The economic impact of regional industrial policies: An empirical research

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Abstract

Empirical literature findings do not provide a clear-cut interpretation of the effects of public aid on performances of firms. We contribute to this literature analysing the effects of public regional financial subsidies on investment using a unique dataset covering all the firms in the Italian province of Trentino with a record of public aid granted to them in the last 15 years. We find a temporary effect on investment level together with a permanent positive effects on size of firms, but no effect is found on factors substitution nor on technical change. Moreover, subsidies do not improve either profitability or productivity. These results help better define the scope of local aid.

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§This research was conducted for the *Provincia Autonoma di Trento (PAT)* within the activities of the *Nucleo per la Valutazione degli Investimenti Pubblici* of the year 2006. *PAT* provided also financial support to the project. We would like to thank especially the Servizio Programmazione Economica of *PAT* for the support provided during the different stages of this research.

Keywords: *Regional policy, public subsidies, propensity score matching.*

JEL-Codes: *R11, R58, C31.*

1 Introduction

In this paper we address the issue of the effectiveness of local public direct subsidies to firms. Recently, the interest of policy makers in the topic has been growing rapidly. While the most traditional justification for public aid to firms was job creation, present policies seem to be driven mainly by the hope of boosting productivity. There is a widespread belief among policy makers, that public subsidies can help the private sector to enter a path of more rapid productivity growth, thus sheltering economies from the competition of low labour cost countries. This argument gains strength when applied to small firms. Innovation and improvement of processes are introduced into the small and medium enterprises mainly through investment and the renovation of capital goods: subsidies to investments are viewed by policymakers as a method to foster the adoption of innovation by SME. The same European regulation gives a looser definition to state aid to SME.

Despite the popularity of state and regional aid to firms, there is no clear rationale nor empirical ground that might help the design of aid schemes.

Theoretical rationale for public aid has been widely discussed. Evidence for the traditional argument for subsidies, namely market failures or imperfections, has been presented to justify subsidies to R&D investments (David, Hall and Toole, 2000), credit subsidies (Vittas and Cho, 1995), and export subsidies (Abbott *et al.*, 1987). Rodik (2004) highlights the problems of underinvestment in innovation that stem from information externalities and the lack of co-ordination. He describes a set of criteria for public intervention coherent with this view. Following this view, the literature on growth stresses the idea that, in presence of diminishing returns of capital, investments should be directed towards less developed areas that, in principle, can give higher returns on investments (Angrist and Hahn, 1999). In fact, the convergence process might occur too slowly to be socially acceptable, hence the need to subsidize underdeveloped areas. On the other hand, the literature on localization (Fujita and Krugman, 1995) stresses the importance of policies aimed at attracting mobile resources, such as capital, entrepreneurs, and specialized labor, that, idiosyncratically combined with local immobile resources, allow for local positive externalities from aggregation. The obvious counter-arguments – see for instance Bergström (2000) – identify two main causes of failures of the subsidizing efforts. First public aid displaces private investments; second, it causes technical and allocation inefficiencies.

Empirical investigation into the effects of subsidies on growth and productivity seems, on the other hand, to be problematic at best. Public policies are seldom designed with clear goals. Policies targeted at productivity growth are not always distinguishable from ones aiming at sustaining employment; moreover, specific schemes are often biased in favor of more influential sectors and industries which capture political

interests. Doubts about the effectiveness of public aid to firms are widespread. Studies on the effect of subsidies in developed Asian countries, for instance, point to a negative effect on productivity (Lee, 1996; Beason and Weinstein, 1996; Harris and Trainor, 2005). Studies about EU countries raise similar doubts – see for instance Roper (2003). Similarly Bagella and Becchetti (1998), studying Italian data, find a set of partly contradictory results. In the short run, subsidies seem to cause a higher level of indebtedness for firms which receive them with non-decreasing costs of debt, so apparently there is no mis-allocation of financial resources. In the long run, subsidized firms exhibit lower levels of productivity when compared to non-subsidized ones, lending support to the idea that this exogenous "shock" is re-absorbed by the market. A couple of studies (Pellegrini and Centra, 2006; Bronzini and De Blasio, 2006) on the effects of the Italian Law 488/92, that drafted an innovative auction scheme for allocating subsidies, report different conclusions on the effects of state aid, but they both share the view that the impact of subsidies on productivity is weak or nil.

Starting from these considerations we aim at giving a contribution to the current empirical literature on public subsidies on two different levels.

The first contribution is methodological: we use a modified propensity score matching models in order to detect the effects of a treatment. Such a methodological approach helped us shed light on the causal link between the grants and firms performances, measured in terms of variation of size, growth rates and productivity and profitability.

A second contribution lies in the evaluation of the effectiveness of aid policy on performance and behaviour of firms. On the basis of a new database we tested the causal effects both in the short and in the long run, attempting to provide an interpretation of the results. In particular, we distinguish wearing off from long lasting effects of public policy, and we discuss them.

The paper is set out as follows: section 2 looks at the economic and social motivations usually associated with public subsidies from a local viewpoint. Sections 3 and 4, respectively, present the dataset and the methodology we used. Section 5 includes a description of the results and analysis and is followed by the conclusions in section 6.

2 The provision of the law and the research hypothesis

Our research into the effect of local subsidies is based on the case of Trentino Province and in particular, as mentioned in the introduction, on the evaluation of the impact of the Provincial Law 4/81 (PL 4/81 henceforth) on the competitiveness of local firms. Due to its special status of autonomy, the Province is responsible for allocating the large majority of public aid that is given to firms: during the time span we studied, the total subsidies directly handled by the local government were between 90% and 95% (Brancati (2005)).

The PL 4/81 was the mean used by local government to intervene in the local economy. in order to stimulate entrepreneurial activity and, in general, to make local industries more dynamic. It was the main means of local government intervention and lasted from 1981 until the early years of the 21^{st} century, when it was substituted by new legislation, namely the PL 6/99.

All firms operating in Trentino which fulfilled the requirements within the scope of the PL 4/81 at the time were eligible to receive a direct financial subsidy from the local government. The participation in the programme was limited to firms that could submit a formal application and respect the stated criteria – e.g. a viable financial condition, a reasonable investment project substantially related to the grant requested.

The local bureau for industrial policy selected the investment projects following the guidelines provided with the PL 4/81 until the allotted funds per year were exhausted. The law put up two main directives for intervention. The first one can be summarized broadly into four different aims: (1) to foster entrepreneurship; (2) to stimulate the enlargement of existing firms; (3) to stimulate the process of industrial conversion, i.e. the innovation of production processes; (4) to sustain the local employment level. The second directive is related to the morphological aspects of the province and it aims to favor and sustain firms acting in depressed areas.

The law also foresaw a list of strategic sectors for intervention. These sectors were singled out, firstly, by looking at the past performance of sectors in Trentino compared to the corresponding Italian National performance. Secondly, the law proposed a comparison of the dynamics of local sectors with that of international sectors. In particular, sectors considered more eligible for interventions were listed: mining and quarrying (with the exclusion of metal), the manufacture of chemicals and chemical products, the manufacture of mechanical products, the manufacture of electrical and electronic machinery, the manufacture of medical, precision and optical instruments, the manufacture of paper and paper products,

publishing and printing, textiles and transports. The law during its long period of validity was modified several times to account for the dynamics of the various sectors and to fine-tune the type of intervention to counter-act the effects of the business cycle, but its inspiring principles remained unchanged.

Using micro-data on firms, we cannot test for the expected effects on the whole region, like the improvement of employment levels. Our goal is, instead, to point out some hypothesis on the effects of aid on firm structure and performance as using firm level micro-data, we cannot test for general equilibrium effects. In particular, we test the following hypothesis:

- Subsidies help firm grow, by capturing market opportunities otherwise neglected (H1);
- Subsidies help firm improve their competitivity, mainly getting higher levels of productivity (H2);
- If both growth and productivity effects were present, this would be a clear indication that subsidies triggered an endogenous growth process (H3).

3 Data description

The empirical research is based on an original and unique database built specially to deal with the issues focused on in the paper. The major advantages of the dataset we used are the following: (a) it covers all the limited liability firms in Trentino – an Italian province in the "North-East" macroregion – during the period 1998-2003 (PINC8199) [1]; (b) it accounts for entry and exit of firms; (c) the unit of observation is given by the individual firm; (d) it contains balance sheet information as well as data on direct public financial subsidies to private firms; (e) the sectoral coverage of the database covers all the sectors within the scope of PL 4/81 regulating the concession of subsidies.

The sources of data are the Local Bureau of Statistics (Servizio Statistico della Provincia Autonoma di Trento, SSPAT) and the *Servizio Industria della Provincia Autonoma di Trento* (i.e. the local *Bureau for Industrial Policy*) which provided, respectively, balance sheet information and public subsidies information about grants given to firms operating in the province.

In particular, SSPAT provided annual data – the *Pitagora* dataset – that contain balance sheet information about each limited firm together with information about the number of employees in each firm. The latter information is taken from the individual forms that employers send monthly to the Italian Social Security Institute (INPS) [2]. The second source is the Local Bureau for Industrial Policy that gave us access to the whole set of administrative archives which record all public grants given to firms operating in the province in the context of the PL 4/81: the INCE dataset. The original unit of observation was

a *single grant given to a firm in a given year*. Subsequently, we re-organized the information in order to match firm level data with the Pitagora yearly database. In particular, we adjusted the data taking into account the timing of each grant.

The PINC8199 panel data was created by merging the two firm level datasets: Pitagora and INCE. We applied a filtering procedure in order to select the industries we wanted to focus on, i.e. within the scope of the PL 4/81 object of study. We selected firms based on the two digits ISIC sectors classification. In particular, we considered the following industries that correspond to the broad definition of "industrial sectors" contained in the PL 4/81: (a) from 10 to 14: mining; (b) from 15 to 40: manufacturing; (c) 45: constructions; (d) from 60 to 62: transport; (e) 72: computer and related activities; (f) 90: sewage and refuse disposal.

The overall number of grants per year is reported in Table 1.

Our analysis refers to the period 1998-2003 when the sample size of treated firms seems to be sufficient with respect to the methodology we used in our study. The overall distribution of yearly frequencies is shown in Table 2.

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In this study we refer mainly to a balanced panel containing the subset of firms that are always present in the database during the time window we refer to. The number of observations per year ensures a good statistical tractability of data. In Table 2 information about the overall number of firms covered by the database is shown.

Information contained in the database can be grouped into three main categories: (1) demographic information regarding each firm that appears in both the *Pitagora* and the INCE datasets[3]; (2) Balance sheet variables (coming from the *Pitagora* dataset) that we used to create the indicators used in the analysis; (3) information on subsidies coming from the INCE dataset, re-classified into yearly dummy variables showing the existence of a grant given to the firm in that given year.

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4 Methodology and assumptions

4.1 Methodology

The evaluation of the impact of public policy can be framed as a problem of inference where a group of units is subject to a specific treatment (i.e. receiving a direct financial aid). In such a framework a comparison is made between the treated group and the control group (the units not included in the programme); nevertheless, this comparison could lead to biased estimations due to selection bias and observable and unobservable differences between the units comprised in the two groups (Dehejia and Wahba, 2002). In our case, the selection bias arises from the fact that incentive programs are not set in the context of a random or a natural experiment in which firms are selected randomly for treatment [4]. As a consequence, the study of the economic effects of public subsidies cannot be carried out using the standard regression techniques.

This is the reason that spurred us to use the propensity score matching literature to deal with the issue. In our framework we can consider the impact of subsidies as a particular example of the general problem of the evaluation of the average effect of a treatment in a sample, in which both treated and untreated units are present (Imbens and Rubin, 1997; Angrist and Hahn, 1999)[5]. In particular, in our study we had to estimate the effect of a public subsidy (treatment) on different aspects of performances of firms (objective variables). As mentioned, the major issue arising in such a framework is that the assignment to treatment might not be completely random: it might be that observable and unobservable characteristics of the firm influence the probability of receiving such a subsidy.

In formal terms we define, for each firm i , an objective variable (Y_i) potentially affected by the treatment. We start with considering the realization of such a variable at time t as the result of the linear combination of the two potential outcomes. Potential outcomes are defined as the values that the variable Y_i can assume in the situation of incompatible events: (a) the firm i benefits from a grant ($D_i = 1$); (b) the firm i did not receive any subsidy ($D_i = 0$). So that we can write for variable Y_i:

$$Y_i = Y_i(1)D_i + (1 - D_i)Y_i(0). \tag{1}$$

From a theoretical point of view we are interested in estimating the causal effect of the treatment D given by the quantity:

$$\Delta Y_i = Y_i(1) - Y_i(0), \tag{2}$$

However, this value is not readily available, being the linear combination of a factual and a counterfactual

observation. In order to make our estimation, we have to abandon the individual level and look at the average effect of the treatment on the sample, that is given by the following:

$$\tau = E[Y_i(1) - Y_i(0)]. \tag{3}$$

Moreover we define the population average treatment effect for the treated (ATT):

$$\tau^{p,T} = E[Y_i(1) - Y_i(0)|D_i = 1], \tag{4}$$

which is of particular interest in our context[6].

In order to obtain an unbiased estimation of the average effect of the treatment, i.e. to make the observable values $E\{Y|D_i = 1, \mathbf{X} = \mathbf{x}\}$ and $E\{Y|D_i = 0, \mathbf{X} = \mathbf{x}\}$ good approximations of their theoretical values we must assume the independence of potential outcomes:

$$D \perp (Y(0), Y(1)). \tag{5}$$

Moreover, in order to neutralize the effects of self selection of firms into the treatment, given by their individual observable characteristics ($\mathbf{X}$), we relax the assumption (5):

$$D \perp (Y(0), Y(1))|\mathbf{X}, \tag{6}$$

The above assumption is called the *unconfoundedness hypothesis* (Rosenbaum and Rubin, 1983; Lechner, 2002); it ensures that given the values of pre-treatment variables the treatment is random. Using (6) we can refer to conditioned average causal effects of treatments:

$$\tau^p = E_x\{\tau_x^p|D_i = 1\}, \tag{7}$$

in which τ_x^p is defined by the following:

$$\begin{aligned} \tau_x^p &= E\{Y_i(1) - Y_i(0)|\mathbf{X} = \mathbf{x}\} = \\ &= E\{Y|D_i = 1, \mathbf{X} = \mathbf{x}\} - E\{Y|D_i = 0, \mathbf{X} = \mathbf{x}\}. \end{aligned} \tag{8}$$

Note again that the particular database we use for the study allows us to consider the population average

effect of the treatment on treated avoiding any bias arising from sampling schemes[7].

If the number of variables is high, then a problem of dimensionality can arise. To overcome such dimensionality problems we can introduce the so-called propensity score (PS). The PS represents the conditional probability of receiving a treatment given the pre-treatment variables. In formal terms:

$$P(\mathbf{x}) = Prob(D = 1|\mathbf{X} = \mathbf{x}). \tag{9}$$

It is possible to show that, if the two following properties hold, the estimation procedure ensures a bias reduction (Rosenbaum and Rubin, 1983)[8]:

- *Balancing property*: $D \perp \mathbf{X}|P(\mathbf{X})$; this guarantees that given the propensity score the treatment and the observable variables are independent;
- *Unconfoundedness property*: if $Y(1), Y(0) \perp D|\mathbf{X}$ then $Y(1), Y(0) \perp D|P(\mathbf{X})$; this ensures that given the propensity score the treatment and the potential outcomes are independent.

Using the propensity score we can estimate the conditional (on the propensity score) causal effect:

$$\tau^p = E_x\{\tau^p_{P(\mathbf{X})}|D_i = 1\}, \tag{10}$$

in which $\tau^p_{P(\mathbf{X})}$ is defined by the following:

$$\begin{aligned} \tau^p_{P(\mathbf{X})} &= E\{Y_i(1) - Y_i(0)|P(\mathbf{X})\} = \\ &= E\{Y|D_i = 1, P(\mathbf{X})\} - E\{Y|D_i = 0, P(\mathbf{X})\}. \end{aligned} \tag{11}$$

τ^p can be computed using different matching algorithms[9].

A matching estimator (ME) is a method that makes it possible to compare one, or more than one, treated unit belonging to a stratum with other non treated member(s) of the sample from the same stratum. In other words, an ME, once the assumptions for the PS are satisfied, makes it possible to compare treated and control units that are alike at least in their observable characteristics, allowing for a bias reduction in the estimation of ATT. In the present study, all the results refer to the stratification matching estimator that seems to ensure a more extensive use of the set of controls available in our database[10]. In formal terms we have the following estimation in each stratum q constructed using the values of PS estimated in the first step of the method:

$$\tau^S_q = E[Y|D = 1, Q = q] - E[Y|D = 0, Q = q], \tag{12}$$

finally, averaging all the strata we obtain the estimator:

$$\tau^S = E_q[\tau_q^S] = E_q[E[Y|D=1,Q=q] - E[Y|D=0,Q=q]], \tag{13}$$

We should note that the above estimator is ill suited to deal with time invariant non-observable heterogeneity of firms. In other words, there may be systematic differences between participant and nonparticipant outcomes even after conditioning on observable variables. Consequently the use of the above estimator could lead to a bias due to unresolved idiosyncratic factors. For this reason we employ a modification of the method exposed. It can be shown that the conditional difference-in-difference estimator (CDID) washes away such idiosyncratic factors (Smith and Todd, 2005). We implemented the panel data version of the ATT estimator which reveals itself as being more bias-reducing. It can be defined by the following:

$$\begin{aligned}\tau_{CDID} = E_q[\tau_{CDID,q}^S] = E_q[E[(Y(t)|D=1,Q=q) - (Y(t')|D=1,Q=q)] + \\ -E[(Y(t)|D=0,Q=q) - (Y(t')|D=0,Q=q)]],\end{aligned} \tag{14}$$

in which t and t' are time periods after and before the treatment inclusion, respectively.

4.2 Assumptions and model design

We used the propensity score matching approach to estimate the average treatment effect on treated units (ATT). We assumed that once firms received the notification of the forthcoming subsidy they would incorporate the event in their decisions. As a result, the behavior of each firm is immediately influenced by the decision of the policy-maker to provide additional funds. In other words, we attribute a high degree of rationality to firms, in line with standard assumptions in economic literature;

Our model formulation design aims at identifying the time lag of the impacts of subisidies. Hence, we estimate a series of models that investigate eventual effects on the performance variables from two to four years after the grant concessions. The choice of the matching algorithm was made taking into account both the constraints arising from the data availability as well as the necessities arising from the study. We calculated different matching estimators. Preliminary results show that estimators that allow the re-use of the same treated variables and that compare each treated unit with an average of the control units are prefereable. In particular we tried: stratification with respect to the score, the nearest-neighbour matching, the radius matching and the kernel matching. In this paper, we only present the results of the

stratification method, as they appear to be more reliable. Results are not significantly different when other estimators are used.

The technical steps that we followed to implement the above-mentioned strategy are the following:

1. We generated a dummy variable ($treat9899$) that assumes the value 1 for those firms who received a subsidy in the two year period 1998-1999 and zero otherwise;

2. we estimated the propensity score with reference to the same two years period using as independent variables a set of indicators which refer to the year 1998, prior to treatment in order to check for causality. Then, we tested for the balancing property. The final specification of the estimated PS model was chosen aiming at satisfying the balancing property;

3. Using the propensity score, we estimated a stratification matching estimator in the conditional difference–in–differences (CDID) specification in which the variables under observation refer to the years 2001, 2002 and 2003.

5 Results and analysis

5.1 The estimation of the propensity score

The first step of our analysis regards the estimation of the PS. We recall that the functional form and the explicative variables of our chosen PS, take into account the need to satisfy the balancing property; nevertheless it is possible to obtain interesting information from its estimation. We refer to years 1998 and 1999 when focusing on the subsidies and to year 1998 for the control variables. The rationale behind this choice is prompted by the need to estimate the probability of being included for treatment given a set of idiosyncratic factors that can be considered logically and causally antecedent to the treatment itself. The control group we refer to is made up of all firms that did not receive aid within the PL 4/81 in the period 1990-2003. The choice helps us avoid any bias arising from past public interventions. As a consequence, the sample size of the balanced panel is reduced to 577 firms. In equation (15) we show the PS functional form used in the analysis in which the logit specification was chosen[11]:

$$\begin{aligned} Pr(treat9899 = 1) = \alpha + \beta_0 dadd99 + \beta_1 add98du + \beta_2 invadd98 \\ +\beta_3 kxadd98 + \beta_4 VAxadd98 + \beta_5 at1dd + \beta_6 + at1df \\ \beta_7 invaddsq98 + \beta_8 roesq98 + \beta_9 roisq98 + \beta_{10} roiroe98 + \varepsilon, \end{aligned} \tag{15}$$

in which we suppressed the index i referring to each firm for $i \in I$ – i.e. in the set of firms belonging to the balanced panel. The variables included in the analysis represent the set of observable characteristics of firms that we thought could influence the probability of being involved in the subsidies programme. In particular, we consider: the growth rate for year 1999 ($dad99$), a dummy for micro-firms -with one or two employees - ($add98du$), investments per employee as a linear term ($invadd98$) and as a quadratic term, the return on equity ($roe98$), the return on investments ($roi98$), the capital intensity ($kxadd98$), the value added per employee ($VAxadd98$) and two macro-sector dummies: $at1dd$ for manufacturing and $at1df$ to indicate the constructions sector. Moreover, to pursue balancing property we added quadratic terms ($invaddsq98$, $roesq98$, $roisq98$) and an interaction term ($roiroe98$).

The specification reported in 15 satisfy the balancing property. Even if the PS method has to be interpreted as a non parametric device to reduce the dimensionality problem related to the high number of control variables introduced we propose a comment on the estimations results we obtain.

Results (see Table 3) show that micro-firms have a lower probability of receiving a grant.

the return on equity on the propensity of firms to be selected for aid, while the value added per employee has a positive influence. As we expected the manufacturing sector prevails. Moreover, a significant negative effect emerges for the capacity of growth of firms. Summary statistics reveal a good degree of fit: the $pseudo - R^2$ indicates that we captures almost 20% of variability and the log likelihood ratio χ^2 test allowed us to reject the null hypothesis of zero value for all the coefficients considered.

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The stratification of subsidized and non-subsidized firms into different blocks left us with seven groups. Such classification allowed us to verify the balancing property and reveals a good degree of overlapping between the two distributions (see Table 4). The estimations of ATTs refer to the common support of these two distributions to reduce the bias of results (see again Rosenbaum and Rubin (1983)). Moreover, we excluded from analysis firms belonging to the last two groups.

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5.2 The estimation of the average treatment effect of direct subsidies to firms

The analysis looks at several aspects of performance of firms. We looked initially at the direct impact of subsidies on investments of firms, in order to check the coherence of our methodology. Indeed, we expected a positive effect for aid recipients at least in the short run, given that we are focusing on subsidies directly related to investment projects. Secondly, we concentrated our attention on economic performances of firms, looking first at sales, then at classic financial indexes, namely the return on investments (ROI), the return on equity (ROE) and the EBITDA (i.e. the gross margin) per employee. Finally, we moved on to the analysis of labour and capital productivity in order to understand whether public aid is able to generate a change in the technological trajectories. To be able to study the latter point in further depth, we analyzed changes in capital intensity to discover evidence for variations in the combination of factors. Finally, we took into consideration the impact on firms growth.

Table 5 shows the results of our comparisons between subsidized and non-subsidized firms for the three years 2001, 2002 and 2003. The table shows ATTs that express the average effect of public subsidies on the first differences of the objective variables for the firms involved in the programme. In particular, the CDID estimator compares the first-differences between, respectively, pre–treatment and post–treatment objective variables under analysis referring to 1998 for pre–treatment variables and to 2001, 2002 and 2003 for post–treatment variables on which the evaluation is made. The estimations are obtained using the stratification method, which allows us to compare each treated firm in a stratum with a linear combination of control firms belonging to the same stratum. Once the effects of treatment on treated in every block has been estimated, a weighted average is calculated using the relative frequency of treated firms in a strata as weights.

The particular estimator adopted (CDID) makes it possible to cancel out the time invariant unobservable heterogeneity. The different units of measurement that each objective variable refers to has to be taken into account when the results are interpreted, e.g. a coefficient of four for the level of employment means that on average a treated firm is able to hire four employees more than if it had not received the subsidy. In addition to ATT, the table shows bootstrapped standard errors and the t values[12].

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If we look at the impact of aid on investments we can note that there is a positive effect one year

after the grant. The second and third year the effect is still positive but its significance wear off. In other words, firms motivated by the subsidy, tend to invest more – compared with firms who do not receive any subsidy – in the short run. This investment corresponds to jump in fixed assets that happens once in time, afterwards the path of asset growth does not differ from non-subsidized firms. We do observe a significant effect of subsidies on the size of firms. If we look at the number of employee the effect is persistent and clear. Slightly different is the situation regarding the total sales. In fact, we still observe a positive significant impact for years 2001 and 2003, while the positive estimate for year 2002 is not significant. The last result could be interpreted in terms of bias existing in data that leads, in turn, to a loose of precision of the estimation.

Nonetheless, the effect on the firms rate of growth seems to be transitory. We have a positive significant estimation for 2002, but such effect wear off in year 2003.

Higher sales for subsidized firms support the view that investments are at least partly aimed at sustaining growth; however, while there is a jump in levels, we do not observe a stable modification of the growth patterns of firms.

We went on then investigate, consulting ROI, whether the investments undertaken using public funds are more profitable. The impact is positive in every year but it is significant only for 2002. However, the – relatively – high magnitude of standard errors do makes difficult to conclude in favour of a higher ROI.

No significant effect emerges regarding ROE and EBITDA per employee confirming that no clear evidence of a link between public funds end firms profitability exists. It worthwhile to note that results regarding profitability indicators could be influenced by the ambiguous choices made by firms about the adoption of the accounting schemes to follow. Put it differently the Italian law allow firms to use different accounting schemes to register the public subsidies. The adoption of different schemes leads to a different results in the profitability indicators that are modified both in the numerator and the denominator. Given that such choices are not clear from balance sheet data we are not able to correctly interpret he results obtained.

The analysis of labor productivity and capital productivity reveals a not significant positive effect. The latter evidence, combined with an insignificant impact on capital intensity, suggests that subsidies do not induce a re-combination of factors leading to more productive techniques.

5.3 Discussion

The collection of empirical evidences we have obtained, calls for some coherent hypothesis of the effects of subsidies on behaviour of firms. The joint increase of capital, labour and sales, with respect to

non-subsidized firms, suggest that firms expand to match market opportunities lending support to our hypothesis (H1). Public aid triggers firms to modify their investment plans. Nonetheless, there is no evidence that investments have important effects on productivity, through capital deepening or technical change suggesting that we should refuse our research assumption (H2). Firms appear, however, to scale up basing themselves on technology they have already used, despite the capital incentives, i.e. using a combination of the same factors [13].

The increase in investment level could suggest that firms are actually taking on projects that in absence of subsidies would have been discarded, probably because of their lower internal rate of returns. Nonetheless, it is not easy to capture such evidence from our results. In fact, the expected lower degree of profitability of additional investments is washed away, because the profitability indices we calculate are influenced not only by the marginal investments we are talking about, but also from the other "normal" investment projects.

Summing up, we have "hints" supporting our hypothesis (H1), while our hypothesis (H2) are not supported by data. Nevertheless, a most important conclusion is that the assumption (H3) has to be rejected, i.e. public aids do not help in starting off any process of growth of the local industrial system. This means that, as a consequence of a public subsidy, firms have the chance to seize some market opportunity, otherwise neglected. Whether this was good or not from a public point of view, it is hard to say. From one hand, it can be asserted that a larger size of firms can be one of the goals of a regional industrial policy aimed at strengthening the local industrial structure. On the other hand, one can maintain the view that a larger size is of value as far as it helps build a more competitive industrial structure, able to sustain an endogenous growth. Occasional growth like the one we observe could instead be induced by the fact that a subsidy makes it possible to accept investments with an internal rate of return lower than market cost of capital adjusted for the risk. If this was the case, the game would end in a displacement effect.

6 Conclusions

In this paper we have dealt with the issue of the effectiveness of public subsidies in a regional industrial system. Using an original database we analyzed the effects of public aid on firms performance in a local economy, the Trentino province, in the North-East of Italy.

The theoretical literature on the role of public subsidies does not offer a clear interpretation, nor a benchmark with which to compare empirical findings. The rationale for aid is under debate: growth

theorists stress the role of subsidies as a tool to help the system reach a desirable growth path; on the other hand, there has been criticism, that highlights the risks of displacement of private investments.

When evaluating a public policy, however, it must be observed that the intervention rarely results from a clear, theoretically sound, rationale. The Trentino economic structure is widely based on small and medium sized firms. Despite the entrepreneurial flair, common to the whole North East of Italy, the Trentino economy suffers from being mainly based on traditional industries and from following a path of slow productivity growth. In the beginning of the 1990s it faced, like the rest of Italy, a slowdown in employment, from which it promptly recovered as a consequence of the depreciation of the lira. The policies under analysis, i.e. those fostering public aid to private firms, were created at the beginning of the 1980s. The same laws were in place until the end of the century (the new regional law dates back to 1999): criteria for granting the aid was adapted from time to time to a changing environment. When evaluating the effects of public aid, it is difficult to focus on the specific goal that a decision maker had in mind.

These considerations lead us to contribute to the debate with an empirical analysis aiming at shedding some light on two main issues. The first one is methodological; the second one is concerned with policy implications.

From a methodological point of view, our contribution can be viewed as an empirical procedure to adapt the matching models (MM) to the field object of study. We coped with problems related to: (a) the definition of variables to be employed in the analysis; (b) the choice of the correct matching estimator and the correct form of the propensity score in presence of lagged variables. Moreover, we explore the time span to evaluate the effects of policies using the panel data.

From a policy perspective, on the other hand, we contributed to three different issues related to three broad categories of goals typically associated with subsidies. The first one regards the ability of public aid to improve the productivity of the factors and/or profitability, which is their ability to promote self-sustained virtuous cycles. While it is true that firms which have access to aid are able to increase their investments, at least in the short run, the only effect we observe is an even shorter term increase in labor productivity, which quickly disappears over time. From this standpoint we can conclude that subsidies do not produce any long lasting effect in terms of productivity.

A second observation deals with the ability of subsidies to promote the adoption of innovative technologies by means of capital investment. In this regard, both the observations regarding capital and labor productivity and the ambiguous (and statistically insignificant) effects of the policy in terms of capital intensification, seem to point to a different explanation. Namely, it looks like the most noticeable effect

of public subsidies lies in the anticipation of investments, through the radial expansion of the current technology.

A third issue deals with the promotion of growth. In this case our data suggest that public aid can promote employment significantly for subsidized firms. This increase translates into a growth in terms of total sales, which, albeit not a legitimate goal *per se*, might be a significant policy objective in a regional environment characterized by a large amount of small firms.

This most glaring effect, together with the previous one, suggests that public aid promote the development of existing firm, without any particular change in technology or factor combination. Whether this could be of any worth for the regional economy or not much depends from the size of displacement that potential entrant or non–aided firms could suffer.

Notwithstanding these results, this work can be seen as the starting point for further researches. A natural methodological extension is related to the possibility to fully exploit the panel structure of the data. In fact, within the current framework it is impossible to account for unobserved heterogeneity stemming from hidden control variables. Moreover, taking such way could help in addressing new research questions related with the effects of subsidies along the business cycle. More precisely, we could investigate if –and how– the economic impact of subsidies on firms performances is different during recessions *vs* expansion periods of business activity. Although, in order to go on with these investigations we need also to work on the theoretical foundations of the statistical properties and the robustness of matching estimators in a panel data framework. Under this point of view White and Chalak (2006) seem to propose a framework, which once fully developed could provide the appropriate methodology to use.

A second extension is linked with the future availability of data regarding a new local law regulating the concession of subsidies, that partly overlaps with the law under analysis: The Provincial Law 6/99. Integrating these new data into the dataset and studying the composition effects arising from the interactions of the two policy interventions is a second straightforward direction for further research. A further empirical extension could be carried out in the direction of a more precise test of the productivity effects of public aid: in this respect, a decomposition of the Malmquist productivity index using a non–parametric frontier estimation would be a promising path.

Notes

[1]Note that in contrast to many datasets used in industrial economics literature we do not suffer from any data loss due to the low size threshold of firms. As a result, we are able to study dynamics of small

firms that are often neglected in other studies. Moreover, we do not have any problem of representativeness of the sample under analysis and our results apply directly to the economy we are referring to.

[2]See Gallo (2003) for more in-depth knowledge about the information contained in the DM10 form and to gain further insights into the underlining legislation on social security registrations.

[3]The fiscal code number (Partita IVA) provided us with a common code where we could merge the data into a single panel.

[4]See Klette *et al.* (2000) Heckman *et al.* (1998) and Jaffe (2002) for exhaustive reviews of the literature on this issue.

[5]See Petersen (2003) and Lechner (2002) for interesting applications of the propensity score matching models to the role of labor market policies.

[6]For a discussion on the use of the average treatment effect on treated in evaluation studies see Heckman and Robb (1984) and Heckman *et al.* (1997).

[7]See on the issue Heckman and Robb (1984) and Imbens (2004).

[8]A third property that the propensity score satisfies is: $0 < P(\mathbf{x}) = Prob(D = 1|\mathbf{X} = \mathbf{x}) < 1$, that ensures meaningful boundaries for the propensity of firms to be included in treatment.

[9]See Becker and Ichino (2002) for a detailed description of matching estimators.

[10]Estimation results do not change significantly considering other matching estimators, such as kernel matching, radius matching and nearest neighbor matching.

[11]The use of the Probit form does not change the results and does not compromise the verification of balancing property.

[12]The bootstrapped standard errors are obtained as standard deviations of the distributions of ATTs, in which each estimation refers to a sample of firms in the control group. The number of replications is set to 500

[13]It can also be that they expand along a radial expansion of a fixed factor Leontief technology.

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year:	1992	1993	1994	1995	1996	1997	1998	1999
frequency:	12	53	146	62	59	81	81	119
year:	2000	2001	2002	2003	2004			
frequency:	98	111	149	102	179			

Source: PINC8199.

Table 1: Provincial Law 4/81: number of firms involved in the program. Yearly distribution of subsidies concessions to firms.

Year	*Incumbent firms*
1998	1727
1999	1781
2000	1785
2001	1931
2002	1941
2003	1841
Balanced Panel	
1998-2003	851

Source: PINC8199.

Table 2: Frequency distribution of firms in the database PINC8199.

treat9899	Coef.	Std. Err.	z	P>z
add98du	-4.200	1.237	-3.390	0.001
dadd99	-0.004	0.000	-2.060	0.028
invadd98	-0.001	0.001	-0.620	0.537
invaddsq98	0.000	0.000	1.400	0.162
roesq98	-0.099	0.073	-1.350	0.176
roiroe98	-0.502	1.154	-0.430	0.664
roe98	-0.385	0.236	-1.630	0.103
roi98	-0.564	1.438	-0.390	0.695
kxadd98	0.000	0.000	-1.070	0.285
at1dd	1.627	0.270	6.030	0.000
at1df	0.439	0.338	1.300	0.193
VAxadd98	0.003	0.001	2.770	0.006
const.	-1.782	0.255	-6.990	0.000
$LR\chi 2(12)$:	117.54		Prob $> \chi^2$:	0.000
Pseudo R^2:	0.1947		Number of obs:	563

Source: PINC8199.

Table 3: The estimation of the propensity score for the treatment *treat*9899: subsidized firms in one of the years 1998, 1999.

Inferior of block of PS	treat9899 0	1	Total
0.01	97	2	99
0.10	99	5	104
0.15	55	20	75
0.20	73	20	93
0.40	76	79	155
0.60	3	0	3
0.80	0	2	2
Total	403	128	531

Source: PINC8199.

Table 4: The distribution of the PS for treated and control groups in the region of common support. Discarded obs: 32.

Variable:	year:	ATT	Std. dev.	t-value
		Direct effects		
Investments	2001	**496.36** **	255.367	1.944
(Mln lire)	2002	2228.726	1940.259	1.149
	2003	264.631	356.804	0.742
		Size		
Employees	2001	**4.506*****	1.653	2.726
(number of)	2002	**5.327*****	1.858	2.868
	2003	**5.202*****	1.598	3.255
Total sales	2001	**1817.052*****	816.298	2.226
(mln lire)	2002	3638.666	2475.407	1.470
	2003	**3460.532*****	981.760	3.525
Growth rates	2001	8.68	5.333	1.628
of total sales	2002	**10.07***	5.883	1.712
(%)	2003	6.129	30.179	-0.203
		Profitability		
ROI	2001	0.013	0.023	0.588
(%)	2002	**0.022** *	0.013	1.663
	2003	0.005	0.013	0.357
ROE	2001	0.108	0.127	0.849
(%)	2002	-0.012	0.435	-0.027
	2003	-0.14	0.119	-1.182
EBITDA/L	2001	4.001	4.502	0.889
(mln lire)	2002	7.735	8.114	0.953
	2003	5.416	6.136	0.883
		Productivity		
VA/L	2001	17.512	12.244	1.43
(mln lire)	2002	12.014	17.841	0.673
	2003	5.33	10.875	0.49
VA/K	2001	0.741	0.979	0.757
(mln lire)	2002	0.5	0.673	0.743
	2003	1.662	0.729	0.28
		Capital intensity		
K/L	2001	6.816	51.061	0.133
(mln lire)	2002	-0.522	91.693	-0.006
	2003	25.914	79.390	0.326

Legenda: significant effects in bold."": 10%, "**": 5%, "***":1%*

Source: PINC8199.

Table 5: Estimation of average treatment effects on treated.