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DEVELOPMENTS IN THE EVALUATION OF CAPITAL SUBSIDY POLICIES

THESIS SUBMITTED IN PARTIAL FULFILMENT OF THE REQUIREMENTS FOR THE DEGREE OF DOCTOR OF PHILOSOPHY IN ECONOMIC STATISTICS

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Introduction: scientific context and motivation

This PhD thesis is comprised of three self-contained but related essays on the evaluation of capital subsidy policies.

Capital subsidies are part of the place-based policies that industrialised countries have been adopting for decades in order to foster self-sustaining growth and boost capital expenditure, employment and competitiveness of firms located in their most underdeveloped areas. The place-based approach relies on the principle that opportunities for growth exist in the entire territory, across all types of regions. Such principle is opposite to the one behind people-based policies - which aim to maximise the opportunities of the people and firms located in lagging regions by allowing them to migrate to and succeed in economic centres. Advocates of the place-based approach suggest that exogenous policy action is needed to trigger endogenous changes in lagging regions with persistent underdevelopment caused by the incapacity of such regions to maintain the pace of growth and development of leading regions and to make productive use of the resources available.

A capital subsidy policy is typically a selective policy that provides grants to finance the most deserving investment projects, i.e. those projects that guarantee the most efficient use of public funds and the creation of new jobs in areas with a struggling industrial sector and high unemployment rates. However, the literature has shown scant systematic evidence on the achievement of such targets.

Although a number of researchers have made substantial progress in improving the credibility of capital subsidies' evaluations, many interesting research issues remain yet to be tackled. This thesis tries to make new developments on the following matters: the reduction of the selection bias, the implementation of a rough cost-benefit analysis, and the estimation of policy spillovers' extent.

One of the main reasons behind the considerable variation in the estimated impact of capital subsidies is due to the difficulties in facing the selection bias, i.e. in disentangling the policy effect from the most relevant confounding factors. Indeed, capital subsidy policies usually pick firms in a non-random manner causing the endogeneity of the assignment process. The policy effect should be measured as the difference between the average outcome of a group of firms composed of financed

firms and the average outcome of the same group in the absence of the policy. Obviously, data relative to the latter group of firms are not directly available; therefore, the challenge is to find a valid control group. To date, only a restricted number of papers have addressed the selection bias using credible approaches.

Besides, at least in theory, the ultimate word on the effectiveness of a capital subsidy policy should lie with a comprehensive cost-benefit analysis. Nevertheless, very few researchers have tried to fill this gap, mainly because it is extremely complex, if not impossible, to include in the evaluation all the costs (especially in terms of distortion of the competition) and the benefits (most notably in terms of long run competitiveness of financed firms) brought about by a capital subsidy policy.

Finally, the policy evaluators' focus (almost) exclusive on the impact of the policy on subsidised firms' outcomes does not seem to be justified considering the likely spatial externalities generated by the new subsidised investment. The lack of studies on capital subsidies spillovers is partially justified by the common reliance of the most adopted econometric methods on the Stable Unit Treatment Value Assumption (SUTVA). Such assumption holds if the causal impact of the subsidies on a firm does not depend on:

1) the intensity of the subsidies and how the subsidies are dispensed;

2) the subsidies that other firms receive, including competitors.

The evaluation strategies based on the SUTVA do not model how firms affect each other's but assume that even if they interact, the subsidies received by one or more of these firms do not influence the future outcomes of the other interacting firms. The SUTVA seems particularly strong especially when we consider competing firms.

After a critical survey of the literature reported in Essay 1, the aforementioned issues are directly faced in Essay 2 and Essay 3.

Essay 1: A critical survey on capital subsidy policies. Despite the long history of capital subsidies in most developed countries and the numerous evaluations of their effectiveness, there is no comprehensive survey in the literature. This essay aims to provide a complete review of the most relevant research works in such literature highlighting their main findings. Besides, the core threats to internal validity and the main issues that a researcher has to face in order to deliver a robust evaluation work are stressed.

Essay 2: The causal impact of capital subsidies: a multiple regression discontinuity design approach. This essay analyses the impact of a policy instrument - Law 488/92 (L488), the main Italian regional policy - that allocates subsidies to private firms by a multiple ranking system. Thanks to the peculiar L488 selection process that creates the conditions for a local random experiment, we are able to assess the effectiveness of these types of incentives for a relevant subgroup of firms. We propose a nonparametric multiple rankings regression discontinuity design that exploits the sharp discontinuities in the L488 rankings and extends the regression discontinuity design (RDD) approach to a context where the treatment is assigned by multiple rankings with different cut-off points. We find that the impact of the subsidies on employment, investment, and turnover is positive and statistically significant, while the effect on productivity is mostly negligible. The new subsidised capital is additional but non-complementary with the owner-financed investment. The results are robust to different specifications and not due to intertemporal substitution.

Essay 3: Beyond the SUTVA: how policy evaluations change when we allow for interactions among firms. The shortage of studies on spatial spillovers of industrial policies is rather surprising considering that such policies are usually designed for generating spatial externalities. In Essay 3 we try to fill this gap proposing a new framework that partially relaxes the SUTVA assuming that a firm might interact only with firms having a limited economic distance from it (e.g. firms that belong to the same sector of activity) and that the intensity of these interactions is diminishing in distance and it does not extend over a certain threshold. This allows us to contrast the positive agglomeration effects with the negative cross-sectional substitution and the crowding-out effect. The global evaluation of the ATT and the spillover parameters shifts the spotlight from the policy effect on subsidised firms to the global effect of the industrial policy on the targeted territory making possible to determine if the subsidies have had a welfare-enhancing role in the underdeveloped regions. Analysing the effectiveness of the Italian L488 policy on firms located in peripheral areas, we find - in line with most of the literature - a positive and large effect of the policy on subsidised firms in terms of investment, turnover, and employment; however, the employment growth is in part determined to the detriment of affected untreated firms located in the very proximity of one or more treated firms that belong to the same sector of activity. This finding suggests that the ATT on itself is not a sufficient parameter to evaluate the effectiveness of an industrial policy and that we cannot rule out the possibility that the substitution effect (firms substitute labour with capital) might be in place.

1. A critical survey on capital subsidy policies

1.1. Introduction

In the last decade there has been a surge in papers and reports on the dispute between place-based and people-based policies (see, inter alia, Glaeser and Gottlieb, 2008; OECD, 2009; World Bank, 2009; Farole et al., 2011; Barca et al., 2012). Advocates of the latter approach support policies that maximise the opportunities of the people (and firms) located in lagging regions by allowing them to migrate to and succeed in economic centres (Garretsen et al., 2013). They highlight the inefficiencies of regional policies that seek to spread growth and activity more evenly amongst regions, suggesting the existence of a "trade-off" between the pursuit of regional equity and national efficiency. Their considerations are supported by the new economic geography models showing that spatial agglomeration raises national economic growth because the localised, self-reinforcing positive externalities and spillovers involved increase innovation and productivity.

On the other hand, the place-based approach relies on the principle that opportunities for growth exist in the entire territory, across all types of regions (see, among others, Pike et al., 2006; Barca, 2009). The aim is to maximise national output by encouraging each individual region to reach its growth potential from within.[1] Advocates of this approach suggest that exogenous policy action is needed to trigger endogenous changes in lagging regions with persistent underdevelopment caused by the incapacity of such regions to maintain the pace of growth and development of leading regions and to make productive use of the resources available.[2] It is from this view of economic development that policies targeting underdeveloped areas have arisen.

[1] A new developmentalist approach suggests that local and regional development policy should not just be about promoting greater growth, but also about reducing levels of inequality, and that mobilising resources in lagging and/or peripheral areas may constitute a valid recipe for both greater overall growth and lower territorial polarisation. More importantly, it suggests that tackling local and regional inequalities may be necessary for the achievement of national wellbeing (Tomaney, 2010). Indeed, an excessive concentration of economic growth in a few areas […] is not only costly in terms of territorial cohesion and equity, but particularly on the ground of economic efficiency itself (Camagni and Capello, 2010).

[2] Such structural inequalities are considered circular and cumulative when skilled human capital emigrates to leading regions, weakening innovative capacities in lagging regions, leading to adverse selection effects for the existing population and for political behaviours and institutions, in a "vicious circle" scenario (Farole et al., 2011). Layard (2006) argues that although migration may lead to higher income, the negative effects of loss of family stability and higher crime rates tend to dominate the income gain.

In a period of growing dispute among proponents of place-based and people-based policies, the referee's role should be played by empirical evidence. This paper aims to contribute to this heated debate surveying one of the most popular place-based policies: business capital subsidies.[3,4] This is typically a selective policy that provides grants to finance the most deserving investment projects, i.e. those projects that guarantee the most efficient use of public funds and the creation of new jobs in areas with a struggling industrial sector and high unemployment rates. The new additional investments should help to modernise production processes, introduce more up-to-date technologies, and generally increase the competitiveness and viability of subsidised firms in terms of their ability to produce new products and processes and/or produce existing products more cost effectively (Harris and Robinson, 2004). Indeed, capital subsidies are supposed to foster self-sustaining growth boosting capital expenditure, employment and competitiveness of firms located in underdeveloped regions but there is scant systematic evidence on the achievement of such targets. In this paper we will try to extrapolate the most relevant findings from the empirical literature on the effectiveness of capital subsidy policies directed to areas or regions with particular problems of underdevelopment and/or unemployment, leaving out of the review innovation policies, programmes directed only to urban areas, and employment subsidies. This is made all the more necessary by literature reviews of papers on business incentive programmes that usually mix the findings on capital subsidies with findings on other place-based policies set up following different rationales, such as R&D, SMEs, FDI, and EZs.

The paper has been organised as follows: the next section summarises the theoretical rationales for and against business incentive programmes. Section 1.3 describes the evaluation problem due to the presence of selection among treated and how it has been faced in the literature, followed by a presentation of the main findings in Section 1.4. A detailed review of the most common problems that policy evaluators have to face when dealing with capital subsidy policies is reported in Section 1.5. Finally, Section 1.6 concludes.

[3] The expressions "investment incentives" and "business incentives" will be used interchangeably with "capital subsidies".

[4] To be more specific, we will focus our analysis on developed countries rather than developing countries. This distinction is necessary because problems such as migration and poverty are much more pervasive in developing countries (see Carvalho et al., 2006).

1.2. Why governments should (or should not) finance capital subsidy programmes?

In the last decades, business incentive programmes have been charged with the most diverse inefficiencies. Possible dynamic inefficiencies have been suggested by several authors. For example, Lee (1996) and Harris and Trainor (2005) suggest that targeting, which could be seen as a form of protection, might make subsidised firms overreliant on "production" subsidies, causing a failure in reorganising their activities and improving their performances to the same extent as non-assisted firms that face the same competitive market pressures. Moffat (2013) denounces another possible problem of dynamic inefficiency due to supporting plants that would otherwise be forced to close. Indeed, capital subsidies may impede the Schumpeterian process of "creative destruction" that creates growth in the economy by shifting resources from low- to high-productivity plants. A related matter is pointed out by Bergstrom (2000), who claims that politicians and bureaucrats might be more interested in maximising political objectives than in economic efficiency; therefore, resources might be suboptimally allocated. It is also likely that incentives are offered in some cases primarily to give politicians "talking points" or "bragging rights" regarding their role in expansions whose true cause cannot be clearly identified by the electorate (Gabe and Kraybill, 2002). In addition, the assignment process of the subsidies might cause an allocative inefficiency incentivising subsidised firms to choose the K/L combination that maximises the likelihood of receiving the subsidy instead of the optimal combination of resources.

Among others, Harris and Trainor (2005) highlight the likely deadweight loss due to the information asymmetry between businesses and the government. Indeed, intertemporal substitution might be in act, i.e. subsidised firms may pocket the subsidies and simply bring forward projects originally planned for the post-intervention period. Moreover, Criscuolo et al. (2012) point out a particular form of deadweight loss: large firms could "game the system", i.e. they could increase employment at subsidised plants at the expense of employment in unsubsidised plants.

What works for subsidised firms does not necessarily work for subsidised territories; in other words capital subsidies might engender the cross-sectional substitution and the crowding-out effect. The former effect implies that subsidised firms take some of the investment opportunities that unsubsidised firms would have exploited in absence of the policy; whereas, the crowding-out effect is in act if subsidised firms crowd-out of the market non-subsidised firms. Furthermore, non-subsidised firms partly finance the subsidies trough taxation (see Bergstrom, 2000) and it is possible

that they experience negative general equilibrium effects such as an increase in the price of capital (see Bronzini and de Blasio, 2006).[5]

On the other hand, capital subsidies have been also considered capable of generating some positive effects as investment in additional capital is a prime determinant of national rates of productivity growth (see De Long and Summers, 1991). Capital subsidies can be adopted to overcome credit market imperfections helping the market to achieve efficiency if some companies are denied access to credit despite the fact that they have viable business projects (Felsenstein et al., 1998).[6]

As noticed by Bergstrom (2000), in the literature on regional economics one line of research argues that various forms of market failure give rise to agglomeration effects. For example, economies of scale and location advantages associated with easy access to large markets, skilled labour and technological knowledge, in combination with migration of the most highly skilled members of the labour force from the lagging regions, might lead to growing polarisation between different regions.[7] Business incentives - supporting firms located in the backward regions or firms that decide to relocate in the backward regions - are seen as a way to shift this development path (see Devereux et al., 2007).

Business incentive programmes might also compensate for local external diseconomies[8] and induce firms to locate their investment in backward areas kick-starting a growth process in underdeveloped areas.[9] Besides, competition in capital subsidies is seen as a way to trigger endogenous changes and move the economy of low-income regions towards a more efficient equilibrium (see Essay 3).

Finally, it is important to stress that investment subsidy policies try to boost the investment level in lagging regions reducing the cost of capital and the theoretical effect that this has on employment is unclear (see May, 1979; Schalk and Untiedt, 2000). Policies targeting regions with high

[5] Criscuolo et al. (2012) argue that in case of policies with funds much smaller than the national GDP, general equilibrium effects are negligible.

[6] Credit constrained firms may not be able to pay the required fixed costs to enter new markets or introduce new production technology. Policymakers might also face such market failure using soft loans (see Bondonio and Greenbaum, 2014, for an empirical comparison between soft loans and capital grants).

[7] Severe regional disparities can strain the fabric of national unity and generate social conflicts. It is argued that regional incentives can reduce that strain and provide a sense of fairness, regional balance, and stability in the country and also minimise welfare dependency by encouraging entrepreneurship and economic self-reliance (Cohen and LeGoff, 1987).

[8] Lagging regions usually supply lower wages than advanced regions but their advantage is limited by lower levels of labour productivity. In fact, what matters to firms in choosing their location are unit labour costs or "efficiency wages": in advanced regions, a high productivity generated by a strong industrial culture, efficient services and good infrastructure may well outweigh the disadvantage of higher salaries and generate a continuing external competitiveness (Camagni and Capello, 2010).

[9] However, incentives might be too small or too broad in scope or offered for too short a period of time to trigger the decision to invest or induce large location shifts away from the optimal location. As a result, incentives will often influence investment decisions at the margin (Cohen and LeGoff, 1987).

unemployment rates usually try to create jobs; this is why some capital subsidy programmes deliberately reward projects with an extra use of labour. In case of creation of both additional and better-paying occupation, capital subsidies may boost worker's skills, self-confidence, and reputation with employers. This greater human capital may increase some workers' long-run employability and wages (Bartik, 2012).

1.3. The Most Common Evaluation Strategies

If public funds would be allocated by a random process, an optimal method to evaluate the impact of capital subsidies would be a simple difference between the outcomes of treated and untreated firms (assuming that the Stable Unit Treatment Value Assumption, SUTVA, holds).[10] Unfortunately, the assumption of random assignment is not credible when the policy instrument determines a deliberate selection process. If a business incentive programme picks firms in a non-random manner, the participation is endogenous and the projects are heavily selected. To avoid selection bias, the policy effect should be measured as the difference between the average outcome of a group of firms composed of financed firms and the average outcome of the same group in the absence of the policy. Obviously, data relative to the latter group of firms are not directly available; therefore, the challenge is to find a valid control group.

Other than being pervasive in most observational studies, selection bias is almost unanimously considered the most relevant problem in the evaluation of capital subsidy policies. Indeed, as well recognised in Bondonio and Greenbaum (2014), any enterprise support policy must be evaluated disentangling programme effects from many confounding factors affecting firms and economic growth outcomes independently from the programme being evaluated.

The typical target of policy evaluators is the average treatment effect on the treated (ATT) parameter, however also the local average treatment effect (LATE) and the intention to treat (ITT) parameters have been recently estimated. In the last two decades different evaluation techniques have been used to estimate such average causal effects of capital subsidies on firms' performances. The most well-known evaluation techniques have been harnessed: from the regression model to the

[10] The SUTVA holds if:
i) there exists only one version of the treatment, i.e. the subsidy intensity is constant for each treated firm;
ii) the subsidies that other firms receive (including competitors) do not affect firm's *i* potential outcomes.
See Section 1.5 for a discussion on what happens when the SUTVA does not hold.

more recent regression discontinuity design. In this section, we will review the most common evaluation strategies highlighting merits and limitations of each methodology.[11]

The Regression Model. The most basic approach to evaluate the impact of investment incentives consists in regressing the outcome variable on a dummy variable that is equal to 1 if the firm received the subsidies. The validity of such approach depends on the nature of the relationship between firms' performances and the policy. If the best performing firms - those with high levels of managerial competence, good products, innovative etc. - are the most likely to get the subsidies, a positive result derived from a simple regression based on ordinary least squares (OLS) is likely subject to an upward bias. Likewise, if the subsidies are more likely to be assigned to poor-performing firms, then a simple regression will probably underestimate the impact of the policy. A first solution to this selection problem is the addition to the regression model of a set of control variables, supposed to exogenously influence the outcome variable.

The regression model requires assumptions on the functional form (often assumed to be linear) of the dependency between the outcome variable and the observed covariates. Even if the addition of control variables would probably ease the selection bias problem, most researchers use this model only as a preliminary method in their analysis, adopting more sophisticated methods to further reduce selection bias.

Matching Methods. Matching methods ex post mimic an experiment by matching each financed firm to one or more non-financed firms as similar as possible with respect to a given set of pre-treatment variables X. The main advantage offered by matching is that being a non-parametric method, unlike the regression model, it does not require functional form assumptions.[12] However, the dimensionality of the space of the matching variables can represent a serious limitation to the implementation of matching. Indeed, if there are a high number of covariates, it may be difficult to identify a non-subsidised firm to match with every subsidised firm. A popular alternative is to match on a function of the X: the probability of assignment given the set of characteristics X. This matching method is named Propensity Score Matching (PSM). The correct use of the PSM requires that firms with the same propensity score must have the same distribution of observable (and non-observable) characteristics independent to the treatment status. This hypothesis is called the

[11] For a more general overview of these methods see Blundell and Costa Dias (2009). Bondonio (2009) reviews and discusses statistical techniques aiming at offering some clear guidance on how to choose the appropriate focus of the evaluation, the policy relevant evaluation parameters and the empirical impact identification methods for evaluating a variety of types of business incentive programmes.

[12] Matching methods represent an improvement over the linear regression model also for two other reasons. First, the regression model makes use of observations outside the common support. Second, the regression model does not allow weighing control observations in order to build ex post a control group with pre-treatment variables distributions similar to the ones of the treated group.

"balancing hypothesis" and can be tested for observable variables. Moreover, PSM can be combined with the Cox proportional hazards model to estimate the impact of capital subsidies on the likelihood of plant exits.

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 (selection on the observables). Second, the common support assumption, i.e. every subsidised firm has at least one counterpart in the control group with the same or very similar observable characteristics. Especially the CIA is a strong assumption and the possible presence of selection on the unobservables is unaccounted for by matching methods.

Difference-in-Differences (DID). The difference-in-differences (DID) estimator exploits some naturally occurring event that makes a certain group of firms eligible to capital subsidies but keeps a similar group ineligible. This method requires longitudinal data (at least 2 time periods) and consists in a before and after comparison across these groups of firms. The DID estimator delivers unbiased estimates of the ATT only if two difficult to meet conditions are satisfied:

i) The assignment process does not depend on temporary shocks;[13]

ii) Without the subsidies, the trends of the performances relative to the treated group and the control group would have stayed unchanged.

In this literature, such approach might not be appropriate as the required naturally occurring events are hard to find for capital grants programmes.

The Difference-in-Difference Matching estimator (MDID). Combining matching methods with the difference-in-differences estimator (MDID) allow formulating the main matching hypothesis with respect to the before-after evolution instead of levels. In fact, first-differencing outcomes with respect to a pre-programme period removes selection on the time-invariant unobservables (individual fixed effects and trend effects), while comparing the first-differentiated outcomes for participants with those of observationally identical non-participants removes selection on the observables. In other words, the MDID represents an improvement over both matching and DID because it weakens the identifying assumption for matching by allowing non-observed time-

[13] E.g. evaluating with the DID an investment incentive policy that targets only firms with a growing investment level in the last years would probably deliver upwardly biased ATT estimates.

invariant variables to influence performance. However, time-variant unobservables cannot be controlled for and after the MDID procedure there might still be some residual selection bias.

The Instrumental Variables (IV) approach. In contrast to matching methods, the instrumental variables (IV) approach deals directly with selection on the unobservables. The IV method requires the existence of at least one variable exclusive to the assignment rule, known as the instrument. Such instrument is supposed to affect only the eligibility to receive the subsidies without having a direct impact on firms' performances; this is why it is not included in the set of conditioning variables. This is known as the exclusion restriction. It implies that the potential outcomes do not vary with the instrument and any difference in the mean observed outcomes of two groups of firms differing only with respect to the instrument can only be due to consequent differences in the eligibility and composition of the treatment group with respect to potential gains from treatment. Depending on the assumptions on the homogeneity/heterogeneity of the policy effects and the specific application, the IV estimator allows retrieving the ITT, the ATT or the LATE parameters. In this literature it is rare to adopt the IV because of the difficulties in finding a good instrument that can be argued to satisfy the exclusion restriction.

The Regression Discontinuity Design (RDD). When capital grants are assigned following an assignment rule in which the probability of receiving the incentives changes discontinuously with some continuous variable *s*, researchers might exploit this particular source of randomisation to estimate the LATE under relatively weak assumptions. In case *s* fully determines the assignment of incentives on the basis of a threshold, s^*, this approach is called sharp regression discontinuity design (RDD).[14] With the sharp RDD treatment impacts are estimated by comparing the outcomes from the applicant firms ranked just above and below the cut-off point that determines the treatment status. This is because in such neighbourhood of the threshold the treatment status can be thought of being nearly randomly assigned. This method is typically considered to have a high internal validity and an external validity depending on the homogeneity of the characteristics of assisted firms throughout the entire population of treated. Evaluations based on the RDD require a capital subsidy policy following the afore-mentioned assignment process and the availability of data on the ranking for both treated and non-treated firms.

The Heckman selection estimator. When selection is on the unobservables, one attractive approach to the evaluation problem is to take the nature of the assignment rule explicitly into consideration in the estimation process. The Heckman 2-step estimator does exactly this, treating

[14] When *s* does not fully determine the assignment of subsidies, it is still possible to retrieve the LATE parameter using *s* as an instrument in the IV method. Such approach is called fuzzy RDD.

the endogeneity of the assignment as an omitted variable problem. In a first step the probability of participation in the investment support scheme is estimated using a probit model and the so-called Inverse Mills Ratio (IMR) set-up which should measure the influence of the unobservable variables in the selection process. Then, in a second step the IMR is introduced into the investment regression equation. By estimating this enhanced equation the correlation between the explanatory variables and the error terms is eliminated. In these terms, a positive (negative) and significant coefficient on the IMR is indicative of a positive (negative) sample selection problem, analysed policy being skewed towards high (low) performance firms. An important issue in operationalising the Heckman type model is the avoidance of too much overlap between the selection and performance models. This is why it is highly advisable to include variables in the first stage probit equation which are not included in the second stage investment equation. The latter approach could also be seen as a variant of the more general IV method. The main threats to validity of this parametric method lie in the untestable strong structural assumptions.

1.4. Main Findings

In the literature, there is a considerable variation in the estimated impact of investment support, which, among others, reflects differences in circumstances between countries,[15] regions, sectors and firms, differences in the design of policy and delivery (policy implementation details) and differences in the quality of the data and the analytical methods used in the empirical studies (Brandsma et al., 2013). With this in mind we will attempt to retrace the main findings of this strand of literature[16] gathering together the findings of 3 groups of relatively homogeneous research works carried out in the last 20 years.[17,18] Table 1.1 presents synthetically all the surveyed studies,

[15] For instance, in this survey we will look at papers on capital subsidy programmes adopted in the UK, the Republic of Ireland, Italy, Germany, Estonia, Sweden, Poland, Finland, Greece, and the USA.

[16] We exclude from our review studies that evaluate the effectiveness of capital subsidies directed to areas or regions with no particular problems of underdevelopment or unemployment (e.g., Gabriele et al., 2006; Bia and Mattei, 2012; Bondonio and Greenbaum, 2014). We also exclude studies that evaluate more than a regional policy together and do not distinguish among policies (e.g., Roper and Hewitt-Dundas, 2001; Girma et al., 2007b). Finally, we do not consider studies that evaluate the effectiveness of investment incentives through the administration of surveys to the subsidised firms (see National Audit Office, 2003; Cannari et al., 2006).

[17] Empirical evaluations of capital subsidies can be divided between area-based and firm-based analyses. In this review we focus on the latter category of studies due to the considerable complexities in disentangling the policy impact from other confounding factors at the area level (see Bondonio, 2009) and the predominance of area-based analyses that do not distinguish the causal effect of capital subsidies from other policies established on different rationales. Besides, even if some of the surveyed studies analyse also the effectiveness of other policies we will refer to them only for the parts on investment incentives.

[18] Until the end of the 1980s, evaluation techniques of business incentive programmes were rarely based on a counterfactual approach. Indeed, trend projections, case studies, surveys concerning a small number of scheme participants, shift-share analyses, basic econometric models, and theoretical models combined with analyses of aggregated data were the most widely used methods to evaluate the effectiveness of capital subsidy programmes.

reporting their main findings.[19] An overview of the most analysed capital subsidy policies is reported in Appendix 1.A.

1.4.1. Studies based on a theoretical framework

A first subgroup of studies is based on the presence of a theoretical framework that drives the empirical analysis. Most of these research works aim to estimate the impact of capital subsidies on the total factor productivity (TFP). Indeed, such type of estimation requires a production function approach (usually a Cobb-Douglas specification).[20]

Using an OLS estimator, Bergstrom (2000) finds that growth through subsidisation has been achieved simply by using more inputs but not by improving on their usage. Moreover, his results suggest that the more subsidies a firm is granted the more inefficient it becomes. Similar results come from Harris and Robinson (2004) using a policy off/policy on model and a GMM estimator. On the other hand, Harris and Trainor (2005) using a similar approach find that without capital subsidies, the TFP would have been between 7-10% per annum lower throughout the analysed period.

Harris and Robinson (2005) break down TFP into its allocative components (entry, exit, within plant, between plant and cross-plant effects) applying a decomposition approach. They find that financed plants experienced negative TFP growth, mostly due to plants with low TFP increasing their market share during the period. A different decomposition procedure allows Skuras et al. (2006) to decompose into three components the TFP (technical change, technical efficiency change, and scale efficiency change). They find that capital subsidies to the food manufacturing sector are not fully additional and affect TFP growth mostly through technical change.

[19] Relevant insights on the effectiveness of business incentives come also from the theoretical literature. In a Diamond-Mirrlees (1971a and 1971b) setting (an economy with competitive markets, constant returns to scale, and flexibility in choosing different commodities taxes for different goods), firm-specific capital subsidies are considered not desirable as they distort the allocation of factors of production causing productive inefficiency. In a similar vein, Sinn and Sinn (1993) argue that regional public support schemes give rise to excessively capital intensive production that contributes to the unemployment problem. On the other hand, Fuest and Huber (2000) develop a theoretical model where union–firm bargaining distorts both employment and investment decisions and investment subsidies dominate employment subsidies in terms of welfare. Finally, Brandsma et al. (2013) find that investment incentive policies that impose additionality in perfectly competitive markets cause distortions in the capital market and lead to lower welfare levels. In contrast, without the enforcement of additionality, the distortions are zero and the investment support fully benefits the firms. In an imperfectly competitive environment, the firm-level investment support may increase investment and may be welfare increasing with and without the enforcement of the investment additionality. However, a complete account of theoretical studies on capital subsidies is beyond the scope of our survey.

[20] Growth of TFP is a productivity measure which reflects the increase of total output that is not explained with increase of capital and labour. The TFP is considered by some authors as the most relevant productivity measure for analysing the efficiency of a subsidised firm. Indeed, while labour productivity (output per worker) may grow simply because of the capital deepening induced by the subsidies, the efficiency with which all inputs are used (measured by the TFP) may not increase at all.

Finally, Devereux et al. (2007) focus on a different research question using a model of firm location choice to study the influence of capital subsidy programmes on the location choice of manufacturing firms. Adopting a conditional logit model they find that grants have a small effect in attracting plants to specific areas. Besides, their results suggest that these subsidies are less effective in influencing firms' location decisions in the face of alternative locations offering countervailing co-location benefits or natural advantages.

1.4.2. Statistically driven studies

We can split the statistically-driven studies in 2 subgroups depending on their credibility. Ideally retracing the steps of the credibility revolution illustrated in Angrist and Pischke (2010),[21] we will use 3 criteria for the afore-mentioned split: i) the internal validity of the evaluation strategy; ii) the quality of the data; and iii) the presence of robustness checks. While the first point has already been discussed in Section 1.3, in the following we briefly illustrate points ii) and iii).

Most evaluation strategies require detailed data on unsubsidised firms to build a valid counterfactual scenario. As the majority of capital subsidy programmes are selective, the best case scenario is the one with data on rejected applicants.[22] Besides, administrative data are usually to be preferred to survey data, as administrative datasets are typically larger, collected in a more consistent way and subject to more rigorous quality checks. Furthermore, in order to increase the statistical power of the methodologies adopted and enlarge the pool of firms from which build the counterfactual, it is important for researchers to have available a large number of treated and non-treated firms. Finally, the presence in the dataset of numerous covariates makes easier to control for selection on the observables.

Another good property of evaluation papers is the use of robustness checks. An estimation result is robust to changes in model specification if the inference a researcher makes with respect to the tested hypothesis or prediction does not change. Among the most used robustness tests there are alternative measures of the dependent variables, additional controls, changes in the sample, alternative estimators, and alternative functional forms. Besides, the robustness of the results should

[21] In the last two decades improvements in empirical work have come from many directions: i) better data and more robust estimation methods; ii) more focus on causal interpretation of the main findings; iii) empirical researchers in economics have increasingly looked to the ideal of a randomized experiment to justify causal inference; iv) more transparent discussion of research designs; and v) robustness checks (see Angrist and Pischke, 2010).

[22] As suggested by Brown et al. (1995), rejected applicants show a propensity for investment very similar to that of subsidised firms. Moreover, control observations should come from eligible areas, as the use of firms located outside eligible areas will probably generate a bias in the causal effect estimates (see Moffat, 2013).

be assessed by employing tests specific to the methodology adopted, such as the balancing test for the PSM or the continuity of the density test for the RDD.[23]

1.4.2.1. Studies that do not meet all the credibility criteria

The last column of Table 1.1 reports why the following studies do not meet all the credibility criteria. Besides, as the selection of research works is not clear-cut, we exercise a degree of discretion in classifying the studies.

Employing a Cox proportional hazards model, Girma et al. (2007a) and Harris and Trainor (2007) find that grant payments have helped recipient firms to survive longer. Using a less rigorous approach, Trzciński (2011) finds a negligible impact on survival rates of subsidised firms.

Evidence is more mixed for employment. Pellegrini and Carlucci (2003) use matching, DID and a rough version of the RDD finding that subsidised firms present an employment dynamic from 3 to 15 percentage points higher than in non-subsidised firms. Along these lines, both Gadd et al. (2009) (adopting the MDID estimator) and Trzciński (2011) (adopting the PSM) find a positive effect on employment. Opposite results have been found by Gabe and Kraybill (2002) using the Heckman 2-step estimator, Kangasharju and Venetoklis (2002) using the DID approach, and Ankarhem et al. (2010) using the MDID estimator.

Concerning profitability outcomes, Gadd et al. (2009) and Ankarhem et al. (2010) find a negligible or negative impact of capital subsidies. Finally, Hartsenko and Sauga (2012) find a positive effect on net sales using the DID estimator, Gadd et al. (2009) a positive effect on net turnover, while in the GEFRA-IAB (2010) report a plethora of evaluation techniques has been used (linear regression model, PSM, DID, Heckman selection model) and the results show that treated manufacturing firms have higher investment per employee of around €9,000-€12,000.

1.4.2.2. State of the art empirical studies

Criscuolo et al. (2012) use an IV approach exploiting exogenous changes to area-level eligibility as key form of identification.[24] They find evidence for a positive ATT in terms of employment and

[23] Another possibility is the use of sensitivity analysis to separate the fragile inferences (based on doubtful assumptions) from the sturdy ones. An example of sensitivity analysis is the extreme bounds analysis that determines the range of estimates that the data could support given a precisely defined range of assumptions about the prior distribution (see Leamer, 2010).

[24] Examining the impact of capital subsidies both at the firm-level and at higher levels of aggregation they capture both extensive (plant entry and exit) and intensive margins (growth by incumbents).

investment. At the area level they also find that the programme raised employment and the higher manufacturing employment seems to come from reducing the level of unemployment. These results are strong for smaller firms but essentially zero for larger firms. Using the MDID estimator, Bernini and Pellegrini (2011) reach similar conclusions on investment and employment. They also find that output and value added grew substantially in subsidised firms. On the other hand, Bronzini and de Blasio (2006) adopt a DID combined with a rough version of the RDD or with an ad-hoc comparison group that mirrors the time-series pattern of the treated group before the programme was launched and find some evidence of intertemporal substitution.

Finally, negligible or negative effects on labour productivity or TFP are found in Bernini and Pellegrini (2011), and Criscuolo et al. (2012). This might be due to firms overshooting the optimal amount of employment in order to gain a subsidy.

Concerning the impact of capital subsidies on plant survival a positive effect has been found in two recent papers. Moffat (2013) combines the PSM and the Cox proportional hazards model finding that capital subsidy grants reduce on average the probability of closure by 15-20%. Adapting the RDD approach to the survival analysis framework, Muccigrosso and Pellegrini (2013) find consistently higher survival probability in subsidised relative to non-subsidised start-ups: a statistically significant difference in favour of the treated firms is observed with respect to firm survival, particularly regarding the seventh year of life (approximately 30%). Nevertheless, the discrepancy decreases thereafter.

From the review of the most credible empirical studies emerges an almost homogeneous set of results that can be summarised in a positive impact of capital subsidies on financed firms' employment, investment and plant survival prospects but a negligible effect on productivity. However, these results are still not sufficient to determine a final conclusion on the effectiveness of this policy and future research should continue on the credibility path addressing in more depth the issues that will be delineated in the next section.

Table 1.1. Research works on the impact of capital subsidies on assisted firms' performances

Author(s)	Policy	Dependent variable(s)	Method(s)	Main findings	Relevant problems and limitations
Bergstrom (2000)	Selective subsidies in Sweden (largely capital subsidies). Period: 1987-1993	TFP	Regression model	Growth through subsidisation seems to have been achieved simply by using more inputs but not by improving on their usage. Moreover, the more subsidies a firm is granted the more inefficient it becomes.	The number of subsidised firms is relatively small. Moreover, while subsidised firms are located in lagging areas, most of the non-subsidised firms are located in more developed areas. Finally, the regression model with a small set of covariates is not the best approach to reduce selection bias.
Harris and Robinson (2004)	RSA in Great Britain. Period: 1990-1998	TFP	Policy on/policy off model with a GMM estimator	RSA assistance does not appear to significantly improve plant productivity (except for Scotland).	
Harris and Trainor (2005)	Selective Financial Assistance (SFA) in Northern Ireland. Period: 1983-1998	TFP	Policy on/policy off model with a GMM estimator	The final results show that for all manufacturing, the TFP would have been between 7 and nearly 10% per annum lower throughout 1983–1998 if SFA had not been in operation. Capital grants were more likely to have a positive impact on TFP compared with the other forms of grant-aid.	
Harris and Robinson (2005)	RSA in Great Britain. Period: 1990-1998	TFP	Decomposition approach	British RSA-assisted plants experienced negative TFP growth, mostly due to plants with low TFP in 1990 increasing their market share, which suggests that capital is being substituted for labour.	
Skuras et al. (2006)	Capital subsidies to the food manufacturing sector in Greece. Period: 1989-1994	TFP	Decomposition approach	Capital subsidies are not fully additional. Capital subsidies affect TFP growth, especially through technical change, and not through scale efficiency change.	

Table 1.1. Continued

Author(s)	Policy	Dependent variable(s)	Method(s)	Main findings	Relevant problems and limitations
Devereux et al. (2007)	RSA in Great Britain. Period: 1986-1992	Location choice	Conditional logit model	Subsidies have a small effect in attracting plants to specific areas. They are less effective in influencing firms' location decisions in the face of alternative locations offering countervailing co-location benefits or natural advantages.	
Girma et al. (2007a)	Capital grants and FDI in the Republic of Ireland. Period: 1983-1998	Plant survival	Cox proportional hazards model combined with PSM	Capital subsidies have helped domestic plants to survive longer. On the other hand, they do not seem to have induced foreign multinationals in not shutting down their plants.	The matching procedure has reduced only slightly selection bias (possibly because of the impossibility to distinguish between rejected applicants and non-applicants)
Harris and Trainor (2007)	Selective Financial Assistance (SFA) in Northern Ireland. Period: 1983-1997	Employment Plant survival	Cox proportional hazards model combined with an IV approach	Positive effect on employment mostly due to the stabilisation of employment by sustaining existing plants rather than through the generation of new plants. Being assisted by government grants (ceteris paribus) lowered the hazard rate of closure by 24.1% on average. Using an instrument this estimate reduces to 14.9%.	A table indicates that assisted plants are not a random sample of population of all plants; however, the use of the instrument does not convincingly face such sample selection bias. Moreover, their approach is invalid because of the non-linear relationship between the explanatory variables and the dependent variable in the Cox proportional hazards model (Moffat, 2013).
Trzciński (2011)	Capital subsidies to SMEs in Poland. Period 2004-2011	Plant survival Employment Foreign sales Income Investments	PSM	Subsidies do not have significant effects on survival rates and financial variable. On the other hand, they caused an increase in employment (14 extra jobs per firm) and investment in machinery and technical equipment.	The analysis lacks important aspects, such as a good data description, the balancing test, and robustness checks.
Pellegrini and Carlucci (2003)	Law 488/92 in Italy. Period: 1994-1998	Employment	Matching DID Rough RDD	Subsidised firms present an employment dynamics from 3 to 15 percentage points higher than in non-subsidised firms.	The dataset is composed of much more treated firms than controls and this might undermine the possibility of creating a good counterfactual scenario. Moreover, the authors do not perform any robustness checks.

Table 1.1. Continued

Author(s)	Policy	Dependent variable(s)	Method(s)	Main findings	Relevant problems and limitations
Gadd et al. (2009)	Regional Investment Grants (RIG) in Sweden. Period: 2000-2003	Employment Return on Total Assets ROE Net turnover Income per employee Operating income	MDID with the propensity score computed by a multinomial logit	Subsidised firms have had a more successful development concerning employment growth and net turnover, than unsubsidised companies. Concerning the profitability measures (operation income, return on assets, ROE, and income per employee) subsidised firms are either found not significantly different from the non-subsidised firms, or significantly worse off than the non-subsidised firms.	This study lacks robustness checks and examines a relatively small number of subsidised firms (roughly 65 considering only treated firms within the common support).
Gabe and Kraybill (2002)	Business incentive policy in Ohio (USA). Period 1993-1995	Employment	Heckman selection estimator	Negligible or negative effect on employment. Establishments misrepresent their hiring plans to receive larger incentives from the government.	The authors do not have at their disposal longitudinal data and use cross-sectional data derived from a survey with a response rate of 50%. Besides their empirical strategy is based on very strong assumptions.
Kangasharju and Venetoklis (2002)	Investment and Operation subsidies in Finland. Period: 1995-1998	Employment	DID	The policy had a very mild positive effect on employment with a cost per job of 440,000€.	The DID on itself is not convincing in the absence of a natural experiment event. Lack of robustness checks.
Ankarhem et al. (2010)	Regional Investment Grants (RIG) in Sweden. Period: 1990-1999	Employment ROE	MDID	Subsidised firms did not have better development of ROE than others that did not receive grants. In addition, in most cases, capital grants did not influence employment either.	It appears as the authors did not perform either the balancing test (the most necessary test to check if the PSM has worked as intended) or any other robustness test.
Hartsenko and Sauga (2012)	Enterprise Estonia (EAS), to promote business and regional developments. Period: 2004-2010	Net sales	DID and fixed-effect panel model	Technology investment grant for industrial enterprises increases net sales on average by 33%	Other than the lack of robustness checks, the authors have available only 20 treated observations. Furthermore, their evaluation strategy is hardly convincing in its ability to accurately reduce selection bias.
GEFRA IAB (2010)	Capital subsidies in Eastern Germany. Period: 2000-2007	Investment per employee (or per volume of	Regression model PSM Heckman selection estimator	Capital subsidies are complementary with the other investment projects of the treated firms (two-thirds of the	The dataset used represents the main limitation of this report. It consists in an unbalanced panel of firms derived from a survey where only a small percentage

Table 1.1. Continued

Author(s)	Policy	Dependent variable(s)	Method(s)	Main findings	Relevant problems and limitations
		sales)	DID	additional investment over non-treated firms is explained by the subsidies and one-third is additional).	of firms are observed more than twice and a change of the support status can only be observed for a few firms.
Bernini and Pellegrini (2011)	Law 488/92 in Italy. Period: 1996-2004	Output (sales) Employment Labour Productivity TFP Profit	MDID	Output, fixed assets, and value added grew substantially in subsidised firms. They also find a positive impact of subsidies on employment, i.e. the output effect prevailed over the substitution effect. On the other hand, the productivity of subsidised firms shows less growth than in the non-subsidised firms.	
Criscuolo et al. (2012)	RSA in Great Britain. Period: 1986-2004	Employment Investment Labour productivity TFP	IV	The programme reduced unemployment and raised employment both through intensive and extensive margins. Positive effect on investment and negligible effect on either labour productivity or TFP.	
Moffat (2013)	RSA in Scotland. Period: 1984-2005	Plant survival	Cox proportional hazards model combined with PSM	The receipt of an RSA grant reduces the probability of closure by 15–20%.	
Pellegrini and Muccigrosso (2013)	Law 488/92 in Italy. Period: 1996-2009	Plant survival	RDD (sharp and fuzzy)	At each age, treated firms exhibit a default probability in the following year that is constantly lower that the non-treated firms.	
Bronzini and de Blasio (2006)	Law 488/92 in Italy. Period: 1996-2001	Gross investment as a percentage of capital (or sales or pre-dated assets)	DID coupled with a rough RDD or a strengthening of the common-trend assumption	The increase in investment triggered by the incentives was counterbalanced by a decline in accumulation by the subsidised firms later in time in one of the auctions analysed. Moreover, they find some evidence of negative spillover effects on investment.	

1.5. Empirical evaluations: common concerns and future challenges

Most policy evaluators identify selection bias as the most pervasive threat to the internal validity of evaluation studies; however, selection bias is only the first of a long series of concerns. In this final section we review the main issues that evaluators have to implicitly or explicitly address in order to carry out a thoughtful evaluation work.

Failure of the first SUTVA assumption: different subsidy intensities. The first SUTVA assumption states that there exists only one version of the treatment. If this was true a capital subsidy policy might be evaluated without any distortions using a binary variable to indicate the assignment of the subsidies (as in the vast majority of the surveyed studies). However, several policies allow for different levels of subsidies, depending on the investment project, the firm dimension, the region and also the firms' choice. Policymakers are particularly interested in exploring the impact of different treatment levels on policy outcomes as this may uncover heterogeneities along different amounts of financial aids and provide some information on the optimal level of incentives (Bia and Mattei, 2012).[25] Indeed, the adoption of a binary variable derives mostly from an econometric literature that has developed evaluation strategies with a binary treatment variable in mind that are difficult to extend to a continuous treatment variable setting.

Different methodologies have been adopted to analyse the impact of different subsidy intensities, such as the 2-step matching estimator (see Adorno et al., 2007), the 3-stage conditional DID (see Bondonio and Greenbaum, 2014), and the generalised propensity score estimator (see Bia and Mattei, 2012).

Failure of the second SUTVA assumption: the presence of policy spillovers. The second SUTVA assumption states that the subsidies that other firms receive (including competitors) do not affect firm's *i* potential outcomes. It is a leap of faith to consider this assumption completely satisfied for investment incentive programmes as policies oriented to the growth of underdeveloped regions are designed for generating spatial externalities (De Castris and Pellegrini, 2012). Potentially, these programmes give rise to several externalities, such as the agglomeration effect, the cross-sectional substitution and the crowding-out of non-subsidised firms. If a selective business incentive policy brings about negative (positive) spillover effects on unsubsidised firms located in the vicinity of one or more subsidised firms, the evaluation strategies described in Section 1.3 will

[25] A continuous treatment level approach might help policymakers to minimise the extent of deadweight loss. For instance, if such approach demonstrates that a capital subsidy programme is subject to decreasing marginal returns in correspondence to high per-firm values of assistance, policymakers might reduce deadweight loss lowering the maximum percentage of the total investment cost covered by capital grants.

deliver: i) an upward (downward) estimate of the ATT; and ii) no estimates of the spillover effects (see Essay 3).

As data cannot reveal the extension of spillovers, the failure of the second SUTVA assumption remarkably complicates the evaluators' work because possible solutions involve the adoption of a set of strong assumptions on the extension and/or the nature of the spillovers.

Data concerns. The probability of success in minimising selection bias is positively related to the richness of data on the selection process and the availability of data on a large pool of non-subsidised firms; yet such information is not always available in the desired amount. In addition, even in situations with plentiful data it might be difficult to find a credible counterfactual for the firms with the best (worst) investment projects as they are almost always subsidised (non-subsidised). As such, even when evaluation works claim to retrieve the ATT they sometimes implicitly exclude the causal effect on the most deserving investment projects or they estimate such causal effect relying on stronger assumptions.

Addressing selection bias is difficult enough that sometimes evaluators overlook the discussion about other empirical concerns such as the ones regarding data availability. Data are not always available for all the eligible firms and in general small and medium enterprises are underrepresented in the sample as the probability of inclusion of an eligible firm in a financial dataset is inversely proportional to its size. Besides, in case of longitudinal data researchers usually assume that missing values in certain years are randomly distributed throughout the panel, conversely, data on small firms are more likely to be missing and this might distort the causal effect estimates. In addition to that, as evaluations of industrial policies often require merging different datasets, they might be subject to a decrease in the number of observations due to the possible typing mistakes and/or the presence of mergers, changes of names and activities of some firms in the time-period considered.

Other than the possible presence of measurement errors and outliers (as in almost every observational study), when dealing with firms' data, a researcher has to take also some important decisions on relevant clusters of observations that affect the subsequent analysis: i) firms that disappear from the dataset during the time span analysed; ii) assisted firms that received only part of the subsidies; and iii) assisted firms for which the grants were revoked. The widespread solution is to exclude these categories from the analysis but this might generate substantial biases.

Moreover, it is not always clear what the best dependent variables to use are and if it is best to use absolute changes or percentage changes of certain dependent variables. A cautious solution might

bring to an empirical analysis with a wide spectrum of dependent variables but this might complicate the interpretation of the results.

Coexisting policies. In most regions, capital subsidy programmes are not implemented in a vacuum, i.e. they coexist with other competing enterprise support programmes and this further complicates their empirical analysis. The coexistence of different policies in the same territory requires an additional assumption for single-programme evaluations based on a counterfactual scenario approach: the probability of firms gaining access to additional unobserved regional or national programmes incentives is the same across both the assisted firms and the comparison group firms that did not receive assistance from the single observed programme being evaluated (Bondonio and Greenbaum, 2014).[26] Comprehensive data on the whole array of the coexisting policies are necessary to test for this additional hypothesis; nevertheless, they are rarely available.

Only a few studies exploit information on all payments of government grant assistance obtaining a causal effect estimate for each programme (e.g., Bondonio and Greenbaum, 2014). The availability of comprehensive data makes their analyses immune from any bias due to the presence of other types of financial assistance. On the other hand, multiple schemes evaluation strategies might limit the use of evaluation strategies based on a natural experiment event and can reduce the pool of control firms making difficult to build a valid counterfactual scenario for each policy.

The cost-benefit analysis. Whenever the topic is public spending, it is important to establish if the money has been used efficiently. This surely applies to industrial and regional policies. Such assessment calls for one of the most valuable tools available to economists: the cost-benefit analysis. Indeed, at least in principle, an all-inclusive cost-benefit analysis is entitled to the last word on the viability of a government outlay. However, in this strand of literature the implementation of a comprehensive cost-benefit analysis is extremely complicated, especially because of the difficulties in performing a complete account of the costs and benefits of investment incentive programmes. General equilibrium effects such as those due to distortions between subsidised and non-subsidised firms and the need for financing the subsidy with distortionary taxes are impossible to gauge using the counterfactual framework.

Two interesting parameters for a rough cost-benefit analysis are the cost per job and the complementarity between the subsidised investment and the rest of the firms' investment activities.

[26] Quite often such assumption is implausible and findings from single-programme evaluations can suffer from attenuation bias (in the most frequent cases in which non-assisted firms are more likely to gain access to other forms of incentives than assisted firms) (Bondonio, 2009). The Italian Law 488/92 is an exception because one of its features is that firms applying for the incentives renounce any other public subsidies, even without any guarantee of receiving the Law 488/92 funds.

However, only a few studies on capital subsidies have retrieved an estimate of the cost per job parameter (Kangasharju and Venetoklis, 2002; Bondonio and Greenbaum, 2014; Criscuolo et al., 2012)[27] or have analysed the complementarity between the subsidised investment and the rest of the firms' investment activities (the GEFRA-IAB report, 2010).

The long term evaluation. The study of the long term effects of an industrial policy is at least as important as the study of the short term effects. Unfortunately, the most common evaluation strategies based on the counterfactual scenario may not be suitable to assess long-lasting impacts of capital subsidies on assisted firms, as such firms are economic units embedded in a network of mutual economic transactions. In the long run, a possible positive program shock on the employment of each single assisted firm is likely to have enough time to generate subsequent impacts on non-assisted firms as well. Those outcome data become endogenous to the treatment and can no longer be considered unaffected by the program incentives and used to retrieve counterfactual estimates (Bondonio and Greenbaum, 2014).

A possible solution is to find non-treated firms that can be argued to have characteristics similar to treated firms but with potential outcomes independent from the policy even in the long-run. Although feasible, such an evaluation strategy would be based on fairly strong assumptions.

A related problem consists of deciding how long one should follow the firms after the subsidies have been granted. If one uses a period that is too short, there is a risk that the evaluation will misrepresent the success/failure of the subsidies. Having too long a time span may make it difficult to isolate the effects of the support. What is too short and too long is difficult to say (Bergstrom, 2000).

A final list of issues. Business incentives are usually directed to a specific investment project and not to a company in general. As evaluation analyses are commonly carried out on companies' key investment variables, they might not reveal the overall impact of the new investment, especially in the short term.

Another aspect that complicates empirical analyses is that a non-negligible portion of firms have two or more establishments, some of which could even be located in different regions or countries. It is possible that some establishments are located in an eligible area, while the others outside of it; this could bring about the substitution of resources from non-eligible establishments to subsidised

[27] This parameter does not factor in the dampening effect on aggregate productivity of keeping open the less productive firms or the money saved by the government from paying less out in unemployment benefits and other forms of welfare for workers who are drawn into employment (Criscuolo et al., 2012).

establishments in order to pocket the money without any additional investment (see Criscuolo et al., 2012).

To conclude this list of issues, it is important to notice that most industrial policies do not have just a single assignment process but a number of them (called bidding rounds); moreover, such policies usually allow the same firms to receive the subsidies more than once for different investment projects. Taking into account these aspects would complicate the evaluation procedure even further (especially the study of long term effects).

1.6. Conclusions

In the words of Pike et al. (2006), policy is bedevilled by the unclear evidence of its impacts and capital subsidy programmes are no exception. Looking at the whole literature, systematic empirical evidence is sketchy, to say the least; however, the studies that we consider the most credible show a much more homogeneous evidence: a positive impact of capital subsidies on financed firms' employment, investment and plant survival prospects but a negligible effect on productivity. This does not mean that researchers are breaking the secret code of perfect evaluations, but only that the credibility path is the one to follow in future research works.

This review has also highlighted the most relevant problems in the evaluation of the effectiveness of the public subsidies on firms' performances. We believe that pointing out the limitations and the challenges of empirical studies is the best way to orientate future research towards more credible evaluations. In the last decade some of these challenges have been undertaken but further research is needed to shed some more light on the effectiveness of capital subsidies.

Appendix 1.A. Description of the most analysed capital subsidy programmes

Despite the general prohibition of state aid in the EU, in some circumstances government interventions is necessary for a well-functioning and equitable economy.[28] Capital subsidy policies of the EU countries comply with the guidelines designating very deprived "Objective 1" areas in which higher rates of investment subsidy can be offered, and somewhat less deprived "Objective 2" areas where lower subsidy rates are offered. There is an upper threshold called the Net Grant Equivalent (NGE) which sets a maximum proportion of a firm's investment that can be subsidised by the government. In the last decades the EU determined maximum subsidy rates have differed over time and across areas.

In the following we present 3 of the most relevant investment incentive policies in Europe.

The Italian Law 488/92 (L488)

See Section 2.2.2 of Essay 2.

The Regional Selective Assistance (RSA) in the UK

The stated chief aim of RSA is to safeguard and create employment opportunities in designated Assisted Areas (disadvantaged areas characterized by low levels of per capita GDP and high unemployment)[29] by offering a discretionary subsidy to plants in the form of a capital grant. That is, RSA provides money towards capital investment projects that secure employment opportunities. RSA is heavily targeted at the manufacturing sector and a sub-aim of RSA is to increase foreign direct investment (FDI) into Assisted Areas.

In addition to offering some sort of commitment to safeguard or expand employment, firms that wish to apply for RSA funding (primarily to expand or modernize) must meet the criteria set out below. First, to be eligible, firms must be located in an Assisted Area. In addition, the firm must

[28] Article 87 of the Treaty of Amsterdam states "any aid granted by a Member State or through State resources in any form whatsoever which distorts or threatens to distort competition by favouring certain undertakings or the production of certain goods shall, insofar as it affects trade between Member States, be incompatible with the common market". However exceptions are allowed in particular circumnstances such as:
i) aid granted to the economy of certain areas of the Federal Republic of Germany affected by the division of Germany, insofar as such aid is required in order to compensate for the economic disadvantages caused by that division;
ii) aid to promote the economic development of areas where the standard of living is abnormally low or where there is serious underemployment;
iii) aid to facilitate the development of certain economic activities or of certain economic areas, where such aid does not adversely affect trading conditions to an extent contrary to the common interest.

[29] Whether an area is eligible for any RSA is determined by a series of quantitative indicators of disadvantage which were changed over time but always included per capita GDP and unemployment (both relative to the EU average).

demonstrate that the project to which the grant is to be directed is itself viable (within 3 years). The project must be shown to be additional, which means that the investment would not have taken place in the way that it did without government support. It is also a requirement that any award of RSA funds does not result in displacement within the Assisted Area (i.e. that by providing funding within a region to a firm, the government is not causing other competing firms to close or cut employment). If successful, the government financed a proportion of the project which was up to 35% in some years.

There are slightly different arrangements in Northern Ireland where the equivalent of the RSA is business support funded under Selective Financial Assistance (SFA).

Regional Investment Grants (RIG) in Sweden

In Sweden, RIG directly to firms go back to the 1960s, when firms were given grants if they made new investments in outlying regions with free capacity. RIG became even more common during the 1970s and more oriented towards reducing distributional differences across regions. However, from 1990 onwards, grants have been targeted more towards promoting economic growth. Grants are limited to firms that have a market outside their own county or that face competition from outside the county. In order to receive RIG, the firm must apply in writing to the County Administration Board or the Swedish Agency for Economic and Regional Growth (NUTEK), including a business plan and a description of the expected results. A processing officer decides whether the application is entitled of receiving support, taking into account the economic situation of the firm. For example, firms with lower probability of receiving financial support from commercial banks are more likely to receive grants so that high-risk projects are overrepresented. It is also evaluated whether the firm can expand and survive in the future. Small firms and investments expected to increase integration and equality in society are also given priority. For a firm to be eligible for a subsidy, it must be used for investments in machinery, equipment, buildings or a service activity that is aimed to increase the market for the enterprise. Depending on the region, grants can cover up to the 40% of the total investment cost. This policy constitutes the largest regional policy-instrument directed towards promoting firm performance in Sweden.

2. The causal impact of capital subsidies: a multiple regression discontinuity design approach

2.1. Introduction

Business support programs are popular industrial policies used by most governments in the EU and other industrialised countries to foster competitiveness, self-sustaining growth and employment, most notably in disadvantaged areas. A huge amount of funds are spent each year on regional policies and subsidies or "state aid".[1] Not surprisingly, several studies have evaluated the extent of the economic payoff of these subsidies (see, inter alia, Roper and Hewitt-Dundas, 2001; Harris and Trainor, 2005; Bondonio and Greenbaum, 2014; Criscuolo et al., 2012); however, the literature is still relatively limited considering the importance of the topic. Moreover, there is little consensus among economists on the effectiveness of investment incentives. In a time of limited public budgets, this is clearly a hot issue. An example is the recent debate raised among scholars with different views about the effectiveness of these types of programs and the presentation of the Report on state aids commissioned by the Italian Government to well-known economists (Giavazzi et al., 2012). In such report, the authors claim that the literature shows no evidence in favour of the effectiveness of incentives to private firms; therefore, they propose cutting €10 billion in state aid to firms and using the money to reduce firms' taxation. Nevertheless, there is hardly a unanimous standing in the literature, as clearly shown in Chapter 2 of the GEFRA-IAB report (2010) and in Essay 1.

Assessing the effectiveness of these types of incentives is basically an empirical question, but evaluating the impact of business incentive programs is a challenging task (Bondonio, 2009). The main problems are due to the difficulties faced in isolating the effects of the subsidies from the confounding effects induced by other factors and in controlling for the high selection bias. This is why credible micro-econometric evaluations are rare in the literature.

[1] Excluding crisis measures, since the early 2000s the share of state aid for industry and services as a percentage of GDP in the EU-27 economies has been stable, amounting to 0.5% in 2011 (EU Commission, 2012). On top of this, national governments themselves also spend large amounts (EU member states devote 1% of GDP on average) on regional policies and subsidies or "state aid" which are allowed by the EU up to a certain degree, which depends on how "disadvantaged" the region is (Dupont and Martin, 2006).

In this paper, we present a robust econometric analysis of the causal effect of capital subsidies to private firms by exploiting an unusual characteristic of an important regional policy in Italy that creates the conditions for a local random experiment. We analyse the impact of subsidies distributed by Law 488/92 (henceforth L488), which has been the main policy instrument for reducing territorial disparities in Italy during the period 1996-2007. This law has been characterised by a rigorous and transparent selection procedure. Each year, subsidies are allocated to a broad range of investment projects through regional "calls for tenders", which mimic an auction mechanism. In each regional "call for tender", the investment projects are ranked on the basis of a score that depends on a number of (known) characteristics of both the project and the firm. Projects receive subsidies according to their position in the ranking system until the financial resources granted to each region are exhausted.

L488 has financed firms in both northern (Objective 2 or 5b) and southern regions (Objective 1) of the country;[2] however, the subsidy intensity is by far higher in the latter areas, following the map of state aid delineated by the European Commission (De Castris and Pellegrini, 2012). This is why we analyse only incentives to the southern regions (Mezzogiorno), i.e. the southern section of the Italian Peninsula (Abruzzi, Basilicata, Calabria, Campania, Molise, and Puglia) and the two major islands (Sardinia and Sicily).[3]

The presence of sharp discontinuities in the L488 rankings allows using a quasi-experimental method deriving from a regression discontinuity design (henceforth RDD) approach, enabling us to identify the causal effect of subsidies on firms' performances. Due to the presence of multiple rankings by regions and years, we use different ranking cut-off points. Therefore, we modify the classical RDD framework, proposing a nonparametric multiple rankings regression discontinuity design (henceforth MRDD) that brings the RDD to our treatment context. The main assumption is that, in each ranking, the best control group for the units just above the cut-off point is represented by the firms ranked just below the cut-off point (the firms that are not treated). Because we focus our analysis on the firms ranked around the cut-off point of each ranking, our parameter of interest is a local average treatment effect (LATE) that reflects the impact of the L488 subsidies on this subgroup of firms.

[2] In the southern regions, L488 has been financed not only with national funds but also with the EU Structural Funds (the southern regions were the only eight Objective 1 Italian regions in the 1994-1999 cycle of EU regional policies).

[3] In the medium-large firms, the subsidy intensity with respect to the total investment is 40-50% in the southern regions and 10-20% in the northern regions (plus an additional 15% for small firms). Moreover, the limited concentration of the funds in northern regions and the circumscribed territorial extension of the Objective 2 and 5b areas (in 2000 about 15% of the northern regions population was covered by L488, i.e. 5.7 million inhabitants) allow neighbouring firms to easily delocalise their industrial plants into these areas, carrying out projects that would also have been realised without L488. Therefore, the additional effect of L488 should be much stronger in the Mezzogiorno.

The data we use come mainly from two sources: an administrative dataset containing detailed information on the instrument and a financial statement dataset covering the period 1995 to 2004. This time span is perfectly suitable for the evaluation of the short-term impact (1995-2001) of the L488 subsidies, as well as the long-term impact (1995-2004).

Over the period from 1995-2001, the tangible capital growth rate is considerably higher in subsidised firms growing each year 14 to 17% faster than in non-subsidised firms (approximately doubling with regard to the median tangible capital in non-subsidised firms), while the yearly growth rate of turnover is as large as 6.5 to 8% higher in subsidised firms. Also the impact of L488 on employment is positive: subsidised firms hire on average from 5 to 8 extra employees in respect to non-subsidised firms. On the contrary, the impact on the output per worker is mostly negligible.

A year by year evaluation during the period 1995-2004 clearly shows that turnover and especially investment markedly increased during the years of the subsidies, and after that they grew at the same rate of the non-subsidised firms. We find that the subsidised investment is additional; however, subsidies do not trigger either positive or negative spillovers on the rest of the owner-financed investment activities. On the whole, the results show that subsidies generate additional capital stock and productive capacity in subsidised firms but without a productivity improvement. Therefore, the analysis suggests that investment incentives cause a boost in private capital accumulation; however, this private benefit does not per se signal the usefulness of the policy from a social welfare perspective. For instance, in a Diamond-Mirrlees (1971a and 1971b) setting, firm-specific capital subsidies are considered not desirable as they distort the allocation of factors of production causing productive inefficiency. Accordingly, we expect that L488 would engender a socially inefficient allocation of resources between treated and non-treated firms. Still, the conclusions of the Diamond-Mirrlees' production efficiency theorem rely on assumptions (competitive markets, constant returns to scale, and flexibility in choosing different commodities taxes for different goods) that are hardly met in the Mezzogiorno. Here, widespread underdevelopment engenders imperfect and incomplete markets, undercapitalised firms, massive unemployment and a local economy far away from the production possibilities frontier. Consequently, L488 could represent a way to move the Mezzogiorno's economy out of the "poverty trap" (see Azariadis and Stachurski, 2005), by increasing productive efficiency and social welfare.[4]

[4] An accurate analysis of the social optimality of L488 would require an investigation of the optimal deviations from production efficiency under a plausible set of assumptions. Such comprehensive evaluation of the social welfare effects is beyond the scope of our paper.

The paper has been organised as follows: the next section summarises the literature and presents the policy in more detail. Section 2.3 details the evaluation method, followed by a presentation of the data in Section 2.4. The results are discussed in Section 2.5, while Section 2.6 assesses their robustness. Section 2.7 concludes the paper.

2.2. The previous literature and the L488 policy

2.2.1. The literature

Different business support schemes have been implemented in developed countries over the last decades, particularly in lagging areas. Non-repayable grants, interest-rate subsidies, equity participation and participation in venture capital are among the most adopted tools in industrialised countries (see Dupont and Martin, 2006). Evaluating the effectiveness of these tools is a pivotal step to orientate policymakers' decisions and thereby optimise the use of taxpayers' money. The evaluation literature has devoted particular attention towards the incentives to R&D as well documented in the surveys by García-Quevedo (2004) and Parsons and Phillips (2007). Recently, another policy has experienced a surge in the number of evaluation studies: the Enterprise Zones (EZs) program[5] (see, among others, Ham et al., 2011; Givord et al., 2012; Busso et al., 2013). Instead, not as many policy evaluators have focused their research on the effectiveness of investment incentives to firms located in lagging areas. Besides, the empirical evidence is mixed: some analysts suggest that regional capital incentives can induce additional investment in subsidised firms (Faini and Schiantarelli, 1987; Harris, 1991; Daly et al., 1993; Schalk and Untiedt, 2000; Bondonio and Greenbaum, 2014; Criscuolo et al., 2012); while others argue that intertemporal substitution effects prevail (Bronzini and de Blasio, 2006). Moreover, the employment impact of capital subsidies is doubtful (Gabe and Kraybill, 2002). Finally, the effect of subsidies on efficiency and productivity seems negligible or negative (Lee, 1996; Bergstrom, 2000; Harris and Trainor, 2005; Criscuolo et al., 2012).

The selection process of the Italian L488 is particularly apt for the empirical evaluation of the investment incentive program. Starting in the late 1990s, a number of scholars have tried to evaluate the effectiveness of this policy[6] (see, inter alia, Chiri et al., 1998; Scalera and Zazzaro, 2000; Carlucci and Pellegrini, 2003; Losurdo, 2004; Vadalà, 2005; Bronzini and de Blasio, 2006; Adorno

[5] In this program, delineated zones - usually neighbourhoods with socio-economic difficulties - are granted "special dispensation" status, and firms that 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).

[6] To increase the transparency and the accountability of the program, the data have been made publicly available by the Ministry of Economic Development.

et al., 2007; Bernini and Pellegrini, 2011), but none of the previous studies have exploited its features in a natural experiment framework like we do.

Bernini and Pellegrini (2011) show evidence of higher growth in output, employment and fixed assets in subsidised firms but a less significant increase in Total Factor Productivity than in unsubsidised firms, while Adorno et al. (2007) highlight a positive but U-reversed relationship between the amount of subsidies and production. Bronzini and de Blasio (2006) investigate the presence of cross-sectional substitution (financed firms may receive some of the investment opportunities that non-financed firms would have otherwise had in the absence of the incentives) and intertemporal substitution (firms may have brought forward investment projects originally planned for the post-intervention period in order to take advantage of the incentives), and find evidence in favour of the latter substitution effect. However, none of these studies have properly exploited the auction mechanism as a source of local randomness. On the other hand, a proper use of the MRDD yields a compelling evaluation strategy: the method is locally equivalent to a random sampling procedure and the internal validity is high (Lee and Lemieux, 2010).

2.2.2. L488

Italy is among the European countries with the highest inequality in the distribution of wealth between different areas. In 1992 the Ministry of Economic Development issued L488, which has been the main policy instrument for reducing territorial disparities in Italy. L488 was fully operational in 1996. During the period 1996-2007, roughly 44,000 projects (over €23 billion) have been financed by L488. Most of this funding has targeted the Mezzogiorno, which comprises the least-developed regions of Italy.

L488 allocates subsidies through a rationing system based on "calls for tender" that mimics an auction mechanism and that guarantees compatibility of demand and supply of the incentives. This policy 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.[7]

The Italian Ministry of Economic Development presides over the selection process. After receiving an application form that includes a technical report and a business plan, the relevant authority

[7] Firms that apply for the incentives renounce any other public subsidies, even without any guarantee of receiving the L488 funds.

performs a preliminary screening, evaluating the funding eligibility of the project. The amounts awarded are paid out in three equal instalments.

Incentives are allocated on the basis of regional competitive auctions. In each auction, the investment projects are ranked on the basis of five objectives and predetermined criteria: 1) the share of owners' funds on total investment; 2) the new job creation by unit of investment; 3) the ratio between the subsidy requested by the firm and the highest subsidy applicable;[8] 4) a score related to the priorities of the region in relation to location, project type and sector; 5) a score related to the environmental impact of the project.[9] The five criteria carry equal weight: the values related to each criterion are normalised, standardised and added up to produce a single score that determines the place of the project in the regional ranking (this normalised score is the forcing variable used in the following analysis). The rankings are drawn up in decreasing order of the score awarded to each project and the subsidies are allocated to projects until funding granted to each region is exhausted. Several checks are made to determine whether subsidised firms have respected their targets. If a treated firm does not reach its goals, the subsidy is entirely or partially revoked.

L488 auctions have been issued on a yearly basis. Our analysis refers to the period 1995-2004 and focuses on three of the four L488 auctions that were concluded by 2001. The timing of the assistance by auction is presented in Table 2.1.[10]

Table 2.1. Timing of the assistance
Source: Bronzini and de Blasio (2006)

Auction	Application deadline	Presumed time of the 1st instalment	Presumed time of the 2nd instalment	Presumed time of the 3rd (last) instalment
1	Jun-96	Nov-96	Nov-97	Nov-98
2	Feb-97	Jul-97	Jul-98	Jul-99
3	Apr-98	Oct-98	Oct-99	Oct-00
4	Nov-98	May-99	May-00	May-01

Note: In many cases, administrative complications and technical and economic problems have increased the time span of the project (estimated at 3.6 years by Bernini and Pellegrini, 2011).

[8] The lower this ratio, the more likely is the firm to obtain the subsidy. The highest subsidy applicable is determined by the EU and varies with the dimension of the firm (favouring small firms) and with the location of the production unit (favouring the most disadvantaged areas).
[9] Criteria 4 and 5 were introduced at the 3rd auction. These criteria are indicators that signal only the absence/presence of certain requirements. Firms that belong to a "target" sector/area and propose a project with a limited environmental impact have a higher probability of receiving the funds.
For a detailed description of the rationale of each indicator, see Section 3 in Bernini and Pellegrini (2011).
[10] The average time span of the financed investment is 3.6 years (Bernini and Pellegrini, 2011).

2.3. Econometric Evaluation Procedure

2.3.1. The Multiple RDD

The assumption of random assignment is not credible when the policy instrument (such as L488) determines a deliberate selection process. If a support programme selects firms in a non-random manner, the participation is endogenous and the projects are heavily selected.

In the case of L488, data are available for the firms that applied for the incentives in the Mezzogiorno but were not financed because they scored too low in the L488 ranking. These non-treated firms are willing to invest and have a valid investment project as checked by a preliminary screening. As a consequence, within each ranking, we can consider these firms as the best control group available; in fact, as suggested by Brown et al. (1995), they show a propensity for investment very similar to that of subsidised firms. Unlike in randomised experiments, this control group is not random, but we can use a quasi-experimental method to minimise the selection bias.

The particular configuration of the L488 dataset - for each auction, there are as many rankings as the number of regions involved and each ranking has a different cut-off point - is similar to the dataset used by Black et al. (2007) in analysing a re-employment services system and the dataset addressed by Gamse et al. (2008) in evaluating the impact of an education program. In these papers, as in the whole empirical literature, there are two different approaches for exploiting the RDD to estimate the treatment effect across different rankings. The first approach consists of two different steps: first, estimating the treatment effect for each ranking; second, pooling the treatment effects in order to get the global treatment effect of the policy under analysis. The other approach pools observations from different rankings into a single dataset re-centring and standardising the forcing variable. In our paper, we apply the first approach because it allows us to exploit all the information available from the dataset, increasing efficiency, and unlike the pooling approach, it does not rely on the strong assumption of random allocation of subsidies to the firms ranked around the unique cut-off point.[11] Nevertheless, we use the pooling approach as a robustness test.

The methodological approach we propose can be named nonparametric multiple rankings regression discontinuity design (MRDD).[12] Coherent to the configuration of the L488 dataset, we

[11] Pooling observations from different rankings could bias the LATE estimates due to the possible different characteristics of treated firms in different rankings.

[12] Deriving from an RDD approach, it is worth stressing that the MRDD results can be applied to each unit that has a positive probability of being located near the relative cut-off point. Lee (2008) shows that if units do not have precise control over the forcing variable, variation in the treatment status in the neighbourhood of the threshold is randomised, as in randomised experiments. Even when units have some influence over the forcing variable, as long as this control is

extend the RDD to a context where the treatment is assigned by multiple rankings with different cut-off points. The main assumption is that in each ranking, the best control group for the units just above the cut-off point is represented by the firms ranked just below the cut-off point (the firms that are not treated). Aggregating the disaggregated estimates, this method exploits all the available observations in the L488 merged dataset; this feature of the MRDD improves the efficiency of the estimation process, making the resulting LATE estimates more reliable.

The MRDD consists of two different steps: first, we apply a sharp RDD in each ranking, exploiting the sharp discontinuity determined by the forcing variable; in this way, we obtain a nonparametric estimation of the LATE in each ranking. Second, we aggregate the different first step estimates by a weight structure, where we use two weighting schemes. In the first, the weights are based on the number of firms close to the normalised cut-off point in each ranking. The second weighting scheme makes use of the inverse of the variances of the LATE estimates as weights. The MRDD estimator mimics a matching estimator in an RDD context: we can define each ranking as a "homogeneous stratum", determine the best matching in each ranking using an RDD approach and compute outcome differences within "strata" using a specific cut-off point in each ranking, and finally integrate such differences over the distribution of the rankings in the treatment population to retrieve the global LATE.

2.3.2. Potential problems with the use of the MRDD

A potential limitation of the MRDD estimator is the small number of observations near some of the cut-off points, which creates a trade-off between bias and precision and makes not possible to determine a consistent estimation in some of the rankings. Moreover, because the auctions are issued roughly every year, another potential problem is the overlapping of subsidies to firms that have received incentives more than once during the period under analysis. However, the construction of the merged dataset has been carried out by attempting to minimise this problem (see Appendix 2.A).

It could be possible that a firm that wins a project in one auction has some type of positive externality on other firms (e.g., increased supply) that then increases the choice probability of related firms in the subsequent auction. In our analysis, the auctions we considered were very close in time with respect to the average project time span. Hence, even if supply or demand spillovers

imprecise - that is, the ex-ante density function of the forcing variable is continuous - the consequence will be local randomisation of the treatment.

are theoretically possible, from an empirical point of view they seem to be negligible (see Section 2.5.2.2).

There are also methodological implications in using a counterfactual approach based on balance sheet data: first, new firms' performances cannot be evaluated; second, our sample is restricted to firms with a meaningful balance sheet (i.e., we use only corporate enterprises already active at least since 1994) that applied to an auction in the southern regions. Therefore, our results can hardly be extended to very small firms and to policy interventions in other (more developed) areas, such as the northern regions of Italy.[13]

2.3.3. The econometric framework

The MRDD approach is used for estimating the LATE of L488 on the economic growth of financed firms. In particular, we use as dependent variables: i) the yearly growth rate of tangible capital;[14] ii) the yearly growth rate of turnover; iii) the cumulative investment from 1995-2001; iv) the per-firm employment change from 1995-2001; v) the output per worker in 2001.

Let us briefly describe the model at the basis of our analysis.[15] Let Y_{ir} (1) and Y_{ir} (0) denote the potential outcomes of firm *i* applied for ranking *r*. Due to the fundamental problem of causal inference (Holland, 1986), even if we are interested in the difference Y_{ir} (1) - Y_{ir} (0), we can only estimate an average treatment effect. Given the features of the MRDD, we focus on the LATE.

Let K_{ir} denote the treatment variable, with K_{ir} =1 if the firm receives the subsidy and K_{ir} =0 if the firm does not receive the subsidy. The outcome (yearly growth rate of tangible capital) for firm *i* can be written as follows:

$$(2.1) \quad Y_{ir} = (1 - K_{ir}) * Y_{ir}(0) + K_{ir} * Y_{ir}(1) = \begin{cases} Y_{ir}(0) & if\ K_{ir} = 0 \\ Y_{ir}(1) & if\ K_{ir} = 1 \end{cases}$$

We consider the pre-treatment variable X_{ir}: receiving the treatment (i.e., receiving the L488 funds) is assumed to depend only on whether the level of X_{ir} is above or below the referring threshold. In our case, X_{ir} is the sum of the indicators normalised for firm *i* applied for ranking *r*. Accordingly,

[13] On the other hand, taking into consideration different macroeconomic cyclical effect, our results are extendable to the subsequent L488 auctions and to other similar policies oriented to reduce territorial disparities in developed countries.
[14] This is the average annual investment (in the period from 1995-2001) as a percentage of capital (1995).
[15] This model derives from the Rubin causal model (see Holland, 1986). We use the yearly growth rate of tangible capital as dependent variable, but it is easily adaptable for the other dependent variables.

for a subsidised firm, the value X_{ir} exceeds the relative cut-off point ($\bar{s}_r$): $K_{ir} = 1\{X_{ir} \geq \bar{s}_r\}$, with $\bar{s}_r$ depending on the ranking.[16]

Firms with X_{ir} below the value $\bar{s}_r$ are assigned to the control group (firms not subsidised because their scores were too low in the ranking). To find evidence of a local average causal effect of the treatment, we need to verify the presence of a discontinuity in the conditional expectation of the outcome:

(2.2) $\lim_{x \downarrow \bar{s}_r} E[Y_{ir} | X_{ir} = x] - \lim_{x \uparrow \bar{s}_r} E[Y_{ir} | X_{ir} = x] \neq 0.$

In the case of sharp RDD, the LATE at each discontinuity point is as follows:

(2.3) $\tau_r^{SRDD} = E[Y_{ir}(1) - Y_{ir}(0) | X_{ir} = \bar{s}_r].$

Of course, it is not possible to observe for each firm *i* both the values $Y_{ir}(1)$ and $Y_{ir}(0)$. This implies comparing the average value of the yearly growth rate of tangible capital for treated firms and non-treated firms at $X_r = \bar{s}_r$.[17]

Accordingly, the average effect for each ranking writes as follows:

(2.4) $\tau_r^{SRDD} = \lim_{x \downarrow \bar{s}_r} E[Y_r | X_r = x] - \lim_{x \uparrow \bar{s}_r} E[Y_r | X_r = x].$

Given this, we need to estimate two limits, approaching each $\bar{s}_r$ from the left and the right. As we will see below, such estimation is carried out using a nonparametric approach. Once every estimated effect and each standard error is computed, we need to aggregate these estimates to obtain the global average effect of subsidies on treated firms.

The aggregation of different estimates is not a trivial problem because it is not easy to find an objective criterion to choose the weights of the estimates. An intuitive but arbitrary solution consists of using the number of observations in each ranking with a forcing variable value "close" to the normalised cut-off point, i.e., within the interval ± bandwidth chosen for the nonparametric analysis in the aggregated sample.

[16] This is a case of "sharp MRDD", as the treatment (receiving the L488 funds) only depends on the level of X_{ir}.

[17] By design, we cannot observe $Y_{ir}(0)$ for the firms with $X_{ir} = \bar{s}_r$. Thus, we exploit units with covariate values arbitrarily close to $\bar{s}_r$ as counterfactual. In order to justify this averaging, we make a smoothness assumption (i.e., we assume that the relation between X_{ir} and Y_{ir} is smooth around $\bar{s}_r$), known in the literature as "continuity of conditional regression functions": $E[Y_r(0)|X_r = x]$ and $E[Y_r(1)|X_r = x]$ are continuous in *X*.

This assumption is stronger than required, as we will only use continuity at $X_r = \bar{s}_r$, but it is not reasonable to assume continuity for one value of the covariate *X*. Under this assumption: $E[Y_r(0)|X_r = \bar{s}_r] = \lim_{x \uparrow \bar{s}_r} E[Y_r(0) | X_r = x] = \lim_{x \uparrow \bar{s}_r} E[Y_r(0) | K = 0, X_r = x] = \lim_{x \uparrow \bar{s}_r} E[Y_r | X_r = x]$.

Thus, the value of the counterfactual outcome in $X_r = \bar{s}_r$ is equal to the limit of the conditional expected value of the outcome for non-treated firms. Similarly, for treated firms: $E[Y_r(1)|X_r = \bar{s}_r] = \lim_{x \downarrow \bar{s}_r} E[Y_r | X_r = x]$.

As a result, the global LATE of the L488 (τ^{MRDD}) and the standard errors (σ) are computed as follows:

(2.5) $\tau^{MRDD} = \sum_{r=Rankings} N_r * \tau_r^{SMRD}/N;$

(2.6) $\sigma = \sqrt{\sum_{r=Rankings} N_r^2 * \sigma_r^2/N^2};$

where,

σ_r is the standard error of the LATE estimate in ranking *r*,

N_r is the number of firms inside the bandwidth interval in ranking *r*, and

N is the total number of firms inside the bandwidth interval.

Another possibility is resorting to an adaptation of the weighting by inverse variance solution common in meta-analysis studies[18] to give more weight to the LATE estimates with smaller variances. This alternative weighting scheme makes use of the inverse of the variances of the LATE estimates as weights:

(2.7) $\tau^{MRDD} = (\sum_{r=Rankings} \tau_r^{SMRD} * 1/\sigma_r^2)/(\sum_{r=Rankings} 1/\sigma_r^2);$

(2.8) $\sigma = \sqrt{1/(\sum_{r=Rankings} 1/\sigma_r^2)}.$

Given the sensitivity of the results to the estimator and the bandwidth in the nonparametric case, we will test the robustness of the MRDD results using the pooling approach.

This additional analysis concerns a joined version of the dataset, in which the observations have been first normalised and then added up. This aggregation has been carried out by re-centring and standardising the forcing variable to create a dataset with a unique cut-off point (where every $\overline{s}_r$ is equal to zero) in order to retrieve the global LATE of subsidies with a simple RDD. We analyse this aggregated dataset both by a parametric and a nonparametric method.

The nonparametric estimator is based on the local linear regression (see Fan and Gijbels, 1996; Hahn et al., 2001) with standard errors computed with the bootstrap; while, we use the OLS estimator with robust standard errors in parametric regressions, as suggested by Imbens and Lemieux (2008). Finally, several robustness tests are carried out to test for possible manipulation in the assignment of subsidies.

[18] For more detail on this procedure, see Marin-Martinez and Sanchez-Meca (2010).

2.4. Data and methodological issues

2.4.1. Data

Our econometric analysis is based on the integration of two different datasets. The first dataset comes directly from the Ministry of Economic Development and records all the firms that applied for an L488 auction, both financed and non-financed, providing important information such as the forcing variable, the firms' ranking at the regional level and the timing of the instalments. This dataset lacks financial and economic information; therefore, we also need to use the AIDA dataset, i.e. a financial statement dataset that basically collects financial statements for corporations (for this reason, it is skewed towards larger firms).[19] The integration between these two datasets requires a complex process of cleaning and merging. Combining these datasets permits us to compare the change in the treated firms' performances to a control group of firms that applied for the incentives but did not receive any subsidy.

The financial statement dataset used in our analysis extends from 1995 to 2004, allowing us to study the impact of the program over a period that includes pre-intervention as well as post-intervention years for the auctions 2, 3, and 4 of L488.[20] For these auctions, the treatment started in 1997 (for firms subsidised in the 2nd auction) and finished by 2001 (see Table 2.1).

By linking the L488 dataset with the financial statement dataset, we reconstruct a merged dataset for the short-term analysis (1995–2001) for 1,702 firms that applied for "calls for tenders" in southern Italy.[21] In the long-run the hypothesis that financed firms do not interact with non-financed firms becomes less and less reliable (see Bondonio, 2009); moreover, the larger the time span, the more problematic becomes to disentangle the causal effect of subsidies from increasingly relevant confounding factors. This is why we give prominence to the short-term analysis, exploiting the long-term analysis only to test for the presence of intertemporal substitution.[22] The merged dataset

[19] AIDA is a large dataset that contains the budgets delivered by over 500,000 Italian corporate enterprises to the Chambers of Commerce (the financial information are standardised and made available by Bureau van Dijk).

[20] The 1st auction has been excluded from the analysis because it included a transitory clause that allowed firms not eligible under L488 to be financed as well (see Bronzini and de Blasio, 2006).

[21] Of the 1,702 firms composing the aggregated dataset, 1,356 are non-duplicate firms (560 subsidised, 796 non-subsidised). Indeed, as we explain in Appendix 2.A, for the MRDD analysis we have decided to keep duplicate non-subsidised firms if they applied for the incentives more than once but were never subsidised until 2001 (for example a firm that scored too low both in the second and the third auction and was never financed until 2001). When we analyse the aggregated dataset in the robustness section we remove the duplicates (the results remain substantially unchanged when we repeat the same analyses keeping the duplicates in the control group).

[22] It is crucial to test whether subsidised firms make additional investment or if they just bring forward investment projects that were originally planned for future periods.

for the long-term analysis (1995–2004) is composed by 709 firms. The detailed construction of the samples is described in Appendix 2.A.

Exploiting the MRDD features, we have tested whether the pre-treatment characteristics of the financed firms are similar to those of the control group. As shown in Table 2.2, we find no evidence of statistically significant pre-treatment differences around the cut-off point between subsidised and non-subsidised firms in terms of investment, turnover, productivity, size, liabilities, and ROE.[23]

Table 2.2. MRDD estimates of the pre-treatment differences in tangible capital, turnover, productivity, size, liabilities ratio, and ROE between subsidised and non-subsidised firms

	Bandwidth	Tangible capital 1995	Turnover 1995	Productivity 1995	Employees 1995	Liabilities ratio 1995	ROE 1995
Triangle Kernel	75% opt.bw	-721.25	-1,733.30	11.53	-3.70	-0.08	5.02
		(890.59)	(1,637.59)	(6.71)*	(5.78)	(0.24)	(7.21)
	opt.bw	-670.70	-1,232.65	8.88	-0.05	-0.09	4.35
		(730.66)	(1,542.58)	(5.61)	(5.52)	(0.19)	(6.82)
	150% opt.bw	-337.87	-1,014.04	5.47	2.99	0.12	-0.79
		(548.98)	(1,295.44)	(4.76)	(5.10)	(0.15)	(6.30)
Epanechnikov Kernel	75% opt.bw	-103.99	-916.65	4.82	2.84	-0.14	-2.72
		(491.07)	(1,315.32)	(4.16)	(4.53)	(0.14)	(6.01)
	opt.bw	-6.73	-900.21	4.65	1.87	-0.15	-3.62
		(460.28)	(1,307.98)	(3.98)	(4.47)	(0.14)	(6.06)
	150% opt.bw	3.30	-891.06	4.48	1.41	-0.15	-4.12
		(454.18)	(1,309.19)	(4.07)	(4.50)	(0.13)	(6.11)
Gaussian Kernel	75% opt.bw	176.10	-992.88	5.21	2.62	-0.13	-1.45
		(565.96)	(1,347.44)	(4.24)	(3.54)	(0.14)	(7.54)
	opt.bw	-71.58	-935.88	5.21	2.26	-0.14	-2.71
		(496.26)	(1,396.32)	(4.38)	(3.56)	(0.13)	(7.39)
	150% opt.bw	-15.06	-903.42	4.57	1.73	-0.15	-3.67
		(449.07)	(1,322.52)	(4.18)	(3.66)	(0.13)	(7.32)

Note: Bootstrapped Standard Errors in parentheses. The interval ± bandwidth chosen for the nonparametric analysis in the aggregated sample is ±1.72. 959 firms (56.3% of the sample) have forcing variable values within this interval (428 treated and 531 not treated). The variable liabilities ratio is defined as the financial expenses divided by the net borrowing. Amounts of tangible capital and turnover are expressed in thousands of euros.
***p<0.01, **p<0.05, *p<0.1.

2.4.2. Descriptive statistics

We have carried out the econometric analysis for the rankings with at least 10 treated and 10 non-treated observations. Therefore we do not evaluate auctions in Basilicata and Molise (the smallest regions in our sample), the 2nd auction in Sicily, and the 4th auction in Abruzzi and Sardinia;[24]

[23] Furthermore, using Hotelling's T-squared generalised means test, we have tested for differences in the means and the medians of a large set of financial variables in the aggregated dataset, finding no evidence of statistically significant differences between subsidised and non-subsidised firms "close" to the cut-off point.

[24] In order to verify if the loss of these observations would substantially affect our results, we have tested whether the integration of these observations into the aggregated dataset significantly modifies the LATE estimates for each dependent variable. The results obtained support the hypothesis that the loss of these observations is not critical; in fact, the inclusion of such observations does not cause any statistically significant change in the LATE estimates.

consequently, there are 15 rankings under analysis. We thoroughly describe the data in Appendix 2.A.

We now turn to a brief presentation of some descriptive statistics. The amount of resources allocated in the 2nd, 3rd, and 4th auctions is roughly €6.5 billion, and of the 27,436 projects that overcame the preliminary screening, 11,722 obtained funding (42.7%). Among the subsidised projects, 65% of the firms are located in the Mezzogiorno, for a total of €5.58 billion allocated by L488 (86% of the total funding).

Using the whole sample reconstructed for the short-term analysis we find that the two groups of firms are very similar in terms of age, ROE, and tangible capital, while financed firms have a higher turnover and are slightly larger (more employees).[25] These statistics are reported in Table 2.3.

Table 2.3. Descriptive statistics

Bandwidth		Subsidised firms	Non-subsidised firms
Tangible capital	Mean	1,784	2,132
	Median	390	380
Turnover	Mean	7,640	4,922
	Median	1,706	1,296
Size (Nb. of employees)	Mean	31.29	22.73
	Median	10	9
Output per worker	Mean	51.28	39.13
	Median	45.20	37.33
ROE	Mean	5.91	7.99
	Median	6.64	5.29
Firm constitution	Mean	1985	1985
	Median	1988	1988

Note: The variables are reported for the pre-treatment year 1995. Amounts of tangible capital and turnover are expressed in thousands of euros.

2.5. Results

2.5.1. Graphical evidence

The estimation procedure begins with some graphical evidence. A simple way to evaluate the effect of L488 is to plot the relationship between each outcome variable and the forcing variable for firms on either side of the cut-off point.[26] Fifteen different rankings have been analysed with the MRDD and it is impractical to graphically represent all of them; therefore, we illustrate only the most representative (the ranking with the highest number of treated firms), i.e. Campania in the 2nd

[25] If we restrict the analysis to the observations "close" to the threshold as described in Section 2.3.3, these differences tail off and become statistically insignificant.

[26] If there is no visual evidence of a discontinuity in the graph, it is unlikely that even the most sophisticated regression methods will yield a significant policy effect (Lee and Lemieux, 2010).

auction. Fig. 2.1 plots this ranking for each dependent variable for the period from 1995-2001 for subsidised firms against non-subsidised firms. In each graph, the cut-off line sharply separates the treated and not-treated firms. Each figure superimposes the fit of a nonparametric flexible polynomial regression model (estimated separately on each side of the cut-off point), together with the 95% confidence bands.

Fig. 2.1 clearly shows that, on average, subsidised firms grow more than non-subsidised firms. To provide some graphical evidence for the entire merged dataset, in Fig. 2.2 we present the figures relative to the pooling approach in which we exploit all the available observations. This figure too displays a systematic difference between financed and non-financed firms in the aggregated sample for each dependent variable but productivity.

2.5.2. Regression results

The standard approach in the RDD analysis is to use a local linear regression, which minimises the bias of the estimates (see Hahn et al., 2001; Lee and Lemieux, 2010). There are two key issues in implementing the estimation by a local linear regression: the choice of the kernel and the choice of the bandwidth. Different types of kernels are available. We present our results using three different types: triangular, Epanechnikov, and Gaussian.

A very delicate part of the analysis is the choice of the bandwidth. In a nonparametric RDD estimation, this involves finding an optimal balance between precision (more observations are available to estimate the regression) and bias (the larger the bandwidth, the larger the differences between treated and not-treated firms). There are several rule-of-thumb bandwidth choosers, but none are completely reliable. A recent contribution by Imbens and Kalyanaraman (2012) presents a data-dependent method for choosing an asymptotically optimal bandwidth in the case of a RDD. However, different bandwidth choices are likely to produce different estimates. We have decided to report three estimates as an informal sensitivity test: the first uses the Imbens-Kalyanaraman formula (the optimal bandwidth); the others reduce the optimal bandwidth by 25% and increase the optimal bandwidth by 50%.[27] Standard errors are estimated with a bootstrap procedure.[28] The results are presented in Table 2.4 (see Nichols, 2011, for more details on the implementation of the RDD estimates and the Stata module rd.ado).

[27] These optimal bandwidths derive from a weighted procedure of the optimal bandwidths computed for each ranking; for this reason, they are not numerically reported in Table 2.4.
[28] We used 200 repetitions in each procedure.

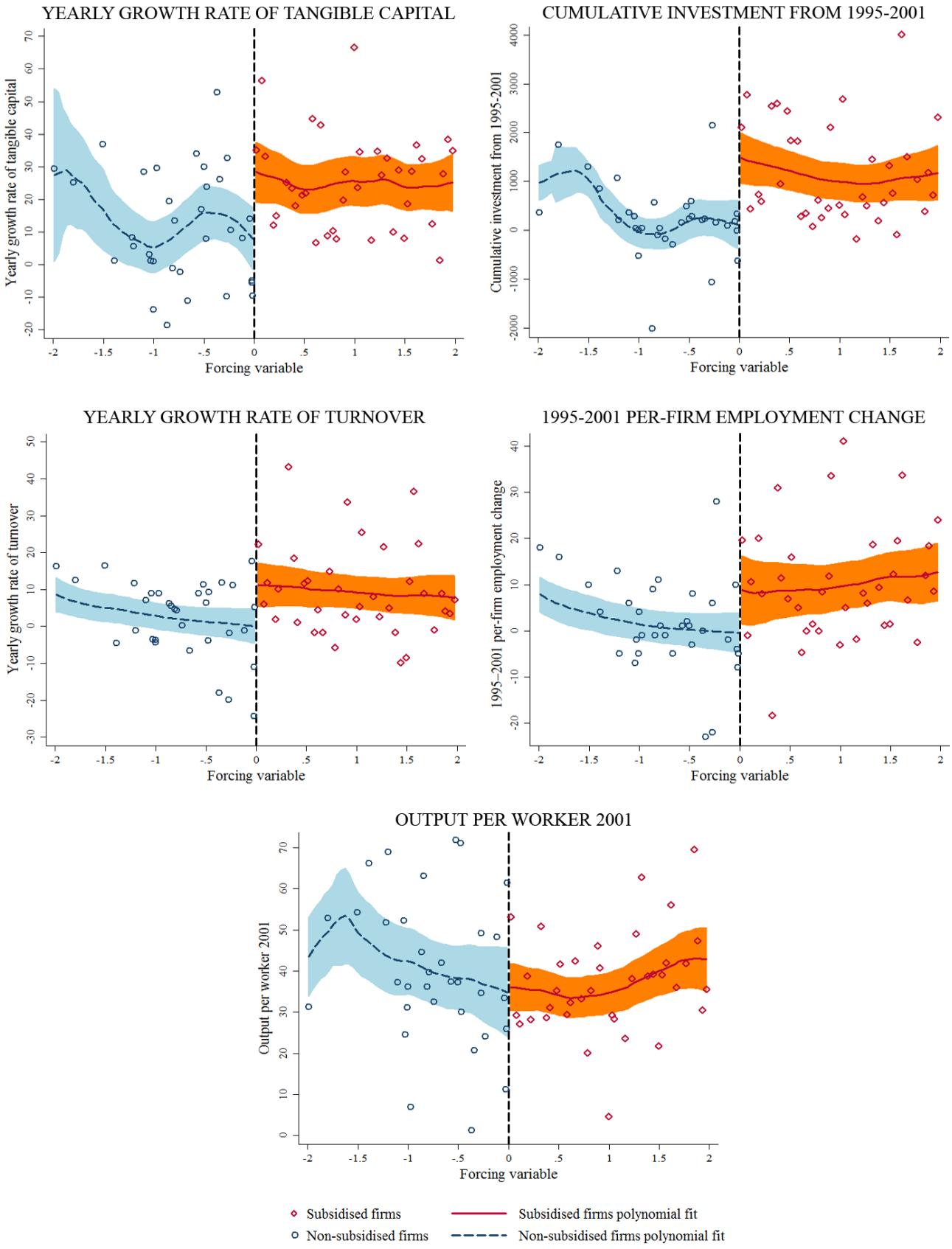

Fig. 2.1. Differences in investment, turnover, employment, and productivity at the discontinuity between subsidised and non-subsidised firms in Campania for the 2nd auction.

Note: Bin size=0.05. We use the Stata's command lpolyci to superimpose the fit of a nonparametric flexible polynomial regression model, separately estimated on both sides of the cut-off point, together with 95% confidence bands. This graphical analysis has been carried out on the 134 observations having forcing variable values within the interval [-2; +2]. Amounts of cumulative investment are expressed in thousands of euros.

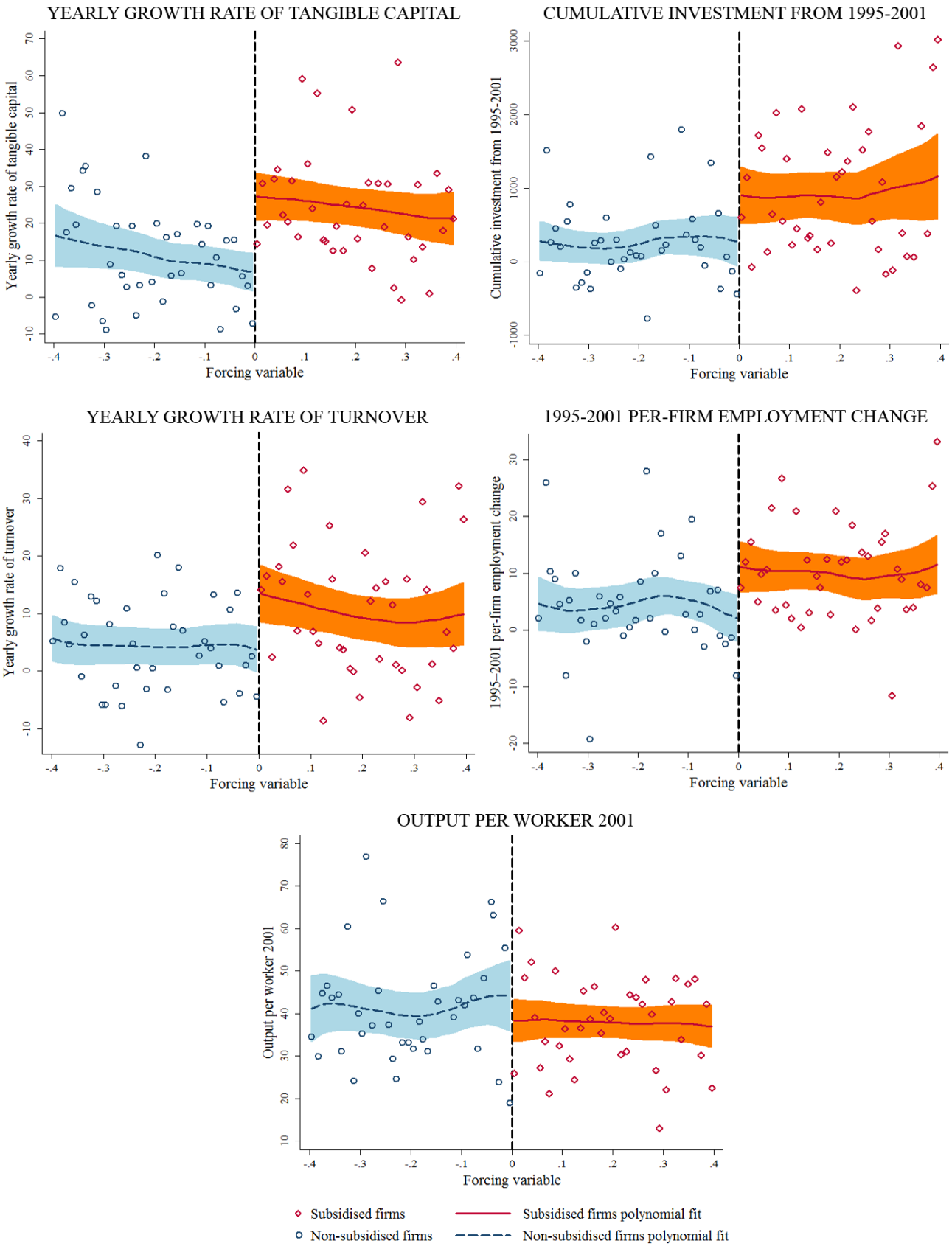

Fig. 2.2. Differences in investment, turnover, employment, and productivity at the discontinuity between subsidised and non-subsidised firms in the aggregated sample.

Note: Bin size=0.01. We use the Stata's command lpolyci to superimpose the fit of a nonparametric flexible polynomial regression model, separately estimated on both sides of the cut-off point, together with 95% confidence bands. This graphical analysis has been carried out on the 270 observations having forcing variable values within the interval [-0.4; +0.4]. Amounts of cumulative investment are expressed in thousands of euros.

The effect of the L488 policy is positive and statistically significant at the 5% level (for each kernel and for every bandwidth) for each dependent variable but productivity.[29] Over the period from 1995-2001, the tangible capital growth rate is considerably higher in subsidised firms growing each year 14 to 17% faster than in non-subsidised firms (columns 1 and 2) and approximately doubling with regard to the median tangible capital in non-subsidised firms (columns 3 and 4), while the yearly growth rate of turnover is as large as 6.5 to 8% higher in subsidised firms (columns 5 and 6).

Also the impact of L488 on employment is positive: subsidised firms hire on average from 5 to 8 extra employees in respect to non-subsidised firms (columns 7 and 8). On the contrary, there is some evidence that the impact on the output per worker is negligible or slightly negative (columns 9 and 10).[30]

2.5.2.1. Long-term analysis

We conduct a long-term analysis especially in order to test the intertemporal substitution hypothesis. Fig. 2.3 displays the evolution of the differences in tangible capital (cumulative investment) between financed and non-financed firms for the period from 1995-2004, estimated each year with the RDD. The gap in cumulative investment between financed and non-financed firms markedly increases in the period from 1995 to 2001 and then slightly decreases in the following years. This gap becomes statistically significant since 1999 and remains in place until 2004. Moreover, analysing the other dependent variables we find that turnover and especially cumulative investment markedly increased in the subsidised firms during the years of the subsidies, while after that period, they grew at approximately the same rate of the non-subsidised firms (see Fig. 2.B1 of Appendix 2.B). The joint evaluation of these results shows that we can safely reject the intertemporal substitution hypothesis. Also the employment level of subsidised firms considerably increased during the 9-year period, with estimates that become statistically significant in 2003, while the effect on productivity is mostly negligible.

[29] These results rely on the assumption that there are no other governmental programs correlated with the allocation of L488 funding. As shown in Section 2.2.2, a feature of L488 minimises the extent of this bias.

[30] We also have results disaggregated by auction. Concerning cumulative investment and employment, the estimates show a positive effect of L488 for every auction, but this effect is statistically significant (at the 1% or 5% level) only for the 2nd and the 4th auction; similarly, the estimates about turnover show a positive effect of L488 for each auction, but this effect is statistically significant only for the 2nd (at the 1% level) and the 4th auction (at the 5% or 10% level). Finally, the productivity estimates are negligible or negative in the auctions considered but the negative estimates are statistically significant (at the 5% or 10% level) only for the 4th auction. The lack of statistical significance in some auctions can be attributed to the smaller number of observations: in presence of high variability, as is common in firms' performance analysis, the number of observations greatly influences the statistical significance of the estimates.

Table 2.4. MRDD estimates

		Dependent variable: yearly growth rate of tangible capital		Dependent variable: cumulative investment from 1995-2001		Dependent variable: yearly growth rate of turnover		Dependent variable: 1995–2001 per-firm employment change		Dependent variable: output per worker in 2001	
		Weighting schemes		Weighting schemes		Weighting schemes		Weighting schemes		Weighting schemes	
		Number of firms close to the cut-off	Inverse-variance weighting	Number of firms close to the cut-off	Inverse-variance weighting	Number of firms close to the cut-off	Inverse-variance weighting	Number of firms close to the cut-off	Inverse-variance weighting	Number of firms close to the cut-off	Inverse-variance weighting
	Bandwidth	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Triangle Kernel	75% opt.bw	17.27	13.89	584.34	720.20	6.86	7.13	8.28	4.95	-7.34	-3.97
		(5.55)***	(4.39)***	(287.44)**	(210.63)***	(3.97)*	(3.17)**	(3.52)**	(2.15)**	(5.78)	(4.25)
	opt.bw	16.57	14.98	622.28	725.71	7.44	7.69	7.83	5.30	-8.06	-4.15
		(4.30)***	(3.65)***	(239.12)***	(177.36)***	(3.35)**	(2.79)***	(2.89)***	(1.95)***	(4.19)*	(3.50)
	150% opt.bw	16.78	16.51	630.51	679.39	6.83	6.91	8.11	6.06	-6.41	-4.55
		(3.70)***	(3.38)***	(186.27)***	(158.20)***	(2.76)**	(2.43)***	(2.41)***	(1.85)***	(3.31)*	(2.95)
Epanechnikov Kernel	75% opt.bw	16.10	15.95	642.25	668.66	6.66	7.99	7.11	5.23	-7.27	-5.44
		(3.60)***	(3.39)***	(187.54)***	(159.08)***	(2.64)**	(2.44)***	(2.19)***	(1.76)***	(3.27)**	(2.81)*
	opt.bw	15.12	15.57	655.45	680.62	6.80	7.76	7.34	5.86	-5.61	-4.13
		(3.38)***	(3.21)***	(173.26)***	(154.12)***	(2.50)***	(2.37)***	(2.06)***	(1.69)***	(3.11)*	(2.70)
	150% opt.bw	14.13	14.86	629.07	662.74	7.31	8.35	6.89	5.93	-3.40	-3.10
		(3.29)***	(3.10)***	(169.84)***	(145.93)***	(2.44)***	(2.31)***	(2.02)***	(1.63)***	(2.93)	(2.62)
Gaussian Kernel	75% opt.bw	15.82	16.30	623.67	693.51	6.78	7.41	7.26	5.55	-7.08	-4.87
		(3.67)***	(3.44)***	(186.21)***	(161.91)***	(2.56)***	(2.37)***	(2.12)***	(1.69)***	(3.31)**	(2.70)
	opt.bw	15.29	15.77	626.07	675.71	6.90	7.50	7.15	5.72	-5.63	-3.52
		(3.51)***	(3.32)***	(178.37)***	(156.89)***	(2.45)***	(2.31)***	(2.04)***	(1.64)***	(3.12)*	(2.63)
	150% opt.bw	14.52	15.17	630.35	668.16	7.16	7.78	6.96	5.95	-3.86	-2.14
		(3.41)***	(3.22)***	(174.34)***	(152.04)***	(2.37)***	(2.27)***	(2.02)***	(1.59)***	(2.96)	(2.58)

Note: Bootstrapped Standard Errors in parentheses. Formulae (2.5) and (2.6) are used to compute the estimates of the number of firms close to the cut-off weighting scheme, while we use formulae (2.7) and (2.8) for the inverse-variance weighting scheme. The interval ± bandwidth chosen for the nonparametric analysis in the aggregated sample is ±1.72. 959 firms (56.3% of the sample) have forcing variable values within this interval (428 treated and 531 not treated). Amounts of cumulative investment are expressed in thousands of euros.

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

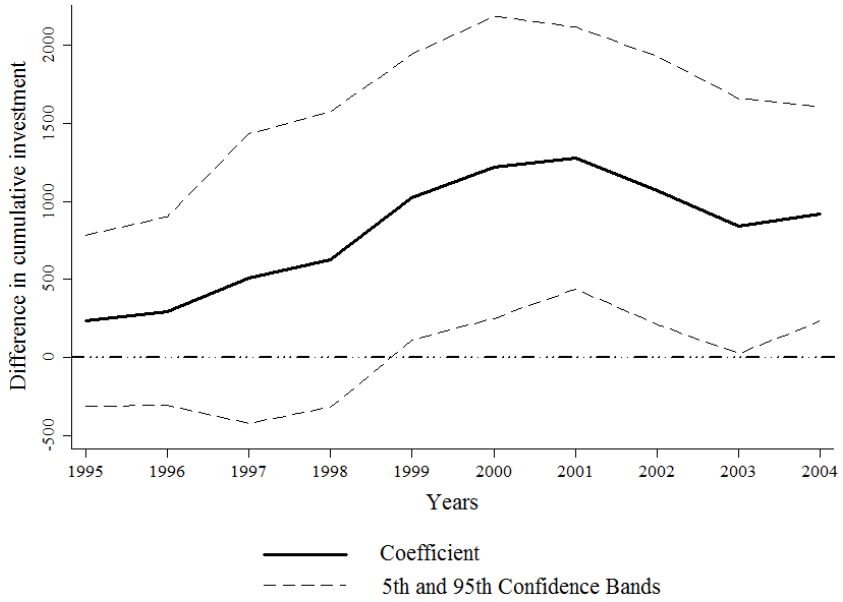

Fig. 2.3. Evolution of the difference between subsidised and non-subsidised firms' cumulative investment during the period 1995-2004.
Note: We carry out a local linear regression in each year from 1995-2004. We use only the 573 non-duplicate firms composing the sample for the long-term evaluation. Amounts are expressed in thousands of euros.

2.5.2.2 Cost per job and complementarity of the subsidised investment

Some interesting parameters for analysing the effectiveness of a business incentive program are the estimation of the cost per job in terms of public expenditure and the evaluation of the possible spillovers of the subsidies on other investment activities of the subsidised firms.[31,32] Dividing the average amount of the subsidies for firms located near the cut-off point by the MRDD estimates of the per-firm employment change we find that each additional new job has cost between €46,343 and €77,520.[33] Clearly, such estimates do not take into account the possible spillovers (negative or positive) engendered by the policy. There is some evidence (see De Castris and Pellegrini, 2012; Essay 3) that the L488 funds have caused a small reduction in the employment level of non-

[31] In this strand of literature the implementation of a comprehensive cost-benefit analysis is extremely complicated, especially because of the difficulties in performing a complete account of the costs and benefits of investment incentive programs. General equilibrium effects such as those due to distortions between subsidised and non-subsidised firms and the need for financing the subsidy with distortionary taxes are impossible to gauge using the counterfactual framework. Also the partial equilibrium costs and benefits expressed as the social return of investment incentives are difficult to retrieve. Such comprehensive cost-benefit analysis is beyond the scope of our paper.

[32] The cost per job is, of course, far from a welfare calculation as we are not factoring in other distortions such as the dampening effect on aggregate productivity of keeping open the less productive firms. On the other hand, the government is also saving money from paying less out in unemployment benefits and other forms of welfare for workers who are drawn into employment (Criscuolo et al., 2012).

[33] These figures are consistent with the nature of L488. Indeed, this program supports the private investment by reducing the cost of capital and the theoretical effect that this has on employment is unclear (see Schalk and Untiedt, 2000; Bernini and Pellegrini, 2011). The policymakers have tried to contrast the substitution effect between capital and labour by adding "the new job creation by unit of investment" as one of the five criteria to determine the assignment of the L488 funds; however, our estimates are larger than those retrieved by Bondonio and Greenbaum (2014) and Criscuolo et al. (2012).

subsidised firms; this implies that the aforementioned figures somewhat underestimate the real cost per job of the program.

The complementarity between the subsidised investment and the rest of the firms' investment activities is evaluated by comparing the total investment response and the funds the government put into subsidised firms. An easy way to do so is evaluating the new investment net of the subsidy amount; therefore, we modify two of the dependent variables (yearly growth rate of tangible capital and cumulative investment from 1995-2001) subtracting from the tangible capital in 2001 the subsidy amount.

Table 2.5: MRDD estimates (net subsidy)

		Dependent variable: yearly growth rate of tangible capital (net subsidy)		Dependent variable: cumulative investment from 1995-2001 (net subsidy)	
		Weighting schemes		Weighting schemes	
		Number of firms close to the cut-off	Inverse-variance weighting	Number of firms close to the cut-off	Inverse-variance weighting
	Bandwidth	(1)	(2)	(3)	(4)
Triangle Kernel	75% opt.bw	2.24	3.05	-83.23	65.89
		(5.42)	(4.94)	(268.11)	(175.56)
	opt.bw	2.00	2.99	-13.67	86.19
		(4.32)	(3.99)	(229.27)	(150.37)
	150% opt.bw	2.88	2.92	-10.61	34.48
		(3.63)	(3.38)	(176.43)	(131.83)
Epanechnikov Kernel	75% opt.bw	2.67	1.47	4.10	5.72
		(3.58)	(3.44)	(165.14)	(136.05)
	opt.bw	1.47	1.12	24.21	15.74
		(3.32)	(3.24)	(159.92)	(131.52)
	150% opt.bw	0.65	0.95	22.35	20.11
		(3.20)	(3.10)	(158.02)	(127.78)
Gaussian Kernel	75% opt.bw	2.08	1.55	-5.28	38.87
		(3.48)	(3.32)	(166.91)	(130.46)
	opt.bw	1.58	1.15	5.97	32.38
		(3.30)	(3.19)	(158.53)	(127.05)
	150% opt.bw	0.98	0.86	12.60	37.79
		(3.19)	(3.09)	(154.52)	(124.78)

Note: Bootstrapped Standard Errors in parentheses. Formulae (2.5) and (2.6) are used to compute the estimates of the number of firms close to the cut-off weighting scheme, while we use formulae (2.7) and (2.8) for the inverse-variance weighting scheme. The interval ± bandwidth chosen for the nonparametric analysis in the aggregated sample is ±1.72. 959 firms (56.3% of the sample) have forcing variable values within this interval (428 treated and 531 not treated). The net subsidy dependent variables are obtained subtracting from the tangible capital in 2001 the subsidy amount that treated firms received. Amounts of cumulative investment are expressed in thousands of euros.
***p<0.01, **p<0.05, *p<0.1.

The results are reported in Table 2.5 and show that the tangible capital growth rate net of the subsidy amount is positive but not statistically significant (columns 1 and 2), while the growth in

cumulative investment from 1995-2001 net of the subsidy amount is approximately nil (columns 3 and 4). These results are not surprising: the owners' financial resources invested by subsidised firms are basically equal to the owners' financial resources invested by non-financed firms. Such analysis points to the absence of complementarity between the subsidised investment and the rest of the firms' investment activities; nevertheless, this does not mean that there are no benefits: the additional capital stock accumulated in subsidised firms is expected to generate income and wealth over time.

Our analysis does not consider the presence of possible spillovers among firms. Therefore, our results crucially depend on the validity of the Stable Unit Treatment Value Assumption (SUTVA), i.e. we assume that interactions among firms are irrelevant in generating firms' potential outcomes (see Rubin, 1986). A recent empirical literature has overcome some of the SUTVA's limitations and estimated the spillover effects of investment incentive policies (see De Castris and Pellegrini, 2012; Essay 3). The evidence points to spillovers which are mostly negligible in terms of investment and turnover and small but negative in terms of employment. On the whole, these results suggest that our main findings on tangible capital and turnover remain substantially unchanged, while we should be cautious in claiming a positive impact of the policy on employment in the eligible areas.

2.5.2.3. Is the effect of investment incentives independent of the firm size?

Several papers note that the effects of the subsidies are highly heterogeneous across firm size (see, e.g., Adorno et al., 2007; Criscuolo et al., 2012). We can gain insight into these effects even with a straightforward application of the RDD using the aggregated dataset.[34] Using only small firms, the effect on the growth rate of tangible capital (turnover) is roughly 75% (30%) larger than the MRDD estimates, while the effect on employment is basically unchanged with respect to the estimates in Table 2.4. Repeating the same procedure for large firms, we obtain estimates considerably lower than those obtained with the MRDD and statistically significant only for the tangible capital.[35] From this straightforward analysis, we find that the effect of subsidies on the growth rates is markedly larger for small firms. Considering that our sample is skewed towards large firms, a plausible explanation is that we are potentially underestimating our growth rate estimates that can be

[34] We have tested that the main RDD assumptions are confirmed for these subsamples.

[35] Small and large firms are defined as firms below and above the median of the firm turnover in 1995, respectively. Similar results are achieved using sales and cost of labour as discriminating variables, while changing the discriminating points of these variables modifies the results in the expected direction (the fewer firms that are considered large, the lower are the extent of the estimates for those firms). These results are in Table 2.B1 of Appendix 2.B.

interpreted as a lower bound of the actual LATE.[36] The effect on productivity is heterogeneous by size but by and large it is still not statistically significant.[37]

2.6. Robustness proofs

2.6.1. Robustness tests with the aggregated dataset

A robustness analysis is carried out comparing the MRDD results with the results obtained through the pooling approach. Because we focus on the average treatment effect of subsidies on firms ranked around the cut-off point, for this analysis, we use only the closest observations to the threshold (25% of the sample). The nonparametric estimates, reported in Table 2.6, are similar to the results obtained with the MRDD. The difference between the two groups of firms is 19-21% for the yearly growth rate of tangible capital, 10-11% for the yearly growth rate of turnover, 500-650 thousands of euros for the cumulative investment, and 7 workers for the per-firm employment change, while the impact on productivity is negligible.

Table 2.6: RDD robustness estimates

Kernel	Yearly growth rate of tangible capital	Cumulative investment from 1995-2001	Yearly growth rate of turnover	1995–2001 per-firm employment change	Output per worker in 2001
Triangle	21.55	661.69	11.43	7.36	1.21
	(6.72)***	(329.52)**	(4.59)**	(3.97)*	(11.60)
Epanech.	19.60	518.38	10.15	6.83	-1.07
	(4.96)***	(257.24)**	(3.82)***	(3.36)**	(6.91)
Gaussian	20.97	524.44	10.12	6.97	-0.82
	(5.22)***	(261.08)**	(4.01)**	(3.25)**	(7.02)

Note: Bootstrapped Standard Errors in parentheses. The estimates are based on the 25% of the sample closest to the normalised cut-off point (keeping constant the percentage of financed firms in the sample). We use only the optimal bandwidth. Amounts of cumulative investment are expressed in thousands of euros.
***p<0.01, **p<0.05, *p<0.1.

The results of the parametric RDD,[38] adding different polynomials and interaction terms, are in Table 2.B2 of Appendix 2.B. In all these cases, the BIC and the AIC criteria choose the simplest

[36] This result is consistent with the empirical evidence that small firms are more prone to financial distress and credit crunches. Moreover, the EU rules favour smaller firms in terms of the share of the subsidies applicable in respect of the project value. Another interpretation is presented in the theoretical model proposed by Criscuolo et al. (2012), suggesting that the larger effect on smaller firms is due to larger firms being more able to "game" the system and take the subsidy without changing their investment and employment levels. Finally, another possible explanation is that large firms ask for an amount of subsidies that is not directly proportional to their dimensions, so that they would need much more money in order to grow proportionally as much as the smaller firms.

[37] In a few cases this effect is positive and statistically significant for the larger firms. This could be due to the higher than average capital intensity of the subsidised investment of these firms.

[38] Lee and Lemieux (2010) argue that, in the case of polynomial regressions, the equivalent to bandwidth choice in the nonparametric regression is the choice of the order of the polynomial regressions. Therefore, it is advisable to try to

specification, i.e. a comparison of each dependent variable on the two sides of the cut-off point. The effect on productivity is negligible, while the effect on the other dependent variables is positive, statistically significant at the 1% or 5% level, equal to 15% for the yearly growth rate of tangible capital, 6.3% for the yearly growth rate of turnover, 638 thousands of euros for the cumulative investment, and 4.44 workers for the per-firm employment change.[39]

2.6.2. Additional robustness tests

Following Imbens and Lemieux (2008), we assess the validity and the robustness of our results adopting various specification tests. First, we rule out possible discontinuities in the conditional density of the forcing variable (the score of the project in the regional ranking), which would indicate evidence of manipulation in the subsidies assignment. The McCrary test (McCrary, 2008) turns out to be negative for each ranking. In Fig. 2.4 we graphically present the negative results of this test in the Campania's rankings, while the remaining 12 McCrary tests are reported in Fig. 2.B2 of Appendix 2.B. Additionally, we exclude that each dependent variable is discontinuous at other values of the forcing variable (different from the cut-off), using different bandwidths (see Table 2.7). We also control for possible jumps in the value of other exogenous covariates (liabilities ratio in 1995 and ROE in 1995) at the cut-off point of each ranking, using the MRDD, to be sure that the discontinuities close to the thresholds are caused by the treatment. Overall, we do not detect any statistically significant discontinuities as reported in Table 2.2 (columns 5 and 6). Accordingly, all these robustness tests confirm that the L488 selection process has been carried out without significant irregularities.[40]

report a number of specifications to see to what extent the results are sensitive to the order of the polynomial. The choice of the order of the polynomial is assessed using a goodness-of-fit criterion.

[39] Analysing with the same procedure the sample for the long-term evaluation, we find that in the period from 1995 to 2004 the long-term effect on all the dependent variables but productivity is positive, statistically significant at the 5% level equal to 8.2%, 5.3%, 494 thousands of euros, and 6.47 workers, respectively, for the yearly growth rate of tangible capital, the yearly growth rate of turnover, the cumulative investment from 1995-2004, and the per-firm employment change from 1995-2004.

[40] These robustness tests are not the only reason why we argue for the absence of significant irregularities. Indeed, considering the possibility of collusion among applicants, we argue that the large number of participants in each considered "call for tender" makes far-fetched any hypothesis of collusive behaviour. On the other side, an internal study at the Ministry of Economic Development shows that only 3.6% of the subsidised projects represented cases of possible corruption, i.e. the projects that were revoked because of a lack of requirements with some evidence of a fraudulent behaviour. Therefore, we cannot exclude the presence of some cases of corruption but this phenomenon should be limited to a physiological level.

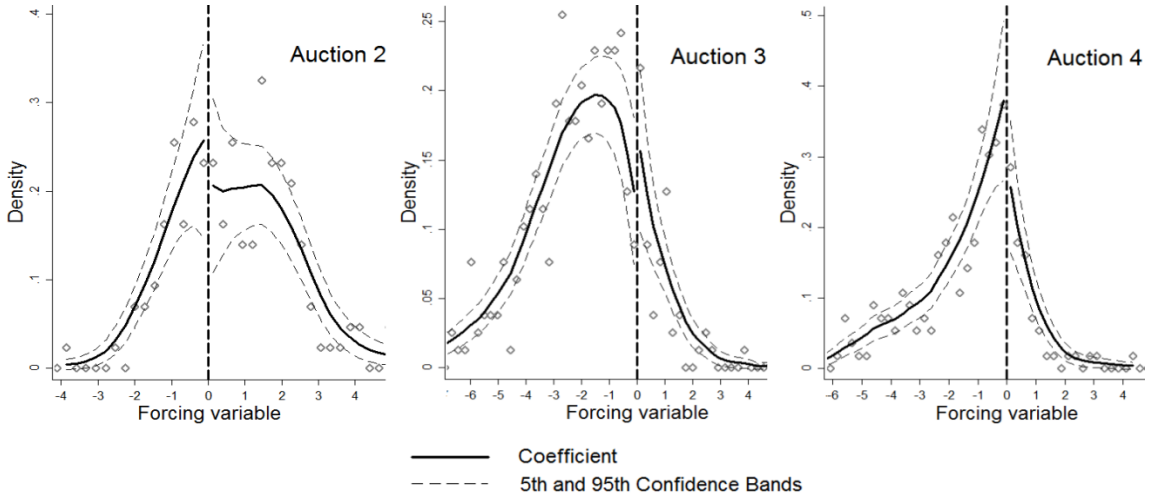

Fig. 2.4. McCrary test for Campania in each auction.
Note: This test is based on an estimator for the discontinuity at the cut-off in the density function of the forcing variable. The test is implemented as a Wald test of the null hypothesis that the discontinuity is zero.

Table 2.7. Test of different cut-off points of the forcing variables for each dependent variable

		Cut-off point						
Dependent variable	Bandwidth	**-1.5**	**-1**	**-0.5**	**0**	**0.5**	**1**	**1.5**
Yearly growth rate of tangible capital	75% opt.bw	9.15	-3.21	3.02	17.99	-4.16	-5.42	3.23
		(5.32)*	(4.76)	(4.90)	(4.20)***	(5.11)	(4.52)	(6.49)
	opt.bw	7.41	-5.68	4.24	17.33	-5.93	-6.28	2.54
		(4.73)	(4.01)	(4.39)	(3.64)***	(4.36)	(4.03)	(5.36)
	150% opt.bw	6.61	-5.49	4.77	16.59	-4.47	-6.76	-1.27
		(4.25)	(3.71)	(3.63)	(3.15)***	(3.64)	(3.68)*	(4.58)
Cumulative investment from 1995-2001	75% opt.bw	-47.82	149.40	-8.86	665.40	-199.42	-371.29	85.81
		(262.35)	(221.27)	(199.87)	(212.42)***	(242.91)	(249.98)	(303.14)
	opt.bw	-119.06	117.60	140.83	695.14	-207.04	-393.83	-6.85
		(229.6)	(190.53)	(175.8)	(178.88)***	(213.27)	(226.08)*	(270.46)
	150% opt.bw	-167.31	-127.97	194.08	661.87	-159.84	-377.48	-287.45
		(185.87)	(167.41)	(148.37)	(146.09)***	(190.1)	(191.52)**	(235.56)
Yearly growth rate of turnover	75% opt.bw	3.73	-3.63	-0.07	8.91	-2.44	1.56	7.95
		(4.45)	(2.72)	(2.54)	(2.88)***	(3.65)	(3.77)	(4.67)*
	opt.bw	3.56	-4.16	0.51	9.34	-2.40	0.32	6.73
		(3.90)	(2.42)*	(2.35)	(2.47)***	(3.11)	(3.42)	(3.95)*
	150% opt.bw	2.36	-3.32	1.09	8.43	-1.25	-1.65	5.72
		(3.03)	(2.07)	(2.09)	(2.02)***	(2.67)	(2.89)	(3.58)
1995–2001 per-firm employment change	75% opt.bw	-3.30	2.85	2.72	6.12	-4.75	0.10	1.35
		(2.68)	(2.87)	(2.13)	(3.17)*	(3.33)	(3.10)	(4.61)
	opt.bw	-4.17	0.85	2.59	5.87	-3.67	0.47	-0.06
		(2.27)*	(2.55)	(1.89)	(2.68)**	(2.83)	(2.93)	(4.07)
	150% opt.bw	-3.44	-0.13	2.35	6.34	-1.70	-0.70	-3.03
		(1.88)*	(2.16)	(1.69)	(2.23)***	(2.33)	(2.67)	(3.57)
Output per worker 2001	75% opt.bw	8.06	-7.62	2.64	-2.13	6.05	1.85	-6.64
		(5.44)	(4.98)	(6.83)	(4.87)	(3.90)	(4.39)	(4.84)
	opt.bw	7.11	-7.19	5.00	-1.42	5.89	2.34	-5.74
		(4.57)	(4.05)*	(5.98)	(4.15)	(3.19)*	(3.81)	(4.29)
	150% opt.bw	6.60	-5.94	1.51	-1.85	4.66	2.88	-3.17
		(3.71)*	(3.37)*	(4.54)	(3.45)	(2.89)*	(3.39)	(3.59)
Nb. obs. on the left of the cut-off		501	607	714	796	971	1103	1190
Nb. obs. on the right of the cut-off		855	749	642	560	385	353	166

Note: Bootstrapped Standard Errors in parentheses. We carry out all the estimates using the local linear regression with the triangular kernel. We use all the 1,356 non-duplicate firms (560 subsidised, 796 non-subsidised) of the dataset for the short-term analysis. Amounts of cumulative investment are expressed in thousands of euros.
***p<0.01, **p<0.05, *p<0.1.

2.7. Conclusions

The aim of this paper is to develop a reliable approach to evaluate the causal effect of public subsidies to investment in lagging regions. Our analysis focuses on the impact of L488 on cumulative investment, turnover, employment, and productivity in a nonparametric RDD framework. We take advantage of the quasi-experimental procedure indicated by the RDD to exploit the discontinuity points of the forcing variable, created by the auction mechanism of L488. Subsequent to an accurate and complex process of merging, we use the RDD for each region in each of the auctions analysed; afterwards, the estimates are aggregated and weighted through the MRDD.

The results suggest that the policy has boosted private capital accumulation in subsidised firms. Over the period 1995-2001, tangible capital and turnover growth rates and employment are considerably higher in subsidised firms. At the end of the period, the subsidised firms' tangible capital approximately doubles with respect to the median tangible capital in non-subsidised firms and this is not due to intertemporal substitution. These estimates are statistically significant and robust to different weighting schemes, bandwidths and kernels. The positive impact of subsidies on firms' growth and the negligible impact on productivity are also consistent with most of the international literature based on different evaluation strategies.

The effects of L488 are in line with at least one of the policymakers' targets: subsidised firms have invested more than they usually would have, and the additionality of the investment is confirmed by analysing the cumulative investment growth net of the subsidy amount. The additional capital stock accumulated is expected to generate income and wealth in the subsidised regions over time; this is why capital incentives are considered a policy instrument for territorial development. We find also that the output effect of subsidies on employment is greater than the substitution effect; however, this is not a policy primarily meant to increase employment in the area. For instance, the average cost per job created by the policy is higher with respect to other instruments more focused on the employment target.

The increase in capital stock does not necessarily entail efficient and productive subsidised firms as shown by the negligible impact of L488 on the output per worker. Such result might derive from the assignment criteria that reward projects with a high number of new employees by unit of investment. In fact, firms are induced to overshoot the optimal amount of employment in order to

gain the subsidy but this behaviour can affect long run efficiency and growth.[41] The effect of L488 on the competitiveness of subsidised firms in the global economy is an interesting topic left for future research.

[41] For a deeper discussion of the impact of L488 on productivity see Bernini and Pellegrini (2011).

Appendix 2.A. Data description

The initial subsidised firms group consisted of all the financed projects according to the rankings under analysis. Projects were eligible for the control group if they were admitted to the evaluation for the regional auctions but were not financed. After the merging procedure (using VAT identification number as firm identifier), the total number of firms under analysis was 2,881 (748 firms for the 2nd auction, 1,243 firms for the 3rd auction and 890 firms for the 4th auction). We then proceeded with the removal of certain categories of observations:

- Concerning duplicate projects, i.e. applications for more than one auction, we have decided to exclude the non-financed projects if the referring firm received the grant in another auction (in this case, the financed project was added to the treated group); on the other hand, we have opted to keep such projects inside the control group if the referring firm was never subsidised.
- Start-ups because their pre-treatment balance sheets are obviously not available.
- Projects that presented anomalies and irregularities.[42]
- Financed projects whose investment program was not yet concluded.
- All the projects started (or scheduled to start) after 1999. This choice is motivated by the impossibility of evaluating these projects because they were missing a sufficient temporal lag with project information after their conclusion.
- Projects pertaining to rankings that did not fulfil the pre-requisite of at least 10 subsidised and 10 non-subsidised observations.[43]

After verifying that the cleaning and integration procedures do not have a different impact on financed projects and the control group, our attention focused on the final dataset on which the evaluation model was implemented. This dataset consists of 560 financed projects and 1,144 non-financed projects (796 if we consider only non-duplicate firms) over the period 1995-2001. The composition of the merged dataset is in Table 2.A1.[44]

[42] 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.

[43] We have excluded 280 observations for this reason: Abruzzi in the 4th auction (6 treated and 41 non-treated firms); Basilicata in the 2nd auction (6 treated and 21 non-treated firms); Basilicata in the 3rd auction (8 treated and 33 non-treated firms); Basilicata in the 4th auction (4 treated and 16 non-treated firms); Molise in the 2nd auction (2 treated and 9 non-treated firms); Molise in the 3rd auction (1 treated and 7 non-treated firms); Molise in the 4th auction (2 treated and 3 non-treated firms); Sardinia in the 4th auction (4 treated and 40 non-treated firms); and Sicily in the 2nd auction (76 treated and 1 non-treated firms).

[44] Before carrying out the analyses, we used a truncation method, wherein extreme values (observations in the first two and last two centiles) are recoded to lowest or highest reasonable values (the value of the 2nd centile and the value of the 98th centile, respectively) to the relative dependent variable.

Table 2.A1. Composition of the merged dataset

Regions	Auction 2		Auction 3		Auction 4		Global	
	Subsidised		Subsidised		Subsidised		Subsidised	
	NO	YES	NO	YES	NO	YES	NO	YES
Abruzzi	54	26	82	15	-	-	136	41
Calabria	19	23	39	17	23	11	81	51
Campania	37	133	183	49	126	42	346	224
Puglia	44	79	167	41	88	36	299	156
Sardinia	18	17	39	10	-	-	57	27
Sicily	-	-	120	24	103	37	223	61
	172	278	630	156	340	126	1,142	560

Note: We have carried out the econometric analysis for the rankings with at least 10 treated and 10 non-treated observations. For this reason, we do not report the sample size for the auctions in Basilicata and Molise, in Sicily for the 2nd auction, and in Abruzzi and Sardinia for the 4th auction.

The sample for the long-term evaluation (1995-2004) consists of 239 financed projects and 470 non-financed projects (334 if we consider only non-duplicate firms). The decrease in the number of firms in the sample is mainly due to misprints (the merging process has been reiterated for each year in the period from 1995 to 2004), the unavailability of balance sheet data over the entire period, the closure or merging of some firms in the period from 2002-2004, and the presence of firms that received the L488 subsidies during the period from 2002-2004 (the presence of these firms in the sample for the long-term analysis would have biased the estimates).

Appendix 2.B. Additional tables and figures

Fig. 2.B1 presents plots of the evolution of the difference between financed and non-financed firms with respect to each dependent variable but cumulative investment, which is presented separately in Fig. 2.3, for the period from 1995-2004.

Fig. 2.B2 graphically reports the McCrary tests for all the rankings but those concerning Campania (reported separately in Fig. 2.4).

Table 2.B1 presents the RDD estimates using the pooling approach when we split the dataset in two halves according to the median dimension of the firms in 1995.

Table 2.B2 shows the results of the parametric RDD using a polynomial order up to 2.

Fig. 2.B1. Evolution of the difference between financed and non-financed firms with respect to investment, turnover, employment, and productivity during the period 1995-2004.
Note: We carry out a local linear regression in each year from 1995-2004. We use only the 573 non-duplicate firms composing the sample for the long-term evaluation. The figure for cumulative investment is reported separately in Fig. 2.3.

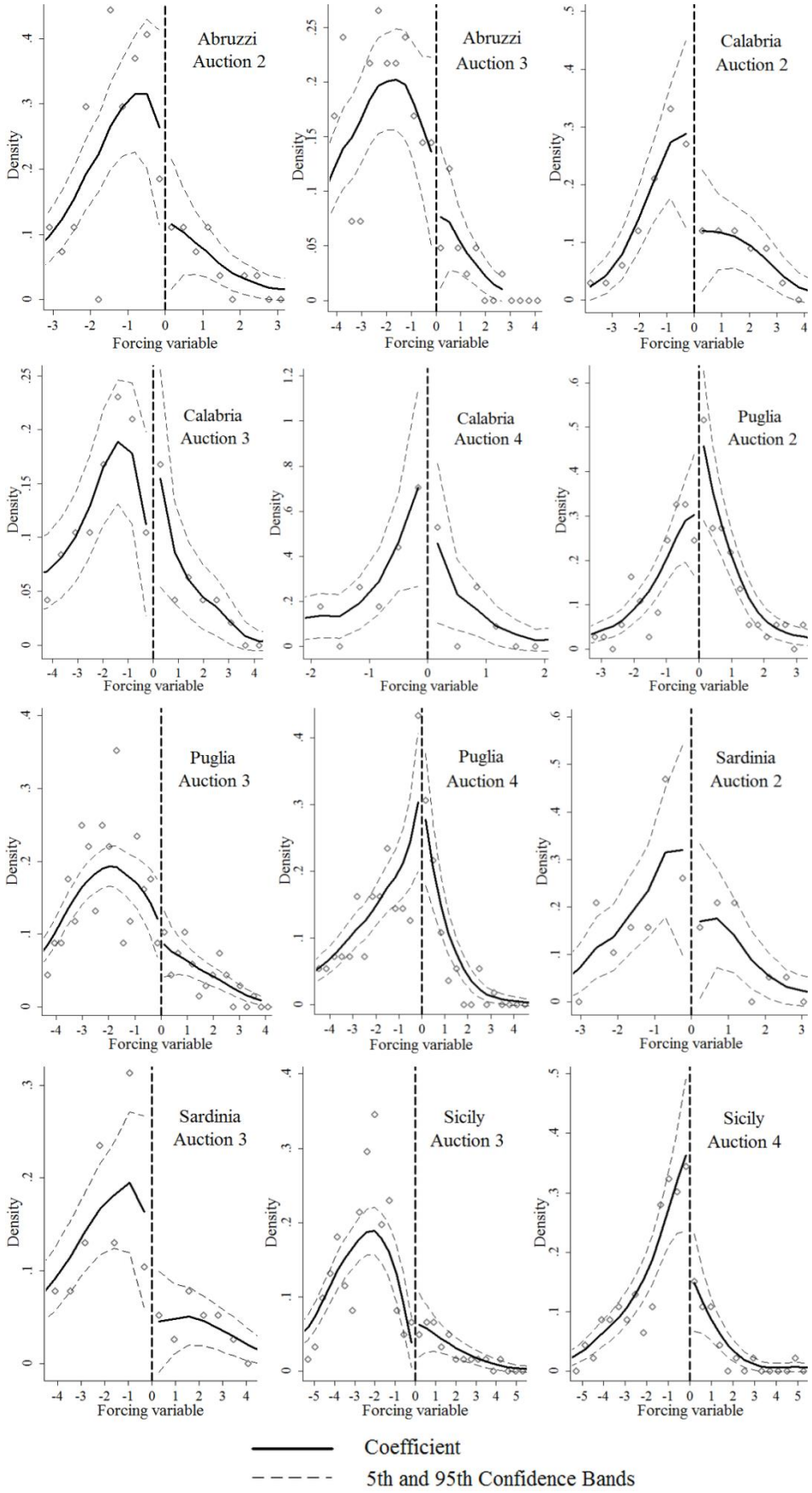

Fig. 2.B2. McCrary test for the analysed rankings (except Campania).
Note: This test is based on an estimator for the discontinuity at the cut-off in the density function of the forcing variable. The test is implemented as a Wald test of the null hypothesis that the discontinuity is zero. Figures for Campania are reported separately in Fig. 2.4.

Table 2.B1. Heterogeneity in the impact of L488 in respect to firm size

	Size of the firms	Yearly growth rate of tangible capital	Cumulative investment from 1995-2001	Yearly growth rate of turnover	1995–2001 per-firm employment change	Output per worker in 2001
Triangle Kernel	small	29.38	740.58	14.18	7.53	-19.58
		(6.72)***	(182.55)***	(5.30)***	(3.09)**	(14.64)
	medium-large	14.05	301.65	2.49	5.87	24.05
		(7.71)*	(526.21)	(4.93)	(12.71)	(15.27)
Epanech. Kernel	small	26.42	548.75	13.40	5.96	-12.83
		(6.32)***	(131.83)***	(4.95)***	(1.84)***	(9.91)
	medium-large	11.70	417.58	4.10	4.42	20.89
		(6.83)*	(368.53)	(4.85)	(7.08)	(11.59)*
Gaussian Kernel	small	27.80	565.67	13.46	6.06	-14.15
		(6.14)***	(125.46)***	(4.31)***	(2.04)***	(9.34)
	medium-large	12.78	398.32	3.50	4.21	21.37
		(6.42)**	(361.77)	(4.67)	(7.56)	(10.48)**

Note: Bootstrapped Standard Errors in parentheses. Small and large firms are defined as firms below and above the median of the firm turnover in 1995, respectively. The estimates are based on the 25% of the sample closest to the normalised cut-off point (keeping constant the percentage of financed firms in the sample). Using only half of this shortened sample means that we use only the 12.5% of the sample for each of these analyses. We use only the optimal bandwidth. Amounts of cumulative investment are expressed in thousands of euros.
***p<0.01, **p<0.05, *p<0.1.

Table 2.B2. Parametric estimates of each dependent variable

Dependent variable: yearly growth rate of tangible capital					
	(1)	(2)	(3)	(4)	(5)
Treatment effect	**15.14**	**17.90**	**21.33**	**22.53**	**20.99**
(standard error)	**(2.70)*** **	**(5.23)*** **	**(6.05)*** **	**(6.90)*** **	**(8.72)** **
Polynomial order	0	1	1	2	2
Interaction term	-	-	-27.15	-	-32.45
(standard error)			(21.11)		(79.41)
Interaction term squared	-	-	-	-	4.74
(standard error)					(19.35)
R-squared	0.0903	0.0914	0.0965	0.0946	0.0965
RMSE	0.2375	0.2377	0.2374	0.2377	0.2378
AIC	**-10.50**	-8.92	-8.80	-8.08	-5.81
BIC	**-2.86**	-2.54	-2.48	7.20	17.13
Nb. of treated	140	140	140	140	140
Nb. of controls	199	199	199	199	199
Dependent variable: cumulative investment from 1995-2001					
Treatment effect	**638.71**	**546.44**	**512.68**	**553.61**	**593.37**
(standard error)	**(142.17)*** **	**(243.95)** **	**(293.68)** **	**(317.57)** **	**(347.91)** **
Polynomial order	0	1	1	2	2
Interaction term	-	-	394.07	-	740.20
(standard error)			(1,237.3)		(1,875.73)
Interaction term squared	-	-	-	-	-1,335.57
(standard error)					(3,781.25)
R-squared	0.0639	0.0644	0.0646	0.0644	0.0655
RMSE	1208.3	1209.7	1211.4	1211.6	1219.3
AIC	**5,741.69**	5,743.51	5,745.44	5,745.51	5,781.27
BIC	**5,749.33**	5,754.97	5,760.72	5,760.79	5,804.21
Nb. of treated	140	140	140	140	140
Nb. of controls	199	199	199	199	199

Dependent variable: yearly growth rate of turnover					
Treatment effect	**6.29**	**8.79**	**10.28**	**10.56**	**9.83**
(standard error)	**(1.88)*** **	**(3.59)** **	**(4.45)** **	**(5.26)** **	**(6.99)**
Polynomial order	0	1	1	2	2
Interaction term	-	-	-11.82	-	-21.69
(standard error)			(16.34)		(61.70)
Interaction term squared	-	-	-	-	14.08
(standard error)					(97.89)
R-squared	0.0356	0.0377	0.0398	0.0387	0.0401
RMSE	0.1620	0.1620	0.1621	0.1622	0.1624
AIC	**-268.61**	-267.35	-266.11	-265.71	-263.51
BIC	**-260.97**	-255.88	-250.83	-250.43	-240.57
Nb. of treated	140	140	140	140	140
Nb. of controls	199	199	199	199	199
Dependent variable: 1995–2001 per-firm employment change					
Treatment effect	**4.44**	**5.10**	**7.20**	**7.91**	**6.56**
(standard error)	**(1.85)** **	**(2.34)** **	**(3.52)** **	**(4.26)* **	**(3.47)* **
Polynomial order	0	1	1	2	2
Interaction term	-	-	-11.90	-	8.66
(standard error)			(14.01)		(23.87)
Interaction term squared	-	-	-	-	-32.42
(standard error)					(78.47)
R-squared	0.0174	0.0176	0.0218	0.0201	0.0229
RMSE	16.485	16.508	16.498	16.512	16.538
AIC	**2,855.65**	2,857.60	2,858.16	2,858.72	2,861.78
BIC	**2,863.30**	2,869.07	2,873.45	2,874.02	2,884.72
Nb. of treated	140	140	140	140	140
Nb. of controls	199	199	199	199	199
Dependent variable: output per worker in 2001					
	(1)	(2)	(3)	(4)	(5)
Treatment effect	**-0.66**	**-4.98**	**-5.29**	**-1.64**	**8.05**
(standard error)	**(2.84)**	**(5.62)**	**(6.85)**	**(8.03)**	**(11.49)**
Polynomial order	0	1	1	2	2
Interaction term	-	-	2.55	-	-37.93
(standard error)			(21.33)		(71.07)
Interaction term squared	-	-	-	-	165.26
(standard error)					(212.20)
R-squared	0.0238	0.0271	0.0280	0.0411	0.0495
RMSE	25.48	25.49	25.53	25.51	25.43
AIC	**3,066.21**	3,067.39	3,069.36	3,068.87	3,068.96
BIC	**3,073.81**	3,078.76	3,084.54	3,084.05	3,091.74
Nb. of treated	140	140	140	140	140
Nb. of controls	199	199	199	199	199

Note: Heteroskedasticity-robust standard errors in parentheses. The estimates are based on the 25% of the sample closest to the normalised cut-off point (keeping constant the percentage of subsidised firms in the sample).The order of the polynomial is 0 if we do not add to the regression the forcing variable, whereas is 1 when we add the forcing variable, and 2 when we also add the squared of the forcing variable. The interaction term (squared) is given by the multiplication of the treatment dummy with the forcing variable (squared). Amounts of cumulative investment are expressed in thousands of euros.
***p<0.01, **p<0.05, *p<0.1.

3. Beyond the SUTVA: how policy evaluations change when we allow for interactions among firms

3.1. 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? This paper does not directly answer these general questions, but rather evaluates the effectiveness of a subgroup of industrial policies aiming to enhance the development of lagging regions.[1] Developed countries have used several place-based policies[2] 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.[3] 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 (see Essay 1). The great extent of evaluation works have focused on the policy impact on subsidised firms with respect to output, investment and employment, while the possible spillovers on other firms have been mostly overlooked. This is because most studies rely on the Stable Unit Treatment Value Assumption (SUTVA), i.e. they assume away any possible interactions among firms due to the policy (see Rubin, 1986). 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.

[1] 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.
[2] Place-based programmes target public resources towards disadvantaged geographic areas rather than towards disadvantaged individuals as the people-based policies (see Barca et al., 2012).
[3] Henceforth, we will refer to this industrial policy using the expressions investment subsidy policies or business incentive programmes.

A strongly related matter is described in Bondonio (2009: 5):

> *In principle, business incentive programmes 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 which they are located is very little. As a result, any actual programme 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 programmes) of a much greater importance than the possible programme-induced improvements in the economic activity of the assisted firms.*

This valuable insight underlies the impossibility to accurately determine the macro effect of an investment subsidy programme; 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 programme 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 or eligible but unsubsidised firms.

Traditional industrial policy analyses - i.e. all the analyses that rely on the SUTVA - put a lot of effort in facing selection bias issues; however, this comes at a price: such studies completely put aside the identification problems linked to spillover effects. Moreover, some of the traditional analyses identify the unsubsidised firms located in the vicinity of the subsidised firms as those firms with the most similar features in respect to the treatment group; nevertheless, in presence of spillovers, even a perfect control of selection bias will not suffice to prevent biased ATT estimates. Indeed, if some control units undergo policy spillovers they will not be suitable to be used as part of the control group, unless perfect knowledge about how spillovers spread is assumed and dealt with. For instance, in case of negative spillover effects on unsubsidised firms located in the vicinity of one or more subsidised firms belonging to the same sector of activity, traditional analyses will deliver an overestimate of the ATT even when selection bias is completely absent.

Potentially, investment subsidy programmes give rise to many externalities, such as agglomeration effects, 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 3.3, it is possible to single out each spillover only resorting to extremely strong assumptions. This is why we adopt a less stringent set of assumptions to retrieve two aggregate spillover parameters: i) the Average Spillover effect on the Affected (ASA) contrasts the positive agglomeration effect on unsubsidised firms with the cross-sectional substitution; and ii) the Average Spillover effect on the New Entrants (ASNE) contrasts the positive agglomeration effect on the new entrants with the crowding-out effect.

Therefore, we clearly distinguish 3 different parameters: the ATT, the ASA and the ASNE, denoting 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 of subsidised firms in terms of investment and employment. However, the positive effect on employment for subsidised firms is mitigated by the negative spillover effect on the affected untreated firms with at least 1 treated firm belonging to the same sector of activity located within a 1 mile distance.

This is not the first paper trying to take into account business incentive policy spillovers, in fact, also Criscuolo et al. (2012) and De Castris and Pellegrini (2012) face this challenging task. However, the present paper is the first to carry out the estimation of the micro spillover effects thanks to a novel evaluation strategy that partially relaxes the SUTVA.

The rest of the paper is organised as follows. Section 3.2 describes the rationales for investment subsidy policies and their potential spillovers with a small survey on other studies concerning industrial policies' spillovers. The welfare computation parameters are discussed in Section 3.3, followed in Section 3.4 by a presentation on how to partially relax the SUTVA. We then turn to the empirical application in Section 3.5, while Section 3.6 concludes.

3.2. Rationales for investment subsidy 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.[4] Indeed, most

[4] 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

governments have tried to reduce regional inequalities resorting to a number of place-based policies (e.g., EU 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;[5] 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 incentive programmes? Policymakers facing with distressed areas primarily try to reduce underdevelopment in a growth-enhancing way.[6] Some of them consider business incentive programmes useful to this purpose for two 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 subsidy 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 incentive 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.[7] In order to make clearer and simplify the empirical spillover analysis, in the remainder of the paper we will distinguish between the positive agglomeration effects on unsubsidised firms and the positive agglomeration effects on new entrants.[8]

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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).

[5] Ulltveit-Moe (2008) divides these tools into six categories: grants, tax deferrals, tax exemptions, soft loans, guarantees and equity participation.

[6] 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).

[7] 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).

[8] 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; however, in the application in Section 3.5 we will focus on localisation economies. This is consistent with Rosenthal and Strange's (2003) findings that localisation effects are much more pronounced than urbanisation effects and with the peculiarities of our application which excludes large urban centres from the analysis. For a survey of the literature on urbanisation and localisation economies see Beaudry and Schiffaurova (2009).

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;[9] rather, in the real world there is room for place-based policies to influence firms' location and investment levels. In fact, the cost of moving firms in lagging areas is considered to be lower than the social cost due to the mobility of workers from depressed regions to more developed areas (primarily large urban agglomerations). Nevertheless, investment subsidy policies try to boost the investment level in the lagging regions reducing the cost of capital and the theoretical effect that this has on employment is unclear. Indeed, firms are incentivised to substitute labour with capital (the substitution effect)[10] 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.[11]

Having said that, we yet have to discuss what are the main potential negative spillovers of business incentive programmes. In the literature, the most quoted negative spillover is 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 crowds-out private investment making the rationale in favour of business incentives less clear. On the estimation side, the evaluation strategies implemented in traditional evaluation works would deliver ATT estimates upwardly biased due to the use of a negatively affected control group. Additionally, some scholars

[9] 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).

[10] 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).

[11] 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).

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; nevertheless, in case policy funds are much smaller than the national GDP, general equilibrium effects might be considered negligible (Criscuolo et al., 2012). Finally, another spillover frequently cited among the failures of business incentive programmes is the crowding-out effect. This is in act if the additional investment of the subsidised firms crowds-out of the market non-subsidised firms.

3.2.1. The previous literature on 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.

This lack of spillover analyses is recently being filled up, in particular by the surge in research works regarding a specific place-based policy: the Enterprise Zones (EZs) programme. In this programme, delineated zones - usually neighbourhoods with socio-economic difficulties[12] - 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 programmes, 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 and Rohlin (2013) 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 programmes 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 incentive programmes are usually

[12] 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).

directed towards firms located in a few large depressed areas and this makes impossible to adopt the same evaluation strategy as the EZs programmes. Criscuolo et al. (2012) and Arpino and Mattei (2013) adopt evaluation strategies that allow addressing specific concerns of interactions among firms at the firm-level.[13] In the former paper, the authors indirectly estimate spillover effects of capital subsidies contrasting the results obtained at the firm-level (positive ATT in terms of employment and investment) with those obtained at the area-level. Overall, they find that the new employees come from the pool of unemployed and they do not find any evidence of reduction of jobs in neighbouring areas or in non-manufacturing industries. On the other hand, Arpino and Mattei analyse the impact of a soft loan policy modelling interactions among firms (specifying which firms interact with each other and the relative magnitudes of these interactions). They do not estimate spillover effects focusing their analysis on the estimation of the ATT. Their results show a positive impact on the employment level of subsidised firms but this positive impact diminishes with the strength of interference.

3.3. Is it empirically possible to disentangle different spillover effects?

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

1) the intensity of the subsidies and how the subsidies are dispensed;[14]

2) the subsidies that other firms receive, including competitors.

The evaluation strategies based on the SUTVA do not model how firms affect each other's but assume that even if they interact, the subsidies received by one or more of these firms do not influence the future outcomes of the other interacting firms. This assumption seems particularly strong especially when we talk about competing firms. For instance, 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.[15]

The potentially contemporaneous presence of different spillovers, such as the crowding-out effect, the substitution effect and the agglomeration effects, makes appealing for a policy evaluator to

[13] Bronzini and de Blasio (2006) make an attempt to empirically detect a conservative estimate of micro spillover effects resorting to an informal test.

[14] The relationship between the causal responses to different subsidy intensities is still an under-researched topic that has been faced only in a few papers (see Adorno et al., 2007; Becker et al., 2012).

[15] It is not possible to retrieve any information about the validity of the SUTVA from observed data. It is only possible to rely on subject-matter knowledge. The SUTVA is a substantive assumption, which is usually maintained, even though it is not always appropriate (Mealli et al., 2011).

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, in this study relaxing the SUTVA does not imply allowing for any possible spillover effects.

In Section 3.2 we have surveyed the most important spillovers linked to the business incentive programmes 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) the Average Spillover effect on the Affected (ASA) contrasts the positive agglomeration effect on unsubsidised firms with the cross-sectional substitution;

ii) the Average Spillover effect on the New Entrants (ASNE) contrasts the crowding-out effect and the agglomeration effect on business births.

3.4. A general framework to relax the SUTVA in industrial policy analyses

Consider a group of firms indexed by $i = 1, \dots, N$. Let the random variable D_i denote a treatment indicator which is equal to 1 if treatment is received by firm i and 0 otherwise. Let $\boldsymbol{D} \equiv (D_1, \dots, D_i, \dots, D_N)$ represent the treatment assignment for all firms. Following Hong and Raudenbush (2013), we describe the potential outcomes for firm i as a function of the firm i's own treatment assignment (D_i), the treatment assignment of other firms ($\boldsymbol{D}_{-i}$), as well as the assignment of the focal firm to a different intensity of treatment (j). For firm i with intensity of the treatment j, the potential outcome is denoted by $Y_i(\boldsymbol{D}, j)$.

It might then seem that causal inference is intractable.[16] Yet, let each firm have its own set of influence $(\boldsymbol{i})$ made up of all the firms that might affect firm's i potential outcomes and of which treatment assignment is represented by $\boldsymbol{D}_{(\boldsymbol{i})}$. Moreover, let $\Pr(q \in (\boldsymbol{i}))$ be a function of a vector $\boldsymbol{Z}$.

Assumption 1: There exists only 1 version of the treatment (1st part of the SUTVA), i.e. $j =$ constant, $\forall i$;

[16] The SUTVA is a special case where $Y_i(\boldsymbol{D}, j) = Y_i(D_i)$. In words, the SUTVA states that the treatment assignments of firms other than i and the intensity of the treatment received by firm i have no effect on firm i's potential outcomes.

Assumption 2: Firm i might interact only with firms belonging to $(\boldsymbol{i})$, so there are $2^{\#(\boldsymbol{i})+1}$ potential outcomes for firm i, and individual causal effects may be defined as a comparison between any two of them: $Y_i(D_i, \boldsymbol{D}_{(\boldsymbol{i})})$ versus $Y_i(D_i', \boldsymbol{D}'_{(\boldsymbol{i})})$; $D_i, D_i' \in \{0,1\}$, and $\boldsymbol{D}_{(\boldsymbol{i})}, \boldsymbol{D}'_{(\boldsymbol{i})} \in \{0,1\}^{\#(\boldsymbol{i})}$.

Assumption 3: Second order spillovers are negligible, i.e. even if firm's h potential outcome, with $h \in (\boldsymbol{k})$**,** is affected by the treatment of one or more firms in $(\boldsymbol{k})$ this will not affect firm's w potential outcome when $w \in (\boldsymbol{h})$ and $w \notin (\boldsymbol{k})$.

The latter is a simplifying assumption that allows to clearly distinguishing between affected untreated firms and not affected untreated firms. Fig. 3.1 illustrates an example of this framework.

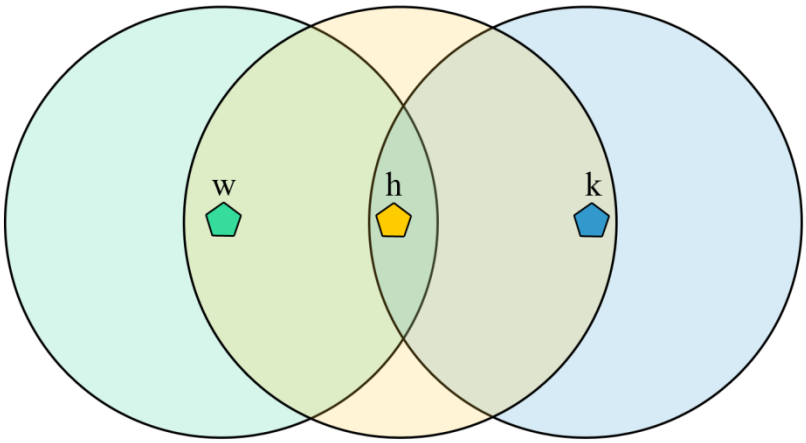

Fig. 3.1: Example of set of influences for 3 firms

In this example, the set of influence of firm w is made up by the other firms located within the buffer around firm w (only firm h). Of the 3 represented firms, only firm k receives a subsidy. Focusing on the potential outcomes of firms h and w, $Y_h(D_h, D_w, D_k)$ and $Y_w(D_w, D_h)$, the third assumption rules out that the possible influence on Y_h of the subsidy received by firm k, affects Y_w.

The present framework allows estimating a large range of causal effects; however, we are particularly interested in two specific causal effects:

Definition 1. The treatment effect[17] for the subsidised firm r:

(3.1) $\quad Y_r\big(D_r = 1, \boldsymbol{D}_{(\boldsymbol{r})}\big) - Y_r(D_r = 0, \boldsymbol{D}_{(\boldsymbol{r})} = \boldsymbol{0})$.

Definition 2. The spillover effect for the unsubsidised firm v:

(3.2) $\quad Y_v\big(D_v = 0, \boldsymbol{D}_{(\boldsymbol{v})} \neq \boldsymbol{0}\big) - Y_v(D_v = 0, \boldsymbol{D}_{(\boldsymbol{v})} = \boldsymbol{0})$.

[17] We might have used "policy effect" instead of "treatment effect" to remark that such effect might depend both on the subsidies received by firm r and on the subsidies received by other firms in its set of influence $(\boldsymbol{r})$.

Because of the fundamental problem of causal inference the aim is to estimate two average effects:

Definition 3. The Average Treatment effect on the Treated (ATT)[18]:

(3.3) $E[Y_i(D_i = 1, \boldsymbol{D}_{(i)}) - Y_i(D_i = 0, \boldsymbol{D}_{(i)} = \mathbf{0})|\boldsymbol{D}]$

Definition 4. The Average Spillover effect on the Affected (ASA):

(3.4) $E[Y_i(D_i = 0, \boldsymbol{D}_{(i)} \neq \mathbf{0}) - Y_i(D_i = 0, \boldsymbol{D}_{(i)} = \mathbf{0})|\boldsymbol{D}]$

In such framework $\boldsymbol{D}'_{(i)}$ is allowed to have a different causal effect than $\boldsymbol{D}_{(i)}$ on firm's *i* potential outcomes, where $\boldsymbol{D}_{(i)} \neq \boldsymbol{D}'_{(i)}$.

The SUTVA is a special case in which $Y_i \perp \boldsymbol{D}_{(i)}\ \forall i$, i.e. the future outcome of a firm does not depend on the treatment received by the firms belonging to its set of influence ⇒

(3.5) $\begin{cases} ATT\text{: } E[Y_i(D_i = 1) - Y_i(D_i = 0)|D_i = 1] \\ ASA\text{: } no\ spillover\ effects \end{cases}$

As shown in Table 3.1, in this framework there are 3 groups of firms while in the traditional approach only 2. In the example reported in Fig. 3.1, firm *k* is the treated, firm *h* the affected untreated, and firm *w* the not affected untreated.

Table 3.1. Differences in the groups of firms in the proposed framework with respect to the traditional framework

	Proposed framework	Traditional framework (SUTVA)
Treated Group	N_T = # of treated firms	N_T = # of treated firms
Affected Group	N_A = # of affected untreated firms	$N_A = \emptyset$
Control Group	N_C = # of not affected untreated firms	N_C = # of untreated firms
where, $N = N_T + N_A + N_C$		

3.4.1. Our framework

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 fairly strong; however, those assumptions will allow partially relaxing the SUTVA and retrieving rough estimates of the spillover effects.

[18] The counterfactual scenario for the ATT does not consist merely in changing the assignment for firm i from $D_i = 1$ to $D_i = 0$ but also in removing the subsidy to all the other firms that belong to $(\boldsymbol{i})$ if necessary, i.e. $\boldsymbol{D}_{(i)}$ is changed to the null vector if $\boldsymbol{D}_{(i)} \neq \mathbf{0}$.

In our case the vector $\boldsymbol{Z}$ that defines the set of influences consists of two variables: the economic distance and the spatial distance. We assume that a firm might interact only with firms having a limited economic distance from it (e.g. with the firms that belong to the same sector of activity).[19] Moreover, drawing on Tobler's first law of geography - everything is related to everything else, but near things are more related than distant things - the intensity of the interactions among firms with a limited economic distance is assumed to be diminishing in distance and to not extend over a certain spatial distance[20]. In short, two firms might interact only if they satisfy the aforementioned conditions about the economic and spatial distances. In such framework, Y_i might depend on the outcomes, the treatment received, and other covariates of the firms with which it interacts, i.e. the firms that belong to ($\boldsymbol{i}$).

The pool of control firms is made up by all the eligible untreated firms with no treated firms in their set of influence. This pool can be enlarged using also firms located in surrounding non-eligible areas with similar characteristics to the eligible areas. The proximity and the similarity of the non-eligible areas with respect to the eligible areas might rule out the presence of relevant territorial shocks; however, it might be possible to control for such shocks exploiting a non-eligible sector not strictly related with the eligible sectors. The most credible assumptions about the spillovers range should be evaluated on a case-by-case basis.

If the SUTVA holds, the new framework will still deliver unbiased ATT estimates and there will only be an efficiency loss with respect to traditional analyses due to the reduced number of controls caused by considering some firms' outcomes to be affected by the policy when they are not. However, if the SUTVA does not hold, traditional evaluation strategies will not deliver any estimate of the spillover effects and will not be capable of retrieving unbiased ATT estimates even assuming that they can perfectly control for selection bias. Indeed, some of the control firms do not represent anymore what would have happened to the assisted firms in case of no intervention. In the case that the economic and spatial distance assumptions are satisfied, the estimation of the 3 welfare evaluation components - the ATT, the ASA, and the ASNE - brings about a sharp improvement in the way capital subsidies are evaluated. The first two parameters might be retrieved contrasting the

[19] 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 2002 classification. In the application in Section 3.5 we subdivide the manufacturing firms in 14 groups considering the 2-digit NACE 2002 classification. 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 much more common to have interactions both in the technology and in the product markets.

[20] 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.

treated and the affected untreated groups of firms with the not affected untreated group of firms (see Table 3.1) using a quasi-experimental method capable of minimising selection bias, such as the matching difference-in-differences (MDID) estimator presented in Section 3.5.1.

On the other hand, relevant confounding factors make more challenging the evaluation of the ASNE. A possible strategy to infer the prevailing spillover effect consists of looking at the ratio of the number of new entrants to the number of firm exits in certain areas.[21] Considering new entrants (closing-down firms) located within a limited distance from the closest treated firm belonging to the same sector as new entrants (closing-down) affected firms and new entrants (closing-down firms) with no treated firms belonging to the same sector within the same limited distance as new entrants (closing-down) not affected firms it is possible to compare the aforementioned ratio for the 2 groups of firms. In case the affected firms' ratio prevails over the not affected firms' ratio, this might be interpreted as evidence that agglomeration effects have prevailed over crowding-out effects and vice versa.

3.5. Application

3.5.1. 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 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. Essay 2); however, when no natural experiment is available, matching methods are a valid alternative.[22] 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 is assumed to have at least 1 counterpart in the control group. In recent years, a number of papers (e.g. Iacus et al., 2012) have highlighted the misapplication of

[21] Of course, capital subsidy policies represent only one of a number of causes that might determine firm births and exits. For recent contributions on the main determinants of firm births and exits see Jofre-Monseny et al. (2011) and Cainelli et al. (2013).

[22] 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).

matching methods by some researchers; thus, a new class of matching methods has emerged - dubbed "monotonic imbalance bounding (MIB)"[23] (see Iacus et al., 2011) - that curtails the misuse of these techniques.

In the subsequent application 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. If different numbers of treated and control units appear in different strata, the econometric model must weight or adjust for the different stratum sizes.[24] This is why a weighted regression of the dependent variable on the covariates is adopted at the end of the matching procedure. 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 (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 paper we 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).

3.5.2. Data

In our application we evaluate 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,

[23] 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).
[24] See Iacus et al. (2012) for an illustration of how the CEM weights are computed.

technological updates, restructuring, relocation and reactivation.[25] L488 allocates subsidies through a rationing system on the basis of regional competitive auctions.

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[26] 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). The estimation results we present below rely on the assumption that there are no other governmental programmes correlated with the allocation of L488 funding.[27]

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.[28] Fig. 3.2 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.[29] 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.[30]

[25] 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).

[26] 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.

[27] 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.

[28] 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).

[29] 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).

[30] Obj. 1 regions receive transfers that are substantially higher in magnitude than transfers under all other lines of the EU's Structural Funds programme (Becker et al., 2013). In particular, for the L488 the medium-large subsidised firms located in Obj.2 areas received capital grants that support up to 10-20% 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% 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 (we are basically assuming that those firms would have effectuated the subsidised investment even in the absence of the policy).

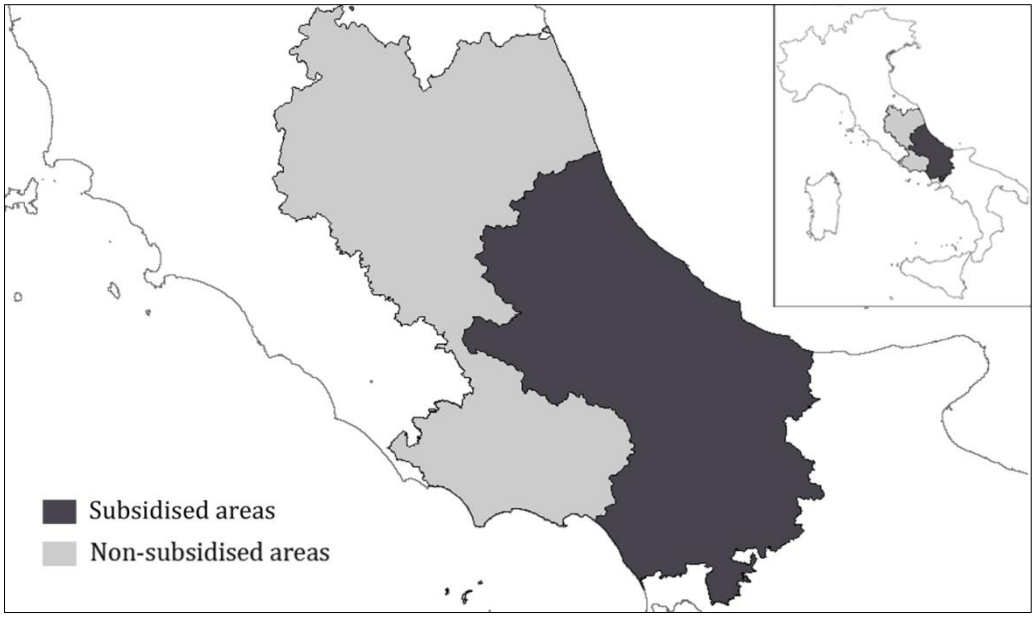

Fig. 3.2. Eligible and Non-Eligible Areas under Analysis

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 2,213 manufacturing firms (code D of the NACE 2002 classification) that were localised in the areas under analysis. Of these firms, 200 firms were subsidised, 679 make up the group of affected untreated firms, and 1,334 the control group (not affected untreated firms). The detailed construction of the sample is described in Appendix 3.A.

3.5.3 Results

The extent of agglomeration economies - not to mention the extent of the crowding-out effects and of the cross-sectional substitution - is still an under-researched topic. However, as well documented in Duschl et al. (2014), some authors have tried to shed some light on the impact of agglomeration economies on start-ups, firm survival, innovative and productive performance, and regional growth. Despite these studies, Frenken et al. (2011) argue that there is still a large gap in the understanding of agglomerations and in particular in the effect of localisation economies on firm performance. Drucker (2012) surveys the findings of a number of empirical studies that consider the spatial aspect of agglomerations. His survey documents a high heterogeneity in findings due especially to the methods adopted, the data at disposal, the type of industries analysed[31] and the countries on which they are located. Still, the majority of the surveyed studies report that externalities exist

[31] Distance may be more of an impediment to the realization of agglomeration benefits in those industries in which time plays a crucial role, [...] while agglomeration externalities for which the transmission mechanism necessitates less frequent face-to-face interactions should operate across greater distances (Drucker, 2012).

primarily in a narrow band surrounding a spillover generator, while a strong decay can be observed after a few miles. In particular, Rosenthal and Strange (2003) find that localisation economies are particularly strong in the first mile; while they attenuate rapidly over the first few miles, do not exceeding 15 miles. Additionally, they find weak evidence of urbanisation economies. Similarly, Henderson et al. (1995) find positive effects for both diversity and specialization externalities for high tech industries while mature industries experienced only localisation economies. Drawing on these findings we define the maximum spatial distance for possible spillover effects to be 20 km[32] and the economic distance limited to firms belonging to the same sector (2-digit NACE 2002 classification).[33] Then we look at three concentric rings of varying size to see how the ASA and the ASNE vary depending on the extension of the spatial distance. We exploit the untreated firms with no treated firms in their set of influence as controls to estimate the ATT and the ASA. 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 identifying controls according to 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 NACE 2002 classification. To this list we add also the number of neighbours within a 10 km distance in order to take into account different economic environments of firms in the same sector and across sectors. We then coarsen the joint distributions of these covariates by creating 203 strata (103 of which were matched).[34] In order to estimate the ATT, each subsidised firm is matched with one or more not affected untreated firms: i) belonging to the same sector of activity; ii) having similar values of the conditioning

[32] However possible, we believe that spillovers over a 20 km threshold are negligible for most industries, most notably in Italy where the small and medium-sized businesses are the backbone of the economy. In order to test for this assumption, we repeat the analyses reported in the following, using an even "safer" spatial threshold (we remove from the control group the non-subsidised firms with the closest treated neighbour belonging to the same sector located within a spatial distance from 20 to 30 km). We find no statistically different estimates with respect to the welfare computation parameters reported in the following and we interpret such result as a rough empirical proof that in our application spillovers are negligible over the 20 km threshold.

[33] Firms' geographical location refers to their registered office, although the vast majority of the firms in our sample only have one branch.

[34] The growth rate of tangible capital from 1993-1995, the turnover in 1995, and the number of neighbours with a maximum distance of 10km are coarsened at the median; the manufacturing firms are divided in 14 subgroups according to the NACE 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). The more strata the larger is the loss in treated/affected observations. This is why we do not add more coarsening variables or we do not coarsen some variables in quartiles instead of at the median. Such strategy allows us to analyse over 90% of the treated/affected firms, controlling for the residual imbalance in the weighted regression where we add also the tangible capital in 1995 and the ROE in 1995 as covariates. Nevertheless, in Appendix 3.C we use additional coarsening intervals and an extra variable to reduce the distance in terms of pre-treatment variables between treatment and control group. Even if this causes a substantial reduction in the number of treated/affected observations, the results are very similar to the ones obtained in the main analysis.

variables; iii) located in an area not interested by spillover effects but not too far away from the treated.

Table 3.2. Pre-treatment differences between the treated/affected and the controls

			Averages computed without using the CEM weights		Averages computed after using the CEM weights	
	Variables	Treatment/ Affected Group	Control Group	Difference	Control Group	Difference
Treated	Tangible Capital 1995	2,313	935	(1,378)***	1,570	(743)*
	Turnover 1995	8,093	4,012	(4,081)***	6,838	(1,255)
	Growth rate of tang. cap. 93-95	21.72	17.62	(4.10)	17.67	(4.05)
	ROE in 1995	17.83	9.65	(8.18)***	10.02	(7.81)**
	Nb. of workers in 1995	42.60	24.25	(18.35)***	37.13	(5.47)
	Nb. Of neighbours in 10km	6.52	9.53	(-3.01)*	5.53	(0.99)
	Added value in 1995	2,168	1,064	(1,104)***	1,636	(532)*
	Liabilities in 1995	7,519	3,508	(4,011)***	5,849	(1670)
Affected (first mile)	Tangible Capital 1995	1,411	929	(482)**	1,066	(345)
	Turnover 1995	4,712	3,633	(1,079)*	4,060	(652)
	Growth rate of tang. cap. 93-95	10.67	12.45	(-1.78)	12.77	(-2.10)
	ROE in 1995	6.95	6.78	(0.17)	6.97	(-0.02)
	Nb. of workers in 1995	27.51	22.23	(5.28)*	26.83	(0.68)
	Nb. Of neighbours in 10km	13.43	6.09	(7.32)***	7.86	(5.57)***
	Added value in 1995	1,114	980	(134)	1,163	(-49)
	Liabilities in 1995	4,459	3,204	(1,255)***	3,740	(719)
Affected (between 1 mile and 10 km)	Tangible Capital 1995	791	786	(5)	740	(51)
	Turnover 1995	2,807	3,423	(-616)	2,918	(-111)
	Growth rate of tang. cap. 93-95	16.45	16.67	(-0.22)	14.43	(2.02)
	ROE in 1995	8.93	7.03	(1.90)	6.50	(2.43)
	Nb. of workers in 1995	17.31	21.64	(-4.33)**	18.33	(-1.02)
	Nb. Of neighbours in 10km	11.02	5.35	(5.67)***	7.91	(3.11)**
	Added value in 1995	706	908	(-202)**	782	(-76)
	Liabilities in 1995	2,657	3,008	(-351)	2,583	(74)
Affected (between 10 and 20 km)	Tangible Capital 1995	887	706	(181)**	716	(171)
	Turnover 1995	2,831	3,383	(-552)	3,447	(-616)
	Growth rate of tang. cap. 93-95	11.05	20.39	(-9.34)***	11.93	(-0.88)
	ROE in 1995	5.81	11.04	(-5.23)***	6.40	(-0.59)
	Nb. of workers in 1995	18.79	21.54	(-2.75)	20.42	(-1.63)
	Nb. Of neighbours in 10km	7.98	17.80	(-9.82)***	11.49	(-3.51)**
	Added value in 1995	803	894	(-91)	860	(-57)
	Liabilities in 1995	2,930	2,812	(118)	3,023	(-93)

Note: Amounts are expressed in thousands of Euros. In computing the averages we use only the firms within the common support after the CEM procedure (191 treated obs. and the 787 controls for the ATT, 168 affected observations and the 615 controls for the ASA within 1 mile, 249 affected obs. and the 811 controls for the ASA between 1 mile and 10 km, and 236 affected obs. and the 924 controls for the ASA between 10 and 20 km).
***p<0.01, **p<0.05, *p<0.1.

Before proceeding to the ATT estimation, it is important to check the similarity of the treatment group with the control group, not only with respect to 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 control firms.[35] The results shown in Table 3.2 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. The residual imbalance between the two groups will be additionally reduced in the weighted regression. Table 3.2 reports also the differences between the affected and control groups used in the estimation of the ASA.

The ATT estimates are reported in Table 3.3 for three different matching specifications: the CEM-DiD; the CEM-DiD without strata having more treated observations than controls[36]; 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 from 8.94 to 10.24% for the yearly growth rate of tangible capital and from 1.78 to 3.08% for the yearly growth rate of turnover, while the effect on employment is of roughly 7-8 extra employees. All these estimates are statistically significant at the 1% level except for the Mahalanobis matching estimate for the yearly growth rate of turnover that is statistically significant at the 10% level.

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 must check this empirically. We adopt the matching specification described in Appendix 3.C 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 3.B). We then estimate the ASA with the same matching specification for each of the 3 affected groups determined upon the distance from the closest treated firm: i) untreated firms with the closest neighbour of the same sector within 1 mile distance, ii) untreated firms with the closest neighbour of the same sector between 1 mile and 10 km distance, and iii) untreated firms with the closest

[35] In Table 3.2 we report the results for other four covariates: the tangible capital in 1995, the ROE in 1995, the added value in 1995, and the liabilities in 1995. These variables are highly correlated with some of the other coarsening covariates; thereby it is not surprising that, if the CEM procedure has worked as expected, their level of imbalance is reduced between the two groups. Still, the results in Table 3.2 strengthen the hypothesis that our matching specification substantially reduces the pre-treatment differences between the two groups.

[36] This can be considered as an informal 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 4 treated observations but only 1 control). Indeed, a few controls are given too much weight in determining the estimates. A similar approach is followed in the estimation of the ASA and the territorial shocks. This informal robustness test is based on a similar rationale of the removal of outliers in the estimation of averages.

Table 3.3. ATT estimates

Bandwidth	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
Yearly growth rate of tangible capital	8.94 (1.29)***	9.25 (1.34)***	10.24 (1.92)***
Yearly growth rate of turnover	3.08 (0.70)***	2.78 (0.71)***	1.78 (0.98)*
Absolute employment change for each firm	8.16 (1.58)***	7.07 (1.59)***	8.17 (2.28)***
Nb. matched subsidised firms	191	181	181
Nb. controls	787	783	783

Note: Standard Errors in parentheses. Of the 787 controls there were 97 firms located in Obj.2 areas 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 9.33% for the yearly growth rate of tangible capital and 3.16% for the yearly growth rate of turnover. These estimates are statistically significant at the 1% level. When we remove the 4 strata with more subsidised firms than controls we lose 14 observations (10 treated and 4 control firms).
***p<0.01, **p<0.05, *p<0.1.

Table 3.4. ASA estimates

	Bandwidth	CEM-DiD	CEM-DiD without the strata with more subsidised firms than controls	Mahalanobis-metric matching after using the CEM to restrict the data and the removal of the strata in the previous step
Within 1 mile	Yearly growth rate of tangible capital	0.67 (1.48)	0.25 (1.47)	1.37 (2.14)
	Yearly growth rate of turnover	-0.02 (0.83)	0.08 (0.84)	-0.17 (1.32)
	Absolute employment change for each firm	-1.93 (0.94)**	-1.96 (0.95)**	-1.80 (1.42)
	Nb. matched affected firms	168	158	158
	Nb. controls	615	609	609
Between 1 mile and 10 km	Yearly growth rate of tangible capital	0.93 (1.21)	0.43 (1.25)	-0.72 (1.95)
	Yearly growth rate of turnover	-0.76 (0.69)	-0.59 (0.71)	-0.69 (1.06)
	Absolute employment change for each firm	-0.64 (0.98)	-0.60 (1.03)	-0.23 (1.44)
	Nb. matched affected firms	249	235	235
	Nb. controls	811	806	806
Between 10 and 20 km	Yearly growth rate of tangible capital	-1.23 (1.27)	-1.33 (1.31)	-0.77 (1.62)
	Yearly growth rate of turnover	0.16 (0.69)	-0.14 (0.69)	-0.25 (1.02)
	Absolute employment change for each firm	-0.51 (1.01)	-0.57 (1.04)	-0.94 (1.56)
	Nb. matched affected firms	236	227	227
	Nb. controls	924	921	921

Note: Standard Errors in parentheses. Correcting for the negative bias caused by the firms located in Obj.2 areas that received L488 funds, we find that the difference between the two groups of firms is i) 0.82%, 1.23%, and -1.08% for the yearly growth rate of tangible capital; ii) 0.02%, -0.70%, and 0.19% for the yearly growth rate of turnover, respectively for the ASA within 1 mile, the ASA between 1 mile and 10 km, and the ASA between 10 and 20 km. These estimates are not statistically significant. When we remove the strata with more affected firms than controls we lose 16 observations (10 affected and 6 control firms), 19 observations (14 affected and 5 control firms), and 12 observations (9 affected and 3 control firms), respectively for the ASA within 1 mile, the ASA between 1 mile and 10 km, and the ASA between 10 and 20 km. ***p<0.01, **p<0.05, *p<0.1.

neighbour of the same sector between 10 and 20 km distance. The results are summarised in Table 3.4.

The ASA in terms of investment and turnover turns out to be of a mixed sign and statistically insignificant. On the contrary, we find negative employment spillovers of magnitude -2 that are statistically significant at the 5% level in 2 of the 3 specifications for firms with at least 1 treated neighbour in their closest set of influence.

However, different set of covariates and coarsening procedures will yield different ATT and ASA estimates; this is why we check the robustness of our results in Appendix 3.C using a slightly different set of covariates and coarsening intervals. In general, the robustness analysis confirms the extent of all the ATT and ASA estimates, except for some weak evidence of negative turnover spillovers for affected firms with at least 1 treated neighbour interacting firm within 10 km distance.

Assuming that the population of manufacturing firms has the same proportion of subsidised and affected 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 ASA estimates (overlooking the statistical insignificance of some estimates) by the number of affected untreated firms and dividing their sum by the number of treated and affected firms, we find that on average each treated/affected firm has hired between 0.72 to 1.39 extra employees because of the L488 funds (we consider also the robustness analysis estimates). Dividing such figures by the average number of employees in 2001 (discounted by the extra employees' estimates) we find an increase of the employment level of manufacturing firms located in an affected area from 2.24 to 4.41 percentage points.

Finally, we estimate the ASNE for the three concentric rings described before. We find some evidence of crowding-out effects that have prevailed over agglomeration effects for the affected firms with at least 1 treated in their closest set of influence. Considering the combined estimates of the first 2 ASNE parameters and assuming the absence of agglomeration effects caused by the policy we find that 2.30% of the closures of manufacturing affected firms in the period 1995-2001 were caused by the L488 funds.[37]

[37] It is possible that the spillover effects engendered by the policy might have determined business births or closures with different characteristics for firms with or without a treated interacting firm in their set of influence. However, our tests show no statistically significant differences in the mean of the main covariates of new entrants and closing-down firms for affected and not affected firms.

3.5.3.1. Does the ATT depend on the number of firms in the set of influence?

Splitting subsidised firms between those with a number of neighbours above the median of their sector (considering only the treated) and those below the median we can gain insight into the different impact of the policy for firms more and less clustered. Considering only treated firms below such median, the effect on the growth rate of tangible capital and on employment is slightly larger than the estimates presented in Table 3.3, while the effect on the growth rate of turnover approximately doubles. These estimates are all statistically significant at the 1% level. Repeating the same procedure for treated firms above the median, we obtain estimates considerably lower for all the dependent variables and statistically significant only in a few instances. The estimates are reported in Table 3.5. Concerning employment, we can consider our results in line with the diminishing impact of the policy with the strength of interference found in Arpino and Mattei (2013).

Table 3.5. ATT estimates by number of neighbours

	Bandwidth	CEM-DiD	CEM-DiD without the strata with more subsidised firms than controls	Mahalanobis-metric matching after using the CEM to restrict the data and the removal of the strata in the previous step
Treated with a number of neighbours smaller than the median of their sector	Yearly growth rate of tangible capital	10.87 (1.87)***	10.87 (1.95)***	10.62 (2.73)***
	Yearly growth rate of turnover	5.38 (0.95)***	4.88 (0.98)***	4.48 (1.38)***
	Absolute employment change for each firm	10.12 (2.04)***	8.27 (1.99)***	8.31 (3.07)***
	Nb. matched subsidised firms Nb. controls	111 509	105 507	105 507
Treated with a number of neighbours larger than the median of their sector	Yearly growth rate of tangible capital	5.12 (2.01)**	7.76 (2.23)***	3.07 (3.16)
	Yearly growth rate of turnover	-0.22 (1.03)	-0.54 (1.12)	-2.05 (1.48)
	Absolute employment change for each firm	3.28 (2.67)	6.08 (2.70)**	6.20 (4.74)
	Nb. matched subsidised firms Nb. controls	74 247	65 241	65 241

Note: Standard Errors in parentheses. Correcting for the negative bias caused by the firms located in Obj.2 areas that received L488 funds, we find that the difference between the two groups of firms is i) 11.47% and 5.26% for the yearly growth rate of tangible capital; ii) 5.49% and -0.19% for the yearly growth rate of turnover, respectively for the treated firms with a number of neighbours below and above the median of their sector. These estimates are statistically significant at the 1% level for firms below the median and statistically significant only in a few instances for firms above the median. When we remove the strata with more treated firms than controls we lose 8 observations (6 treated and 2 control firms) and 15 observations (9 treated and 6 control firms), respectively for the treated firms with a number of neighbours below and above the median of their sector.
***p<0.01, **p<0.05, *p<0.1.

3.6. Conclusions

The main aim of this paper is to make business incentive programmes' empirical evaluations more thorough and more pertinent to the policymakers' targets. We do this by proposing a new framework to evaluate capital subsidy policies in order to avoid biased ATT estimates and to retrieve the main spillover effect estimates. This enables us to recover a global estimate of the effect of capital incentives on the regional economy. The present 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 effects 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 statistically significant negative employment spillovers for unsubsidised firms located within one mile of 1 or more subsidised firms that belong to the same sector of activity. 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 both investment and employment. The ATT estimates are in line with other evaluations of the L488 impact, such as Bernini and Pellegrini (2011) and Essay 2; however, the latter studies report even higher estimates of the policy impact on investment. Yet, in the present paper the positive effect on employment for the treated is partially determined to the detriment of affected firms located within a one mile distance of a treated firm in terms of both spillover parameters. This suggests that the subsidised manufacturing firms located in the eligible area attract part of 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 consonant with the De Castris and Pellegrini' spatial crowding-out finding and casts some doubts on the extent of the positive L488 impact on employment reported by previous literature.

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 product 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 findings are extendable to policies similar to L488 that reward projects with a high labour component. Policies only focused on capital deepening might engender different spillover effects.

Our study leaves room to some extensions; most notably, the flourishing literature on agglomeration indexes (see Ellison and Glaeser, 1997; Duranton and Overman, 2005; Marcon and Puech, 2010: Espa et al., 2013) could be exploited to move forward the estimation of average spillover effects, investigating the heterogeneity of industrial spillovers for different sectors. This direction of research might identify the sectors that most benefit from being targeted and, even more importantly, might help answering a crucial question for policymakers: is it best to incentivise areas with a large number of firms or sparsely industrialised areas?

Appendix 3.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,[38] 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 ASA:

- 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[39] have not been considered.
- Financed firms whose investment programme 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, we turned our attention to the final dataset on which the evaluation model was implemented. This dataset consists of 200 subsidised firms, 173 affected firms with the closest treated neighbour of the same sector within 1 mile distance, 269 affected firms with the closest treated neighbour of the same sector between 1 mile and 10 km distance, 237 affected firms

[38] 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%.

[39] 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.

with the closest treated neighbour of the same sector between 10 and 20 km distance), and 1,334 control firms.

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 NACE 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 eligible areas and of 759 firms located in non-eligible areas.

Appendix 3.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 - reported at the beginning of Appendix 3.C - except for the number of neighbours variable. Coarsening the joint distributions of the selected covariates we create 272 strata (139 of which were matched). The results are shown in Table 3.B1.

Table 3.B1. Territorial shocks estimates

Bandwidth	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
Yearly growth rate of tangible capital	1.13 (1.09)	1.23 (1.11)	0.87 (1.44)
Yearly growth rate of turnover	0.31 (0.52)	0.49 (0.55)	0.72 (0.73)
Absolute employment change for each firm	0.20 (0.33)	0.38 (0.35)	0.04 (0.52)
Nb. matched service firms located in the eligible area	433	412	412
Nb. controls	631	626	626

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 from 0.87 to 1.23% for the yearly growth rate of tangible capital, from 0.31 to 0.72% for the yearly growth rate of turnover, and from 0.04 to 0.38 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 3.C. Robustness analyses

We check the robustness of our results using the same set of conditioning variables used in Section 3.5.3; except for the fact that now we do not include the subdivision of manufacturing firms in 14

Table 3.C1. Pre-treatment differences between the treated/affected and the controls with a different coarsening specification

			Averages computed without using the CEM weights		Averages computed after using the CEM weights	
	Variables	Treatment/ Affected Group	Control Group	Difference	Control Group	Difference
Treated	Tangible Capital 1995	2,311	861	(1,450)***	2,010	(201)
	Turnover 1995	7,837	3,542	(4,295)***	7,090	(647)
	Growth rate of tang. cap. 93-95	22.58	19.58	(3.00)	20.75	(1.83)
	ROE in 1995	17.54	8.82	(8.72)***	9.75	(7.79)**
	Nb. of workers in 1995	42.53	22.64	(19.89)***	43.85	(-1.32)
	Nb. Of neighbours in 10km	6.55	16.24	(-9.69)***	11.62	(-5.07)*
	Added value in 1995	2,170	974	(1,196)***	1,948	(222)
	Liabilities in 1995	7,467	3,197	(4,270)***	6,822	(645)
Affected (first mile)	Tangible Capital 1995	1,354	1,155	(199)	1,174	(180)
	Turnover 1995	4,439	4,541	(-102)	4,281	(158)
	Growth rate of tang. cap. 93-95	10.31	13.35	(-3.04)	11.88	(-1.57)
	ROE in 1995	4.07	4.18	(-0.11)	4.59	(-0.52)
	Nb. of workers in 1995	27.12	28.34	(-1.22)	26.91	(0.21)
	Nb. Of neighbours in 10km	13.48	14.07	(-0.59)	16.57	(-3.11)
	Added value in 1995	1,061	1,227	(-166)	1,175	(-114)
	Liabilities in 1995	4,421	4,120	(301)	4,011	(410)
Affected (between 1 mile and 10 km)	Tangible Capital 1995	888	1,007	(-119)	835	(53)
	Turnover 1995	3,297	4,127	(-830)*	3,542	(-247)
	Growth rate of tang. cap. 93-95	14.92	21.62	(-6.70)**	16.07	(-1.15)
	ROE in 1995	5.25	7.23	(-1.98)	4.35	(0.90)
	Nb. of workers in 1995	18.96	26.67	(-7.71)***	21.67	(-2.71)
	Nb. Of neighbours in 10km	10.59	15.28	(-4.69)**	16.16	(-5.57)**
	Added value in 1995	787	1,135	(-348)***	955	(-168)
	Liabilities in 1995	3,093	3,707	(-614)	3,160	(-67)
Affected (between 10 and 20 km)	Tangible Capital 1995	902	983	(-81)	913	(-11)
	Turnover 1995	2,937	3,991	(-1,054)**	3,431	(-494)
	Growth rate of tang. cap. 93-95	10.56	20.71	(-10.15)***	14.11	(-3.55)
	ROE in 1995	5.18	9.40	(-4.22)*	5.74	(-0.56)
	Nb. of workers in 1995	19.20	24.86	(-5.66)**	21.05	(-1.85)
	Nb. Of neighbours in 10km	7.98	15.50	(-7.52)***	14.56	(-6.58)***
	Added value in 1995	827	1,080	(-253)**	940	(-113)
	Liabilities in 1995	3,007	3,623	(-616)	3,247	(-240)

Note: Amounts are expressed in thousands of Euros. In computing the averages we use only the firms within the common support after the CEM procedure (185 treated observations and the 968 controls for the ATT, 162 affected observations and the 765 controls for the ASA within 1 mile, 239 affected observations and the 965 controls for the ASA between 1 mile and 10 km, and 226 affected observations and the 971 controls for the ASA between 10 and 20 km). ***p<0.01, **p<0.05, *p<0.1.

subgroups, i.e. with this specification firms from different subgroups can be matched. At the same time, we add the investment in 1995 among the coarsening variables and we coarsen such variable, the growth rate of tangible capital from 1993-1995, and the turnover in 1995 into quartiles. Table 3.C1 reports the pre-treatment differences between the treated/affected and the controls, while Table 3.C2 and Table 3.C3 report the results for the ATT estimates and for the ASA, respectively.

Table 3.C2. ATT estimates with a different coarsening specification

Bandwidth	CEM-DiD	CEM-DiD without the 3 strata with more subsidised firms than controls	Mahalanobis-metric matching after using the CEM to restrict the data and the removal of 3 strata
Yearly growth rate of tangible capital	11.15 (1.18)***	11.36 (1.20)***	9.60 (2.03)***
Yearly growth rate of turnover	3.40 (0.68)***	3.04 (0.69)***	2.34 (1.06)**
Absolute employment change for each firm	7.43 (1.56)***	6.38 (1.57)***	8.83 (2.39)***
Nb. matched subsidised firms	185	178	178
Nb. controls	968	965	965

Note: Standard Errors in parentheses. Of the 968 controls there were 93 firms located in Obj.2 areas 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 11.40% for the yearly growth rate of tangible capital and 3.47% for the yearly growth rate of turnover. These estimates are statistically significant at the 1% level. When we remove the 3 strata with more subsidised firms than controls we lose 10 observations (7 treated and 3 control firms).
***p<0.01, **p<0.05, *p<0.1.

Table 3.C3. ASA estimates with a different coarsening specification

	Bandwidth	CEM-DiD	CEM-DiD without the strata with more subsidised firms than controls	Mahalanobis-metric matching after using the CEM to restrict the data and the removal of the strata in the previous step
Within 1 mile	Yearly growth rate of tangible capital	0.50 (1.46)	0.55 (1.47)	-0.66 (2.10)
	Yearly growth rate of turnover	-1.36 (0.83)	-1.52 (0.86)*	-1.77 (1.40)
	Absolute employment change for each firm	-2.40 (1.01)**	-2.44 (1.07)**	-1.97 (1.55)
	Nb. matched affected firms	162	153	153
	Nb. controls	765	761	761
Between 1 mile and 10 km	Yearly growth rate of tangible capital	-0.08 (1.19)	-0.06 (1.25)	-0.45 (1.85)
	Yearly growth rate of turnover	-0.71 (0.68)	-1.19 (0.70)*	-0.60 (1.01)
	Absolute employment change for each firm	-1.77 (1.04)*	-1.52 (1.06)	-0.47 (1.36)
	Nb. matched affected firms	239	225	225
	Nb. controls	965	960	960

Between 10 and 20 km	Yearly growth rate of tangible capital	-0.18	-0.22	-0.97
		(1.18)	(1.21)	(1.75)
	Yearly growth rate of turnover	-0.19	-0.19	0.01
		(0.68)	(0.70)	(1.09)
	Absolute employment change for each firm	0.15	0.86	-0.34
		(0.99)	(1.02)	(1.53)
	Nb. matched affected firms	226	216	216
	Nb. controls	971	965	965

Note: Standard Errors in parentheses. Correcting for the negative bias caused by the firms located in Obj.2 areas that received L488 funds, we find that the difference between the two groups of firms is i) 0.63%, 0.15%, and -0.07% for the yearly growth rate of tangible capital; ii) -1.30%, -0.66%, and -0.15% for the yearly growth rate of turnover, respectively for the ASA within 1 mile, the ASA between 1 mile and 10 km, and the ASA between 10 and 20 km. These estimates are not statistically significant. When we remove the strata with more affected firms than controls we lose 13 observations (9 affected and 4 control firms), 19 observations (14 affected and 5 control firms), and 16 observations (10 affected and 6 control firms), respectively for the ASA within 1 mile, the ASA between 1 mile and 10 km, and the ASA between 10 and 20 km.
***p<0.01, **p<0.05, *p<0.1.

We now combine the coarsening specification of the main analysis with the one of the first robustness analysis in order to reduce the distance in terms of pre-treatment variables between the treatment/affected group and the control group (Table 3.C4). Even if this causes a substantial reduction in the number of treated/affected observations, the tables below (Table 3.C5 and Table 3.C6) show that the results are very similar to the ones obtained in the main analysis.

Table 3.C4. Pre-treatment differences between the treated/affected and the controls (restricted sample)

			Averages computed without using the CEM weights		Averages computed after using the CEM weights	
	Variables	Treatment/ Affected Group	Control Group	Difference	Control Group	Difference
Treated	Tangible Capital 1995	1,670	865	(805)***	1,607	(63)
	Turnover 1995	6,961	3,703	(3,258)***	6,503	(458)
	Growth rate of tang. cap. 93-95	24.35	18.62	(5.73)	21.88	(2.47)
	ROE in 1995	17.44	10.57	(6.87)**	13.39	(4.05)
	Nb. of workers in 1995	34.14	22.18	(11.96)***	35.42	(-1.28)
	Nb. Of neighbours in 10km	6.85	7.46	(-0.61)	5.63	(1.22)
	Added value in 1995	1,820	1,013	(807)***	1,658	(162)
	Liabilities in 1995	5,961	3,210	(2,751)***	5,792	(169)
Affected (first mile)	Tangible Capital 1995	1,500	1,129	(371)	1,233	(267)
	Turnover 1995	5,238	4,415	(823)	4,605	(633)
	Growth rate of tang. cap. 93-95	9.46	11.67	(-2.21)	9.53	(-0.07)
	ROE in 1995	7.21	5.35	(1.86)	5.74	(1.47)
	Nb. of workers in 1995	29.52	27.01	(2.51)	28.33	(1.19)
	Nb. Of neighbours in 10km	13.54	7.33	(6.21)***	8.13	(5.41)***
	Added value in 1995	1,189	1,140	(49)	1,194	(-5)
	Liabilities in 1995	4,854	3,738	(1,116)	3,961	(893)

Affected (between 1 mile and 10 km)	Tangible Capital 1995	820	1,134	(-314)**	808	(12)
	Turnover 1995	2,909	4,280	(-1,371)**	3,307	(-398)
	Growth rate of tang. cap. 93-95	13.86	15.72	(-1.86)	13.69	(0.17)
	ROE in 1995	5.84	4.34	(1.50)	4.88	(0.96)
	Nb. of workers in 1995	18.50	26.54	(-8.04)***	20.45	(-1.95)
	Nb. Of neighbours in 10km	10.99	6.48	(4.51)***	7.86	(3.13)**
	Added value in 1995	778	1,151	(-373)***	898	(-120)
	Liabilities in 1995	2,752	4,033	(1,281)**	3,053	(-301)
Affected (between 10 and 20 km)	Tangible Capital 1995	859	770	(89)	762	(97)
	Turnover 1995	2,737	3,205	(-468)	3,232	(-495)
	Growth rate of tang. cap. 93-95	11.55	17.73	(-6.18)	13.75	(-2.20)
	ROE in 1995	4.94	5.28	(-0.34)	5.42	(-0.48)
	Nb. of workers in 1995	18.86	21.13	(-2.27)	20.58	(-1.72)
	Nb. Of neighbours in 10km	8.38	14.98	(-6.60)***	10.94	(-2.56)
	Added value in 1995	815	880	(-65)	845	(-30)
	Liabilities in 1995	2,856	2,923	(-67)	2,940	(-84)

Note: Amounts are expressed in thousands of Euros. In computing the averages we use only the firms within the common support after the CEM procedure (129 treated observations and the 342 controls for the ATT, 130 affected observations and the 261 controls for the ASA within 1 mile, 193 affected observations and the 357 controls for the ASA between 1 mile and 10 km, and 187 affected observations and the 372 controls for the ASA between 10 and 20 km).
***p<0.01, **p<0.05, *p<0.1.

Table 3.C5. ATT estimates (restricted sample)

Bandwidth	CEM-DiD	CEM-DiD without the strata with more subsidised firms than controls	Mahalanobis-metric matching after using the CEM to restrict the data and the removal of the strata in the previous step
Yearly growth rate of tangible capital	11.43 (1.76)***	11.96 (1.78)***	9.72 (2.66)***
Yearly growth rate of turnover	1.90 (0.93)**	2.14 (0.96)**	1.80 (1.36)
Absolute employment change for each firm	5.56 (1.99)***	7.02 (2.00)***	8.50 (2.71)***
Nb. matched subsidised firms	129	123	123
Nb. controls	342	339	339

Note: Standard Errors in parentheses. Of the 342 controls there were 40 firms located in Obj.2 areas 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 11.69% for the yearly growth rate of tangible capital and 1.96% for the yearly growth rate of turnover. These estimates are statistically significant at the 5% level. When we remove the strata with more subsidised firms than controls we lose 14 observations (10 treated and 4 control firms).
***p<0.01, **p<0.05, *p<0.1.

Table 3.C6. ASA estimates (restricted sample)

	Bandwidth	CEM-DiD	CEM-DiD without the strata with more subsidised firms than controls	Mahalanobis-metric matching after using the CEM to restrict the data and the removal of the strata in the previous step
Within 1 mile	Yearly growth rate of tangible capital	1.21	1.73	-0.46
		(1.66)	(1.76)	(2.35)
	Yearly growth rate of turnover	-1.27	-1.10	-1.89
		(0.99)	(1.00)	(1.47)
	Absolute employment change for each firm	-2.63	-3.22	-1.86
		(1.71)	(1.79)*	(2.59)
	Nb. matched affected firms	130	120	120
	Nb. controls	261	256	256
Between 1 mile and 10 km	Yearly growth rate of tangible capital	0.78	0.54	0.72
		(1.44)	(1.49)	(2.00)
	Yearly growth rate of turnover	-1.00	-1.24	-1.48
		(0.87)	(0.89)	(1.30)
	Absolute employment change for each firm	-0.74	-1.07	0.05
		(1.26)	(1.33)	(1.82)
	Nb. matched affected firms	193	181	181
	Nb. controls	357	353	353
Between 10 and 20 km	Yearly growth rate of tangible capital	0.59	-0.35	-0.87
		(1.50)	(1.51)	(2.08)
	Yearly growth rate of turnover	0.67	1.03	1.28
		(0.84)	(0.87)	(1.16)
	Absolute employment change for each firm	0.74	0.64	-0.11
		(1.23)	(1.27)	(1.84)
	Nb. matched affected firms	187	179	179
	Nb. controls	372	370	370

Note: Standard Errors in parentheses. Correcting for the negative bias caused by the firms located in Obj.2 areas that received L488 funds, we find that the difference between the two groups of firms is i) 1.41%, 0.94%, and 0.71% for the yearly growth rate of tangible capital; ii) -1.22%, -0.95%, and 0.68% for the yearly growth rate of turnover, respectively for the ASA within 1 mile, the ASA between 1 mile and 10 km, and the ASA between 10 and 20 km. These estimates are not statistically significant. When we remove the strata with more affected firms than controls we lose 15 observations (10 affected and 5 control firms), 16 observations (12 affected and 4 control firms), and 10 observations (8 affected and 2 control firms), respectively for the ASA within 1 mile, the ASA between 1 mile and 10 km, and the ASA between 10 and 20 km.
***p<0.01, **p<0.05, *p<0.1.

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