

THE IMPACT OF CAPITAL SUBSIDIES: NEW ESTIMATIONS UNDER CONTINUOUS TREATMENT

VALENTINA ADORNO, CRISTINA BERNINI AND GUIDO PELLEGRINI*
Department of Statistics, University of Bologna

Received: March 2007; accepted: April 2007

Most of the relevant literature on the evaluation of the impact of public subsidies to private firms deals with the estimation of causal effects of a binary treatment. However, several policies allow for different levels of subsidies, depending on the investment project, the firm dimension, the region and also on the firms' choice. The aim of the paper is to evaluate the causal effect of a policy intervention in the case of a continuous treatment, exploring the impact of differences in treatment level on policy outcome. As an empirical application, we estimated the impact of subsidies allocated by L. 488/1992, the main regional policy in Italy, in the southern regions of the country in the period 1996-2000. We compare two estimation methods: a parametric method, based on a more traditional DID estimator adapted to the continuous treatment case, and a non parametric estimator, based on a novel two-step matching method developed in our recent work (Adorno, Bernini and Pellegrini, 2007a). On average, our results support the conclusions derived from methods based on the binary treatment: subsidies have a positive and often statistically significant effect on employment, fixed assets and turnover. However, the strong heterogeneity of the treatment outcome with respect to different levels of treatment is highlighted. We find that higher the level of incentive, higher the policy effect until a certain point, from which the marginal impact decreases. The results are robust to changes in the estimation method.

JEL Classification: R38, C14 and H71

Keywords: Continuous treatment, Matching estimator, Industrial policy evaluation, Subsidies to capital accumulation.

1. INTRODUCTION

Most of the relevant literature on the evaluation of the impact of public subsidies to private firms deals with the estimation of causal effects of

*Acknowledgment: We are grateful to participants at seminars in Venezia, Rome and Firenze, especially Fabrizia Mealli, Enrico Rettore and Alessandro Sembenelli for helpful discussions on a previous version of the paper. An anonymous referee gave us substantial suggestions. However, we have full responsibility for any remaining errors. Founding from Ministero dell'Università e della Ricerca Scientifica e Tecnologica, Research Project "Statistical methods for the evaluation of educational, training, and development policies", is gratefully acknowledged.

a binary treatment on one or more outcomes, as employment and investments (Bronzini and De Blasio, 2006, and Bernini, Centra and Pellegrini, 2006, for Italy; Roper and Hewitt-Dundas, 2001, for Ireland). However, several policies allow for different levels of subsidies, depending on the investment project, the firm dimension, the region and also on the firms' choice. This is the case of the subsidies allocated by Law 488/1992, the most important policy intervention to subsidise private capital accumulation in the poorest Italian regions in the last decade. In the dose-response analysis jargon, the subsidized firms by L. 488 are exposed to different levels or doses of treatment. Therefore, studying the impact of such treatment as if it was binary, not only neglects its heterogeneity, but can also hide some important features of the policy intervention. For instance, policy makers might be interested in the different effects related to differences in the level of the treatment.

The aim of the paper is to evaluate the causal effect of a policy intervention in the case of a continuous treatment. We want to explore the impact of differences in treatment level on policy outcome. We confront two estimation methods: a parametric method, based on a more traditional difference-in-differences (DID) estimator adapted to the continuous treatment case, and a non parametric estimator, based on a two-step matching method recently developed (Adorno, Bernini and Pellegrini, 2007a). The novel estimator matches treatment and comparison units that are similar in terms of their observable characteristics in both selection processes, related to the choice to be exposed to the treatment and to the choice about the level of the treatment: in the first step, treated and not treated units are matched on the basis of a set of covariates; in the second step, among matched units in the first step, another matching procedure pairs units with similar treatment level. The main empirical advantages of the method is that we can easily incorporate in the matching procedure some recognized restrictions on the relation between the two processes, avoiding matching between units with similar values in the variables that specify one process but with different values in the others.¹ These features are very important in the empirical application, because several policy instruments, as the case of L. 488, have institutional characteristics affecting the typology of firms that can have access to specific subsidization levels.

As an empirical application, we estimated the impact of subsidies allo-

¹We are also evaluating the statistical properties of the two-step matching estimator in the case of continuous treatment, using a Montecarlo experiment (Adorno, Bernini and Pellegrini, 2007b). The preliminary results show slightly better finite sample properties of the proposed estimator if institutional rules define the level of treatment with respect to the characteristic of the treated units.

cated by L. 488 in the southern regions of Italy in the period 1996-2000. The literature concerning the ex-post evaluation of the impact of L. 488 is recently growing: positive effects of L. 488 on investment are found in Bronzini and De Blasio (2006) and Bernini, Centra and Pellegrini (2006). Pellegrini and Carlucci (2003) present empirical evidence of a positive employment effect, Bernini, Centra and Pellegrini (2006) also on turnover but not on productivity. Bronzini and De Blasio (2006) indicate the presence of (moderate) intertemporal substitution: financed firms slowdown significantly their investment activity in the years following the program. All the previous papers consider the L. 488 as a case of a binary treatment, and do not exploit the richness of the data set that includes information on the level of the subsidy by firm. In our paper we also check if the previous results are confirmed using a continuous treatment estimator. Moreover, the application includes the estimation of the empirical dose-response function related to the L. 488 subsidies, i.e. the entire function of average treatment effects over all possible values of the treatment levels.² Also this function is estimated by parametric and non parametric methods, using a flexible functional form. Therefore we can determine the fraction of treatment effect heterogeneity that can be attributed to the different levels of treatment.

The paper is structured as follows. Section 2 contains a brief description of the L. 488 and of its selection mechanism. In Section 3 we present a concise analysis of the relevant literature on programme evaluation with a continuous treatment, evidencing the pros and cons of the main proposed methods; Section 4 describes the continuous treatment estimators, explaining the modified DID estimator and two-step matching approach proposed in the paper. Section 5 illustrates the data and Section 6 the empirical findings. Finally, Section 7 offers some concluding remarks.

2. THE LAW 488/1992: THE INSTITUTIONAL FRAMEWORK AND THE SUBSIDIES ALLOCATION MECHANISM

State aids to manufacturing and to service sectors, in the form of grants and subsidies, have been for many years a key component of regional policy in European countries. The use of such policy instruments

² As will be explained below, the function correctly identifies the impact of all different treatment levels to the treated firms. However, it cannot infer how much a change in the amount of subsidy affects the magnitude of its impact. In fact, the dose-response function is affected by a composition effect: firms at different level of treatment could have dissimilar characteristics, and discrepancies in the level of treatment can be imputed to this heterogeneity.

has been aimed at influencing the regional allocation of investments and employment, in order to increase competitiveness, self-sustaining growth, and new employment in low income regions. L. 488 was developed in this policy framework, as a instrument to subsidize investment in the so-called disadvantaged areas, founding private capital accumulation by project-related capital grants.³

The L. 488 allocates subsidies through a “rationing” system based on an auction mechanism which guarantees compatibility of subsidies demand and supply. Auctions are run usually on a yearly basis and take the form of a project-related capital grant. The first auction was run in 1996, the last auction took place in 2006. In our paper we consider the first four actions (period 1996-2000), which projects were concluded in the period 1998-2003.

Bernini, Centra and Pellegrini (2006) indicate three main features of L. 488 important for evaluation analysis: i) the L. 488 makes clear the targets of the policy intervention; ii) the selection mechanism of L. 488 identifies projects that are viable but cannot be subsidized due to limited financial resources; iii) the law operates at a regional level.

L. 488 is basically a regional tender for incentives where the subsidies allocation is based on general criteria expressing the policy preference. In each regional auction the investment projects are ranked using five pre-determined criteria:⁴ 1) quota of owner capital invested in the project; 2) number of new employees per unit of investment; 3) ratio between the subsidy requested by the firm and the higher subsidy applicable, given the rules determined by area by the EU Commission; 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 first three criteria carry equal weight: the values related to each criteria are normalized, standardised and added up to produce a single score. The latter two indicators are basically additional points to be added to the score if the project is in line with regional and environmental priorities. The total score determines the position of the project in the regional ranking. The rankings are drawn

³ The L. 488 auctions are run usually on a yearly basis and take the form of a project-related capital grants. Eligible for assistance are manufacturing and extractive firms; starting from 2001, the L. 488 scheme has been extended though separate auctions to the tourism and transport sectors. Investment qualified for the intervention by the L. 488 are: setting-up, extension, modernization, restructuring, reactivation and relocation. For a description of L. 488 mechanism see, among others, Bronzini and De Blasio (2006) and Pellegrini and Carlucci (2005).

⁴ The criteria 4 and 5 were introduced starting from the 1998 (3rd auction).

up through the decreasing order of the score awarded to each project and the subsidies are allocated to projects until the financial resources granted to each region are exhausted.⁵

The five indicators are an explicit expression of the policymakers' preferences. The share of the own funds invested in the project can be considered an (imperfect) proxy of the entrepreneur assessment of the project viability and success: higher the share, greater the commitment of the owner to the project (Chiri and Pellegrini, 1995). The number of new jobs per unit of total investment is used to re-equilibrate the negative substitution effect of the capital subsidy to the firm labour demand. The policy makers express a preference for new projects and for labour-intensive investments. The amount of aid applied for by the firm, relative to ceilings established by the European Union, is the key indicator that transforms the allocation procedure to an auction mechanism.

The second interesting feature of the auction mechanism is the presence of a set of firms willing to invest and having a valid investment project, as checked by a preliminary screening carried over by a set of appointed banks, but not subsidized. They were admitted into the ranking, but they did not receive any subsidies because their scores were too low in the ranking. Moreover, the administrative data set contains several information on these firms.

Another important feature of L. 488 is that the financial resources to be allocated in every auction are different across regions. Consequently, for every auction exists a specific regional ranking and a regional allocation threshold. This characteristic implies that firms with the same propensity to be financed can be subsidized or rejected, depending on region, size of the firm and auction.

The institutional framework is important also for the choice about the level of the treatment. The maximum amount of subsidy (relative to the level of investment) allowable to a project depends on both the region where the investment is localised and the size of the firm. Therefore, it exists some institutional constrains related to the level of subsidies received by each firm, depending on some of its characteristics. This aspect can be fully exploited in the estimation of the treatment level decision.

The selection procedure of L. 488 can help in policy evaluation analysis. The procedure uses the indicators as selection variables: most part of the difference between the group of subsidized firms and the group of non subsidized firms is explained by the indicators. Therefore, the indicators can be

⁵ There are also special rankings for large projects and reserved lists for small and medium-sized firms

very helpful in the construction of the counterfactual scenario in the evaluation analysis: the selection processes can be reconstructed, and the selection effect in the control group estimated. The idea is that controlling for observable differences (in performances and characteristics) allows to control for pre-program not observable temporary shocks affecting the probability of being subsidized. In this contest the not subsidized firms are eligible to be part of a control group, as they show both a propensity for investment and a need to invest very similar to the subsidised firms (Brown et al., 1995; Pellegrini and Carlucci, 2003; Bronzini and De Blasio, 2006).

Another important feature is the expected absence of a double subsidisation or the use of other public subsidies by subsidized firms. Actually, the L. 488 regulation imposes that firms financed by L. 488 have to give up to other public subsidies, because the subsidy can not be combined with other source of public financing.

The presence of multiple ranking by region produced by L. 488 is a fundamental element in implementing the evaluation analysis. As previously noted, firms with the same selection variables are subsidized or not subsidized depending on the regional threshold, and on the regional amount of financial resources dedicated to the intervention. If the matching procedure is applied by auction and region, a common support problem arises, the matching estimator is not adequate and the regression discontinuity design should be applied. Instead, in our case, by pooling the projects by region and auction, an overlapping area of firms with the same propensity to be subsidised that are in both the treated group and in the control group is available. Essentially, the probability of being subsidized in a single auction depends also on "local" conditions, related to the interaction between individual firm and the "group of peers" in the same auction, as the score of all the participants to the auction or the amount the top ranked projects applied for.⁶ The identification assumption used in the matching procedure is that the "local" conditions do not affect the outcome of the policy instrument. Therefore, pooling the projects by region and auction is an adequate procedure for identifying the instrument effects, because we can contrast firms with the same characteristics but with different selection results, as the matching evaluation technique requires. L. 488 mechanism allows to mimic the treatment group among the non treated, re-establishing the experimental conditions in a non-experimental setting, and to construct a reasonable counterfactual scenario for the evaluation analysis by both the non parametric matching method and the parametric approach (the DID estimator).

⁶ We thank an anonymous referee to make clear this point.

3. DIFFERENT APPROACHES TO PROGRAMME EVALUATION WITH CONTINUOUS TREATMENT

In the last ten years the interest on the generalization of the programme evaluation framework from a binary treatment setting to a more general structure for the treatments has increased rapidly. The most important reason was the growing implementations of public policies or interventions characterised by a more complicate structure than the binary one. These are the cases in which the policy consists of a variety of different programmes, or different discrete levels of treatment, or a strictly continuous treatment. In these situations the continuous treatment setting allows to investigate the form of the entire function of average treatment effects over all possible values of the treatment levels and to study if the effects change when the level of the treatment changes.

The continuous treatment setting is appropriate in the case of firm incentive programs, where firms receive different amounts of subsidies. Even more, in this situation, it is not so unlikely to find non random treatment level assignment. That is, we are away from an experimental data framework because there is a non random selection process not only on the participation decision but also with respect to the treatment level assignment. This means the policy instrument (as L. 488) determines a deliberate selection process, and the subsequent selection bias problem has to be tackled using an appropriate estimation method.

The common approach followed from the evaluation literature in analyzing a binary treatment is the potential outcome approach developed by Rubin (1974). It might be easily extended to the continuous case: $y_i(T)$ represents the set of potential outcomes, for each unit i , and T represents the continuous variable indicating the treatment level. The extension of the potential outcome approach represents the basis of all the studies on the program evaluation in a continuous treatment framework. The recent studies, viewed as extensions of the binary treatment framework, might be classified with respect to the adopted approach: parametric versus non parametric.

In the first group may enter all the works which propose a generalization of the propensity score approach in a regression contest. It is the case of the work of Imbens (1999), and Hirano and Imbens (2004), where the propensity score method is extended in a continuous treatment setting. They define the Generalized Propensity Score (GPS) as the conditional density of the treatment given the covariates. In the practical implementation they use a flexible parametric approach: after a specification of the GPS has been chosen, they model the conditional expectation of the outcome variable Y given the treatment variable T and the estimated GPS. Finally, estimates of aver-

age potential outcomes at the treatment levels of interest t , $E[Y_t]$, are obtained by averaging on the covariates values X entering in the GPS function, at the level t . A parametric approach, allowing for a continuous treatment variable in a industrial subsidies context, it is also followed by Bondonio and GereenBaum (2006). They estimate a parametric DID model to measure mean employment impacts of the “Objective 2”, using the value of these subsidies as the treatment variable.

On the other side, a non parametric approach is followed by Imai and Van Dyk (2004). They develop the theoretical properties of the propensity function, which is a generalization of the propensity score of Rosenbaum and Rubin (1983). Subclassification or matching estimators, given this propensity function, are suggested to estimate $E[Y(t)]$. The non parametric approach proposed by Flores (2004) is different with respect to the parameter of interest. He focuses on the entire curve of average potential outcomes or dose-response function, the treatment dose at which the curve is maximized and the maximum value achieved by the curve. The analysis starts by following a non-parametric regression approach. Then, to deal with the problem of “dimensionality” when there are a large number of covariates, the paper discusses the use of GPS. Different approaches are proposed such as matching and weighting. A matching approach is also followed by Behrman, Cheng and Todd (2004). Estimation of the expected values for participants and non-participants, conditional on the X and on the level t of treatment, are obtained using local nonparametric regression methods. In estimating average treatment effect on the treated, they propose to use a matching approach over the region of common overlapping support between the estimated expected values for participants and non-participants. A similar matching approach is the one proposed by Hornik *et al.* (2001). The difference regards comparison among units: matching is carried out among units who are similar in terms of baseline characteristics, but receive different exposures to the programme. They propose a distance measure that decreases both as the propensity scores become similar and as the assigned treatments become dissimilar.

As mentioned before, the case of L. 488 firm incentive programs may well be placed in these continuous treatment contests. Also in our case, the idea is to start from the methods developed in the traditional literature on the binary treatment setting in order to widen their applicability to the case of the continuous treatment policy instrument. In particular we will focus on two particular methods: we will compare a parametric approach (DID) with a non parametric solution (matching). The main difference with the methods proposed in the continuous treatment literature regards the type of the effects we are interested in. While the previous methods mainly focus on the comparison among treated individuals, in this paper we propose a com-

parison between units treated at different levels and non-treated units. It follows that the selection process that identifies participants versus non-participants becomes a fundamental source of non-random selection in the identification of any policy effects, together with the process governing participation into alternative program doses. Furthermore, as regards the quantity to estimate, there are two important considerations that might be pointed out. First, when the focus is on estimation of policy effects, there is often an underestimation of the continuity dimension; the information given by the continuous treatment variable is lost because what is estimated is an average effect among different levels of treatment. On the other side, when the estimation is a function of the doses, the parameter of interest is the potential outcome $Y(t)$ rather than the difference $Y(t) - Y(0)$.

Starting from these considerations, in our analysis we investigate the policy effects on the treated versus the non-treated units at different treatment level. Thus, the idea is to compare participants with non-participants, in order to estimate the effects of an intervention for each level of treatment. Furthermore, the analysis allows to evaluate how these effects are related with the continuous treatment variable, identifying the empirical relation among treatment levels and treatment effects. This approach implies an analysis of the heterogeneity of the effects with respect to the level of treatment, consistently with the recent literature on the heterogeneity of the effects in the program evaluation contest (Athey and Imbens, 2006). The methodology is explained in the next paragraph.

4. THE CONTINUOUS TREATMENT ESTIMATORS

The common potential outcome approach followed by the evaluation literature in a continuous treatment framework remains also valid in our context: $y_i(T)$ represents the set of potential outcomes, for each unit i , given a random sample indexed by $i=1 \dots N$ and T represents the continuous variable indicating the treatment level. Furthermore, the general observed outcome Y can be written as:

$$y_i = d_i y_i(t_i) + (1 - d_i) y_i(0) \quad (1)$$

where D is a dummy variable indicating the treatment status: in particular $D=1$ if the individual has been treated and $D=0$ otherwise, and $y_i(t_i)$ is the particular potential outcome at the observed level t_i . In practice, when $D=1$ we observe $y_i(t_i)$, when $D=0$ we observe $y_i(0)$.

Selection into treatment determines both the treatment status d_i and the treatment level t_i . The participation decision assignment will determine the

treatment status d_i , while the treatment level process will determine the dose t_i . Even if they can occur simultaneously, we suppose a sequential logic in the estimation, where the decision on participation comes before the decision on the level of treatment. Let assume assignment to treatment is made on the basis of:

$$t_i = \begin{cases} g(z_i) + u_i & \text{if } d_i = 1 \\ 0 & \text{otherwise} \end{cases} \quad (2)$$

where

$$d_i = \begin{cases} 1 & \text{if } I_i > 0 \\ 0 & \text{otherwise} \end{cases} \quad (3)$$

and

$$I_i = h(w_i) + v_i \quad (4)$$

where W , Z and U , V represent a set of observable and unobservable variables respectively available at a fixed time, when the selection processes occur.

This structure represents the basis of our approach: differently from the previous literature, it specifies separately the two selection processes (see Adorno, Bernini and Pellegrini, 2007a). It can be relevant if there are strict rules that relate the amount of subsidies to the characteristics of the firms, as the case of L. 488. For instance, these affect the set of variables influencing the selection rule and the treatment level assignment, that can be different. Adopting different specifications for the two processes we can improve the selection process estimation, incorporating information on the institutional framework.

4.1 The parameter of interest

When the treatment is continuous, the treatment effects are affected by three components: the treatment level, the heterogeneity among the units and the stochastic component. Apart from the error term, the heterogeneity issue can be interpreted in two ways. First, for each level of treatment, the effects may vary among units: this is the traditional heterogeneity problem in the literature of the programme evaluation with binary treatment. This aspect is not considered in our analysis, even it is relevant, because we focus on another source of heterogeneity, that is the differences in the effects across levels of treatment. The second source of variability is handled by evaluating average effect among units treated at different levels. The idea is to use the information on the treatment level to estimate different treatment effects as a function of the doses. Assuming to have an infinite number of observations, a natural development of the treatment effects estimation in

the continuous case is the difference between the outcome of the units treated at each level with the outcome of the untreated units. Therefore the average treatment effect on the treated at the t -th level is estimated as:

$$\alpha(T) = E[Y(T) - Y(0) | T = t] \quad (5)$$

for a person randomly drawn from the subpopulation of the participants at the level t . This parameter $\alpha(T)$ is the average treatment level effect (ATLE). ATLE can be estimated using two different approaches: a matching estimator and a DID estimator. We propose a comparison between the two methods, as a robustness analysis of our estimation.

4.2 Treatment level effects: a two-step matching approach

The estimator proposed considers the two different processes stated above, estimating the ATLE by a two step procedure. The first step will identify the participation decision rule and units will be matched on the basis of similar value of the set of covariates identifying this process. Among matched units in the first step, another matching procedure (the second step) will pair units with similar value in the covariates identifying the treatment level assignment. Then, participants and non participants are comparable in the sense that the only difference between the two groups is programme participation. A general form for the proposed two-step matching estimator is given by:

$$\hat{\alpha}(t) = \frac{1}{n_t} \sum_{i|t_i=t} \left\{ y_i(t_i) - \sum_{j \in C} m_{ij} y_j(0) \right\} \quad (6)$$

where C represents the comparison group, t_i stands for the observed level of treatment for the i -th unit and the expected value is among units at the same treatment level t_i . n_t is the number of observations for which $t_i=t$. This means that the estimator can be computed only at the observed treatment levels t . m_{ij} is the weight placed on comparison observations j for individual i that comes from the two matching procedures. In particular,

$$m_{ij} = \frac{w_{ij}^1 w_{ij}^2}{\sum_{j \in C} w_{ij}^1 w_{ij}^2} \quad (7)$$

where w_{ij}^1 and w_{ij}^2 are the weight placed on comparison observations j for individual i , respectively in the first and second matching. The matching estimator can be easily extended into a matching DID estimator, as it will be in the next application.

The first step of the proposed procedure, related to the participation decision process, uses the propensity score matching. Rosenbaum and Rubin

(1983) show that the conditional independence assumption, required for the estimator identification, is also valid if controlling for the propensity score $p(w) = P(D_i|W_i)$ instead of W . The sequential structure of the proposed matching procedure requires the choice of an algorithm that assigns more than one control unit to a single treated unit. However, in literature there is a wide choice among this kind of procedures (Becker and Ichino, 2002).

The second step regards the treatment level selection process. We extend the propensity score method applying it to a continuous treatment regime in order to reduce the dimension of the covariates set Z . Following the work of Imai and Van Dyk (2004), what is proposed here is to find an appropriate “propensity” function of the variables Z to model the treatment variable T . Let $\theta(Z) = E(T|Z)$ the parameter that uniquely represents the propensity function. The authors show that the conditional independence assumption holds with Z replaced by the propensity function. Matching on the propensity function can be easily accomplished by matching on θ , regardless of the dimension of Z . The difference with the matching strategies proposed by Hornik *et al.* (2001) regards the groups on which the pairs are accomplished. In particular, while the authors suggest matching pairs among the group of treated units on the basis of similar values of θ but dissimilar values of the treatment dose, here pairs are performed among the group of treated versus non treated group. Our method is able to estimate the causal effect due to the participation, ruling out the differences between these two groups. However, there could be some differences among treated units (at different levels of treatment). Participants might be different not only with respect to the level of treatment, but also with respect to other covariates. Therefore, we evaluate if there is some heterogeneity with respect to treatment level. However, differences in impacts related to different treatment levels cannot be attributed only to dissimilarities in the level of treatment. Differences in impacts can be interpreted as a causal treatment effects only with respect to the non-treatment status.

We do not exclude that the two selection processes are in some way related. In particular, it is reasonable to argue that the treatment level assignment might depend on the participation decision rule. It is the case of L. 488: given the auction mechanism, higher is the probability of receiving the subsidy, lower is the amount of requested subsidies. Therefore, the treatment level assignment is conditioned to the participation decision rule. Empirically, the function of the observable variables Z , used to model the treatment variable T , is conditioned to the propensity score:

$$\theta(Z) = E(T|Z, p(W)) \quad (8)$$

and θ becomes the new parameter that uniquely represents the propensity function modelling the treatment variable T .

4.3 Treatment level effects: a DID approach

DID estimators are become an increasingly popular way to estimate the effects of a public policy. DID estimation consists of identifying a specific intervention or treatment and then comparing the difference in outcomes before and after the intervention for groups affected by it to this for unaffected groups. If the (non observable) differences among subsidized and non subsidized firms are constant over time, the DID estimator can capture the outcome changes caused by the policy intervention.

DID estimator can be useful represented in a regression framework (Blundell and Costa Dias, 2002). Formally, let assume that the performance of a firm i in period r , denoted y_{ir} , is given by

$$y_{ir} = \beta_0 + \beta_1 d_i + \beta_2 r + \alpha_{DID}(d_i r) + \beta X_{ir} + \varepsilon_{ir} \quad (9)$$

where d_i is a dummy variable which is one if the i firm is subsidized and zero otherwise, r is a time dummy which is 0 when firm is observed in the pre-treatment period and 1 in the post-treatment period. $d_i r$ is equal to 1 if firm i participates in the program and is observed in the post-period treatment; 0 otherwise. ε_{ir} represents temporary fluctuations in unobservables. This representation also allows to control for observable covariates X which affect the outcome and vary between the two groups reducing the selection bias. The correlation between ε_{ir} and the probability to be selected into the incentive program produces the well know selection bias problem. In presence of a positive temporary shock to a specific sector, firms in this sector are more willing to invest and to apply for the subsidy. In this case there is a positive relation between the firms that apply to the scheme and the short term market perspectives, and the DID method tends to overestimate the effect of the policy intervention. The recent econometric literature has suggested that this kind of selection bias can be reduced by augmenting the DID estimator and incorporating conditioning variables reflecting the pre-program performance (Heckman et al., 1999; Klette, Møen and Griliches, 2000; Blundell and Costa Dias, 2000 and 2002). The idea is that conditioning for the observable differences (in performances but also in characteristics), we should control for pre-program temporary shocks that influence the probability of being subsidized.

The main parameter of interest is the coefficient of the interaction term α_{DID} that gives the DID estimate of the treatment effect. Assuming that data are available before and after the subsidized firms have received the subsidy, i.e., at times r_0 and r_1 , this gives the estimator

$$\hat{\alpha}_{DID} = (\bar{y}_{r_1}^S - \bar{y}_{r_0}^S) - (\bar{y}_{r_1}^{NS} - \bar{y}_{r_0}^{NS}) = \Delta \bar{y}^S - \Delta \bar{y}^{NS} \quad (10)$$

where $\Delta\bar{y}^S$ and $\Delta\bar{y}^{NS}$ are the average changes in performance from before to after the incentive scheme was operating, and the superscripts S and NS refer to the subsidized and non-subsidized firms, respectively. The estimated parameter represents the average effect of the subsidy on the subsidized firms (also known as ATT in this literature), and it could not be extended to the not subsidized firms.

The DID estimator can easily be developed in the continuous framework evaluating the average changes in performance from before to after the incentive scheme for different range of the treatment level,

$$\hat{\alpha}_{DID}(t) = \Delta\bar{y}^S(t_h < t_i < t_k) - \Delta\bar{y}^{NS}(0) \quad (11)$$

where $\Delta\bar{y}^S(t_h < t_i < t_k)$ is the average change in performance from before to after the incentive scheme was operating, for the subsidized firms which level of treatment is in the interval $[t_h, t_k]$. The idea is to use the information on the treatment level to select group of subsidized firms (homogenous by grant amount) and to estimate their average treatment effects with respect to the whole control group.

5. THE DATA

The data used in the analysis are collected in two different surveys: the L. 488 administrative dataset and the AIDA, which contains the budgets delivered by a subset of Italian firms to Chambers of Commerce. The integration among the different sets of data has required a complex process of cleaning and merging. The final dataset used in this paper is the same presented in Bernini, Centra and Pellegrini (2006).

The final dataset consists of 665 financed projects and 1.228 non financed projects in the years 1996-2000. For the validation of the control group, a comparison of the main characteristics of the projects and of the ranking indicators in the sample of subsidized and non subsidized firms is presented (Table 1). As far as the distribution of projects is concerned, a substantial homogeneity between the two groups is found, according to ranking indicators, regions and investment typologies. Some difference are found with respect to different auctions, that can be attributed to the dissimilarity in the levels of financial resources allocated among waves of auctions, that determines a different share of subsidized firms with respect of the total number of applicants.

Similarities in the two sample referring to budgetary data for year before the start of the project are evaluated. We use this information on the years of the effective beginning and the effective end of the investment proj-

ect to better interpret the impact of the subsidy comparing the balance sheet the year before the project is really started and the year after the investment is really concluded.⁷ The alternative would be to choose a fixed investment time for all firms, and to evaluate the policy effects at this time. This procedure could generate serious problem in the interpretation of the estimated impacts, due to the strong heterogeneity in the investment time lengths among firms. A correct matching procedure requires to impute an ending date also for the not subsidized firms. We assume that the ending date for the scheduled investment in not subsidized firms is equal to the date scheduled for the investment beginning, augmented by the average investment period by auction, estimated for the subsidized firms sample.

TABLE 1 – *Summary of the project and ranking indicators in the final dataset*

		MEDIAN		
		NOT FINANCED	FINANCED	TOTAL
Investment(Euro)*		1.358.452	1.415.674	1.372.096
Ranking Indicator	Own capital indicator	0,448	0,560	0,484
	Employment indicator	0,003	0,004	0,003
	Subsidy share indicator	1,333	1,351	1,333
Auction	auction 1	0,65%	10,98%	4,28%
	auction 2	12,70%	44,96%	24,04%
	auction 3	57,08%	22,26%	44,85%
	auction 4	29,40%	21,80%	26,73%
	Total	100,00%	100,00%	100,00%
Area	area 1 (Calabria)	8,71%	6,17%	7,82%
	area 2 (Basilicata, Sicilia, Campania, Sardegna, Puglia)	88,76%	86,47%	87,96%
	area 3 (Molise)	2,52%	7,37%	4,23%
	Total	100,00%	100,00%	100,00%
Project typology	Modernization	17,67%	13,83%	16,32%
	Extension	42,18%	53,08%	46,01%
	Setting up	34,28%	28,27%	32,17%
	Reactivation	0,08%	0,45%	0,21%
	Reconversion	0,24%	0,00%	0,16%
	Restructuring	5,46%	4,36%	5,07%
	Relocation	0,08%	0,00%	0,05%
	Total	100,00%	100,00%	100,00%

Source: our elaboration on L. 488 e AIDA data.

* We have deflated profitability and leverage variables by the investment deflator (base=2000)

⁷ The final estimated impact has to be read as the effect of the subsidy on the project outcome one year after the investment is concluded. In order to consider the different time span we use dummy variables for each year (that is each auction) and deflate variables by the investment deflator.

The median values of the covariates do not show strong differences between the two samples in the budgetary data (Table 2). The subsidized firms are slightly greater, more profitable and more capital intensive, as expected, than the non subsidized firms.

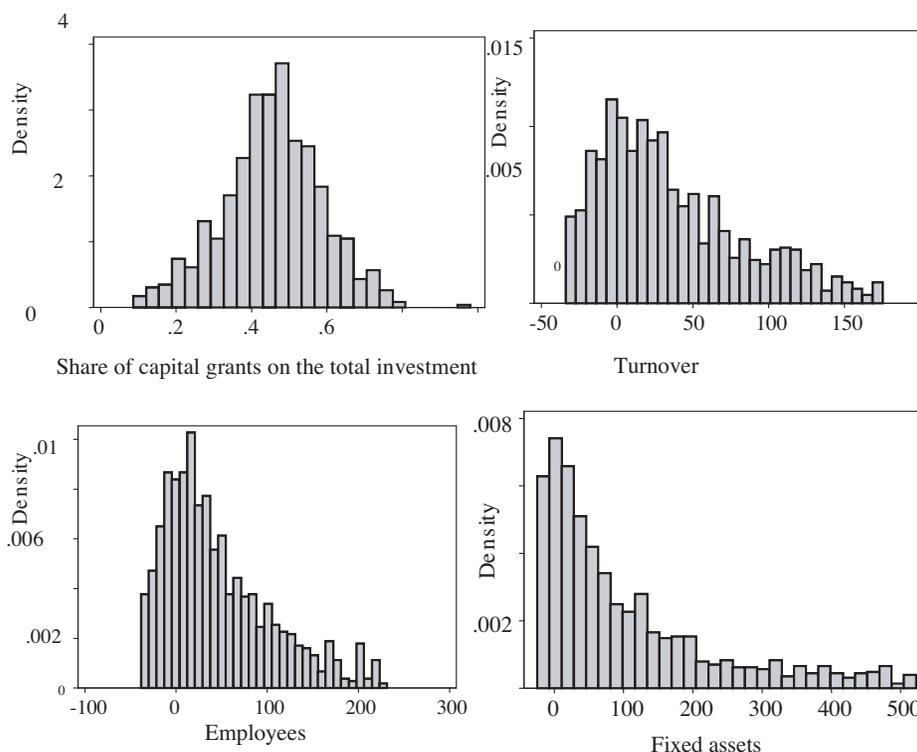
As treatment variable, in order to reduce variability and to limit its range, what is proposed here is to use the ratio of the subsidies on the investment (named "quota"), instead of amount of incentives expressed in levels: in this way the treatment variable takes value in the interval $[0,1]$. As outcome variables, on which we want to estimate the treatment effects, three firm growth variables are considered in the analysis: turnover, number of employees and fixed assets, expressed in growth rates terms between subsidized and non subsidized firms. The sample homogeneity is put at risk by the presence of several anomalous data, therefore we trim the subsidized and non subsidized firm samples at the 10th and 90th percentiles of each of the outcome variables. Figure 1 shows the distributional graphs of the treatment variable, for treated units, and of outcomes variable for the full sample of firms. The share of capital grants seems to have a symmetric distribution

TABLE 2 – *Summary of the main covariates in the final dataset*

		MEDIAN		
		NOT FINANCED	FINANCED	TOTAL
Year 0	Turnover	3.541.644	4.125.601	3.746.266
	Employees	25	30	26
	Fixed assets	1.244.287	1.537.239	1.302.372
	Gr. margin / Turnover	0,09	0,10	0,09
	ROI	4,20	5,57	4,58
	ROE	4,02	6,99	5,05
	Fin. charges / Turnover	0,03	0,03	0,03
	Turnover / Employees	169.380	159.485	165.399
	Fin. charges / Debt	0,05	0,05	0,05
	Value Added	928.895	1.203.206	1.000.404
Year 1	Turnover	4.680.689	5.863.183	5.090.816
	Employees	29	42	32
	Fixed assets	1.984.960	3.054.411	2.412.743
	Gr. margin / Turnover	0,09	0,10	0,09
	ROI	3,47	3,26	3,37
	ROE	2,61	1,93	2,30
	Fin. Charges / Turnover	0,02	0,03	0,02
	Turnover / Employees	185.048	154.387	169.901
	Fin. Charges / Debt	0,03	0,03	0,03
	Value Added	1.135.065	1.778.691	1.335.058

Source: our elaboration on L. 488 e AIDA data.

* We have deflated profitability and leverage variables by the investment deflator (base=2000)

FIGURE 1 - *Distribution of the treatment and the growth rate of the outcome variables*

over its mean value, equal to 46%. The outcomes variable shows a non symmetric distribution, with an higher density of values corresponding to lower growth rates.

6. THE EMPIRICAL FINDINGS

6.1 The two-step matching approach

The first step in the evaluation procedure is the specification of the propensity score model. We adopt a logit specification of the treatment variables. For the covariates identification, we take advantage of the selection mechanism that is used to allocate the incentives under the L. 488 by including the selection indicators in the equation of the propensity score. The main ranking indicators are introduced, either in level or squared and cubed. Dummy variables relative to the different auctions are considered, being also a proxy of the investment year. Moreover, the interaction between main

indicators and dimension (large dimension by European Union Commission definition) is introduced in the model specification, together with regional and sectorial indicators. To test the “balancing hypothesis” we follow the procedure proposed in Becker and Ichino (2002). Splitting the sample by propensity score values and imposing a common support restriction, we test that the balancing hypothesis is satisfied.

The second step of the evaluation procedure is the specification of the treatment level model. Conditioning of the treatment level assignment on participation decision is obtained by estimating the (share of) subsidy at different point of the propensity score distribution. Empirically, we have split the sample in equal average propensity score values between treated and non treated units and estimate a linear regression model for the share of subsidies, for each chosen blocks of the propensity score. In order to select the covariates to include in the analysis, the standard goodness of fit statistics together with the common specification model tests have been carried out. Some covariates are common for each model: they mainly refer to the variable reflecting the subsidies limitation imposed by the law (firm size and areas). In some cases other variables may be found in some specification but not in others: they are the sectorial dummies, the kind of investment qualified for the intervention or the amount of debts charges on debts stock. Given the estimated coefficients of the models and the values of covariates, the predicted values are also computed for the group of the non-participants, in order to use this variable to compute the second step of the proposed matching procedure. To test the “balancing hypothesis” we follow the same procedure proposed in Becker and Ichino, (2002). We test that the balancing hypothesis is satisfied for each model.

Once estimated the two models we compute the two-step matching (DID) procedure, as described above, choosing as outcome variables the firm growth rates. Among the matching with replacement methods proposed in literature we chose, for each step, the radius matching, with four different size for the radius. The average treatment level effects are estimated by eq. (6). The average treatment effects on the treated are reported in Table 3.

As expected, the growth impact of 488 on subsidized firms is positive and statistically significant: the turnover decreases from 12 to 9 points lower in the subsidized firms than in non subsidized ones, depending on the radius of the matching algorithm; the number of employees decreases from 25 to 11 per cent points lower and the fixed assets decrease to 25 per cent points. The results confirm the findings reported in Pellegrini and Carlucci (2003) using a parametric approach and the results of Bernini, Centra and Pellegrini (2006) using a matching approach but in a binary case. The effects of the L. 488 are in line with the (explicit or less explicit) targets: the subsi-

TABLE 3 – *ATT – matching estimates*

	RADIUS=0,1 ¹					RADIUS=MEAN ²				
	NOT		STD			NOT		STD		
	SUBSID	SUBSID	ATT	ERROR	T-TEST	SUBSID	SUBSID	ATT	ERROR	T-TEST
Turnover	410	592	16,811	9,412	1,786	490	592	8,893	3,728	2,385
Employment	430	555	24,796	11,017	2,251	514	555	10,995	4,503	2,442
Fixed assets	442	591	39,898	26,318	1,516	518	591	27,470	10,128	2,712

	RADIUS=0,1 ¹					RADIUS=MEAN ²				
	NOT		STD			NOT		STD		
	SUBSID	SUBSID	ATT	ERROR	T-TEST	SUBSID	SUBSID	ATT	ERROR	T-TEST
Turnover	438	592	20,185	11,978	1,685	471	592	12,195	5,080	2,401
Employment	465	555	27,663	14,198	1,948	497	555	16,496	6,283	2,625
Fixed assets	473	591	39,321	33,747	1,165	501	591	26,065	14,402	1,810

¹equal to 10% of the propensity function range

²equal to the mean of all distances among treated and non-treated

³equal to the maximum of the minimum of all distances among treated and non-treated

⁴equal to the standard deviation of all distances among treated and non-treated

dized firms have invested more (in percent terms) than the non subsidized ones, achieving more turnover, more employment and more fixed assets.

Our method allows to compute the impact of the amount of subsidy on the treatment level. In order to investigate if the treatment level differently affects the response variable, we use an OLS estimator imposing a quadric relation between effects and level of subsidies. We restrict the analysis only for the radius values that return significant average treatment effects (see Table 3). Estimates are reported in Table 4.

TABLE 4 – *OLS estimate: impact of the amount of subsidy on the treatment level*

	EMPLOYMENT			TURNOVER		FIXED ASSETS
	RADIUS FIX	RADIUS MEAN	RADIUS STD	RADIUS MEAN	RADIUS STD	RADIUS MEAN
quota	2.998*	3.439*	3.769*	1.159	3.348*	0.108
quota^2	-0.035*	-0.034*	-0.039*	-0.012	-0.040*	-0.009
Const	-31.910	-68.550*	-65.860*	-15.380	-49.390*	40.180
Obs	58	65	61	65	58	64
Adj. R2	0.089	0.201	0.127	0.003	0.187	0.008
Prob F	0.029	0.000	0.007	0.405	0.001	0.290

* p<0.05

Note: The number of observations corresponds to the different treatment levels (groups) considered in the estimation. The number changes across outcomes because the trimming procedure is applied to each outcome variable.

The results show a significant quadratic relation between effects and treatment levels for turnover and employment. In particular, given the sign of the coefficient for the level (quota), the analysis evidences an increasing positive impact of the amount of incentives for low values of treatment level with respect to non treated firms. After reaching a maximum level, the effect of the subsidies on the outcomes decreases. Figure 2 shows the graphs of the predicted values of these OLS estimates.

This result is also confirmed by adopting a non parametric estimator for the relation between effects and treatment levels. Figure 3 shows the results of the non parametric mean regression estimator using the Nadayara-Watson kernel estimator (Nadayara, 1964) for the outcome variables. Again, the results confirm heterogeneity of the treatment outcome with respect to different levels of treatment, with an increasing relation for low values of subsidies. This evidence confirms that the level of the treatment affects differently the performance of firms with respect to non treated firms. In particular, the graphical analysis evidences an increasing positive impact of the

FIGURE 2 - *Predicted OLS estimates for impact of the amount of subsidy on the treatment level*

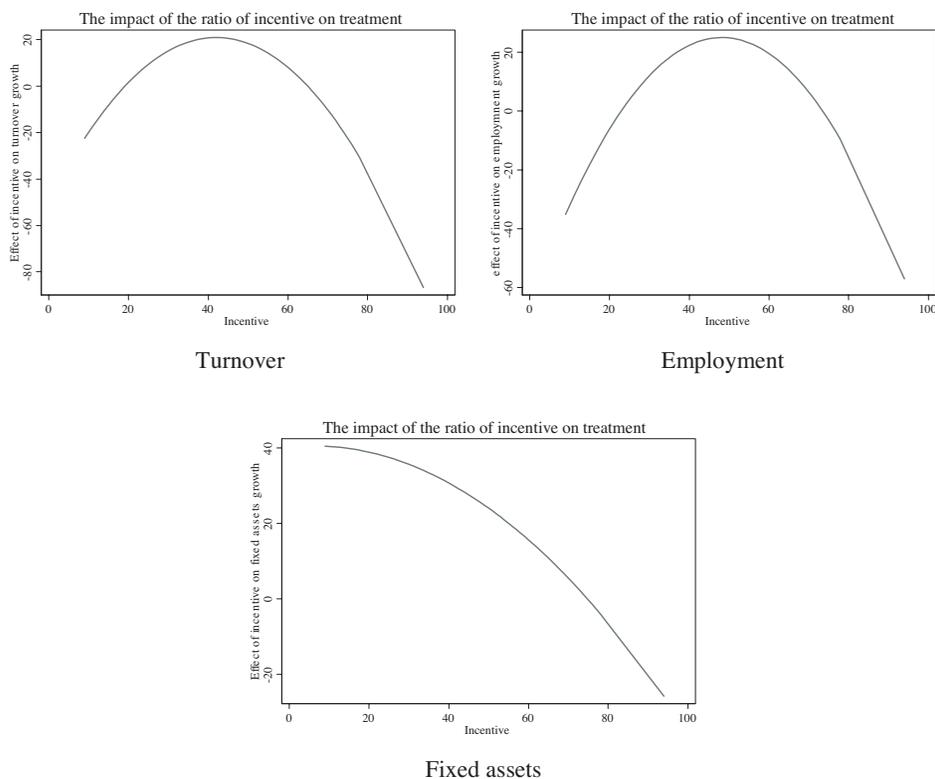
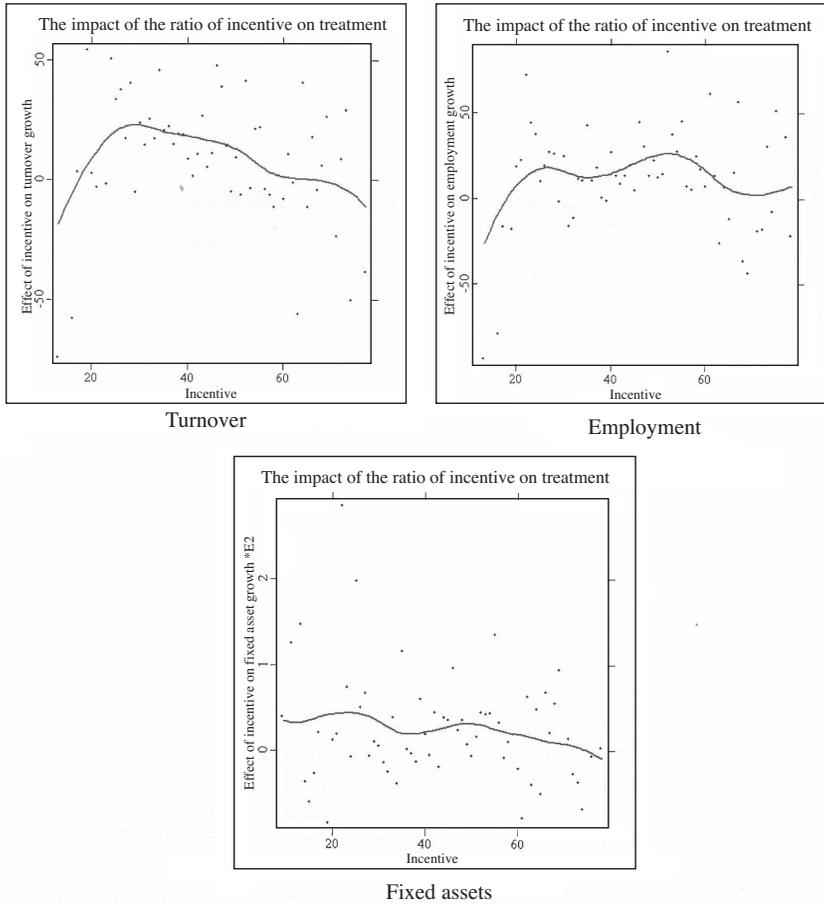


FIGURE 3 - Kernel estimates for impact of the amount of subsidy on the treatment level



amount of incentives until a peak is reached. After that, the effect of the subsidies on the outcomes is decreasing. The value, on which this maximum refers to, is different for the three outcome variables: for turnover it is between 40 and 50% of the share of subsidies on the investment. For employment the impact is increasing until the grant is equal to 50-60% of the total investment. For fixed asset this peak is not so relevant, and also the OLS estimator returns not significant values for the coefficients.

6.2 The parametric DID estimator

The analysis of the effects of the treatment level on outcomes is also performed using a parametric technique, as DID, extended in the continuous framework. We use the specification of eq. (9), where the outcomes are ex-

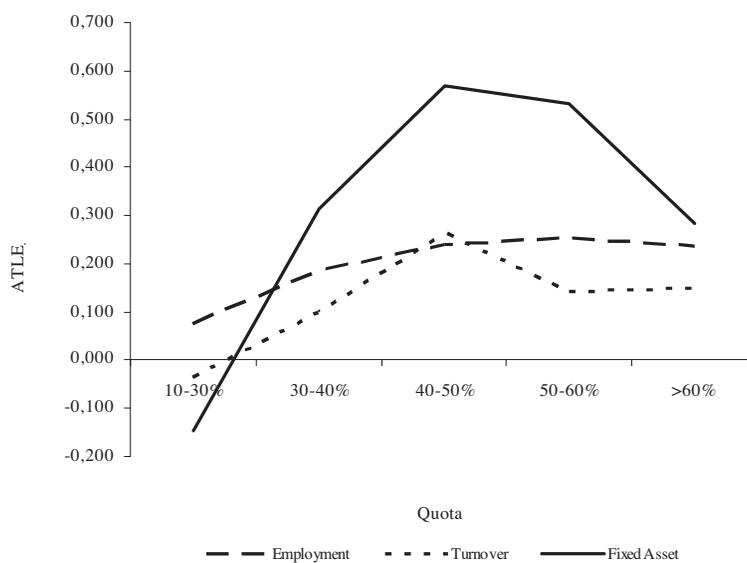
pressed in log terms. For the covariates, we select the same subset of observable variables used in the Propensity Score function previously described; we also introduce in the model specification the investment time length, in order to control for the different time span of the projects.

The model is estimated on the whole sample of treatment and control groups and with respect to different range of the treatment level. The variable quota is split in six intervals: 10-30%, 30-40%, 40-50%, 50-60% and more than 60% (Table 5).

TABLE 5 – ATLE - DID regression estimates

Quota	EMPLOYMENT				TURNOVER				FIXED ASSET			
	ATT	P> t	n	Adj-R ²	ATT	P> t	n	Adj-R ²	ATT	P> t	n	Adj-R ²
Whole sample	0,220	0,000	665	0,318	0,164	0,000	665	0,295	0,423	0,000	665	0,350
10-30%	7,50	0,692	49	0,323	-3,50	0,860	44	0,277	-14,80	0,535	46	0,356
30-40%	18,80	0,201	82	0,326	9,80	0,506	80	0,295	31,20	0,088	82	0,355
40-50%	24,10	0,017	197	0,303	26,20	0,009	198	0,289	56,70	0,000	199	0,358
50-60%	25,40	0,021	154	0,315	14,40	0,185	157	0,298	53,10	0,000	157	0,356
>60%	23,70	0,092	89	0,316	14,90	0,282	90	0,289	28,20	0,099	89	0,359

FIGURE 4 - ATLE- DID parameters plot with respect to the treatment level



Estimates on the whole sample are very close to those found with the matching approach and highly significant. The results of DID estimation of the relation between the outcomes and the level of subsidy confirm the heterogeneity of the treatment outcome with respect to different levels of treatment. The analysis evidences an increasing positive impact of the amount of incentives, especially for fixed asset, until the grant is the 40-50% of the total investment. After this peak, the effect of the subsidies on the outcomes is decreasing: firms investing in the project less than the half of their capital achieve less favourable performance. In general, the effect of very small and very large grants with respect to the level of the firm's investment is positive but low.

The results are robust to different estimators: the comparison of the matching and DID estimates shows that the average treatment effects on the outcomes are similar. In both cases the profile of the outcome effects on the subsidies is non linear, with a initial increasing phase up to a maximum: the difference between the estimators is basically only on the peak value. This result might be justified by the features of DID estimator in the case of continuous treatment. By construction, DID is implemented comparing a limited sample of treated firms (homogeneous in quota interval) with the whole sample of the control group: this can influence the coefficient estimates and reduce efficiency. These drawbacks are overcome by the proposed two step matching because of, for each treatment level, it compares treated units with the most similar non treated firms with respect to the observable variables. Another interesting feature of the matching in the continuous case is the ability to evaluate the effects at each level of the subsidies and not at predefined treatment intervals, as in the case of DID.

7. CONCLUSION

The paper presents a comparison of parametric and non parametric methods to the evaluation of the effects of a continuous treatment. A modified DID estimator is contrasted with a novel double matching estimator, easy to apply and computationally not heavy. Both methods allow to explore the impact of differences in treatment level on policy outcome. As an empirical application, we estimated the impact of subsidies allocated by L. 488/1992.

Our results basically support the conclusions derived from methods based on the binary treatment. Using the double matching method, the impact of L. 488 on subsidized firms is positive and statistically significant: the turnover decreases from 12 to 9 points lower in the subsidized firms than in non subsidized ones, depending on matching algorithm; the number of em-

ployees decreases from 25 to 11 per cent points lower and the fixed assets decrease to 25 per cent points. The findings are robust to different estimation methods: the parametric estimator gives similar results. Overall, the results reported in Pellegrini and Carlucci (2003) using a parametric approach and in Bernini, Centra and Pellegrini (2006) using a matching approach but in a binary case are confirmed by our empirical analysis. The effects of the L. 488 are in line with the (explicit or less explicit) targets: subsidized firms have invested more (in percent terms) than the non subsidized ones, achieving more turnover, more employment and more fixed assets.

However, our methods show the strong heterogeneity of the treatment outcome with respect to different levels of treatment. The share of the impact variance explained by differences in the subsidies is about the 20% using the non-parametric method, 30-35% using the parametric method. We find that higher the level of incentive, higher the policy effect until a certain point, from which the marginal impact is decreasing. The results confirm the empirical evidence that the additional effect of very small grants is low (Ministero delle Attività Produttive, 2006).

Several economic policies use continuous policy variables. Therefore, both methods we used can have a wide application field. Nevertheless, the non parametric two step matching method offers more information to the empirical instrument evaluation: a correct measure of the impact of all different treatment levels to the treated firms can be derived; moreover, it evaluates the effects at each level of the subsidies and not at predefined treatment intervals, as in the case of DID. In addition, the comparison between the treated and the non treated firms is more homogeneous with respect to the treatment level.

However, the method can explain heterogeneity but cannot suggest that, in the lower (higher) part of the curve, an increase in the amount of subsidy can increase (decrease) the impact. In fact, firms at different level of treatment could have dissimilar characteristics, and discrepancies in the level of treatment can be imputed to this heterogeneity. Only if we use the same sample (the same firms' mix) at different levels of treatment this comparison is meaningful. We left this analysis for future research.

REFERENCES

- ADORNO A. - BERNINI C. AND PELLEGRINI G. (2007a), *Evaluating the effect of policy intervention with a continuous treatment. The case of subsidies to capital accumulation*, presented at Miur 40% Workshop "La valutazione dell'impatto di interventi pubblici: metodi e studi di caso", Università degli Studi di Firenze, January 2007.

- ADORNO, A. - BERNINI C. AND PELLEGRINI, G. (2007b), *Comparing matching methods in policy evaluation*, submitted to CLADAG Conference in Macerata (April 2007).
- ATHEY, S. AND IMBENS, W. (2006), "Identification and inference in nonlinear difference-in-difference models", *Econometrica*, vol. 74, n. 2, 431-497.
- BECKER, S. O. AND ICHINO, A. (2002), "Estimation of average treatment effects based on propensity scores", *The Stata Journal*, vol. 2, n. 4, 358-377.
- BEHRMAN J. - CHENG, Y. AND TODD, P. (2004), "Evaluating preschool programs when length of exposure to the program varies: a non parametric approach", *Reviews of Economics and Statistics*, vol. 86, n. 1, 108-132.
- BERNINI, C. - CENTRA, M. AND PELLEGRINI, G. (2006), "Growth and efficiency in subsidized firms", presented at the Workshop *The evaluation of Labour Market, Welfare and Firms Incentive Programmes*, Venice, 11-13 May 2006.
- BLUNDELL, R. AND COSTA DIAS, M. (2002), "Alternative Approaches to Evaluation in Empirical Microeconomics", *Portuguese Economic Journal*, vol. 1, 91-115.
- BLUNDELL, R. AND COSTA DIAS, M. (2000), "Evaluation Methods for Non-Experimental Data", *Fiscal Studies*, vol. 21, n. 4, 427-468.
- BONDONIO, D. AND GREENBAUM, ROBERT T. (2006), "Do Business Investment Incentives Promote Employment in Declining Areas? Evidence from EU Objective-2 Regions", *European Urban and Regional Studies*, vol. 13, n. 3, 225-244.
- BRONZINI, R. AND DE BLASIO, G. (2006), "Evaluating the Impact of Investment Incentives: The Case of the Italy's Law 488/1992", *Journal of Urban Economics*, vol. 6, n. 2, 327-349.
- BROWN, M. A. - CURLEE, R. T. AND ELLIOTT, S. R. (1995), "Evaluating Technology Innovation Programs: The Use of Comparison Groups to Identify Impacts", *Research Policy*, vol. 24, 669-684.
- CARLUCCI, C. AND PELLEGRINI, G. (2005), "Nonparametric analysis of the effects on employment of public subsidies to capital accumulation: the case of law 488/92 in Italy", presented at the *Congress AIEL 2004*, Modena.
- CHIRI, S. AND PELLEGRINI, G. (1995), "Gli aiuti alle imprese nelle aree depresse", *Rivista economica del Mezzogiorno*, n. 3, 166-181.
- FLORES, CARLOS A. (2004), "Estimation of dose response functions and optimal doses with continuous treatment", *Working paper*, University of Miami, 2004.
- HECKMAN, J.J. - LALONDE, R. AND SMITH, J. (1999), "The economics and econometrics of active labour market programs", in A. Ashenfelter and D. Card, Eds. *Handbook of Labour Economics*, vol. 3, Amsterdam.

- HIRANO, K. AND IMBENS, G. (2004), "The propensity score with continuous treatment". Draft of a chapter for *Missing data and bayesian methods in practise: contributions by Donald Rubin's Statistical family*, 2004, forthcoming from Wiley.
- HORNIK, R. - ROSENBAUM, P.R. - LU, B. - ZANUTTO E. (2001), "Matching with doses in an observational study of a media campaign against drug abuse", *Journal of the American Statistical Association*, vol. 96, n. 456, 1245-1253.
- KLETTE, T.J. - MØEN, J. AND GRILICHES, Z. (2000), "Do Subsidies to Commercial R&D Reduce Market Failures?", *Microeconomic Evaluation Studies, Research Policy*, vol. 29, 471-495.
- IMAI, K. AND VAN DYK, D. (2004), "Causal inference with general treatment regimes: Generalizing the propensity score", *Journal of the American Statistical Association*, vol. 99, n. 467, 854-866.
- IMBENS, G. W. (1999), "The role of the propensity score in estimating dose-response functions". NBER *Technical Working Papers* 0237, National Bureau of Economic Research, Inc, April 1999. Available at <http://ideas.repec.org/p/nbr/nberte/0237.html>.
- MINISTERO DELLE ATTIVITÀ PRODUTTIVE (2006), Relazione sugli interventi di sostegno alle attività economiche e produttive predisposta ai sensi dell'art. 1 della l. 7 agosto 1997 n. 266, Roma, giugno 2006.
- NADAYARA, E. A. (1964), "On estimating regression", *Theory and probability and its applications*, vol. 9, 141-142.
- PELLEGRINI, G. AND CARLUCCI, C. (2003), "Gli effetti della legge 488/92: una valutazione dell'impatto occupazionale sulle imprese agevolate", *Rivista Italiana degli Economisti*, n. 2, 267-286.
- ROPER, S. AND HEWITT-DUNDAS, N. (2001), "Grant Assistance and Small Firm Development in Northern Ireland and the Republic of Ireland", *Scottish Journal of Political Economy, Scottish Economic Society*, vol. 48, n. 1, 99-117.
- ROSENBAUM, P. AND RUBIN D. B. (1983), "The central role of the propensity score in observational studies for causal effects", *Biometrika*, vol. 70, 41-55.
- RUBIN, D. (1974), "Estimating causal effects of treatments in randomized and non-randomized studies", *Journal of Educational Psychology*, n. 66, 666-701.