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# Beyond the SUTVA: how industrial policy evaluations change when we allow for interactions among firms

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**Abstract:** *Investment subsidies to private firms have been one of the most popular place-based policies in developed countries; however, the empirical evidence to date is still mixed and there is no general consensus on the effectiveness of such policy. Most evaluation studies have focused on the policy impact on subsidised firms, whereas the possible spillovers on other firms have been mostly overlooked. This is due to the dependence of these analyses on the Stable Unit Treatment Value Assumption (SUTVA), i.e. they assume away any possible interactions between subsidised and non-subsidised firms. We propose a new approach that allows to consistently estimate not only the ATT, but also spillover parameters contrasting the positive agglomeration effect with the negative cross-sectional substitution and the crowding-out effect. Econometrically we adopt a Matching difference-in-differences (MDiD) using the recent coarsened exact matching (CEM). Our application concerns the Italian Law 488 (L488). We find that capital subsidies engender a growth process in the eligible area in terms of investment but not with respect to employment. Indeed, the positive effect on employment for subsidised firms is entirely compensated by the negative effect on eligible but unsubsidised firms.*

**Keywords:** *SUTVA; spillovers; policy evaluation; public subsidies; business support policy.*

**JEL:** C14, H23, R58.

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# 1. Introduction

For a long time the expression “industrial policy” has been on most economists’ lips. The long and heated debate on this topic, documented in Aiginger (2007) and Chang (2011), calls for a boost in the policy evaluators’ contribution on answering two crucial questions: is the government intervention justified? In this case, what policies work? In this paper we will not directly answer these general questions, but rather we will try to evaluate the effectiveness of a subgroup of industrial policies aiming to enhance the development of lagging regions.<sup>1</sup> Developed countries have used several place-based policies<sup>2</sup> to address the socioeconomic underdevelopment of these regions ranging from tax exemptions to soft loans. Such place-based policies are usually adopted in order to attract new investment, to decrease the unemployment level and, ultimately, to generate self-sustaining growth in lagging regions. One of the most popular policies in the EU to boost depressed regions’ growth consists in investment subsidies to private firms.<sup>3</sup> This policy is typically selective and provides financial assistance to the eligible firms with investment projects that better meet policymakers’ targets. The empirical evidence to date is mixed and there is no general consensus on the effectiveness of such policy (for a recent survey see chapter 2 of the GEFRA-IAB report, 2010). The great extent of evaluation works have focused on the policy impact on subsidised firms with respect to sales, investment and employment, while the possible spillovers on other firms have been mostly overlooked. This is because these studies rely on the Stable Unit Treatment Value Assumption (SUTVA)<sup>4</sup>, i.e. they assume away any possible interactions among firms. There are several situations in which this assumption is not plausible; however, severe empirical difficulties in disentangling the spillover effects from more relevant confounding factors have hindered the relaxation of the SUTVA.

This standing is accurately described in Bondonio (2009: 5):

*In principle, business incentives programs of all sorts are somehow capable of affecting distant outcomes, such as macro-economic or long-run indicators of the well-being of residents measured at the level of the entire provinces, regions, or states in which eligible firms are located. In the vast majority of cases, however, the economic importance of the group of assisted firms, compared to the size of the province/region/state economy in*

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<sup>1</sup> Defined here as regions with per capita GDP substantially below the country average and/or regions with output and employment levels well below the country average.

<sup>2</sup> Place-based programs target public resources towards disadvantaged geographic areas rather than towards disadvantaged individuals as the people-based policies (see Barca et al., 2012).

<sup>3</sup> Henceforth, we will refer to this industrial policy using the expressions investment subsidies policies or business incentives programs.

<sup>4</sup> For more details, see Rubin (1986).

*which they are located is very little. As a result, any actual program impact (in the form of a positive impulse given to the province/region/state economy) becomes virtually undetectable from the changes to the outcome variable of the evaluation caused by many confounding factors (including, in many cases, the presence of other business incentive programs) of a much greater importance than the possible program-induced improvements in the economic activity of the assisted firms.*

This valuable insight underlies the impossibility to accurately determine the macro effect of the investment subsidies programs, yet, evaluators should keep in mind that one of the founding rationales of such policy consists in generating positive externalities, such as a general improvement of the eligible areas' socioeconomic situation. Thus, policy evaluators should strive for detecting potential spillovers turning to evaluation strategies that use firms as units (micro effects) instead of local areas (macro effects). Indeed, even if any actual program impact is virtually undetectable at the province/region/state level, this does not entail that it is impossible to detect the indirect effect that the policy has on new entrants and on eligible but unsubsidised firms. Traditional industrial policy analyses<sup>5</sup> explicitly face selection bias issues spending a few paragraphs in the underpinnings of their evaluation strategies. However, this comes at a price: such studies completely put aside the identification problems linked to spillover effects. Moreover, traditional analyses correctly identify the unsubsidised firms located on the eligible territory as those firms with the most similar features in respect of the subsidised firms; nevertheless, in presence of spillovers, even a perfect control of the selection bias will not suffice to prevent a biased ATT estimate. Indeed, using as counterfactual the non-subsidised firms that undergo policy spillovers will automatically make those firms not suitable to be used as control group, unless perfect knowledge about how spillovers spread is assumed. Checking for the presence of spillovers requires a counterfactual scenario built drawing firms from the pool of firms located in non-assisted areas. Conversely, this entails the further complication of controlling for possible territorial shocks in the time period under analysis.

For example, in case of negative spillover effects on eligible but unsubsidised firms, the traditional control group will bring about an overestimate of the ATT even when the selection bias is completely absent. Still, if we are able to estimate the spillover effects we could subtract this estimate from the biased ATT. Exploiting this additional information can be considered as an important step towards a more comprehensive welfare evaluation.

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<sup>5</sup> With traditional industrial policy analyses we mean all those studies relying on the SUTVA that use as control group firms located in the same territory of the financed firms.

Potentially, investment subsidies programs give rise to many externalities, such as the agglomeration effect, the cross-sectional substitution and the crowding-out of non-subsidised firms. In principle, policy evaluators should try to inform the policymakers on the extent of each externality but, as we will show in Section 4, it is possible to single out each spillover only resorting to extremely strong assumptions. This is why we try to adopt more conceivable assumptions and estimate two aggregate spillover parameters: i) the spillover effect 1 contrasts the positive agglomeration effect on unsubsidised firms with the cross-sectional substitution; ii) the spillover effect 2 contrasts the positive agglomeration effect on new entrants<sup>6</sup> with the crowding-out effect.

Therefore, we clearly distinguish 3 different parameters: the ATT, the spillover effect 1 and the spillover effect 2. We denote as “welfare computation” the comprehensive evaluation of the industrial policy that originates from the combined assessment of the 3 parameters. In our application on an Italian industrial policy, the welfare computation suggests that capital subsidies engender a growth process in the eligible area in terms of investment but not in respect of employment. Indeed, the positive effect on employment for subsidised firms is entirely compensated by the negative effect on eligible but unsubsidised firms.

This is not the first paper trying to evaluate business incentives policy spillovers, in fact, both Bondonio and Greenbaum (2006) and De Castris and Pellegrini (2012) face this challenging task. However, making use of local areas as units, both papers evaluate the macro effects of the policy, while we prefer to evaluate the micro effects of investment subsidies using firms as units (this considerably reduce the risk of picking up confounding effects instead of the real spillover effects).

The rest of the paper is organised as follows. Section 2 describes the rationales for investment subsidies policies and their potential spillovers, while Section 3 presents the empirical results of other works on industrial policies’ spillovers. The welfare computation parameters are discussed in Section 4, followed in Section 5 by a presentation on how to partially relax the SUTVA. We then turn to the empirical application in Section 6, while Section 7 concludes.

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<sup>6</sup> Considering small and highly subsidised eligible areas (Enterprise Zones), Hanson et al. (2012) find that these eligible areas benefit from this agglomeration effect but probably to the detriment of the non-eligible areas.

## 2. Rationales for investment subsidies policies and their potential spillovers

Distressed areas are among the most pressing policymakers' concerns not only in developing countries - where regional inequalities can be striking - but also in developed nations.<sup>7</sup> Indeed, most governments have tried to reduce regional inequalities resorting to a number of place-based policies (e.g., the European countries have been financing the EU Structural Funds and the Cohesion Fund). Developed nations have spent large amounts of money for their state aid instruments;<sup>8</sup> in particular, the EU has spent €13.6 billion for the 2007-13 programming period (Barca, 2009) for enterprise support policies. Why is that, i.e. what are the rationales behind business incentives programs? Policymakers facing with distressed areas primarily try to reduce underdevelopment in a growth-enhancing way.<sup>9</sup> Some of them consider business incentives programs useful to this purpose for 2 reasons. First, inefficient lock-ins of firms location and development can potentially be overcome and a shift to a more efficient equilibrium be induced through competition in capital subsidies. An inefficient lock-in pertains if, for historical accident or other reasons, an inferior stable equilibrium is chosen (Borck et al., 2012). The presence of a discontinuity in the changes needed to move towards a more efficient equilibrium calls for an external intervention. Investment subsidies policies are seen as a way to trigger endogenous changes and generate a self-sustaining growth that will maximise the development potential of low-income regions. This means that business incentives policies are not only expected to improve the economic situation of subsidised firms but also to generate a virtuous circle that will benefit unsubsidised firms. Furthermore, drawing on the firms' location literature (e.g. Devereux et al., 2007), we argue that an improved local economy may facilitate the opening of new firms. In the long run, this process might beget enough critical mass giving rise to agglomerations in depressed areas.<sup>10</sup> In order to make clearer and simplify the empirical spillover analysis, in the remainder of the paper we will distinguish between the positive

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<sup>7</sup> At a broad level, the Article 174 of the Treaty on the Functioning of the European Union states: "the Union shall aim at reducing disparities between the levels of development of the various regions and the backwardness of the least favoured regions". In the period 2007-2013, a substantial share of the EU budget - around 36 per cent (€347 billion) - is aimed at this purpose. The majority of EU Regional Policy funds, known as the Structural Funds and the Cohesion Fund, targets the most disadvantaged European regions, identified on the basis of EU statistical indicators and criteria (Pellegrini et al., 2012).

<sup>8</sup> Ulltveit-Moe (2008) divides these tools into six categories: grants, tax deferrals, tax exemptions, soft loans, guarantees and equity participation.

<sup>9</sup> The presence and the extent of a trade-off between efficiency and equity for economic growth are keenly debated issues (see, among others, Barca, 2009; Farole et al., 2011).

<sup>10</sup> The desirability of agglomerations is largely debated as it is not clear if the positive spillovers - external economies of scale due to firms sharing customers, suppliers, the labour and the capital markets - outperform the negative spillovers - urban slums with high social and environmental costs (see, among others, Melo et al., 2009).

agglomeration effects on unsubsidised firms and the positive agglomeration effects on new entrants.<sup>11</sup>

Second, low-income regions usually have unemployment levels considerably higher than the country average. Policymakers - and politicians in general - are deeply concerned with employment issues and often see industrial policies as a valuable tool to increase employment; however, in a world with perfect mobility of both labour and capital there would be no need for any place-based policies. As well documented in Barca et al. (2012), this is not the case of the world we live in;<sup>12</sup> rather, in the real world there is room for place-based policies to influence firms' location and investment levels. In fact, the costs of moving firms in lagging areas are considered to be lower than the social costs due to the mobility of workers from depressed regions to more developed areas (most notably large urban agglomerations). Nevertheless, investment subsidies policies try to boost the investment level in the lagging regions reducing the cost of capital and the theoretical effect that this has on the employment is unclear. Indeed, firms are incentivised to substitute labour with capital (the substitution effect)<sup>13</sup> but, at the same time, the pursued output effect may arise for two reasons. First, the reduced cost of capital makes subsidised firms more competitive, this increase the demand for their products and induce such firms to expand production and purchase more of all inputs, including labour. Second, interregional differences in the user cost of capital stimulates investors in the non-assisted areas to shift production into the assisted areas, again leading to an increase in capital and labour demand (Schalk and Untiedt, 2000). Even in the case in which these policies just reshuffled jobs among geographic areas, such reshuffling may benefit the nation.<sup>14</sup>

Having said that, we yet have to discuss what the main potential negative spillovers of the business incentives programs are. In the literature, the most quoted negative spillover is

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<sup>11</sup> Agglomeration of firms can assume two forms: clustering or co-location of several firms that belong to the same industry, and clustering of many firms that belong to many different types of industries. The former may cause localisation economies, while the latter is expected to bring about urbanisation economies, where diversity and size of demand are essential features (Johansson, 2004). When we talk about agglomeration effects we mean both localisation and urbanisation economies.

<sup>12</sup> Labour is often immobile, and union agreements often restrict the ability of firms to offer lower wages in regions of higher unemployment in order to take advantage of the underutilised resources (Faini, 1999). Moreover, labour's economic position, for instance in the housing market, and ties of social reproduction, for instance through family and the education of children, form attachments to places that can often militate against geographical mobility (Pike et al., 2006).

<sup>13</sup> Capital grants are intended to increase productive capacity and thus generate employment, although indirectly and at the risk that capital may be substituted for labour if capital grants make the price of capital cheap relative to that of labour (Harris and Trainor, 2007).

<sup>14</sup> In low-unemployment areas, most individuals who place a high value on getting a job will get one fairly quickly. In high-unemployment areas, many individuals who place a high value on getting a job will remain unemployed for a long time. As a result, the average unemployed individual in high-unemployment areas will "need" a job more in the sense of placing a higher dollar value on getting one than the average unemployed individual in low-unemployment areas (Bartik, 1991).

arguably the cross-sectional substitution. This externality occurs when subsidised firms take some of the investment opportunities that unsubsidised firms would have exploited in the absence of the policy. In presence of cross-sectional substitution, publicly funded investment partially crowd-out private investment making the rationale in favour of business incentives less clear. On the estimation side, the evaluation strategy implemented in traditional evaluation works would deliver ATT estimates upwardly biased due to the use of an inappropriate control group. Moreover, some scholars have pointed out that in case a substantial amount of public money has been put on the market, this might engender different types of spillover effects called general equilibrium effects, e.g. a change of the price of capital in a region as a whole. To Goolsbee (1998) this could shift the industrial policy's benefits from investing firms to suppliers of capital through higher prices.<sup>15</sup> Another spillover frequently cited among the failures of business incentives programs is the crowding-out effect. This is in act if the additional investment of the subsidised firms has crowded-out of the market non-subsidised firms. In addition to that, if such programs do not subsidise the most competitive firms, they may prevent or delay market forces from rewarding the most competitive firms, thereby decreasing overall competitiveness.

We conclude this section highlighting other two relevant aspects of business incentives programs. First, investment subsidies policies might be used to bail out struggling firms; this merely shifts the burden of restructuring on to more efficient firms, reduces the incentives to innovate and be efficient, while delaying inevitable restructuring - the result being lower future growth (Ulltveit-Moe, 2008). Second, drawing on the literature about Enterprise Zones we argue that business incentives programs might enhance local demand for shops, infrastructures, cultural activities, thereby creating new employment opportunities but also new perspectives regarding the quality of life for residents of distressed regions (argument adapted from Mayer et al., 2012).

### **3. The previous literature on empirical evaluation of industrial policies' spillovers**

As well recognised in De Castris and Pellegrini (2012), the lack of an extensive literature on the spatial effect of incentives is rather curious, because several industrial policies, especially the policies oriented to the growth of underdeveloped regions, are designed for generating spatial externalities.

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<sup>15</sup> Criscuolo et al. (2012) argue that in the event of policies with funds much smaller than the national GDP, general equilibrium effects are negligible.



This lack of empirical spillover analyses is recently being filled up, in particular by the surge in analyses regarding a specific place-based policy: the Enterprise Zones (EZs) program. In this program, delineated zones - usually neighbourhoods with socio-economic difficulties<sup>16</sup> - are granted “special dispensation” status, and firms who choose to locate and invest in these zones benefit from temporary incentives such as tax rebates, job-trainings or relaxed regulatory barriers (Givord et al., 2012). Indeed, there is a rapidly growing literature on the evaluation of EZs programs, and some of the authors have shown relevant attention on the empirical estimation of the spillovers of such policies. For example, Ham et al. (2011) find positive but statistically insignificant spillover effects to neighbouring areas in terms of unemployment and poverty rate, while Hanson et al. (2012) find negative spillover effects to neighbouring areas in the number of establishments and employment. Concerning capital subsidies to private capital, De Castris and Pellegrini (2012) find a modest spatial crowding out, whereby subsidised regions attract employment and investment from neighbouring areas

The studies just presented make use of local areas as units, i.e. they try to evaluate the macro effects of a regional policy. EZs programs target a large number of small areas with relatively large amount of public money; this arguably simplifies the spillover evaluation of the macro effects using areas as units (see Bondonio, 2009). On the other side, business incentives programs are usually directed towards firms located in a few large depressed areas and this makes impossible to adopt the same evaluation strategy as the EZs programs. To our knowledge, the present paper is the first to evaluate the spillover effects of investment subsidies policies using firms as units (micro effects).<sup>17</sup>

#### **4. Is it empirically possible to disentangle different spillover effects?**

In business incentives policy terms, the SUTVA holds if the causal impact of the subsidies on a firm does not depend on:

- 1) how the subsidies are assigned and dispensed;
- 2) the subsidies that other firms receive, including competitors.<sup>18</sup>

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<sup>16</sup> Areas with numerous disadvantages, such as the shortage of a skilled labour force, a lack of public services and amenities such as security, a dearth of inputs and poor market potential (Givord et al., 2012).

<sup>17</sup> Bronzini and de Blasio (2006) make an attempt to empirically detect a conservative estimate of micro spillover effects resorting to an informal test.

<sup>18</sup> Especially the second assumption seems particularly strong as it is reasonable to suppose that if two firms located in the same area are direct competitors but only one of them receives public money, this will negatively affect the non-subsidised firm's future performance.

The potentially contemporaneous presence of different spillovers, such as the crowding-out effect, the substitution effect and the agglomeration effect, makes appealing for a policy evaluator to single out each effect. If we had perfect information on the mechanism generating local demand, we could completely relax the SUTVA and come up with an evaluation strategy capable of detecting the extent of each spillover. Unfortunately, perfect information is just an economists' utopia and we had better settle for a less ambitious aim. In fact, as shown in Section 5, in this study relaxing the SUTVA does not imply allowing for any possible spillover effects.

In Section 2 we have surveyed the important spillovers linked to the business incentives programs highlighted in the literature. In order to estimate their extent we will focus on the four that we consider the most relevant. Due to the very limited information on how spillovers spread, we will turn to assumptions that allow retrieving an estimate of two spillover parameters, each of which contrasts a positive and a negative spillover:

- i) Spillover effect 1: the spillover emerging from the contrast between the growth of eligible but unsubsidised firms due to the agglomeration effect and the cross-sectional substitution;
- ii) Spillover effect 2: the spillover emerging from the contrast between the crowding-out effect and the agglomeration effect on business births.

## **5. How to partially relax the SUTVA in industrial policy analyses**

As claimed in the introduction, in presence of spillovers it is not possible to rely on eligible but unsubsidised firms to estimate without bias the ATT. This insight leads to asking the central question of what firms make up the best counterfactual possible in case of spillovers. We claim that the answer depends on what types of spillovers are present and how they stretch to surrounding firms. In case of spillovers limited to the eligible area, we draw on Tobler's first law of geography - everything is related to everything else, but near things are more related than distant things - and claim that the firms located in surrounding non-eligible areas represent the best pool of firms from which drawing the control group. In general, we argue that resorting to firms located in non-eligible regions to estimate both the ATT and the spillover effects is the best way to proceed. Nonetheless, using this subgroup of firms entails the additional complication of controlling for territorial shocks that could non-homogeneously affect different regions.

This new evaluation strategy requires some assumptions about how spillovers spread among territories and sectors. It would be appealing if data could reveal the extension of the spillovers

but this is an extremely difficult endeavour, not least because of the remarkable firms' heterogeneity. Consequently, we will have to turn to assumptions that inevitably are a priori and should still be considered quite strong; however, those assumptions will allow partially relaxing the SUTVA and retrieving rough estimates of the spillover effects.

We assume that only intrasectoral spillovers<sup>19</sup> are present and additionally we differentiate between 2 assumptions: i) spillovers do not spread outside the eligible area; ii) spillovers spread outside the eligible area but within a certain range.<sup>20</sup> If the first assumption holds, we should find one or more surrounding non-eligible areas with similar characteristics to the eligible area and use the most suitable non-eligible firms as control group. We claim that the proximity and the similarity of the areas might rule out the presence of relevant territorial shocks - even if it is always better to empirically check for such similarity. In case the second assumption holds, we should find one or more external non-eligible areas with characteristics as similar as possible to the eligible area and use as control group the most suitable non-eligible firms located in the external regions. In this case we should control for the territorial shocks and we might do so using a non-eligible sector not strictly related with the eligible sector. The most credible assumptions about the spillovers range should be evaluated on a case-by-case basis.

In the remainder of this Section we will describe increasingly realistic stylised frameworks and for each of them we will propose an evaluation strategy (see Table 1). Let us suppose a framework in which there exists an eligible area surrounded by several non-eligible areas and the subsidies to firms are directed towards a specific sector.<sup>21</sup>

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<sup>19</sup> Depending on the case under analysis, it is possible to use different classifications of sectors: from the classic division among primary, secondary, and tertiary sector to considering the first 4 digits of the NACE classification. In the application in Section 6 we will consider all the manufacturing firms as belonging to the same sector. This can be modified considering for example subgroups of manufacturing firms determined by a different classification. The rationale is that within each subgroup of firms it is easier to have interactions both in the technologies and in the goods market. Furthermore, it is possible that the interactions intensity depends not only on the "economical distance" but also on the spatial distance. From the empirical point of view, there are no pre-established classifications to adopt and it would be interesting to see how the results change using different classifications of sectors; however, allowing for interactions among subgroups of firms calls for the introduction of more structure into our framework. This could be faced turning to spatial analyses that take into account the distance among firms. This is the direction we are willing to pursue in a subsequent version of this paper.

<sup>20</sup> The importance of proximity in business relationships has been stressed by different strands of literature (see Lublinski, 2003). For example, Johansson (2004) suggests that the formation of links between firms may be a distance-sensitive activity and hence be more frequent inside a region than between regions. Indeed, proximity can facilitate knowledge spillovers and affect transaction costs when firms buy distance sensitive inputs.

<sup>21</sup> We use the yearly growth rate of capital as dependent variable ( $Y$ ), but this framework is easily adaptable to other outcomes such as turnover, sales, productivity, and employment.

Table 1: Possible evaluation strategies with 3 different assumptions about spillovers

Spillovers	Random assignment	
	YES	NO
None	A) ATT and spillovers estimated using as control group unsubsidised firms located in the eligible areas	B) As A) but using a quasi-experimental method (e.g., regression discontinuity design, RDD; Matching Diff in Diffs, MDiD)
	C) ATT and spillovers estimated using as control group firms located in surrounding but non-eligible areas	D) As C) but using a quasi-experimental method (e.g., Matching Diff in Diffs, MDiD)
Extend over the eligible area but within a certain range	E) ATT and spillovers estimated using as control group firms located in non-eligible external areas	F) As E) but using a quasi-experimental method (e.g., Matching Diff in Diffs, MDiD)

A) Random assignment and no interactions among firms: if the SUTVA holds and the subsidies assignment is random, we have no need to turn to data on other areas to estimate the ATT. Indeed, the average growth rate of capital ( $\bar{Y}_S^E$ ) in the subsidised firms minus the average growth rate of capital ( $\bar{Y}_{NS}^E$ ) in eligible but non-subsidised firms will deliver an unbiased estimate of the ATT. In this case there are no spillovers and the ATT coincide with the welfare computation.

B) Non-random assignment and no interactions among firms: this is the situation for which traditional analyses are tailored for. As in the case of random assignment, the control group comes from a pool of eligible but unassisted firms and the main, if not the only, concern is the selection bias. Different quasi-experimental methods have been adopted to minimise this bias, e.g. Cerqua and Pellegrini (2011) use an extension of the regression discontinuity design.

C) Random assignment and spillovers limited to the eligible area: if the SUTVA does not hold and the subsidies are randomly assigned, the eligible but non-assisted firms are not anymore a valid control group. Indeed, in case of spillovers among firms in the same region the non-assisted firms do not represent what would have happened to the assisted firms in case of no intervention. In this situation, firms located in surrounding non-eligible regions represent the best pool of firms from which constructing counterfactuals to estimate the ATT and the spillover effects. In the time period considered, different areas might be exposed to different territorial shocks; however, using firms located in surrounding non-eligible areas with similar characteristics to the eligible area might make unnecessary to explicitly control for territorial

shocks. In this case, an unbiased estimate of the ATT derives from the difference between the average growth rate of capital ( $\bar{Y}_S^E$ ) in the subsidised firms and the average growth rate of capital ( $\bar{Y}_{NS}^{NE}$ ) in firms located in non-eligible areas. This is not conclusive about the effectiveness of the policy because the ATT is just the first component of the welfare computation. A way to compute the spillover effect 1 is the average growth rate of capital ( $\bar{Y}_{NS}^E$ ) in the eligible but unsubsidised firms minus the average growth rate of capital ( $\bar{Y}_{NS}^{NE}$ ) in firms located in non-eligible areas. Finally, we have to check which one between the crowding-out effect and the agglomeration effect on business births has prevailed. Relevant confounding factors make more challenging the evaluation of the spillover effect 2, but we could infer the prevailing spillover looking at the sign of the difference between the ratio of the number of new entrants to the number of closing-down firms in the eligible and non-eligible areas during the time span examined.

D) Non-random assignment and spillovers limited to the eligible area: the analysis proceeds likewise as in case C) except for the fact that in this scenario we have to control for the selection bias too. A quasi-experimental method capable of minimising this bias is the matching difference-in-differences (MDiD) estimator presented in Section 6.

E) Random assignment and spillovers extending over the eligible area but within a certain range: assuming that the spillovers are potentially present only within the eligible area might be excessively restrictive. Another assumption allows for the presence of spillovers in the surrounding non-eligible areas up to a certain threshold. In this case, we should find one or more external non-eligible areas with characteristics as similar as possible to the eligible area and, after exploiting data on a non-eligible sector to control for the presence of territorial shocks, use the non-eligible firms located in the external regions as control group. We could use these firms as control group both for estimating the ATT and the spillover effects with methodologies specular to the ones proposed for scenario C).

F) Non-random assignment and spillovers extending over the eligible area but within a certain range: the analysis proceeds likewise as in case E) except for the fact that now we should control for the selection bias too. A quasi-experimental methodology capable of minimising this bias is again the MDiD estimator.

## 6. Application

### 6.1 Methods

The main complexities in evaluating business support policies are due to the non-random assignment of capital incentives. Indeed, in observational studies the treatment group has usually features substantially different from the ones of the control group; therefore, there is a need for methods capable of controlling for the selection bias. In absence of randomised studies, the second best is to find natural experiments in which capital subsidies might be considered randomly assigned for a subgroup of firms (e.g. Cerqua and Pellegrini, 2011); however, when no natural experiment is available, matching methods are a valid alternative.<sup>22</sup> Such nonparametric methods match each financed firm to one or more non-financed firms as similar as possible with respect to a given set of pre-treatment variables. Matching methods mainly rely on two crucial assumptions. First, the conditional independence assumption (CIA), i.e. they assume that all the relevant differences between subsidised and non-subsidised firms are captured in their observable attributes. Second, the common support assumption, i.e. every subsidised firm has at least 1 counterpart in the control group.<sup>23</sup> In recent years, a number of papers (e.g. Iacus et al., 2012) have highlighted the misapplication of the matching methods by some economists; thus, a new class of matching methods has emerged – dubbed “monotonic imbalance bounding (MIB)”<sup>24</sup> (see Iacus et al., 2011) - that curtails the misuse of these techniques.

We will resort to one of the MIB methods: the coarsened exact matching (CEM). The idea of the CEM is to temporarily coarsen each conditioning variable into substantively meaningful groups, exact match on these coarsened data, and then retain only the original (uncoarsened) values of the matched data.<sup>25</sup> Iacus et al. (2011) show that the CEM dominates commonly used existing matching methods in its ability to reduce imbalance, model dependence, estimation error, bias, variance, mean square error, and other criteria. Nonetheless, the inherent trade-off of matching is reflected in the CEM too: larger bins (more coarsening) will result in fewer strata; fewer strata will result in more diverse observations within the same strata and, thus, higher imbalance

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<sup>22</sup> Matching techniques have been used by several scholars to analyse the effectiveness of place-based policies (see, among others, Bernini and Pellegrini, 2011; Accetturo and de Blasio, 2012).

<sup>23</sup> For a detailed description of the main matching methods, see Morgan and Harding (2006).

<sup>24</sup> In this class of matching methods the balance between the treated and the control groups is chosen by ex-ante user choice rather than being discovered through the usual laborious process of checking after the fact, tweaking the method, and repeatedly reestimating (Blackwell et al., 2009).

<sup>25</sup> If different numbers of treated and control units appear in different strata, the analysis model must weight or adjust for the different stratum sizes. In this situation, we can try to span the remaining imbalance via a weighted regression of the dependent variable on the covariates (see Iacus et al., 2012, for an illustration of how the CEM weights are computed)

(Blackwell et al., 2009). As well recognised by Ho et al. (2007), matching methods are data-preprocessing techniques and analysts must still apply statistical estimators to the data after matching. In this text we will combine the CEM with the difference-in-differences estimator (DiD). In fact, using differences or growth rates as outcome variables, we accommodate unobserved determinants of the non-treated outcome affecting the selection process for as long as these are constant over time. The main matching hypothesis is now stated with respect to the before-after evolution instead of levels (Blundell and Costa Dias, 2009).

## 6.2 Data

In our application we analyse the Italian Law 488/92 (L488), which has been the main policy instrument for reducing territorial disparities in Italy during the period 1996-2007. L488 operates in the less-developed areas of Italy, i.e. the areas designed as Obj. 1, 2 or 5b for the purpose of EU Structural Funds. L488 makes available grants on capital account for projects designed to build new productive units in less-developed areas or to increase production capacity and employment, increase productivity or improve ecological conditions associated with productive processes, technological updates, restructuring, relocation and reactivation.<sup>26</sup> L488 allocates subsidies through a rationing system on the basis of regional competitive auctions.<sup>27</sup>

L488 auctions have been issued on a yearly basis. Our analysis refers to the period 1995-2001 and focuses on the four L488 auctions that were concluded by 2001. Data relative to the auctions derive from two different datasets: the administrative L488 dataset of the Ministry of Economic Development and a financial statement dataset, collecting data from AIDA<sup>28</sup> and other sources of financial information. The first dataset records all the firms that applied for an L488 auction, both financed and non-financed, providing important information, such as the sector and the location of the firms. This dataset lacks financial and economic information such as investment and turnover; therefore, we use the financial statement dataset that basically collects financial statements for corporations (this means that it is skewed towards larger firms).

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<sup>26</sup> For the areas and time period under analysis, L488 financed mostly projects designed to build new productive units (64.1%), to increase production capacity (25.6%), and for technological updates (7%). Much less projects were financed for the other purposes (3% on restructuring, 0.2% on reactivation, and 0.1% on relocation).

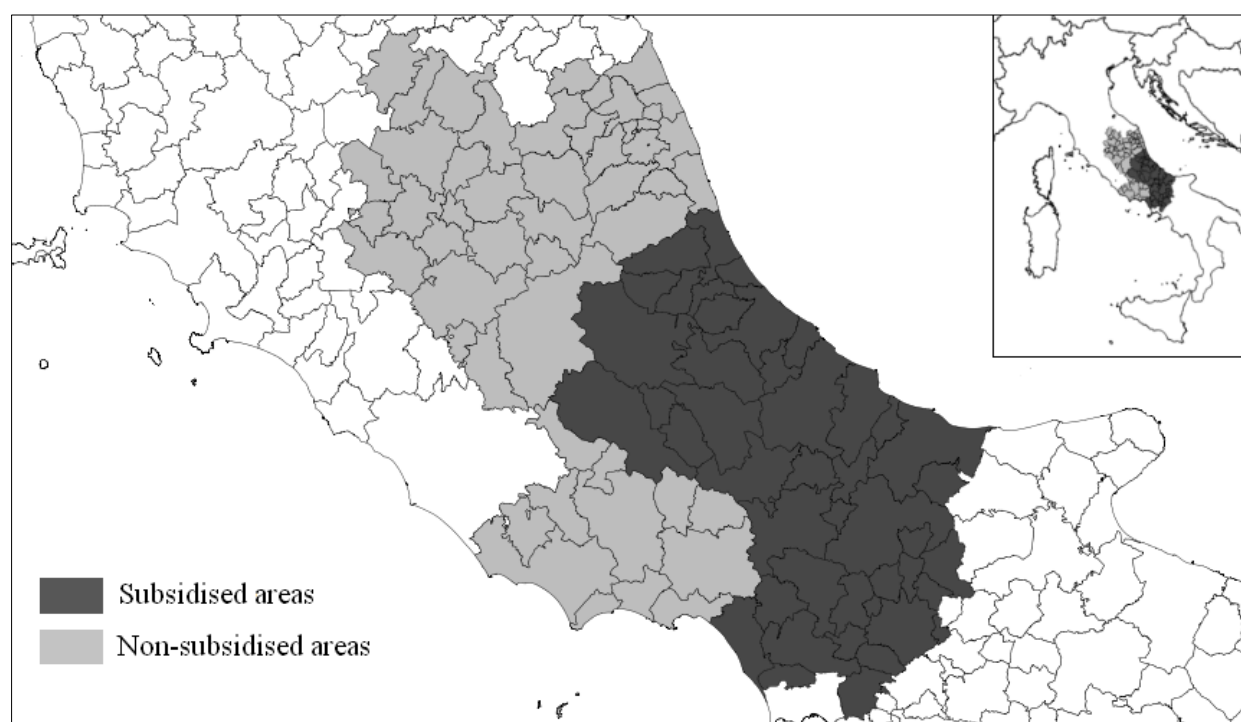
<sup>27</sup> For a detailed description of L488, see Section 3 in Bernini and Pellegrini (2011).

<sup>28</sup> AIDA is a large dataset that contains the budgets delivered by a subset (only corporate enterprises) of over 500,000 Italian firms to the Chambers of Commerce.

The estimation results we present below rely on the assumption that there are no other governmental programs correlated with the allocation of L488 funding.<sup>29</sup>

In order to gauge the ATT and the spillover effects we restrict our empirical analysis to neighbouring areas with socio-economic characteristics rather similar, whereby only some of the areas were eligible for receiving public subsidies.<sup>30</sup> Fig. 1 shows in the darker shade of grey the eligible areas and in the lighter shade of grey the non-eligible areas analysed in the paper.<sup>31</sup> In the programming period 1994-1999 the eligible areas qualified for Obj.1 transfers, while the non-eligible areas did not qualify for Obj.1 transfers, even if some small areas were considered areas with declining industrial production and received Obj.2 transfers.<sup>32</sup>

*Fig. 1: Eligible and non-eligible areas under analysis*



<sup>29</sup> Actually, a feature of L488 minimises the extent of this bias, requiring that firms applying for the incentives renounce any other public subsidies, even without any guarantee of receiving the L488 funds.

<sup>30</sup> Although in 1995 the non-eligible areas had a higher share of workers in the secondary sector - 35.5% - than the eligible areas - 28.2% (30.9% in Italy), the value added per employee in the manufacturing sector was rather similar: €34,498 in the non-eligible areas and €33,846 in the eligible areas (€38,716 in Italy).

<sup>31</sup> The eligible provinces (NUTS 3) are Benevento, Campobasso, Caserta, Chieti, Isernia, L'Aquila, Pescara, Teramo, and Naples (only the local labour system of Nola); whereas the non-eligible provinces are Ascoli Piceno, Frosinone, Latina, Macerata, Perugia, Rieti, Terni, and Rome (only the local labour systems of Colleferro, Velletri, and Subiaco).

<sup>32</sup> The medium-large subsidised firms located in Obj.2 areas received capital grants that support up to 10-20 percent of the total investment expenditures, while the medium-large subsidised firms located in Obj.1 areas received capital grants that support up to 40-50 percent of the total investment expenditures (plus an additional 15% for small firms). Given the large difference in the share of the capital grant on total investment between these areas, in our application we consider all the firms located in the non-eligible areas as non-subsidised firms, even if they received the Obj.2 funds.



By linking the L488 dataset with the financial statement dataset, we reconstruct a merged dataset for the period from 1995-2001 and after a complex process of cleaning and merging we have 2,266 manufacturing firms (code D of the Istat ATECO 2002 classification) that were localised in the areas under analysis. These firms can be partitioned into four groups: 205 firms were subsidised (eligible and subsidised, E+S), 134 firms applied for the subsidies but did not receive them (eligible and unsubsidised, E+US), 645 eligible firms did not apply for the subsidies (eligible non applicants, E+NA), and 1,292 firms were localised in non-eligible areas (non-eligible, NE). The detailed construction of the sample is described in Appendix A.

### *6.3 Results*

Our case study is not very different from scenario D) described in Section 5. Indeed, we hypothesise spillovers limited to the eligible areas and exploit the surrounding non-eligible regions to estimate the ATT and the spillover effects. We focus the analysis on 3 outcome variables: i) the yearly growth rate of tangible capital; ii) the yearly growth rate of turnover; iii) the absolute employment change for each firm. As the traditional analyses, we start from the estimation of the ATT, but the pool of firms from which we draw the control group is made up only of firms located in non-subsidised areas, i.e. we confront E+S firms with NE firms. We identify controls based on the following set of covariates: the growth rate of tangible capital from 1993-1995, the tangible capital in 1995, the turnover in 1995, the ROE in 1995, the number of workers in 1995, and 13 dummy variables that subdivide the manufacturing firms in 14 subgroups according to the Istat ATECO 2002 classification. We then coarsen the joint distributions of these covariates by creating 292 strata (107 of which were matched).<sup>33</sup> Before proceeding to the ATT estimation, it is important to check the similarity of the treatment group with the control group, not only in respect of the conditioning variables, but also in terms of other pre-treatment characteristics that might systematically differ between the two groups in case the conditioning variables do not capture all the relevant differences between subsidised and non-subsidised firms.<sup>34</sup> The results shown in Table 2 are relative to the firms within the common support and demonstrate that the CEM procedure has allowed us to substantially reduce the pre-treatment differences between the treatment and the control group. Indeed, after

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<sup>33</sup> The growth rate of tangible capital from 1993-1995, the investment in 1995, the turnover in 1995, and the ROE in 1995 are coarsened at the median; the manufacturing firms are divided in 14 subgroups according to the Istat ATECO 2002 classification; and the number of workers in 1995 is coarsened using three intervals (micro firms, i.e. 0-9 employees; small firms, i.e. 10-49 employees; medium firms, i.e. 50-249 employees).

<sup>34</sup> We add three covariates: the added value in 1995, the liabilities in 1995, and the cash flow in 1995. These variables are highly correlated with the other five covariates; thereby it is not unexpected that, if the CEM procedure has worked as expected, they are not systematically different between the two groups. Still, the results in Table 2 strengthen the hypothesis that our matching specification substantially reduces the pre-treatment differences between the two groups.

using the CEM weights we see that there are no statistically significant differences between the two groups concerning 8 pre-treatment covariates.

*Table 2: Pre-treatment differences between the treatment and the control group*

Variable	Averages computed without using the CEM weights			Averages computed after using the CEM weights		
	Treatment Group	Control Group	Difference	Treatment Group	Control Group	Difference
Tangible Capital 1995	2,147	938	(1,209)***	2,147	1,795	(352)
Turnover 1995	7,641	4,067	(3,574)***	7,641	7,427	(214)
Growth rate of tang. cap. 93-95	21.34	19.60	(1.74)	21.34	20.12	(1.22)
ROE in 1995	16.75	9.44	(7.31)**	16.75	12.88	(3.87)
Nb. of workers in 1995	42.42	24.93	(17.49)***	42.42	39.70	(2.72)
Added value in 1995	2,129	1,107	(1,022)***	2,129	1,943	(186)
Liabilities in 1995	7,162	3,510	(3,652)***	7,162	6,608	(554)
Cash flow in 1995	899	345	(554)***	899	707	(192)

Note: Amounts are expressed in thousands of Euros. In computing the averages we use only the firms within the common support, i.e. the 173 treated observations and the 689 controls selected by the CEM procedure.

\*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

The ATT estimates are reported in Table 3 for three different matching specifications: the CEM-DiD; the CEM-DiD without strata having more treated observations than controls;<sup>35</sup> and the Mahalanobis-metric matching after using the CEM to restrict the data to areas of common empirical support and removing the aforementioned strata (see Blackwell et al., 2009, for more details on the implementation of the CEM estimates and the STATA module *cem.ado*). The difference between the two groups of firms is 8.92-10.09 percent for the yearly growth rate of tangible capital, 1.74-2.54 percent for the yearly growth rate of turnover and these estimates are statistically significant at the 5% level. The effect on employment is of roughly 5 extra employees and it is statistically significant at the 10% level.

<sup>35</sup> This can be considered as a robustness test of the CEM-DiD results. In our sample the number of controls is far higher than the number of treated units, therefore we argue that strata with more treated units than controls represent a subgroup of firms with characteristics for which it is rare to find reliable controls (the most manifest case in our sample is a stratum with 5 treated observations but only 1 control). Indeed, a few controls are given too much weight in determining the estimates. In the estimation of the spillover effect 1 and the territorial shocks the number of controls is not anymore far higher than the number of treated units. Using the same rationale, we will remove strata with a number of subsidised firms more than double of the number of controls. This informal robustness test is based on a similar rationale of the removal of outliers in the estimation of averages.

Table 3: ATT estimates

	CEM-DiD	CEM-DiD without the 5 strata with more subsidised firms than controls	Mahalanobis-metric matching after using the CEM to restrict the data and the removal of 5 strata
Bandwidth			
Yearly growth rate of tangible capital	8.92 (1.25)***	9.80 (1.32)***	10.09 (2.07)***
Yearly growth rate of turnover	1.74 (0.76)**	2.54 (0.76)***	2.23 (1.09)**
Absolute employment change for each firm	4.81 (1.58)***	5.55 (1.61)***	4.92 (2.53)*
Nb. matched subsidised firms	173	156	156
Nb. controls	689	682	682

Note: Standard Errors in parentheses. When we remove the 5 strata with more subsidised firms than controls we lose 24 observations (17 treated and 7 non-treated firms). Of the 689 controls there were 69 firms that received L488 funds. To take into account the negative bias that these subsidies bring about we subtract the amount subsidised to those observations from the numerator of the first two dependent variables. Repeating the CEM-DiD estimation, we find that the difference between the two groups of firms is 9.10% for the yearly growth rate of tangible capital and 1.81% for the yearly growth rate of turnover. These estimates are statistically significant at the 5% level.

\*\*\*p<0.01, \*\*p<0.05, \*p<0.1.

In this application we argue that the proximity and the similarity of the eligible and non-eligible areas should rule out the presence of relevant territorial shocks, but we have to check this empirically. We adopt the same matching specification described above to compare the non-subsidised service firms' outcome variables in eligible and non-eligible areas. We find slightly positive territorial shocks but none of the estimates are statistically significant (see Appendix B). We then estimate the spillover effect 1 confronting the E+NS and the E+NA firms with the NE firms using again the same matching specification. The results are summarised in Table 4.

Table 4: Spillover effect 1 estimates

	CEM-DiD	CEM-DiD without the 4 strata where the subsidised firms more than doubled the controls	Mahalanobis-metric matching after using the CEM to restrict the data and the removal of 4 strata
Bandwidth			
Yearly growth rate of tangible capital	0.37 (0.79)	0.43 (0.81)	0.48 (1.07)
Yearly growth rate of turnover	-0.48 (0.46)	-0.70 (0.47)	-1.16 (0.63)*
Absolute employment change for each firm	-1.74 (0.66)***	-1.81 (0.68)***	-2.05 (0.94)**
Nb. matched service firms located in the eligible area	693	668	668
Nb. controls	1078	1074	1074

Note: Standard Errors in parentheses. When we remove the 4 strata with a number of subsidised firms more than double of the number of controls we lose 29 observations (25 firms located in the eligible area and 4 firms located in the non-eligible area).

\*\*\*p<0.01, \*\*p<0.05, \*p<0.1.

The spillovers are positive for investment and negative for turnover but these estimates are statistically insignificant. On the contrary, we find negative employment spillovers of magnitude

-2 that are statistically significant at the 5% level. However, different set of covariates and coarsening procedures will yield different ATT and spillovers estimates; this is why we check the robustness of our results in Appendix C using a slightly different set of covariates and coarsening intervals. In general, the robustness analysis confirms the extent of all the ATT and spillover effect 1 estimates, except for the negative employment spillover estimate that turns out to be nearly halved.

Assuming that the population of all the eligible manufacturing firms has the same proportion of subsidised firms that we have in our sample, we can estimate the total effect on employment of the L488 funds. Multiplying the ATT estimates by the number of subsidised firms and the spillover effect 1 estimate by the number of eligible but unsubsidised firms, we find a negative balance for the effect of the subsidies on the total employment level of the manufacturing firms in the eligible area in four cases out of six (considering also the robustness analysis estimates).

Finally, we estimate the spillover effect 2 using as a proxy the difference between the ratio of the number of business births to the number of closing-down firms in the eligible and non-eligible areas during the time span examined. This difference is positive but close to zero, and we interpret this as evidence that the agglomeration effect has at the very least effectively contrasted the crowding-out effect.

## **7. Conclusions**

The main aim of this paper is to make business incentives programs' empirical evaluations more thorough and more pertinent to the policymakers' targets. We do this by proposing possible ways to avoid biased ATT estimates and to retrieve the main spillover effects. This enables us to recover a global estimate of the effect of capital incentives on the regional economy. Our paper moves the spotlight from the policy effect on subsidised firms to the global effect of the industrial policy on the targeted territory and this makes possible to determine if the subsidies have had a welfare-enhancing role in the underdeveloped regions.

Our novel approach allows evaluating the presence and the extent of micro spillover effects. Contrasting the agglomeration effect with the cross-sectional substitution and the crowding-out effect we do not find in our application statistically significant spillovers with respect to investment and turnover; however, we find negative employment spillovers that are statistically significant at the 5% level. This finding emphasises that the ATT on itself is not a sufficient parameter to evaluate the effectiveness of an industrial policy.

The welfare computation, i.e. the combined assessment of the 3 parameters, suggests that capital subsidies engender a growth process in the eligible area in terms of investment but not in respect of employment. This suggests that the subsidised manufacturing firm located in the eligible area attracts their extra employees from firms located in the same area but not subsidised and that we cannot rule out the possibility that the substitution effect (firms substitute labour with capital) might be in place. This result is in line with the De Castris and Pellegrini' spatial crowding-out finding.

A possible interpretation of our results originates from a simple observation: in the factor market there is, to some extent (at least within a small area), labour mobility; whereas capital is a very deep-rooted factor (at least in the short-run). Besides, in the goods market firms located in the same area compete on the same job-market, while they often do not compete on the same product market. Therefore, it is plausible that spillovers are much stronger with respect to employment than capital.

Our study leaves room to some extensions; most notably, it is possible to include more structure into our framework that would require the adoption of spatial tools capable of exploiting the physical distance among firms for estimating the micro spillover effects.

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## Appendix A. Data description

In our application we looked for subsidised areas neighbouring non-subsidised areas. The main criterion adopted to choose these areas was their similarity with respect to GDP per capita,<sup>36</sup> industrial composition, and cultural traditions. Moreover, we excluded the areas of Rome and Naples because such large urban agglomerations have special features, clearly distinct from the other peripheral areas analysed.

The initial pool of firms consisted of all the subsidised and non-subsidised firms in the areas under analysis. After the merging procedure (using VAT identification number as firm identifier), the total number of firms under analysis was 6,446. We then proceeded with the removal of certain categories of observations in order to estimate the ATT and the spillover effect 1:

- The non-manufacturing firms.
- Concerning duplicate projects, i.e., applications for more than one auction, we decided to exclude the non-financed projects if the referring firm had already received L488 funds in a previous auction.
- We considered only firms having a meaningful balance sheet at least since 1993.
- Projects that presented anomalies and irregularities<sup>37</sup> have not been considered.
- Financed firms whose investment program was not yet concluded in 2001 have been discarded.
- Large firms, i.e. firms with 250 or more employees, were discarded because of their particular characteristics (especially in the Italian context whereby they are quite rare).

After verifying that the cleaning and integration procedures do not have a different impact on the treatment and the control group, our attention focused on the final dataset on which the evaluation model was implemented. This dataset consists of 205 subsidised firms (E+S), 134 firms that applied for the subsidies but did not receive them (E+US), 645 eligible firms that did not apply for the subsidies (E+NA), and 1,292 firms localised in non-eligible areas (NE).

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<sup>36</sup> Of course, being the eligible areas part of regions (NUTS 2) with per capita GDP lower than 75% of the Community average in 1994 (Obj. 1 regions), they had generally a lower GDP per capita, but not very different from the non-eligible areas considered. The percentage of their per capita GDP with respect to the Italian per capita GDP in 1995 for the eligible provinces was: Benevento 64%, Campobasso 73.7%, Caserta 56%, Chieti 89.5%, Isernia 67.2%, L'Aquila 90.3%, Pescara 81.5%, and Teramo 81.1%. Concerning the non-eligible provinces the same ratio was: Ascoli Piceno 91%, Frosinone 91.1%, Latina 80.9%, Macerata 92.8%, Perugia 98.3%, Rieti 75.6%, and Terni 91.4%.

<sup>37</sup> We have decided to exclude from the analysis the subsidised firms from which the Ministry of Economic Development has revoked more than 25% of the L488 funds.

In order to check for territorial shocks we have constructed another sample discarding from the initial pool of 6,446 firms all the firms that do not belong to the tertiary sector (code G of the Istat ATECO 2002 classification) and that do not have a meaningful balance sheet at least since 1993. This subsample of service firms consists of 510 firms located in the eligible area and of 759 firms located in the non-eligible area.

## Appendix B. Checking for the presence of territorial shocks

When we check for the presence of territorial shocks we use the same matching specification adopted in the robustness analysis. Coarsening the joint distributions of these covariates we create 331 strata (177 of which were matched). The results are shown in Table B1.

*Table B1: Territorial shocks estimates*

	CEM-DiD	CEM-DiD without the 4 strata where the subsidised firms more than doubled the controls	Mahalanobis-metric matching after using the CEM to restrict the data and the removal of 4 strata
Bandwidth			
Yearly growth rate of tangible capital	1.38 (1.10)	1.33 (1.11)	1.17 (1.39)
Yearly growth rate of turnover	0.61 (0.56)	0.82 (0.57)	0.95 (0.74)
Absolute employment change for each firm	0.11 (0.38)	0.48 (0.38)	0.36 (0.55)
Nb. matched service firms located in the eligible area	426	405	405
Nb. controls	606	601	601

Note: Standard Errors in parentheses. When we remove the 4 strata with a number of eligible firms more than double of the number of controls we lose 26 observations (21 firms located in the eligible area and 5 firms located in the non-eligible area).

\*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

The difference between the two groups of service firms is 1.17-1.38 percent for the yearly growth rate of tangible capital, 0.61-0.95 percent for the yearly growth rate of turnover, and 0.11-0.48 for the absolute employment change. Although all these estimates are positive, they are never statistically significant. Moreover, their extent is almost negligible and even subtracting them from the ATT the results would change only marginally.

## Appendix C. Robustness analysis

We check the robustness of our results using the same set of conditioning variables used in Section 6.3; except for the fact that now we do not include the subdivision of manufacturing firms in 14 subgroups, i.e. with this specification firms from different subgroups can be matched. At the same time, the growth rate of tangible capital from 1993-1995, the investment in 1995, the turnover in 1995, and the ROE in 1995 are now coarsened into quartiles instead of at the median. Table C1 and Table C2 report the results for the ATT estimates and for the spillover effect 1, respectively.

*Table C1: ATT estimates*

	CEM-DiD	CEM-DiD without the 4 strata with more subsidised firms than controls	Mahalanobis-metric matching after using the CEM to restrict the data and the removal of 4 strata
<b>Bandwidth</b>			
Yearly growth rate of tangible capital	11.40 (1.29)***	11.38 (1.34)***	11.18 (2.08)***
Yearly growth rate of turnover	2.94 (0.74)***	3.09 (0.77)***	2.88 (1.13)**
Absolute employment change for each firm	4.66 (1.81)**	4.36 (1.81)**	7.00 (2.67)***
Nb. matched subsidised firms	161	151	182
Nb. controls	704	698	1086

Note: Standard Errors in parentheses. When we remove the 4 strata with more subsidised firms than controls we lose 16 observations (10 treated and 6 non-treated firms). Of the 704 controls there were 63 firms that received L488 funds. To take into account the negative bias that these subsidies bring about we subtract the amount subsidised to those observations from the numerator of the first two dependent variables. Repeating the CEM-DiD estimation, we find that the difference between the two groups of firms is 11.54% for the yearly growth rate of tangible capital and 3.02% for the yearly growth rate of turnover. These estimates are statistically significant at the 1% level.

\*\*\*p<0.01, \*\*p<0.05, \*p<0.1.

*Table C2: Spillover effect 1 estimates*

	CEM-DiD	CEM-DiD without the 6 strata where the subsidised firms more than doubled the controls	Mahalanobis-metric matching after using the CEM to restrict the data and the removal of 6 strata
<b>Bandwidth</b>			
Yearly growth rate of tangible capital	0.22 (0.81)	0.19 (0.83)	0.19 (1.10)
Yearly growth rate of turnover	-0.86 (0.48)*	-0.83 (0.49)*	-0.48 (0.65)
Absolute employment change for each firm	-1.17 (0.59)**	-1.20 (0.62)*	-1.51 (1.01)
Nb. matched service firms located in the eligible area	676	652	652
Nb. controls	1059	1053	1053

Note: Standard Errors in parentheses. When we remove the 6 strata with a number of subsidised firms more than double of the number of controls we lose 30 observations (24 firms located in the eligible area and 6 firms located in the non-eligible area).

\*\*\*p<0.01, \*\*p<0.05, \*p<0.1.