A CRITICAL SURVEY ON CAPITAL SUBSIDY POLICIES[1]

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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.[2] 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.[3] It is from this view of economic development that policies targeting underdeveloped areas have arisen.

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 the recent empirical evidence of one of the most popular place-based policies: business capital subsidies.[4] This is typically a selective policy that provides grants to finance the

[1] This paper is a revised version of the first chapter of the author's PhD thesis "Developments in the evaluation of capital subsidy policies" at the Sapienza University of Rome.

[2] "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).

[3] 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.

[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).

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.[5] For a more general survey on place-based policies see Neumark and Simpson (2015), while for surveys on R&D see Cerulli (2010) and Becker (2014).

The paper has been organised as follows: the next section summarises the theoretical rationales for and against business incentive programmes. Section 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 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 5. Finally, Section 6 concludes.

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 (2015) 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.

[5] 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.

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).

On the other hand, capital subsidies have also been 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).

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.[6] 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 and induce firms to locate their investment in backward areas kick-starting a growth process in underdeveloped areas. 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 (Cerqua and Pellegrini, 2014B).

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 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

[6] "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).

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).

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).[7] 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 more recent regression discontinuity design. In this section, we will review the most common evaluation strategies highlighting merits and limitations of each methodology.[8]

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.

[7] See Section 5 for a discussion on what happens when the SUTVA does not hold.

[8] 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.

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. 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 "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;
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) make it possible to formulate 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-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 makes it possible to retrieve 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). 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 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 main threats to validity of this parametric method lie in the untestable strong structural assumptions.

4. Main Findings

In the literature, there is considerable variation in the estimated impact of investment support, which, among others, reflects differences in circumstances between countries, 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[9] gathering together the findings of the most credible research works carried out in the last 20 years.[10,11]

Ideally retracing the steps of the credibility revolution illustrated in Angrist and Pischke (2010),[12] we will use 3 criteria for selecting the most credible studies: 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 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.[13] 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, 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.

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[9] 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., 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., 2007).

[10] 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 also analyse the effectiveness of other policies we will refer to them only for the parts on investment incentives.

[11] 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.

[12] 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).

[13] As suggested by Brown et al. (1995), rejected applicants show a propensity for investment very similar to that of subsidised firms.

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.

In this survey we discuss only the studies which meet all the credibility criteria. Of course, as the selection of research works is not clear-cut, we exercise a degree of discretion in classifying the studies.[14]

Criscuolo et al. (2012) investigate the effects of the Regional Selective Assistance (RSA) by using a combination of IV and plant- or firm-level fixed effects. They find evidence for a positive ATT in terms of employment, investment and net entry. 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) analyse the Italian Law 488 (L488) reaching similar conclusions to Criscuolo et al. (2012) on investment and employment. They also find that output and value added grew substantially in subsidised firms.

A positive impact of the subsidies on employment, investment, and turnover is also found in Cerqua and Pellegrini (2014A). The authors exploit the sharp discontinuities in the L488 policy assignment process using a nonparametric approach based on the RDD; furthermore, they find that the new subsidised capital is additional but non-complementary with the owner-financed investment. On the other hand, Bronzini and de Blasio (2006) analyse the same policy adopting 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.

Recently, Moffat (2014) adopts PSM to examine whether receipt of a RSA grant has a causal impact on plant TFP. Similar to Criscuolo et al. (2012), for high-tech and medium high-tech manufacturing, the effect is not statistically significant. However, for medium low-tech and low-tech manufacturing, receiving an RSA was found to reduce TFP. Negligible or negative effects on labour productivity or TFP are also found in Bernini and Pellegrini (2011), and Cerqua and Pellegrini (2014A). These negative results might be due to firms overshooting the optimal amount of employment to gain a subsidy. The relationship between capital subsidies and TFP has also been studied using a decomposition approach. Harris and Robinson (2005) break down TFP into its allocative components (entry, exit, within plant, between plant and cross-plant effects) and find that RSA financed plants experienced negative TFP growth, mostly due to plants with low TFP increasing their market share during the period. Skuras et al. (2006) decompose TFP into three components (technical change, technical efficiency change, and scale efficiency change). They find that capital subsidies to the Greek food manufacturing sector are not fully additional and affect TFP growth mostly through technical change.

Concerning the impact of capital subsidies on plant survival a positive effect has been found in two recent papers. Moffat (2015) combines the PSM and the Cox proportional hazards model finding that RSA capital subsidy grants reduce on average the probability of closure by 15-20%. Adapting

[14] A discussion of the studies which do not meet all the credibility criteria is available upon request to the author.

the RDD approach to the survival analysis framework, Muccigrosso and Pellegrini (2013) find consistently higher survival probability in L488 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.

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.

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.

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, such studies are marred by a long series of concerns and selection bias is merely the first one of them. 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).[15] 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).

[15] 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.

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 3 will deliver: i) an upward (downward) estimate of the ATT; and ii) no estimates of the spillover effects (see Cerqua and Pellegrini, 2014B).

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).

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 also to take 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).[16] 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 the analysis 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 public 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. Also the partial equilibrium costs and benefits expressed as the social return of investment incentives are difficult to retrieve (Cerqua and Pellegrini, 2014A).

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. However, only a few studies on capital subsidies have retrieved an estimate of the former parameter (Bondonio and Greenbaum, 2014; Criscuolo et al., 2012; Cerqua and Pellegrini; 2014A)[17] or of the latter parameter (the GEFRA-IAB report, 2010; Cerqua and Pellegrini, 2014A).

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 programme 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

[16] 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).

[17] 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).

can no longer be considered unaffected by the programme 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 is to decide 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 establishments 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 (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).

6. Concluding remarks

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 and the study of new solutions. 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.

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