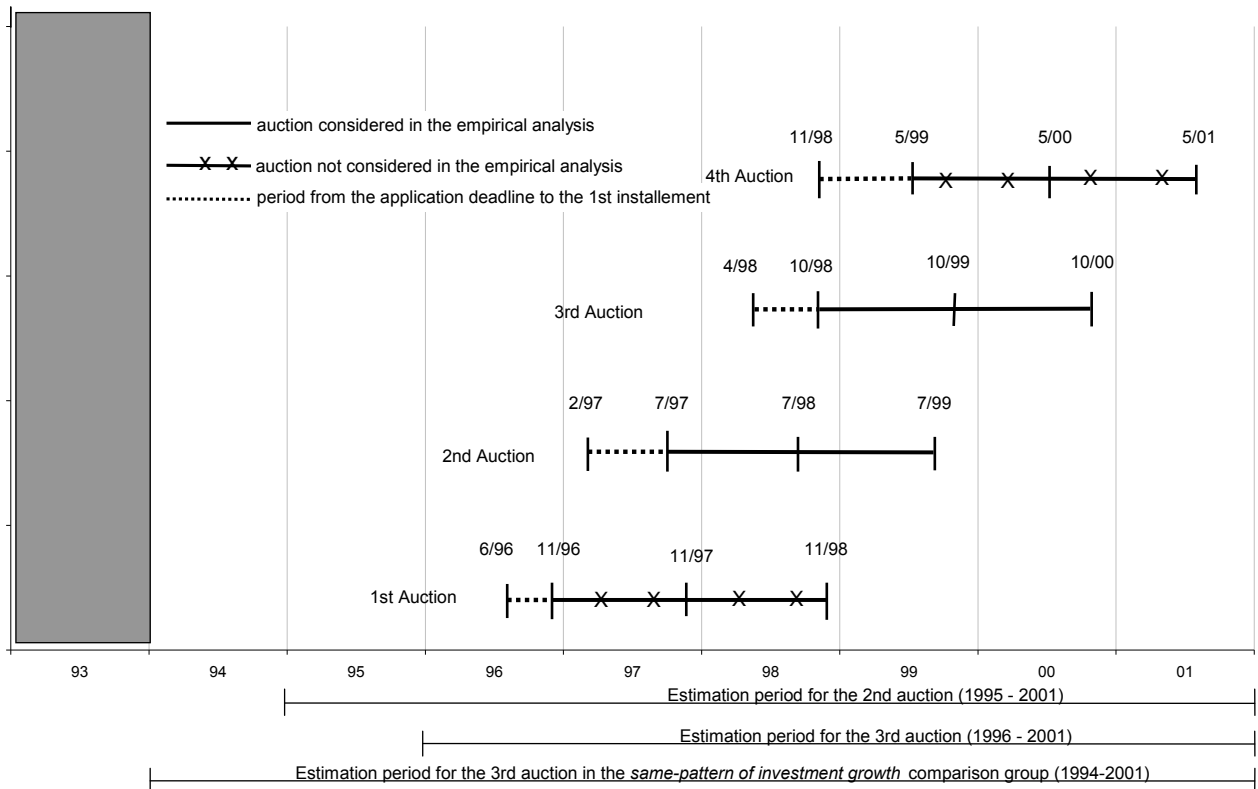
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Figure 1. Timing of the assistance provided by Law 488



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

**Figure 2. Time pattern of the *linked* dataset**



Note: Figure 2 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 2<sup>nd</sup> and the 3<sup>rd</sup> auctions. Moreover, as explained in the text, as for the 4<sup>th</sup> auction that have been substantial and very erratic delays.

**Table 1. Descriptive Statistics**

<i>A. 2<sup>nd</sup> 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. 3<sup>rd</sup> 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 2<sup>nd</sup> and 3<sup>rd</sup> Auctions.

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

Year	2 <sup>nd</sup> Auction							3 <sup>rd</sup> Auction					
	1995	1996	1997	1998	1999	2000	2001	1996	1997	1998	1999	2000	2001
Treatment			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. Standard errors in brackets. The sample includes 6,344 observations for the 2<sup>nd</sup> Auction and 7,177 observations for the 3<sup>rd</sup> Auction.

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

Year Treatment	2 <sup>nd</sup> Auction					3 <sup>rd</sup> Auction			
	1997 X	1998 X	1999 X	2000	2001	1998 X	1999 X	2000 X	2001
<i>A. Baseline (Dependent Variable <math>I_t/K_{t-1}</math>)</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 <math>I_t/S_{t-1}</math></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 <math>I_t/A_{t-1}</math></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. Standard errors in brackets. The sample includes 6,344 observations for the 2<sup>nd</sup> Auction and 7,177 observations for the 3<sup>rd</sup> 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 <sup>nd</sup> Auction					3 <sup>rd</sup> 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. Standard errors in brackets. The sample in Panel A includes 6,344 observations for the 2<sup>nd</sup> Auction and 7,177 observations for the 3<sup>rd</sup> Auction. The sample in Panel B includes 6,818 observations for the 2<sup>nd</sup> Auction and 8,834 observations for the 3<sup>rd</sup> Auction. The sample in Panel C includes 6,344 observations for the 2<sup>nd</sup> Auction and 7,177 observations for the 3<sup>rd</sup> Auction.

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

Year Treatment	2 <sup>nd</sup> Auction					3 <sup>rd</sup> 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 <sup>nd</sup> Auction					3 <sup>rd</sup> Auction			
	1997	1998	1999	2000	2001	1998	1999	2000	2001
	X	X	X			X	X	X	
	<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. Standard errors in brackets. The sample in Panel A sample includes 6,344 observations for the 2<sup>nd</sup> Auction and 7,177 observations for the 3<sup>rd</sup> Auction. The sample in Panel B1 includes 3,590 observations for the 2<sup>nd</sup> Auction and 2,935 observations for the 3<sup>rd</sup> Auction. The sample in Panel C2 includes 3,409 observations for the 2<sup>nd</sup> Auction and 3,585 observations for the 3<sup>rd</sup> Auction. The sample in Panel C1 includes 2,617 observations for the 2<sup>nd</sup> Auction and 2,951 observations for the 3<sup>rd</sup> Auction. The sample in Panel C2 includes 3,727 observations for the 2<sup>nd</sup> Auction and 4,226 observations for the 3<sup>rd</sup> Auction. The sample in Panel D1 includes 4,324 observations for the 2<sup>nd</sup> Auction and 6,070 observations for the 3<sup>rd</sup> Auction. The sample in Panel D2 includes 4,251 observations for the 2<sup>nd</sup> Auction and 5,990 observations for the 3<sup>rd</sup> 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 Law 488 Cutoff. Balanced Panel. Dependent Variable:  $I_t/K_{t-1}$ . *Diff-in-Diffs* Estimates**

Year Treatment	2 <sup>nd</sup> Auction					3 <sup>rd</sup> Auction			
	1997	1998	1999	2000	2001	1998	1999	2000	2001
	X	X	X			X	X	X	
<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. Standard errors in brackets. The sample in Panel A includes 6,344 observations for the 2<sup>nd</sup> Auction and 7,177 observations for the 3<sup>rd</sup> Auction. The sample in Panel B includes 1,914 observations for the 2<sup>nd</sup> Auction and 2,901 observations for the 3<sup>rd</sup> Auction. The sample in Panel C includes 1,186 observations for the 2<sup>nd</sup> Auction and 1,264 observations for the 3<sup>rd</sup> 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 <sup>rd</sup> 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. Standard errors in brackets. 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 <sup>nd</sup> Auction					3 <sup>rd</sup> Auction			
	1997	1998	1999	2000	2001	1998	1999	2000	2001
	X	X	X			X	X	X	
<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. Standard errors in brackets. The sample in Panel A includes 6,344 observations for the 2<sup>nd</sup> Auction and 7,177 observations for the 3<sup>rd</sup> Auction. The sample in Panel B1 includes 1,095 observations for the 2<sup>nd</sup> Auction and 1,182 observations for the 3<sup>rd</sup> Auction. The sample in Panel B2 includes 808 observations for the 2<sup>nd</sup> Auction and 875 observations for the 3<sup>rd</sup> Auction. The sample in Panel C1 includes 1,093 observations for the 2<sup>nd</sup> Auction and 1,169 observations for the 3<sup>rd</sup> Auction. The sample in Panel C2 includes 1,115 observations for the 2<sup>nd</sup> Auction and 1,243 observations for the 3<sup>rd</sup> 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 <sup>nd</sup> Auction					3 <sup>rd</sup> Auction			
	1997	1998	1999	2000	2001	1998	1999	2000	2001
	X	X	X			X	X	X	
	<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. Standard errors in brackets. The sample includes 6.344 observations for the 2<sup>nd</sup> Auction and 7.177 observations for the 3<sup>rd</sup> Auction.

## APPENDIX 1

### Data description

The Law 488 Dataset includes 3,358 and 3,731 corporations, respectively for the 2<sup>nd</sup> and 3<sup>rd</sup> auction. As explained in the text, we study the impact of the program by contrasting the subsidized firms with the firms that have applied for the incentives but 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 Law 488 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 that have nonetheless won the award under any auction before 2001. By implementing those exclusions, we are left with 2,433 and 2,881 firms respectively for the 2<sup>nd</sup> and 3<sup>rd</sup> auction.

Subsequently, we link the Law 488 data set with the CERVED data set to reconstruct an uninterrupted financial-statement sample. In the linking procedure, firm identifier (*fiscal* and *chambre of commerce* codes) misprints and the unavailability of balance-sheet data over the entire period reduce the sample to 1,196 and 1,498 firms, respectively for the two auctions. Moreover, we select only firms with non-negative values for capital stock, assets, and sales, and trimmed the (firm × year) sample at the 5 and 95 percentiles. As a result, we are able to reconstruct uninterrupted financial statements from 1994 to 2001 (1995-2001) for 1,008 (1,329) firms that participated at the 2<sup>nd</sup> (3<sup>rd</sup>) auction (the shares of financed firms are 64 and 32 percent in the two auctions). This sample is labeled *Full Ranking Balanced Sample*. Note that we lose 1993 data to construct our dependent variable, which is defined as investment, calculated as the time difference between the stocks of physical capital measured in two successive years plus depreciation (the CERVED evaluates physical capital *net* of the depreciation), as a percentage of (pre-dated) capital.

For each auction we use only two yearly pre-treatment observations. Therefore the estimation period is 1995-2001 and 1996-2001, respectively for the two auctions. 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 data set 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 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).



**APPENDIX 2**

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

	2 <sup>nd</sup> Auction		3 <sup>rd</sup> 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 <math>I_t/K_{t-1}</math> 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. Standard errors in brackets. Mean and median differences are calculated with reference to the first year of the pre-intervention period (1995 for the 2<sup>nd</sup> Auction and 1996 for 3<sup>rd</sup> Auction). The sample in Panel A includes 883 observations for 2<sup>nd</sup> auction and 1,195 for 3<sup>rd</sup> Auction. The sample in Panel B includes 274 observations for 2<sup>nd</sup> Auction and 483 for 3<sup>rd</sup> Auction. The sample in Panel C includes 473 observations for mean and 328 for median.