Modelling and measuring the effects of public subsidies on business R&D: theoretical and econometric issues

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ABSTRACT. It is the aim of this paper to review the principal econometric models used so far to measure the effect of government's support to private R&D expenditure; in order to reach this task, we first present a basic theoretical framework to identify the effects of public subsidies on business R&D, going on by extending it to the case of dynamic complementarities and presence of subsidy spillovers. The review of the econometric models, the core of the paper, starts from section 3. We first classify econometric models according to three dimensions: 1. *structural* (based on a system of equations) and *non-structural* (based on a reduced-form equation and, possibly, a counterfactual) models; 2. models using the subsidy variable in a *continuous* or in a *binary* form; and finally, 3. studies exploiting a *cross-section* versus a *longitudinal* (panel data) structure. The final part of the paper is an original contribution providing some guidelines to implement R&D policy evaluation in a dynamic subsidization setting.

KEYWORDS: Business R&D; Public incentives; Econometric evaluation; Dynamic treatment

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INTRODUCTION

In the last thirty years a great bulk of empirical evidence has put in evidence the essential role played by business R&D efforts in fostering technological change, innovation and economic growth. As a consequence, it is not surprising that governments of industrialized countries have been long since engaged in providing incentives for the enlargement of the national R&D outlay.

Although the traditional "public good attribute" of knowledge seems still the most known and accepted justification for policy intervention, other "market failures" such as capital market imperfections, barriers to entry and exit, coordination failure and so on, seems produce an insufficient provision of private R&D effort.

Even though policy interventions trying to promote firms' R&D effort date back at least to the middle of the past century, only in recent years economists and econometricians have provided reliable scientific studies aimed at understanding the set of complex factors explaining the rationale for R&D subsidization, the functioning of firm R&D strategy and the techniques to measure incentives' effectiveness: we believe, in fact, that these three elements needs to be increasingly understood to provide a sound basis for policy guidance in this field.

It is the aim of this paper to review the principal econometric models used so far to measure the effect of government's support to private R&D, by tacking into account also some crucial theoretical aspects. In order to reach this aim, we present in the next section a basic theoretical framework widely adopted in the literature to identify the effects of public subsidies on business R&D, trying to extend it to the case of dynamic complementarities in firm R&D strategy (section 1.1) and to the presence of subsidy spillovers (section 2.1).

The review of the econometric models, the core of the paper, starts from section 3 (just after a brief introduction in section 2.2). We decide to classify econometric models roughly according to three dimensions: the first relies on the distinction between *structural* models (that is, models based on a system of equations) and *non-structural* or *reduced-form* models (that is, models based on one equation and, in some cases, a counterfactual); the second dimension hinges on the distinction between models using the policy variable (i.e., the public R&D subsidy) in *level* or in a *binary* form (i.e., supported vs. non-supported status); the third dimension, finally, concerns the type of dataset exploited, basing our analysis on the distinction between studies exploiting a *cross-section* versus those having access to a *longitudinal* (panel data) structure.

Section 3 and its subsections present methods based on structural models where the subsidy is known in levels; section 4 presents the "matching method" identified as a more "empirical-based" approach (compared to structural models) and particularly suitable when the subsidy policy takes a binary form; section 5 comes back to a structural model (the Heckman selection model) where, this time, the subsidy policy is a binary (rather than a continuous) variable; section 6 and its subsections, then, deal with the analysis of subsidy additionality in a longitudinal data setting (applicable both in case of binary and level subsidy), while section 6.2 will treat, more in depth, the important case of "dynamic subsidization"; finally, in section 7, some concluding remarks follow.

1. A THEORETICAL FRAMEWORK TO IDENTIFY THE EFFECTS OF PUBLIC SUBSIDIES ON BUSINESS R&D

The "measurement without theory" long-standing controversy of the econometric discipline seems to have found in the study of the effects of public subsidies on firm R&D expenditure an unexpected revival. The most of the works in this field, in fact, seems to have embraced the only purpose of measuring the presence or absence of "additionality" of public incentives by skipping, at least implicitly, the essential step of going into an explicit theoretical framework explaining this causal relation.

David *et al*. (2000) and David and Hall (2000) denounced this attitude of the econometric literature and tried to provide more sound theoretical bases for the understanding of the effect of public subsidies on R&D private investment[1].

Their structural model identifies the optimal level of R&D investment as the point in which marginal rate of returns (MCC) and marginal capital costs associated to R&D investments are equal. This is, on the side of firms, a classical profit maximization strategy. The MRR curve derives from sorting R&D projects according to their *internal rate of returns*, as in a usual investment plan. This curve is a decreasing function of R&D expenditures, since firms will first implement projects with higher internal rate of returns and then those presenting lower rates. The MCC curve, instead, reflects *opportunity costs* of investment funds, at any level of R&D. This curve has an upward slope due to the assumption that, as soon as the number of projects to implement increases, firms have to shift from financing them by retained earnings to equity and/or debt funding (i.e., from internal to external and more costly sources)[2].

Obviously, both curves depend on a number of variables other than R&D expenditure that can move them either downward or upward. In fact, according to the David et al. (2000) structural model we can write:

[1]
$$MRR = f(R, \mathbf{X})$$
$$MCC = g(R, \mathbf{Z})$$

where **X** and **Z** are variables that shift accordingly the curves. In particular the **X**-variables contain some proxies of:

1. technological opportunities;
2. state of demand;
3. appropriability conditions.

Variables contained in **Z** depend instead on:

1. technological policy tools;
2. macroeconomic conditions;
3. external costs of funds;
4. venture capital availability.

The technological policy tools depend in turn on tax treatment, public subsidies and public-private cost-sharing research projects activated by governmental procurement[3].

The equilibrium condition, *MRR = MCC*, provides the optimal level of firm R&D investment (that we label R^*). In explicit form, in fact, it becomes:

[2]
$$R^* = h(\mathbf{X}, \mathbf{Z}) .$$

Provided that **X** and **Z** are all exogenous factors, equation [2] is the “reduced form” associated to the structural model [1].

[1] In particular, they distinguish between *contracts* and *grants*, as they are different incentive tools on the side of the government. In what follows, nevertheless, we will focus primarily on grants, even if many conclusions can be also extended to contracts.

[2] Actually David et al. maintain that the MCC curve starts with a flat shape becoming increasing only later after a given threshold; this form of the MCC curve is due to the self-financing effect: firms first use retained earnings (flat part) and only after they run them out, they address to the debt and/or equity markets (increasing part).

[3] The distinction among these forms of subsidization is remarkable. In particular, the analysis of contracts differs substantially from that of grants. According to the works of Lichtenberg (1987) and David and Hall (2000) two main elements contribute to the occurrence of additionality/crowding-out effects in the case of contracts: the first relies on the research inputs price increase due to changes in the labour demand for scientists and engineers activated by the contract (especially when the researchers' total supply is assumed to be fixed and the government is budget-constrained); the second is drawn upon spillover effects generated by contracts especially when they are the bases for future (expected) contracts and/or when they envisage to sell products to the government at the end of the R&D program. Both these causes can bring about additionality as well as crowding-out, even if the first of them (labour market effects) seems more likely to provide ground for crowding-out, while the second (spillover effects) for potential additionality (for a formal model see David and Hall, 2000).

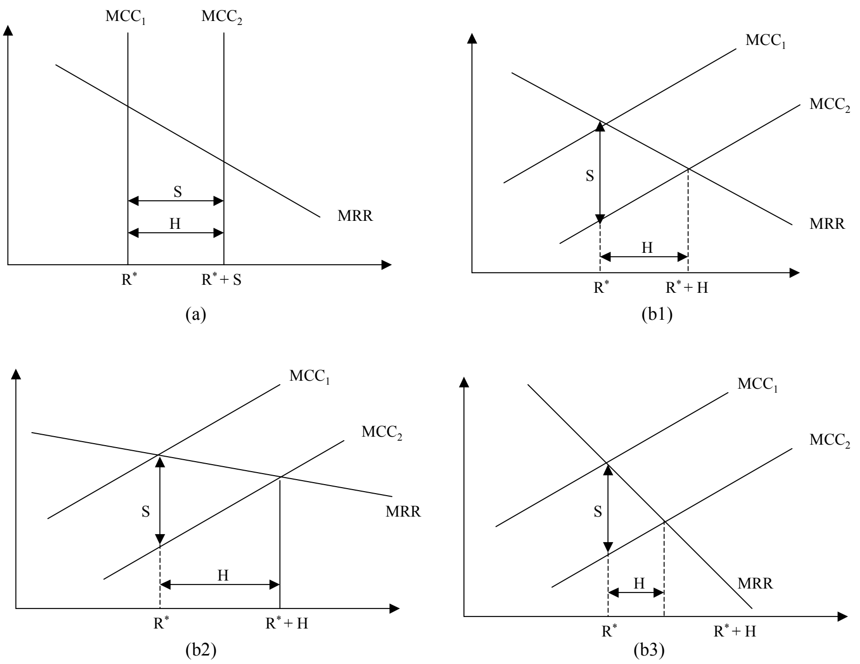

FIGURE 1. SUBSIDY EFFECTIVENESS ON BUSINESS R&D EFFORT ACCORDING TO DIFFERENT SHAPES OF THE MCC AND MRR SCHEDULES (PART I)

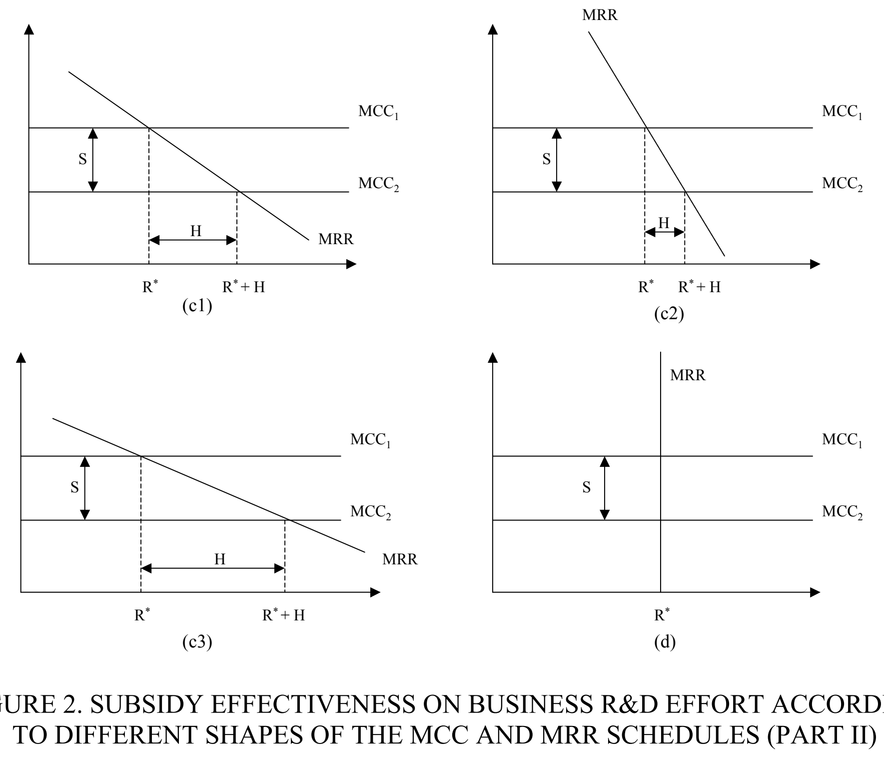

FIGURE 2. SUBSIDY EFFECTIVENESS ON BUSINESS R&D EFFORT ACCORDING TO DIFFERENT SHAPES OF THE MCC AND MRR SCHEDULES (PART II)

According to this framework we can ask for what kind of effect a subsidy would have to the equilibrium level of the R&D expenditure R^*. If we indicate the amount of subsidy with the letter S and with H the incremental R&D expenditure activated by the subsidy S, we can observe that:

[3] $$R = R^* + H \ ,$$

so that we can outline the following five cases:

1. H = S: no additionality, nor crowding-out occurs;
2. H > S: additionality occurs;
3. 0 < H < S: crowding-out takes place;
4. H = 0: full crowding-out occurs;
5. H < 0 < S: more than full crowding-out takes place.

Each of these possibilities can arise according to the following different settings, whose graphical representation is reported in figures 1 an 2.

Setting (a): the firm is asset-constrained so that it operates with a perfectly vertical MCC schedule. In this case, *ceteris paribus*, the MCC schedule moves to the right augmenting the level of R&D outlays exactly of the same amount of the subsidy S (see graph (a) in figure 1). Note that independently of the MRR schedule's shape the level of H coincides with S. This implies no additionality, nor crowding-out (case 1).

Setting (b): the firm faces an upward sloping MCC schedule. In this case the level of R&D expenditure after the subsidy can increase depending on the slope of the downward sloping MCC: in the case (b1) of figure 1 we have that S = H (case1: no additionality, nor crowding-out); in the graph (b2) we get that H > S (case2: additionality); finally, in the graph (b3) we obtain that H < S (case 3: crowding-out).

Setting (c): the firm MCC is infinitely elastic (horizontal). According to the shape of the MRR schedule, again, we can have (following figure 2) no additionality, nor crowding-out (c1), crowding-out (c2) and, finally, additionality (c3).

Setting (d): the firm copes with a vertical MRR schedule (see figure 2, graph (d)). In this case H = 0 < S and full displacement of the public subsidy occurs, independently of the MCC schedule's shape (case 4).

1.1 Dynamic complementarities

So far we have assumed that movements of the MCC schedule induced by subsidy provision are independent of *potential correlated* movements of the MRR. Nevertheless, suppose as example that the subsidies allow the firm to improve its technological opportunities because, for instance, some fixed costs can be now more easily overcome. In this case, even in static absence of additionality such as, say, the case (b3) in figure 1, the firm would be likely to reach an higher than S additional R&D expenditure. The graph (e) in figure 3 shows this occurrence: after the subsidy injection the new equilibrium is in $R^* + H_1$ as in graph (b3) where $H_1 < S$ (we are moving along the MRR_1 schedule); the subsidy, however, could produce new technological opportunities that increase the number of R&D projects that become more profitable: in that case, also the MCC schedule will move to the right allowing to reach a new (long-run) equilibrium $R^* + H_2$ where $H_2 > S$ and where, therefore, there is additionality.

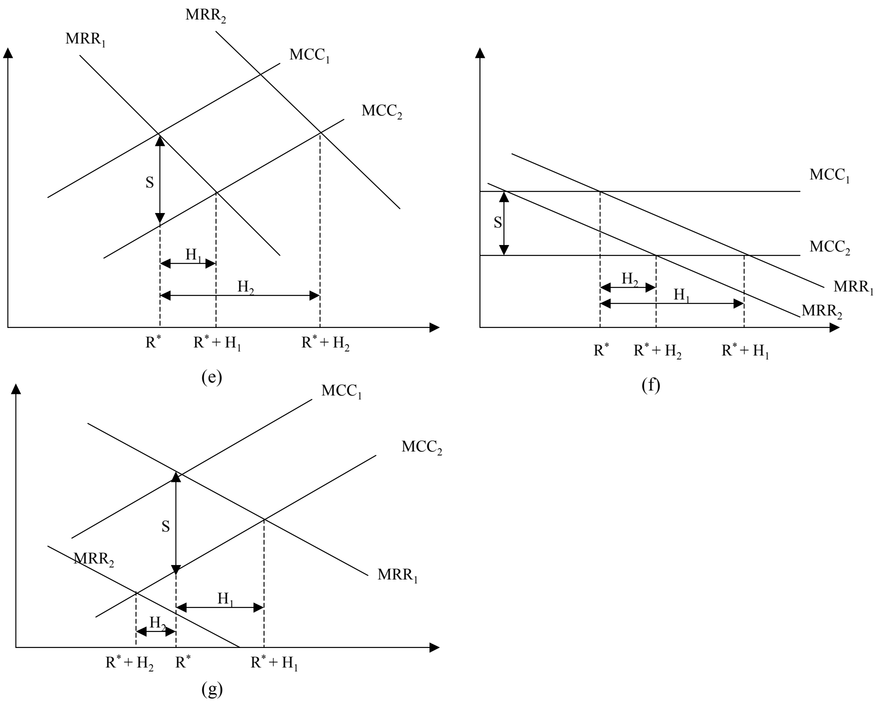

FIGURE 3. DYNAMIC COMPLEMENTARITIES BETWEEN THE MRR AND MCC SCHEDULES ACTIVATED BY THE PUBLIC SUBSIDY

From a theoretical point of view also the opposite case would be possible. Suppose to be in the case (c3) where additionality is fully reached (H > S). If we assume that the subsidy, for some unexpected reason, reduces the state of demand[4] (moving the MRR schedule to the left) the new equilibrium could be characterized by an H < S, determining in so doing a crowding-out result (see, the graph (f) where, at the end, $H_2 < S$).

The last, didactic case, could be the possibility the subsidy generates a "more than full crowding-out". In this case the correlated leftward movement of the MRR schedule would be so strong to generate a situation in which H < 0 < S (case 5). Graph (g) shows this potential, even if little realistic, event.

In conclusion: many different situations can arise where different subsidy effects can be produced. Crowding-out, as well as additionality are both consistent with the framework represented above. Clearly a reduced form such as equation [2], can only put in evidence the "net effect" of the various MCC and MRR movements without ascertain which of them has been changing. Many econometric works start by adopting equation [2], without specifying the structural model laying below it[5].

[4] This occurrence is not so unlikely as it might seem. Indeed, suppose to have two complementary technologies: A and B whose combination is needed in order to get a given finished product. Suppose, then, that the government is budget-constrained and that, for some myopic strategy, is deciding to fund only research on technology A by excluding technology B from grants. In this case, firms producing technology A could have very pessimistic foresights about the future level of demand for the finished product, since they can expect the quality of the good to be too low for future customers' tastes. Beyond some extent, this event can discourage those firms to engage in programmed R&D efforts producing some (previously unexpected) crowding-out effects.

[5] As we will clarify later, this is the approach followed by scholars using "matching methods" to R&D policy evaluation.

2. THE RATIONALE FOR R&D SUBSIDIZATION

What is the rationale for R&D subsidization? Neoclassical theory based on a positive externality argument suggests that, because of the public good characteristics of the R&D activity, the level of private R&D expenditure would be systematically lower than the socially optimal level (Arrow, 1962). This occurs since the benefits associated to R&D activities are easily and freely available to subjects that are not engaged in R&D efforts[6]. Indeed, the lack of full appropriability of R&D outcomes reduces the incentive to do R&D on the side of private for-profit firms so that, as in a classical Pigouvian context, a government intervention through subsidization can reduce the extent of this "market failure".

This argument has been widely criticized by several scholars. From an evolutionary perspective, for example, Cohen and Levinthal (1989) have argued that knowledge cannot be so easily absorbed unless imitative firms invest in their turn on a certain level of R&D effort: imitation is not costless and needs for some preexisting R&D activity's "hard core"[7]. This standpoint could convey a paradoxical consequence: in an environment characterized by a great amount of spillover effects firms could have greater incentives to perform R&D since, in so doing, they might enlarge their absorptive capacity, i.e., their ability to benefit from others' R&D efforts. In this way, they could more easily imitate and exploit market surpluses. Paradoxically and as a consequence, the level of R&D could be too high (rather than too low), since many firms could undertake too much R&D effort than that required to reach the same social results (for example, by an increase of duplications in R&D expenditures).

Other scholars, on the contrary, have suggested that R&D should not be taken as a pure public good: a firm has a great amount of tools to protect its inventive capacity, such as patents, secrecy, and so on (Nadiri, 1993).

The need for subsidization, other than that due to positive externalities, can be invoked since other market failures can be at work such as: 1. imperfect markets of capital, 2. missing markets for high-risk investments (such as undersized venture capital markets), 3. too high barriers to entry and exit, 4. excessive market power or, on the contrary, excessive fragmentation of market power (depending on what Schumpeter argument is invoked: Mark I opposed to Mark II), 4. lack of technological infrastructures and bridging institutions, 5. coordination failure of profitable R&D joint venture, producing duplications in R&D efforts and other resource wastes, and so on (see, for a general discussion, Martin and Scott, 2000).

In the first case, the failure can arise because R&D investment could be too risky and asymmetric information between lenders and borrowers too high, generating in that way high funds' rationing; in the third case, instead, imperfect competition due to barriers such as too high fixed costs to enter the market and/or too high costs to get out (sharp "sunk costs"), can produce a sub-optimal level of R&D expenditure; in the third case, the market structure and firms' size determine the industrial R&D performance according to the complex system of incentives this market structure induces also at different sectoral level[8]; the fourth and five cause, finally, could depend on scarce material and immaterial knowledge infrastructures and on various "traps" in the functioning of the national system of innovation (Mowery, 1995; Metcalfe, 1995; Malerba, 1993).

Coming back to spillovers, one important aspect that should be taken into account is what type of effect a subsidy can generate in their presence. As suggested by Klette, Møen and Griliches (2000), in fact, a subsidy can in its turn generate additional spillover effects, so that non-subsidized firms can profit from the R&D effort undertaken by subsidized firms. This fact generates another paradoxical conclusion: one uses a subsidy as a tool to internalize positive externality and correct market failure, while the same subsidy could generate additional spillovers by causing incremental market failure. This is something similar to the "dynamic complementarities" we saw above (since not only

[6] Through imitation mechanisms such as, for example, the "reverse engineering".

[7] This originates from a conception of the firm as a "competence-based" structure.

[8] Martin and Scott (2000) suggest that policy intervention to promote R&D activity should be targeted and sector-specific rather than widespread and generic; they make use of the Pavitt (1984) taxonomy to identify: 1. main sectoral mode of innovation, 2. sources of sectoral innovation failure, and 3. suitable policy instruments.

"direct", but also "indirect" effects are at work). Does it make subsidies completely useless? As I will argue in the next section, under relevant spillovers, a subsidy could be ineffective statically, but effective and useful dynamically.

2.1 The effect of R&D grants in presence of spillovers

As any other positive externality the presence of spillovers, as we argued before, brings about *static inefficiency*. Nevertheless, they generate *dynamic efficiency* in that they afford to reach Pareto-superior allocations than in the case of their lack. This phenomenon resembles quite faithfully the case of the passage from a competitive towards a monopolistic market structure, when monopoly produces significant cost reductions (*scale economies*). Figure 4 shows this aspect.

For a better understanding of this figure, suppose that R = Q (i.e., one hour of R&D is held equal to one unit of product)[9]. We start, just to fix ideas, from the case in which there aren't spillovers (i.e., no positive externalities exist). In this case, all firms do the level of R&D they desire (with perfect revelation of their preferences), and the equilibrium is on the point S_0 where marginal social benefits and marginal social costs of doing R&D are equal (and the same thing happens on the side of commodities).

This is a classical demand-supply equilibrium where we have posed that the cost, under absence of spillovers, are constant and equal to C_0 (marginal and average costs are the same). The optimal level of R&D under these conditions is R_0, that is the social optimum competitive equilibrium (i.e., the Pareto allocation).

If now we allow for spillover effects, we are under ordinary positive externalities, since some firms can profit from the knowledge freely available in the industry. According to the standard positive externality results, we are tempted to say that the new equilibrium is now in M_0, where only *some* firms reveal their preferences. This equilibrium falls on the crossing between the curve D_1, i.e., the marginal private benefits, and the same marginal costs curve C_0. The point M_0 is clearly sub-optimal since society has to bear a price of C_0 per unit hour of R&D, obtaining only R_1 hours of R&D when it would be possible, under this technology and preferences, to reach S_0 which presents the same cost but a higher level of R.

But the question at stake is another one. Indeed, under pervasive spillover effects, the actual equilibrium is no longer in M_0, as one could expect, but rather in M_1, whose cost (and level) of R&D is not only lower (and higher) if compared with the point M_0, but also lower (and higher) than in the (previous) social optimum S_0. In fact, the presence of spillovers reduces the cost of production of the free-riding firms, that aggregately lowers the "industrial" marginal social costs from C_0 to C_1. It follows that M_1 rather than M_0 is the new (actual) equilibrium.

Note, however, that M_0 is not the socially optimal equilibrium, given the *new* status of the technology (C_1), but it is again a sub-optimal allocation since now society could, at least potentially, reach the level S_1 where, at the same cost, it is possible to get an higher level of R&D (namely, R_3).

In conclusion: spillover effects surely generate *static inefficiency*, whereas they potentially can produce high *dynamic efficiency* in the provision of R&D. This is in the same spirit of the basic conclusion of the theory of endogenous growth driven by R&D spillovers (Romer, 1986; 1990).

What happens when the government wants to correct for this failure? Assuming that the government knows where the economy is placed, providing a subsidy can move the curve D_1 towards the curve D_0 until they coincide. The new equilibrium is now S_1, the social optimum associated to the social costs C_1. Nevertheless, if we allow the subsidy to generate additional spillovers the situation will change as explained in the second graph of figure 4.

[9] Throughout this exposition we overlap the market of R&D with the market of goods. This choice has only an explicative-didactic purpose. In particular, we are assuming that the presence of externality reduces the cost of producing R&D and goods to the same extent.

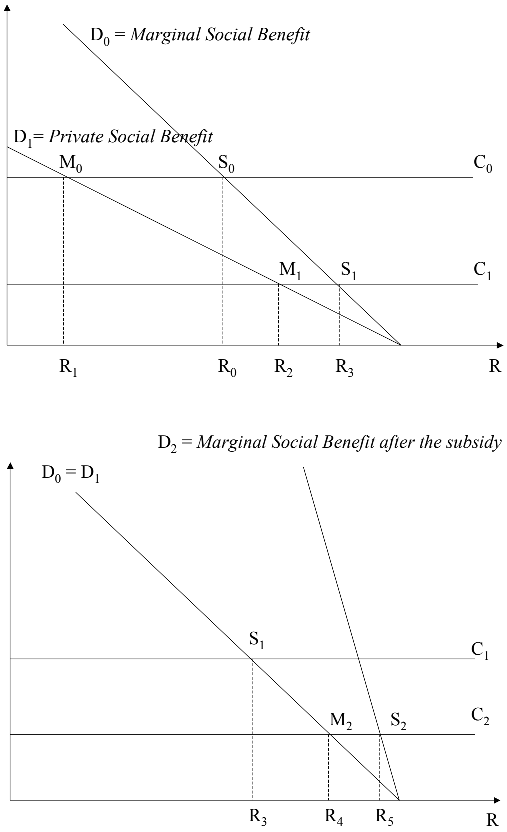

FIGURE 4. THE EFFECT OF PUBLIC GRANTS ON THE OPTIMAL ALLOCATION OF BUSINESS R&D IN PRESENCE OF SPILLOVERS

Here, the new equilibrium (the long-run equilibrium) is in M_2 which is, in turn, a sub-optimal point since subsidy spillovers have in the meantime created a new curve of marginal social benefits (D_2) determining the social optimum in the point S_2 where the cost is C_2. In other words, any attempt to correct market failure by injecting subsidies could fail in presence of significant spillover effects since dynamic structural change (cost reductions) activated by the subsidy through the spillovers modify the expected allocation. The subsidy, nevertheless, will produce a substantial technical change and production growth (as long as directed towards sectors with higher spillovers).

According to the analysis of Klette, Møen and Griliches (2000, p. 483), a full cost-benefit analysis of an R&D support would take into account the estimation of the following expression:

[4] $$w(s) = \sum_{i \in Sub} \Delta\pi_i(s_i; \underline{s}) + \sum_{j \in NSub} \Delta\pi_j(s) + \sum_{k \in Rest} \Delta\pi_k(s) + \sum_{l \in C} \Delta(CS_l) - d(s)$$

where *Sub* is the set of subsidized firms, *NSub* that of non-subsidized, *Rest* that of the rest of the economy, *CS* the consumer surplus and *d(s)* the loss associated to the subsidy. The first term on the right-hand-side of the [4] represents the change in profits of subsidized firms, that depends on the subsidy received (i.e., the direct effect of s_i) and the effect of the change in profits of the other supported firms (i.e., the indirect effect labeled $\underline{s}$); the second term is the change in profits of non-subsidized firms (belonging to the same industry) that are a mixture of rent and knowledge spillovers; the third is the pure rent spillovers pouring into the firm profits of the rest of the economy; the fourth, finally, is the change in consumer surplus.

Whether these effects determine welfare gains or welfare losses is an open question. It critically depends on the relationships among the different subjects involved and, as we tried to explain before, on the firm reaction to subsidies. Therefore, the suitability of a subsidy should be judged according to the potential welfare changes defined by the [4]. The previous example in figure 4 seems to suggest that a subsidy is always suitable; nevertheless, by combining these graphs with the previous results on dynamic complementarities (figure 3), we can conclude that a non-ambiguous answer to the suitability of supports to firm R&D activity appears to be quite hard, if not impossible, to be found.

2.2 Econometric techniques: a taxonomy

According to the previous analysis it should not be surprising that the econometric efforts trying to measure the effects of R&D subsidies on private R&D expenditure have had to cope with a really complex system of interrelated "direct" and "indirect" effects.

Two main philosophies have been followed to address this complexity: the first and more extensively adopted approach, came up especially in the latest years, seems to prefer a more empirical-based point of view, where not a great deal of theoretical speculations have been brought into models except for those specific factors accounting for the selection criteria of supporting programs; examples of this kind are econometric exercises such as those based on the *control function* and *matching* estimators; the second stream of research, on the contrary, have tried to make more explicit the theoretical background laying behind the data, by building proper "structural models" in which causal relations are more explicitly and clearly identified.

Sometimes the boundary between these two viewpoints is less pervasive and sharp than it can appear at a first glance. Nevertheless, for the sake of clearness, we provide a possible *taxonomy* of R&D evaluation models by distinguishing among three analytical dimensions:

1. *type of specification*: distinguishing between models adopting a *structural-analytical* approach, where the outcome equation and the selection-into-program equation are separately modelled in a system of simultaneous equations, and *non-structural* models where only the outcome equation (the so-called "reduced form") is estimated, once controlling for some specific covariates[10];
2. *type of data used*: models based on a *cross-section* dataset and models exploiting a l*ongitudinal* one (in so allowing also for dynamic and long-run analysis);
3. *type of policy variable*: models using a *binary* policy variable (generally in the form of "subsidized" versus "non-subsidized" units), and models using the policy variable in *levels* (i.e., in a continuous form).

Table 1shows some representative studies we met in the literature according to this classification.

[10] As it will be clearer later, this distinction between structural and non-structural (or reduced-form) models couples with that between model taking into account endogeneity due to both "selection on observables" and "selection on unobservables" (the structural models), and those dealing with endogeneity due only to "selection on observables" (the non-structural or reduced-form models).

TABLE 1. R&D POLICY EVALUATION STUDIES ACCORDING TO THE TYPE OF SPECIFICATION, DATASET AND POLICY VARIABLE. CF-OLS: OLS ESTIMATION BASED ON A CONTROL FUNCTION, MATCHING: MATCHING MODELS, SELECTION: HECKMAN SELECTION MODEL, DID: DIFFERENCE-IN DIFFERENCES; IV: INSTRUMENTAL VARIABLES (2SLS OR 3SLS) ESTIMATION

METHOD	TYPE OF SPECIFICATION		TYPE OF DATASET		TYPE OF POLICY VARIABLE		REPRESENTATIVE STUDIES
	Structural	Reduced-form	Cross-section	Longitudinal	Binary	Level	
CF-OLS		X	X			X	Lichtenberg (1987)
MATCHING		X	X		X		Almus and Czarnitzki (2003)
SELECTION	X		X		X		Busom (2000)
DID		X		X	X		Lach (2000)
IV	X		X			X	Wallsten (2000)

The majority of works uses *cross-section* datasets while few studies make use of *longitudinal data*. Nevertheless longitudinal data, as it will be clearer later, allow also for dynamic (and long-run) treatment analysis, an aspect neglected by cross-section studies; also the distinction between works using subsidy *in levels* and those using subsidies in a *binary* form (supported vs. non-supported units) seems important: many econometric techniques, derived essentially from the microeconometrics of labor market evaluation, have been developed in setting where the policy factor (subsidies) is a binary variable; nevertheless, when possible, using levels is more informative than using a binary variable, since it allows not only to estimate the presence or absence of additionality, but also the strength of this effect in term of derivative.

In what follows we start by presenting an overview of the structural models employed in the literature, their econometric estimation and potential improvements; then we will deal with techniques based on a reduce-form analysis paying particular attention, at the end of that part, on the longitudinal data and dynamic treatment setting.

3. STRUCTURAL MODELS WITH SUBSIDY IN LEVEL

They are the first generation of models trying to measure the effect of public subsidy on business R&D. In more recent years, nevertheless, more sophisticated structural models have been proposed; we review them starting from a very simplified model, going on presenting more sophisticated approaches in next sections.

3.1 A (basic) model with exogenous subsidy

To begin with, we consider a simple model drawn from Lichtenberg (1987) since it seems very useful and instructive to derive more complex (structural and non-structural) models; this model recalls the analytical model [1] and takes the following form:

[5]
$$
\begin{aligned}
MCC &= a_0 + a_1 PRD + a_2 SUB + \varepsilon_1 \\
MRR &= b_0 + b_1 PRD + b_2 SALES + \varepsilon_2 \\
MCC &= MRR = M
\end{aligned}
$$

where *PRD* is the private R&D expenditure, *SUB* the subsidy received and ε_1, ε_2 are uncorrelated i.i.d. error terms. Lichtenberg assumes that all the right-hand-side variables of this model are *strictly exogenous*, so that the equilibrium condition (*MCC* = *MRR* = *M*) leads to the following "reduced form" for *PRD* (there exists, of course, also a reduced form for *M*):

[6]
$$PRD = \beta_0 + \beta_1 SUB + \beta_2 SALES + u$$

that can be easily consistently estimated by ordinary least squares (OLS) or generalized least squares (GLS) in case of heteroskedasticity and/or autocorrelation.

Equation [6] can also be seen as a "control function regression": once controlled for sales and other additional variables (such as, for example, a sectoral dummy) we can assume that the covariance between *SUB* and *u* is zero, so that *SUB* becomes exogenous; this is a very simplified application of the so-called conditional independence assumption (Rubin, 1977): we restore exogeneity once conditioning on suitable covariates; as we will see more in-depth later, this assumption is at the basis of both "control function" and "matching" methods; nevertheless, models based on the previous assumption works well only when selection into subsidization is due to observable-to-analyst individual characteristics (such as "sales" and "sector" in the previous example); but when also unobservable-to-analyst characteristics affect the selection-into-program mechanism, these methods fail to consistently estimate the parameter β_1; the central question, at the end, is "how to deal with subsidy endogeneity" and the next sections provide some definitions and possible solutions.

3.2 The issue of subsidy's endogeneity

The previous model is quite a naïve one, since it assumes that the policy variable, *SUB*, is strictly exogenous. This assumption, nevertheless, seems to be too strong in this context for at least three reasons: simultaneity, omission of variables and measurement errors.

Simultaneity

It is likely that *PRD* and *SUB* are contemporaneously codetermined. This is due to the fact that the funding choice operated by the government is not independent of the level of firm *PRD*. For example, if a "picking-the winner" strategy is at work, firms with higher R&D activity are more likely to receive supports from government than weaker R&D performing firms. In this case, observing an high significant and positive level of β_1 in an OLS regression of equation [6] could be seriously misleading since part of this high partial regression effect of *SUB* on *PRD* could be due to the specific strategy operated by the government, rather than to the "direct" causal effect of *SUB* on *PRD*. To better appreciate this point, suppose to derive equation [6] by *SUB*:

[7]
$$\frac{\partial PRD}{\partial SUB} = \beta_1 + \frac{\partial u}{\partial SUB},$$

the OLS estimation of the [6] is exactly the [7] and it takes into account both the "direct effect" of *SUB* on *PRD* (β_1) and the "indirect effect" of *SUB* on *PRD* ($\partial u / \partial SUB$); the latter is that component of the causal relation between *PRD* and *SUB* passing through the link between *SUB* and *u*: what equation [7] points out is that the level of *SUB* is correlated to those unobservable factors determining the level of *PRD*[11].

[11] When, in case of endogeneity, the OLS estimator takes into account "direct" as well as "indirect effects" of a

Suppose that the government knows these factors (for example, the intrinsic quality of the proposed R&D projects and the firm economic soundness) while the econometrician (the external observer) doesn't, and suppose that the "only" factors affecting the endogeneity of *SUB* is the government funding strategy, then we can distinguish among three different cases:

1. $\partial u / \partial SUB > 0$, that implies that SUB is positively correlated with *u* (firm/project quality), so that a picking-the-winner strategy occurs and the unobserved (to econometrician) government strategy (hidden in the error *u*) brings about an *upward bias* of the OLS estimator.

2. $\partial u / \partial SUB < 0$, that implies that SUB is negatively correlated with those unobserved (to econometrician) factors increasing R&D performance and an "aiding-the-poor" strategy occurs (the government tends to finance weaker R&D performing firms) conveying a *downward bias* of the OLS estimator.

3. $\partial u / \partial SUB = 0$, that implies no bias (the government funding scheme can be taken as random), since no correlation exists between *SUB* and *PRD*. The OLS estimation is, in this case, fully consistent.

Omission of variables

The problem arising in the previous case may be included within the more general case of "omission of relevant variables". In fact, if the analyst were able to control for the variables used by the government to select potential receivers of funds, unless other unobservable variables were at work, an extended OLS regression such as the [6] augmented for those variables, would consistently estimate β_1 . As we will see later on, this principle, known as "selection on observable", is indeed at the basis of both OLS and matching estimators. Of course, if part of the government selection strategy remains unobserved, these estimators could continue to provide biased results: for example, many evaluation works do not have information about the quality of the proposed R&D projects, while they have substantial information on the economic soundness of the firms. This aspect could produce problem in the augmented-OLS (the "control function") as well as in "matching" estimators.

Error in measuring variables

Another common problem determining endogeneity of the variable *SUB* could be errors in its measurement. Even if less important than in other fields of econometrics and statistics, also in our case these errors can produce substantial biases. We do not enter too much into this aspect since biases from errors in variables can be recovered by the same solution provided for the "simultaneity bias" (i.e., instrumental variables).

3.3 A structural model with subsidy endogeneity

When in equation [6] the policy variable *SUB* is supposed to be endogenous (for the reasons explained above), then it is no longer a reduced form; equation [6] is, instead, a single part of a "larger structural model" that needs to be uncovered.

Lichtenberg (1988), recognizes the need to take the endogeneity problem seriously. His approach starts from keeping equation [6] by considering now the variable *SUB* as endogenous. In order to obtain a consistent estimation of β_1 he proposes a two-stage least squares (2SLS) estimation, i.e., an instrumental variables estimation where he instruments *SUB* with the "value of competitive contracts that were *potentially* awardable" to each firm (and that we label *W*); he supposes this variable to be correlated with *SUB*, but uncorrelated with *u*.

This assumption, nevertheless, assumes a theoretical (although implicit) standpoint that can be

certain variable on another, the sum of these two effect is called "pseudo-true value" (see, Cameron and Trivedi, 2005, p. 94).

assessed by making explicit its underlying structural model shaped according to the following system of two equations:

[8] $$\begin{cases} PRD = \beta_0 + \beta_1 SUB + \beta_2 SALES + u & \text{(structural equation for } PRD\text{)} \\ SUB = \delta_0 + \delta_1 SALES + \delta_2 W + \varepsilon & \text{(reduced form for } SUB\text{)} \end{cases}$$

where u and ε are correlated (what makes *SUB* endogenous in the *PRD* structural equation). The meaning of this structural equation is that unobservable (to analyst) factors affecting *SUB* (that is, ε), affect contemporaneously the unobservable variables affecting *PRD* (that is, u), so that u and *SUB* are correlated (and *SUB* is endogenous). How can we estimate consistently the structural parameter β_1?

By substitution of the reduced form of *SUB* into the structural equation of *PRD*, we obtain the two reduced form for the two endogenous variable of the model:

[9] $$\begin{cases} PRD = \pi_0 + \pi_1 SALES + \pi_2 W + v & \text{(reduced form for } PRD\text{)} \\ SUB = \delta_0 + \delta_1 SALES + \delta_2 W + \varepsilon & \text{(reduced form for } SUB\text{)} \end{cases}$$

where $\pi_0 = \beta_0 + \beta_1\delta_0$, $\pi_1 = \beta_2 + \beta_1\delta_1$, $\pi_2 = \beta_1\delta_2$ and $v = \beta_1\varepsilon + u$. Since both the equations in system [9] are reduced forms we can estimate them by OLS and obtain consistently the structural parameters β_0, β_1, and β_2. This approach is equivalent to the 2SLS estimation, but it has the virtue to put in evidence its structural derivation. Observe, however, that we can estimate this system only because we are in a just-identified setting, i.e., we can derive the structural parameters from the reduced form parameters[12].

One of the problems in systems like [8] and [9] is that the exogeneity of the instrument chosen (*W*) is not testable, since we are in a just-identified setting. Only in an *over-identified* setting we can test the combined exogeneity of the instrument chosen. To obtain an over-identified setting we need more than one instrument for *SUB*, a situation that is of course not so common and easy to get in applications. Observe, finally, that the type of instrument chosen can modify substantially the estimation, so this choice has to be done really carefully (see Greene, 2003, p. 385-400).

3.3.1 Estimation improvement by 3SLS

Compared to Lichtenberg (1988), Wallsten (2000) proposed an efficiency improvement of the 2SLS estimation of system [8], by introducing also a third equation and estimating the new system by three-stage least squares (3SLS). The improvement of 3SLS compared to 2SLS comes from considering, as additional sample information, the correlation between u and ε. Consider the reduced form system [9] (we overlook, for simplicity, the Wallsten's third equation). It is easy to see that it is equivalent to a "seemingly unrelated regression" (SUR) model of the type:

[10] $$\begin{cases} y_1 = x'\beta_1 + u_1 \\ y_2 = x'\beta_2 + u_2 \end{cases}$$

where the variables of the system [9] have been renamed for the sake of simplicity. In matrix form system [10] becomes:

[11] $$\begin{bmatrix} y_1 \\ y_2 \end{bmatrix} = \begin{bmatrix} x' & 0 \\ 0 & x' \end{bmatrix} \begin{bmatrix} \beta_1 \\ \beta_2 \end{bmatrix} + \begin{bmatrix} u_1 \\ u_2 \end{bmatrix}$$

[12] Indeed, the model is *just-identified* because we have *six* reduced form parameters (π_0, π_1, π_2, δ_0, δ_1, δ_2) and six structural parameters (β_0, β_1, β_2, δ_0, δ_1, δ_2).

or, more compactly:

[12] $$\mathbf{Y} = \mathbf{XB} + \mathbf{U}$$

If we define $E(\mathbf{UU}') = \mathbf{\Omega}$, we have that the 3SLS is equivalent to the following SUR-generalized lest squares (SUR-GLS) estimation:

[13] $$\hat{\mathbf{B}} = [\mathbf{X}'(\mathbf{I_N} \otimes \mathbf{\Omega^{-1}})\mathbf{X}][\mathbf{X}'(\mathbf{I_N} \otimes \mathbf{\Omega^{-1}})\mathbf{X}] .$$

A consistent estimation of $\mathbf{\Omega}$ is:

[14] $$\hat{\mathbf{\Omega}} = \hat{\mathbf{U}}\hat{\mathbf{U}}' / N$$

obtained by the residuals of the OLS of the two single equations in [10][13], so that a consistent feasible-GLS estimation of **B** can be obtained by substituting [14] into [13]. Observe that the 2SLS estimation of the previous section is obtained by [13] plugging-in $\mathbf{\Omega} = \mathbf{I}$; in other words, 2SLS do not take into account the information carried by the correlation between the error terms of the two equations, whereas 3SLS does it improving estimation efficiency.

Observe, finally, that the model of Wallsten assumes a just-identified setting as Lichtenberg does, so the exogeneity of the instrument used (that is also the same used firstly by Lichtenberg) for *SUB* remains not testable.

3.4 A model with barriers to innovation

Gonzalez, Jaumandreu and Pazo (2005) recently proposed a more sophisticated structural model tacking into account also the presence of barriers to the R&D activity. Their work takes into explicit consideration the presence of "fixed cost" on the side of R&D performing firms.

They model the firm R&D choice as a maximizing problem where the firm net revenue from R&D activity is $Y(R)$ where R is the level of R&D expenditure. They assumes Y to be an increasing function of R ($\partial Y / \partial R > 0$) but facing decreasing return ($\partial^2 Y / \partial^2 R < 0$). The profit function in presence of the subsidy is:

[15] $$\Pi = Y(R) - R + S = Y(R) - R + sR = Y(R) - (1-s)R$$

where the maximization condition gives:

[16] $$\frac{\partial \Pi}{\partial R} = 0 \quad \Leftrightarrow \quad \frac{\partial Y}{\partial R} = (1-s) .$$

Since for each assigned level of profits the isoprofit line is:

[17] $$Y(R) = (1-s)R + \Pi$$

the optimal level of R&D can be find graphically in the point where this line is tangent to the net revenue function *Y(R)*.

According to equation [16], for any given level of *s*, firms determine the optimal level of R&D expenditure $R^*(s)$. Nevertheless, there exists a level of *R* that makes the firm indifferent between doing or not doing R&D activities and that continues to be optimal to implement for the firm. This *threshold* level (that we indicate with $\overline{R}$) satisfies the following two requirements:

[13] Indeed, since *x* are variables supposed to be strictly exogenous, separate OLS estimation of the two equations of [10] produces consistent estimation of β_1 and β_2, and therefore consistent estimations of u_1 and u_2.

$$\begin{cases} 1. \quad \Pi(R) = \Pi(0) \\ 2. \quad \partial\Pi / \partial R = 0 \end{cases}$$

i.e., it maximizes the profits (requirement 2) and it provides a level of profits equal to a null level of R&D expenditure (requirement 1). Figure 5 clarifies this condition by a graphical representation of the model. As it is immediate to see, if the firm finds optimal to perform R_1 units of R&D, then the level of profits reached will be Π_1 that is lower than the level of profits achievable by a null level of R&D activity (i.e., $\overline{\Pi}$); in this case the firm will find optimal *do not* perform any R&D effort, since firm will reach a level of profits equal to $\overline{\Pi}$ (with $\overline{\Pi} > \Pi_1$). Until the firm optimal level of R&D corresponds to a profit lower than $\overline{\Pi}$ the firm will prefer not performing any R&D activity. When the firm finds optimal to produce exactly $\overline{R}$ units of R&D expenditure it will be indifferent between performing $\overline{R}$ or zero. On the contrary, when the firm finds optimal to perform an $R^* > \overline{R}$ we will have that $\Pi^* > \overline{\Pi}$ and the firm will have an incentive to produce exactly R^*.

In conclusion: when the firm finds optimal to perform a level of $R < \overline{R}$, then it will prefer to produce a null level of R&D; on the contrary, when it finds optimal to perform a level of $R > \overline{R}$, it has an incentive to produce exactly an R^* amount of R&D effort.

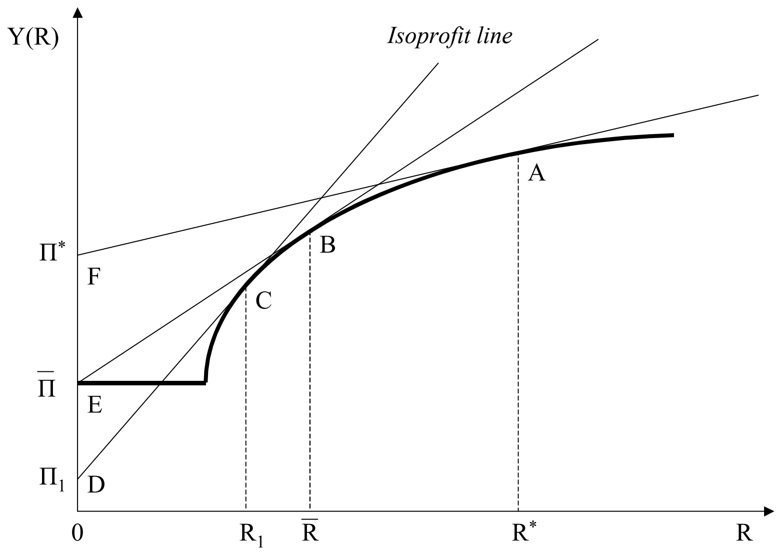

FIGURE 5. DETERMINATION OF THE OPTIMAL R&D EFFORT IN PRESENCE OF *FIXED COSTS*

The next step of the González *et al*. (2005) model is to provide an econometric counterpart of this theoretical model.

To begin with, they consider R^* and $\overline{R}$ by specifying a function for both these variables; hence, the following system has to be estimated:

[18] $$\begin{cases} R^* = x'\beta + \gamma s + u_1 \\ \overline{R} = z'\lambda + x'\omega + \theta s + u_2 \end{cases}$$

where x are variables other than the subsidy affecting both the total firm R&D expenditure and the threshold, s is share of the subsidy S on total R&D outlays, z are variables affecting only the barriers to R&D activities encountered by the firm and (u_1, u_2) are correlated error terms.

A fundamental characteristic of this kind of models is that R^* is observable only when $R^* > \overline{R}$; furthermore, $\overline{R}$ is in its turn unobserved. From system [18] we can write:

[19] $$R^* - \overline{R} = x'\rho - z'\lambda + s\tau + v$$

where $v = (u_1 - u_2)$, $\rho = (\beta - \varsigma)$, $\tau = (\gamma - \theta)$. By posing into [19] $x'\rho - z'\lambda + s\tau = w'\delta$ we obtain the following system:

[20] $$\begin{cases} R^* = x'\beta + \gamma s + u_1 \\ R^* - \overline{R} = w'\delta + v. \end{cases}$$

The variable $R^* - \overline{R}$, a we said, is not observable; we only know that R^* is observable when $R^* - \overline{R} > 0$; according to this setting system [20] becomes:

[21] $$\begin{cases} R^* = x'\beta + \gamma s + u_1 \\ y = 1[R^* - \overline{R} > 0] = 1[w'\delta + v > 0] \end{cases}$$

where we assume that: (a) w is always observed while R^* is observed only if $y = 1$; (b) (u_1, v) are independent of w and have zero mean; (c) $v \sim normal(0;1)$; (d) E($u_1|v$) = φv (i.e., u_1 and v are correlated).

According to Amemiya (1985) classification, such a model is called a "type II Tobit model" and can be consistently estimated by a two-step Heckman (1979) procedure. In fact, a simple OLS of R^* on x and s would be inconsistent. To appreciate this point, consider the expectation of R^* conditioned on all the variables (observable and unobservable); we have[14]:

[22] $$E(R^* \mid x, w, s, v) = x'\beta + \gamma s + E(u_1 \mid v) = [x'\beta + \gamma s] + \varphi v$$

since, as stated before, (u_1, v) are independent of w. If $\varphi = 0$, then no sample selection appears and OLS of R^* on x, s would be consistent. When, on the contrary, $\varphi \neq 0$, then OLS becomes inconsistent; in fact, by applying the law of iterated expectations to equation [21], we have:

[23] $$E(R^* \mid w, y) = [x'\beta + \gamma s] + \varphi E(v \mid w, y) = [x'\beta + \gamma s] + \varphi h(w, y).$$

If we knew $h(w, y)$, we could obtain consistent estimation of β and γ by an OLS of R^* on x, s and $h(w, y)$; furthermore, since R^* is observable only when $y = 1$, we only need $h(w,1)$; it can be proved that:

[24] $$h(w,1) = E(v \mid v > -w'\delta) = \psi(w'\delta)$$

[14] This part draws on Wooldridge (2002, pp. 560-566).

where $\psi(\cdot) = \phi(\cdot)/\Phi(\cdot)$ is the inverse Mills ratio. Therefore, we can substitute [24] into [23] getting:

$$[25] \qquad E(R^* \mid w, y = 1) = [x'\beta + \gamma s] + \varphi \cdot \psi(w'\delta) .$$

Now, a consistent estimation of δ, β and γ can be obtained by an OLS regression of R^* on x, s and $\psi(w'\delta)$, provided that a consistent estimation of $\psi(\cdot)$ is previously available. Heckman (1979) provided the following two-step estimation procedure of equation [24]:

Step 1. Obtain a consistent estimate of δ by estimating the following probit model:

$$\Pr(y = 1 \mid w) = \Phi(w'\delta)$$

using all N observations.

Step 2. Obtain an estimate of β and γ from an OLS regression of :

$$R^* \text{ on } x, s \text{ and } \hat{\psi} ,$$

using the selected sample (i.e., the group identified by $y = 1$)[15]. Finally, a t-test can be used to check the hypothesis $\varphi = 0$, i.e., the absence of selection bias, once standard errors corrected for generated regressors are used[16].

Under the additional hypothesis of joint normality of u_1 and v, also a partial maximun liklihood estimation (partial-MLE) can be implemented, since the model becomes fully parameteric. Under these circumstances, partial-MLE is more efficient of the two-step procedure, even if it could present substantial problem of convergence[17].

3.4.1 Measuring the effects of subsidy by *profitability gaps*

The forgone model allows us for estimating consistently the sign, magnitude and significance of the subsidy parameter γ. According to the significance of this parameter we can conclude about the occurrence or lack of additionality, as in the previous structural model.

Nevertheless a threshold model, can give us also additional insights on firms' behavior when compared with more standard structural models; from the previous equations, indeed, we can compute:

1. the individual optimal *non-zero* level of effort (R^*)
2. the threshold estimates ($\hat{\bar{R}}$).

According to this two values we can define the "profitability gap", that is, the difference between the optimal non zero R&D effort in absence of subsidy and the estimated threshold effort:

$$\text{Profitability gap} = (R^* - S) - \hat{\bar{R}} .$$

[15] Observe that, in this model, identification condition requires that $num(x) \leq num(z) + 1$.

[16] Since $\hat{\psi}$ is a generated regressor, usual standard errors are incorrect. However a new formula from generated regressors analysis can be drawn (see: Wooldridge, 2002, pp. 115-117).

[17] Gonzalez et al. use partial-MLE for their model.

If the profitability gap is negative, it identifies a firm that, without the subsidy, would engage in a R&D effort lower than the threshold: without the subsidy, hence, such a firm would have not been engaged in any R&D effort. Receiving the subsidy, on the contrary, allows this firm to reach an optimal non-zero R&D effort.

If the probability gap is positive, on the contrary, it identifies the non-zero level of R&D that a certain firm would have performed even in absence of the subsidy; for such a firm, the amount of the subsidy only adds to the firm own positive R&D effort.

In such a model, hence, the additionality comes from firms with a negative profitability gap, since they would have been "non-performers" in absence of the subsidy[18].

3.5 Barriers to innovation adding subsidy endogeneity

Although theoretically rich, the threshold model proposed by Gonzalez *et al*. (2005) does not take into account formally the endogeneity of the subsidy. As we stated above, in fact, the level of the subsidy depends on the government (and, at least partially, on firm) decision and it could be function of R&D and other firm characteristics.

To take into account this occurrence we add an equation for s to system [21] obtaining[19]:

[26]
$$\begin{cases} R^* = x'\beta + \gamma s + u_1 \\ y = 1[w_1'\delta_1 + \tau s + v > 0] \\ s = m'\omega + \varepsilon \end{cases}$$

where $w_1'\delta_1 = x'\rho - z'\lambda$ and where m can contain either new variables or part of those in x and w and where we allow for arbitrary correlation between u_1, v and ε. We suppose that variables contained in x, w and m are all *strictly exogenous*, while $Cov(s;u_1) \neq 0$. By substituting the third equation into the second we get:

$$y = 1[w_1'\delta_1 + m'\iota + \upsilon]$$

where $\upsilon = \tau\varepsilon + v$ and $\iota = \omega\tau$. Observe that w and z are all exogenous for υ. The system becomes:

[27]
$$\begin{cases} R^* = x'\beta + \gamma s + u_1 \\ y = 1[w_1'\delta_1 + m'\iota + \upsilon]. \end{cases}$$

To derive a consistent estimation of γ write the first equation as:

[27.1]
$$R^* = x'\beta + \gamma s + g(x, m, w, y) + e_1$$

where $g(x, m, w, y) = E(u_1 \mid x, m, w, y)$ and $e_1 = u_1 - E(u_1 \mid x, m, w, y)$ with, by definition, $E(e_1 \mid x, m, w, y) = 0$. Equation [27.1] can be estimated by 2SLS on the selected sample (the group where $y=1$) using as instruments x, m, w and $g(x, m, w, 1)$, where we know that $g(x, m, w, 1) = E(u_1 \mid x, m, w, 1) = \theta\psi(x\delta_x + m\delta_m + w\delta_w)$. According to these conditions, a two step

[18] Observe that, whereas for the model estimation Gonzalez *et al*. use all N = 2214 firms (with a total of 9455 observations, since they have an unbalanced panel for 1990-99), for the calculus of the profitability gap they only use firms with non-zero R&D effort (i.e., performing firms) and positive subsidy.

[19] See Wooldridge (2002, p. 567-570).

procedure can be applied for consistent estimation:

Step 1. Obtain and estimate of $\delta_x, \delta_m, \delta_w$ by a probit regression of y on x, m, w and take the estimated inverse Mills ratios $\hat{\psi}(x'\hat{\delta}_x + m'\hat{\delta}_m + w'\hat{\delta}_w)$;

Step 2. Using only the selected sample estimate the equation:

$$R^* = x'\beta + \gamma s + \lambda\hat{\psi} + error$$

by 2SLS using x, $\hat{\psi}$, m and w as instruments; as before the hypothesis of no-selection problem can be test by H_0: $\theta = 0$ with usual 2SLS t-test[20]. Note, nevertheless, that standard errors should be corrected for the generated regressors problem as before.

3.6 Lagged endogenous subsidy with auto-correlated errors: a note

Introducing lagged values of s into the previous structural models of the effect of subsidy in R&D expenditure such as:

[28] $$R_{it} = x'_{it}\beta + \gamma_0 s_{it} + \gamma_1 s_{it-1} + \gamma_2 s_{it-2} + ... + u_{it}$$

does not provide estimation problems provided that s is "strictly exogenous" or "predetermined". When s is only contemporaneously endogenous, on the contrary, some estimation problems can arise, even if we rule out the contemporaneous subsidy from the previous regression.

Suppose that s is only contemporaneously endogenous; it means that:

[29] $$\begin{cases} E(s_{ir} \cdot u_{it}) \neq 0 & \text{if} \quad r = s \\ E(s_{ir} \cdot u_{it}) = 0 & \text{otherwise} \end{cases}$$

and suppose then that the error term in [28] is auto-correlated by order one:

[30] $$\begin{cases} u_{it} = \rho u_{i,t-1} + \varepsilon_{it} \\ \varepsilon_{it} : i.i.d. \end{cases}$$

Suppose, finally, that equation [28] reduces to:

[31] $$R_{it} = x'_{it}\beta + \gamma_0 s_{it} + \gamma_1 s_{it-1} + u_{it} ;$$

it is easy to show that, under these assumptions:

$$Cov(s_{i,t-1}; u_{it}) = Cov(s_{i,t-1}; \rho u_{it-1} + \varepsilon_{it}) = \rho Cov(s_{i,t-1}; u_{it-1}) + Cov(s_{i,t-1}; \varepsilon_{it}) = \rho Cov(s_{i,t-1}; u_{it-1}) \neq 0 ,$$

proving that s_{it-1} is now endogenous. By substitution of [30] into [31] we get that:

[20] Observe that for parameters' identification this procedure asks for the presence of all exogenous variables in the linear projection of s on x, $\hat{\psi}$, m and w.

$$R_{it} = x'_{it}\beta + \gamma_0 s_{it} + \gamma_1 s_{it-1} + \rho u_{it-1} + \varepsilon_{it} \,;$$

therefore, provided that a consistent estimation of u_{it-1} is available, equation [32] can be estimated by GLS. A consistent estimation of u_{it-1} can be obtained by a 2SLS regression of [31] that should provide as residuals:

$$\hat{u}_{it-1}^{2SLS}$$

as long as at least one instrument for s_{it} in [31] is available.

4. METHODS BASED ON A BINARY SUBSIDY VARIABLE: ESTIMATION OF THE AVERAGE TREATMENT EFFECT BY *CONTROL FUNCTION*, *MATCHING* AND *SELECTION MODELS*

So far we have considered estimation methods based on the availability of a continuous subsidy variable. In this section we address the problem of testing the presence of additionality when only a *binary* subsidy variable is at our disposal, by continuing to refer to a cross-section data structure. This setting, however, can be encompassed within the more general framework of the *average treatment effect* (ATE) estimation. Therefore, in what follows we first provide a concise introduction of the ATE estimation approach by presenting the main core concepts we need to get through the subject of this section.

4.1 The ATE setting

The main estimation problem arising in non-experimental statistical designs is that the traditional estimation procedure based on the simple comparison between average values of treated and untreated individuals (in our case: supported and non-supported firms) fails to estimate consistently the hypothesis of "additionality" of treatment on a certain target variable.

In non-experimental designs, in fact, treatment is non-random since firms can (at least to some extent) decide their status of participation (*self-selection*), as well as government can select to finance particular subjects according to specific objective functions (for ex., by adopting the principle of "aiding-the-poor" or, on the contrary, of "picking-the-winner"); we saw that point to be at the basis of subsidy endogeneity into equation [6].

In econometric terms it means that the treatment variable *w* (assuming, this time, the value 1 for treated and 0 for untreated units) and the outcome variable *y* (assuming the value y_1 for treated and y_0 for untreated units) are *stochastically dependent*. In this case we cannot trust the usual approach of the classical inference, such as the simple comparison between the mean of treated and untreated units.

In the classical inference, in fact, where *y* and *w* are supposed to be mean-independent[21], we have that the mean of *y* conditional on *w* is equal to the unconditional mean of *y*, i.e., E(y|w) = E(y). By defining the Average Treatment Effect (ATE) as:

[32] $$\text{ATE} = E(y_1 - y_0)$$

and the Average Treatment Effect on Treated (ATET) as:

[33] $$\text{ATET} = E(y_1 - y_0 \mid w=1),$$

[21] As we will state later, "mean-independency" is less restrictive than overall independency.

we can observe that, under mean-independence: E(y | w=1) = E(y_1 | w=1) = E(y_1) and E(y | w=0) = E(y_0 | w=0) = E(y_0); therefore we obtain:

$$\text{ATE} = \text{ATET} = E(y \mid w=1) - E(y \mid w=0)$$

that is, ATE and ATET coincide with the "difference-in-mean estimator" of basic statistics (i.e., the average of *y* for treated minus the average of *y* for non-treated individuals); this estimator, as it is well known, is unbiased, consistent and asymptotical normal (see Wooldridge, 2002, p. 606).

When the mean-independence hypothesis does not hold, then the *ATE* and *ATET* generally differ and, most importantly, the "difference-in-mean estimator" cannot estimate consistently both these parameters.

To overcome this estimation problem econometricians have suggested different approaches under specific hypotheses: *control function* using OLS, *matching*, *instrumental variables* and *selection models* methods are the most known. All these approaches are alternatively suitable according to the underlying process generating the data, sharing in turn differential advantages and drawbacks (for a concise review see Heckman, 2001).

Implementing an Instrumental Variables approach solves the problem of *selection on unobservables*[22]. In this case, as we said above, the researcher needs to know a full set of exogenous variables (the instruments) correlated with the treatment variable *w* and uncorrelated with the outcome *y*, in order to build a 2SLS estimation of the evaluation equation. Generally specking, as in many other field of econometrics, finding appropriate instruments is not easy and asks also for some degree of arbitrariness (especially in a *just-identified* specification). The Selection Model approach (as in the Heckman (1978) two-stage selection model) is a powerful method to deal, as in the case of the instrumental variables, with selection on unobservables, but it requires some specific distributional hypothesis that other models do not need.

The Control Function and the Matching Estimators, on the contrary, ask for less requirements to be applied than the previous methods, but are not suitable to deal with important aspects such as the selection on unobservables. They are suited just in the case of *selection on observables*[23]. In fact, they both start from the idea that the treatment status is correlated with specific *observed characteristics* of firms that, once controlled for, restores the condition of a randomised experiment (this hypothesis is known as *ignorability of treatment*). Hence, by conditioning on these observable characteristics, these methods consistently estimate the *ATE* and *ATET* even in case of *treatment's non-observable heterogeneity* and *selection on results*[24]. Although their limits, if the researcher has at his disposal a wide set of observed variables, the problem of selection on unobservables should be attenuated. For this reasons the majority of studies in the field of microeconometric policy evaluation makes use of OLS and matching[25].

Matching, nonetheless, seems to be preferable to control function based on OLS at least for three reasons. First, it is a non-parametric estimation procedure, so that it does not need to specify a particular parametric relation between the dependent variable and its regressors as in the case of OLS

[22] We have *selection on unobservables* when idiosyncratic characteristics unobservable to the researcher are correlated with the treatment status variable. Without controlling for these characteristics, estimates can be inconsistent since these features can behave as potential confounders (see, Heckman, Urzua and Vytlacil, 2006).

[23] We have *selection on observables* when only characteristics observable to the researcher are correlated with the treatment status variable so that, controlling opportunely for them, ATE and ATET estimates will be consistent.

[24] We have *treatment's non-observable heterogeneity* when the effect of treatment is different in different treated units. We have *selection on results* when treatment's non-observable heterogeneity is correlated with the treatment variable.

[25] For the effect of public subsidy on business R&D or on innovative preformance using matching methods see: Almus and Czanitzki (2003), Duguet (2003), Aerts and Czanitzki (2004), Kaiser (2004), Lööf and Heshmati (2007) and Bérubé and Mohnen (2007).

(where an additive/linear form is assumed); second, the matching procedure considers only treated and non treated units in the *common support* by dropping all the controls whose variables' value is higher or smaller than that of the treated. Third and more importantly, matching reduces the number of non-treated to a sub-sample (the *selected controls*) with characteristics more homogeneous to the treated units. These properties of the matching method prevent those biases in the *ATE* and *ATET* estimation that simple OLS estimation cannot solve (Cameron and Trivedi, 2005, pp. 871-878).

In the next section, we present a concise overview of the OLS and matching procedure, to continue presenting the selection model in the case of a binary treatment variable.

4.2 The matching estimator

Different kinds of matching estimators have been proposed in the literature. Among them the most applied are those based on propensity scores (*propensity score matching*). Defined as the probability for an individual to get treated, conditional on a certain numbers of observable characteristics, the propensity score is an index function summarizing in a single number (the score) the wide set of observable characteristics affecting the probability of becoming treated. It is obtained from a probit regression where *w*, the treatment status, is the dependent variable and observable characteristics are the regressors. The propensity score approach solves the dimensionality problem arising when the number of covariates is high and exact matching is not possible (see, for example, Dehejia and Wahba, 2002 and Ichino, 2006)[26].

Various propensity score matching have been developed, such as: "stratification", "radius", "kernel" and "nearest neighbour" matching. All these methods can lead to different estimates of the *ATET*, so that a robust strategy should take into account this aspect by comparing or averaging on them[27].

Before explaining our matching procedure, it seems of worth to better clarify what kind of statistical problem we face in our setting. As we said, we are interested in estimating the average treatment effect on treated (ATET) defined as:

$$\text{ATET} = E(y_1 - y_0 \mid w=1) = E(y_1 \mid w=1) - E(y_0 \mid w=1).$$

As it is clear, whereas we can observe the quantity $E(y_1 \mid w=1)$ since it is equal to the outcome of treated units when they were treated, we "do not observe" the quantity $E(y_0 \mid w=1)$. From observation, in fact, we only know the variable $E(y_0 \mid w=0)$, i.e., the (average) level of the outcome

[26] Instead of the propensity score, another class of matching estimators use a specific metric (such as the Mahalanobis or the Euclidian one) to measure the distance between a treated and an untreated unit. Recently, also hybrid approaches have been developed using, for example, a Mahalanobis metric whose arguments are contemporaneously the covariates and the propensity score (see, for example, Lechner, 2001). It is not clear, however, which is the efficiency gain of hybrid models (see Zaho, 2004).

[27] Few studies have compared the performance of different kinds of matching estimators. Dehejia and Wahba (2002) found that "The choice among matching methods becomes important when there is minimal overlap between the treatment and comparison groups" (p. 158) concluding that, either in presence of greater or smaller overlap, the nearest neighbour matching performs quite well; in fact, when the true ATET coming from the benchmark (in their work, a previous experimental setting) is about \$ 1,794, the nearest neighbour's ATET is equal to about \$ 1,360 in the case of greater overlap and \$ 1,890 in the case of smaller overlap. Starting from the same database of Dehejia and Wahba (2002), Cameron and Trivedi (2005, pp. 893-896) have shown, on the contrary, that the nearest neighbour matching performs worse than other matching methods when slight modifications in the controls' selection criteria are implemented (such as, the "common support" restriction). They obtain a nearest neighbour's ATET of about \$ 2,385 that overestimates the true value of \$ 1,794 using the same Dehejia and Wahba (2002) propensity score specification. Zhao (2004), finally, compared various matching models in a Monte Carlo experiment; he concludes that "Monte Carlo experiments show that the different methods do not dominate each other in term of performance" (p. 100). Generally speaking, methods perform very differently according to the availability of good controls, their number, and the specification of the propensity score equation.

for non-treated units. Knowing what would have been the outcome for treated units if they had not been treated is impossible, since we can see only one of the two participation status for each single unit. This falls into the general statistical setting of a "missing observation" (Lee, 2005).

In a cross-section dataset, the idea behind the matching procedure is to estimate $E(y_0 \mid w=1)$ using non-treated units that are "similar" to treated units. This similarity can be checked according to several firms characteristics such as size, turnover, sector in which the firm operates and so on. When for each treated unit a similar non-treated unit has been selected among all potential non-treated units a comparable sub-sample is produced and it can be proved that the ATET is consistently estimated. In other words, we estimate $E(y_0 \mid w=1)$ with those non-treated firms that are like "twins" of the treated ones. More precisely, we hold:

[34] $$E(y_0 \mid w=1, X=x) = E(y_0 \mid w=0, X=x).$$

Relation [34] is valid only under *conditional independence assumption* (Rubin, 1977; Rosenbaum and Rubin, 1983): conditional on some pre-treatment observables (the variables *X*), we assume *y* and *w* to be independent[28]. In this case, the conditional *ATET* estimate becomes:

[35] $$\text{ATET}(x) = E(y_1 \mid w=1, X=x) - E(y_0 \mid w=0, X=x).$$

Equation [35] allows for identifying "cells" within which *y* and *w* are independent. To clarify this point, suppose that *X* is formed by two dichotomous variables *A* and *B* taking modalities *a1 ,a2* and *b1, b2* respectively. In this case four cells can be built. According to the *conditional independence assumption*, within each of these cells the experimental setting is restored and the "difference-in-mean estimator" consistently estimate the ATET(*x*). To obtain the ATET overall estimation we have only to integrate on *X* (obtaining its marginal distribution). It means that we have to take the mean of the various *ATET(x)* calculated in each cell weighted by the distribution of *X* conditional on *w=1*. If *X* is a discrete random variable:

$$ATET = \sum_x ATET(x) \cdot Pr(X = x \mid w = 1) \cdot$$

When *X* is highly dimensional or is a continuous variable, an exact matching is not possible. In general too many cells have to be built, running the risk of obtaining a large number of cells with zero observations. To avoid this drawback (the *dimensionality problem*), Rosenbaum and Rubin (1983) proposed to match individuals according to a single variable: the propensity score. As said above, it is obtained from a probit regression with regressors equal to the variables contained in *X*. Each treated and untreated unit has its own propensity score, and units with close propensity score are matched. In practise, the authors propose a procedure to form strata according to the propensity score in which is tested the so called "balancing property": in each stratum and for each variable (included the propensity score) the mean on treated and non treated has to be equal. This procedure generates the optimal number of strata as soon as the balancing property is satisfied in each stratum. Once obtained this partitioning we can averaging on the "difference-in-mean estimator" on strata obtaining a consistent estimation of the ATET (see, Becker and Ichino (2002) for a software implementation). This procedure is called the "Stratification matching".

Even if one makes use of matching procedures other than the Stratification matching, the balancing property has always to be satisfied. Therefore, we have first to test this property on our

[28] "Conditional-independence-assumption" is another name to call the already cited "ignorability of treatment". In any case, to obtain consistent matching estimate, we only needs "conditional-mean-independence" that is a less restrictive hypothesis (Wooldridge, 2002, p. 607).

data (in order to ascertain that our probit specification for the calculus of propensity scores is correct) and then applying each matching procedure.

We have now all the ingredients to describe the general protocol adopted in matching models. We implement the following steps:

1. we specify a probit regression on a given set of covariates (*x*) estimating the propensity scores $\hat{p}(x)$;
2. according to the estimates obtained in the previous step, we test the balancing property taking the specification satisfying it, and reducing observations on treated units to those in the *common support*;
3. according to the considered matching method and for each treated unit, we select the potential control(s), that is, those non-treated units more similar to the treated ones;
4. once obtained the matched comparison group, we calculate the estimated *ATET* using the appropriate formula.

Of course, different matching methods require different formulas for the calculus of the *ATET*; application generally use: 1. stratification, 2. one-to-one nearest neighbour, 3. three-nearest neighbours, 4. kernel, 5. radius (with various callipers) matching.

We have already qualitatively explained in which way the stratification matching works. The corresponding formula for the estimated ATET is[29]:

$$ATET^{S} = \sum_{b=1}^{B} ATET_{b}^{S} \cdot \left[\frac{\sum_{i \in I(b)} w_i}{\sum_{i} w_i} \right] \qquad \text{with:} \qquad ATET_{b}^{S} = \frac{1}{N_{b}^{T}} \sum_{i \in I(b)} y_{1i} - \frac{1}{N_{b}^{C}} \sum_{j \in I(b)} y_{0j} \,,$$

where: *I(b)* is set of units present in block *b*, N_b^T is the number of treated units in block *b*, N_b^C is the number of control units in block *b*.

Other matching methods deserve some further explanation. In the case of the one-to-one nearest neighbour each treated is matched with only one control (always in the common support), whose propensity score is the closest to that of the treated one according to some specific metric (for example, the Mahalanobis metric). In this case the set of control units is defined as:

$$C(i) = \left\{ \; j \;\; | \;\; \min_{j} \left\| p_i - p_j \right\| \; \right\}$$

that, for each unit *i* is a singleton unit *j* (or three units in the case of the three-nearest neighbours). Instead, the set of control units in the case of the “radius” matching is:

$$C(i) = \left\{ \; j \;\; | \;\; \left\| p_i - p_j \right\| < r \; \right\}$$

representing all the non-treated units falling (always in terms of their propensity score) in the radius of dimension *r*. A general formula for all these matching methods is the following:

[29] This part of the section draws on Ichino (2006).

[36]
$$ATET^{M}=\frac{1}{N^{T}}\sum_{i\in T}\left[y_i^T-\sum_{j\in C(i)}\omega_{ij}y_j^C\right]$$

where $0<\omega_{ij}<1$ is the weight given to the control unit *j-th* in the comparison with the unit *i-th* (with: $\sum_{j\in C(i)}\omega_{ij}=1$). For each treated unit *i*, the sum in the square brackets is thus a weighted average of its (selected) control units. In the case of the "arithmetic mean", the weights become $\omega_{ij}=1/N_i^C$ and the previous formula reduces to:

$$ATET^{ArM}=\frac{1}{N^{T}}\sum_{i\in T}\left[y_i^T-\frac{1}{N_i^C}\sum_{j\in C(i)}y_j^C\right].$$

Therefore, for the nearest neighbour matching, since $N_i^C=1$ (so that *j=i*), the formula becomes:

$$ATET^{NN}=\frac{1}{N^{T}}\left(\sum_i y_i^T-\sum_i y_i^C\right),$$

while for the three-nearest neighbours, it takes the following form:

$$ATET^{3NN}=\frac{1}{N^{T}}\sum_{i\in T}\left[y_i^T-\frac{1}{3}\sum_{j\in C(i)}y_j^C\right].$$

Furthermore, the kernel matching comes up from equation [36] when:

$$\omega_{ij}=\frac{K(p_j-p_i)}{\sum_{j=1}^{N_i^C}K(p_j-p_i)},$$

where K is the kernel function.

Finally, provided that outcomes are considered independent across units, it can be proved that the analytical variance of the estimator in equation [36] is equal to:

$$Var\left(ATET^{M}\right)=\frac{1}{(N^T)^2}\sum_{j\in T}Var(y_i^T)+\sum_{j\in C}(\omega_j)^2Var(y_j^C)$$

where $\omega_j=\sum_i\omega_{ij}$. It is quite clear that there is a penalty for using the same controls more than one time.

4.3 Matching estimation in presence of R&D subsidy spillovers

As we said above, the matching procedure is based on comparing a treated unit with a non-treated one resulting very similar to the first in term of economic structure. As suggested by Klette, Møen and Griliches (2000), nevertheless, the presence of high spillover effects induced by the subsidy on non-supported firms could severely underestimate the level of the (actual) additionality; indeed, since more similar firms are likely to be more “linked” (than dissimilar firms), when the R&D support is provided only to some firms, then its beneficial effect will be transmitted also to other firms according to their “closeness” to the first ones; since the “control group” is defined exactly as those non-supported units that are very similar to the supported units (in terms of economic structure), then it is likely that even these particular non-supported firms will benefit “indirectly” of the support, in so *augmenting* their R&D effort through their linkages with supported units.

In this sense, it would be not surprising if the level of additionality were underestimated; econometrically, it is like a sort of “omission of relevant variable” (as we saw above), that should be taken into serious account when getting results.

In this setting, one possible solution could be that of introducing into the structural equation governing the effect of support on R&D effort a spillover measure due to the support; nevertheless, when the support variable is binary and levels are unknown (as in the matching case), how can we produce a sound measure of the spillover variable? The previous authors do not provide any specific answer to this important issue; they only seems to look at that as a “cautionary footnote” for those implementing matching procedure.

What is now quite clear is that, when using matching rather than control function based on OLS, this problem is surely exacerbated; in such a situation, therefore, the benefits of using matching could not outweigh those of using simple OLS.

5. A STRUCTURAL SELECTION MODEL WITH BINARY TREATMENT

So far, we have considered a binary support variable both in a control function (based on OLS) and in a matching context; these methods are strongly empirical since the only included theoretical aspects are those concerning the choice of the control variables. Nevertheless, even in a setting where subsidization takes a binary form a structural model can be used.

According to Busom (2000), a selection structural model has two main advantages compared to matching and OLS: 1. it can overcome the problem of “selection on unobservables” that matching (as well as OLS) are unable to treat; 2. It can make more explicit the underlying theoretical model by the specification of a system of behavioural equations. We briefly present this approach.

If some unobservable variables affect simultaneously the outcome and the treatment status, even by conditioning on the right observables, the estimation of the ATET could be inconsistent since, by definition, w and y are still correlated. To take into account the presence of selection on unobservables, Heckman (1978) and Maddala (1983) provided an estimation procedure for a model with endogenous selection; the model is composed of two (correlated) equations: one for the outcome and one for the selection equation, and takes the following form:

[37]
$$\begin{cases} R_i = \mu + \gamma x_i + \alpha w_i + u_i \\ w_i^* = \eta + \beta z_i + v_i \\ w_i = \begin{cases} 1 & if \quad w_i^* \geq 0 \\ 0 & if \quad w_i^* < 0 \end{cases} \\ Cov(u_i; v_i) = \rho \neq 0 \end{cases}$$

where x and z are covariates and u and v are unobservable components (error terms) with zero unconditional mean, but supposed to be correlated. Under this assumptions $E(w_i \cdot u_i) \neq 0$, so that the OLS estimate of the outcome equation is inconsistent. We could rewrite the first equation of [37] in the two different regimes:

$$\begin{cases} w_i = 1: \quad y_i = \mu + \gamma x_i + \alpha + u_i \\ w_i = 0: \quad y_i = \mu + \gamma x_i + u_i. \end{cases}$$

It would seem possible to run two OLS regressions on them, obtaining α as the difference between the two (estimated) intercepts. The problem of this procedure, unfortunately, is that under both the regimes the error term has not zero unconditional mean; in fact:

$$\begin{cases} E(u_i \mid v_i \geq -\eta - \beta z_i) \neq E(u_i) = 0 \\ E(u_i \mid v_i < -\eta - \beta z_i) \neq E(u_i) = 0. \end{cases}$$

This is a typical case of "omitted variable specification error", that can be solved by adding the non-zero means into the equations, obtaining:

[38]
$$\begin{aligned} w_i = 1: \quad y_i &= \mu + \gamma x_i + \alpha + [u_i - E(u_i \mid v_i \geq -\eta - \beta z_i)] \\ w_i = 0: \quad y_i &= \mu + \gamma x_i + [u_i - E(u_i \mid v_i < -\eta - \beta z_i)]. \end{aligned}$$

Now, the errors terms in the squared brackets have zero mean. The problem, nevertheless, is that we cannot observe $E(u_i \mid v_i \geq -\eta - \beta z_i)$ and $E(u_i \mid v_i < -\eta - \beta z_i)$ directly. Nevertheless, we can estimate them by using the participation equation and the joint normality of u and v. From the joint normality it can be proved that:

$$\begin{aligned} E(u_i \mid v_i \geq -\eta - \beta z_i) &= -\lambda_1 M_{1i} \\ E(u_i \mid v_i < -\eta - \beta z_i) &= -\lambda_0 M_{0i} \end{aligned}$$

where: $M_{1i} = \phi(-\eta - \beta z_i)/[1 - \Phi(-\eta - \beta z_i)]$ and $M_{0i} = \phi(-\eta - \beta z_i)/[\Phi(-\eta - \beta z_i)]$ are the *Mill's ratios* (with ϕ and Φ being the normal density function and its cumulative respectively), while $\lambda_1 = \sigma_u \cdot \sigma_{u,v}$ and $\lambda_0 = -\sigma_u \cdot \sigma_{u,v}$[30].

We can estimate equations [38] by a two-step procedure or via maximum likelihood (Maddala, 1983). In the two-step we first estimate M_{1i} and M_{0i} (once obtained a consistent estimation of η and β from a probit regression of the participation equation); secondly, with these estimations at hand, we can estimate λ_1 and λ_0 by simple OLS[31]. We might then calculate also the coefficient of correlation ρ between u and v (since $\rho = \lambda_1 / \sigma_u^2$): if $\rho = 0$ then there is not endogenous selection in the equation (once controlling also for observable covariates) while, on the contrary, if $\rho \neq 0$ there

[30] The estimation procedure of this model is very close to that of section 3.4.

[31] Remember, again, that the standard errors of parameters have to be corrected for the generated regressors.

is endogenous selection and the sign of ρ shows if participation and outcome are positively or negatively correlated. Since this methodology is fully parametric a maximum likelihood approach can be used to estimate consistently all the parameters.

The specification of the two equation in system [37] depends on the theory the analyst has in mind: different specifications can produce substantial different estimations of the subsidy parameter; in this sense, this model is more flexible even though less robust than matching or OLS estimations. Nevertheless, it is always possible to compare these methods by holding $x = z = matching\ covariates$; in this way we reduce any arbitrariness in choosing different sets of x of z and a comparison among these methods becomes possible[32].

6. AVERAGE TREATMENT EFFECT WHEN A LONGITUDINAL DATASET IS AVAILABLE

Thus far, apart from section 3.6, we have taken into account estimation methods only in a cross-section data structure. Nevertheless, the availability of a longitudinal dataset can convey additional insights and estimation improvements into two directions: 1. in the possibility of taking into account unobservable elements (such as specific firm ability, and so on) through, for example, a fixed effect estimation (FE) that allows, at least partially, to reconcile OLS estimation with "selection on unobservables" (without introducing, for example, "ad hoc" instrumental variables); 2. in the possibility of exploiting data for a dynamic analysis of subsidy effectiveness, by drawing on a *dynamic treatment* approach otherwise impossible to do in a cross-section setting.

In what follows we first present the difference-in-differences (DID) estimator and its properties (section 6.1) to go on by extending it in a dynamic treatment setting (section 6.2 and 6.3); finally, in section 6.4 we provide a comparison between the FE and the DID estimator of the effect of public support on business R&D effort.

6.1 The difference-in-differences (DID) estimator

When a panel dataset is available we can observe the same firm *before* and *after* it receives a subsidy[33]. Suppose to have two times, t_0 and t_1, and that the subsidy occurs in between them, say, at τ so that: $t_0 < \tau < t_1$. In t_0 the firm hasn't received any subsidy, whereas in t_1 it is already treated. The firm *i*'s gain in t_1 after having been treated in t_0 is defined as:

$$\Delta_{i,t_1} = R^1{}_{i,t_1} - R^0{}_{i,t_1}$$

where $R^0{}_{i,t_1}$ is the level of R&D expenditure of the firm in t_1 , had it not received any subsidy in t_0. It is quite clear that $R^0{}_{i,t_1}$ is non observable and represent, as in the case of the cross-section setting, the *missing counterfactual*: again in each *t,* we only can observe a firm in a given status.

We define, in this new context, the ATET as:

$$\alpha = E(R^1{}_{i,t_1} \mid w_{i,t_1} = 1, w_{i,t_0} = 0) - E(R^0{}_{i,t_1} \mid w_{i,t_1} = 1, w_{i,t_0} = 0)$$

where, compared to a cross-section setting, we have imposed the condition $w_{i,t_0} = 0$, that means we want to know the average treatment effect on the sub-group of firms that was not treated in t_0 but

[32] For an extension of this selection model in a non-parametric environment see Hussinger (2003).

[33] This section is based on the mathematical appendix of the work by Lach (2000).

becomes treated in t_1. This average, for the problem of missing counterfactual, is not known and has to be estimated.

A possible idea could be that of calculating separately the average outcome of firms treated in t_1, the average outcome of firms non-treated in t_1, and then make the difference. By calling this estimator as α_1 we get:

$$\begin{aligned}
&\alpha_1 = E(R_{i,t_1} \mid w_{i,t_1} = 1, w_{i,t_0} = 0) - E(R_{i,t_1} \mid w_{i,t_1} = 0, w_{i,t_0} = 0) = \\
&= E(R^1{}_{i,t_1} \mid w_{i,t_1} = 1, w_{i,t_0} = 0) + [E(R^0{}_{i,t_1} \mid w_{i,t_1} = 1, w_{i,t_0} = 0) - E(R^0{}_{i,t_1} \mid w_{i,t_1} = 1, w_{i,t_0} = 0)] \\
&- E(R^0{}_{i,t_1} \mid w_{i,t_1} = 0, w_{i,t_0} = 0) = \\
&= \alpha + \underbrace{[E(R^0{}_{i,t_1} \mid w_{i,t_1} = 1, w_{i,t_0} = 0) - E(R^0{}_{i,t_1} \mid w_{i,t_1} = 0, w_{i,t_0} = 0)]}.
\end{aligned}$$

From this relation we can observe that $\alpha_1 = \alpha$ *if and only if* the underscored quantity is null:

$$E(R^0{}_{i,t_1} \mid w_{i,t_1} = 1, w_{i,t_0} = 0) = E(R^0{}_{i,t_1} \mid w_{i,t_1} = 0, w_{i,t_0} = 0)$$

that is, "if the missing counterfactual $E(R^0{}_{i,t_1} \mid w_{i,t_1} = 1, w_{i,t_0} = 0)$ can be thought to be equal to the *observable* quantity $E(R^0{}_{i,t_1} \mid w_{i,t_1} = 0, w_{i,t_0} = 0)$"; this is, in other word, a *ceteris paribus condition*.

As in the cross-section case in a *randomized setting* the previous equality always hold since w is independent of the outcomes: in this case no bias exists and the sample counterpart of α_1 will be a consistent estimator of α.

In a non-randomized setting we can introduce, as we did before, the hypothesis of *selection on observable*, so that:

$$E(R^0{}_{i,t_1} \mid x, w_{i,t_1} = 1, w_{i,t_0} = 0) = E(R^0{}_{i,t_1} \mid x, w_{i,t_1} = 0, w_{i,t_0} = 0)$$

that is the equivalent version of condition [34] in the cross-section setting.

We can now proceed to the estimation of α under *selection on observable* using the usual regression methods. Indeed, according to Lach (2000), we can derive the so-called *Difference-In-Differences* (DID) estimator for the effect of government subsidy on R&D expenditure in a panel data setting (in our case the α).

Suppose to have two R&D expenditure equations for supported and non-supported firms at t of this kind:

[39]
$$\begin{cases} R^0{}_{it} = x'_{it}\beta + \varepsilon^0{}_{it} \\ R^1{}_{it} = x'_{it}\beta + \delta_i + \varepsilon^1{}_{it} \end{cases}$$

where x are assumed *uncorrelated* with both the error terms: $E(\varepsilon^0 \mid x) = E(\varepsilon^1 \mid x) = 0$. As usually, we can derive the following *switching regression*:

[40]
$$R_{it} = w_{it}R^1{}_{it} + (1 - w_{it})R^0{}_{it} = x'_{it}\beta + w_{it}(\delta_i + \varepsilon^1{}_{it} - \varepsilon^0{}_{it}) + \varepsilon^0{}_{it},$$

which is a regression model characterized by a *random coefficient* for the regressor w_{it}. This model allows for a different effect of subsidy across firms (δ_i) and time ($\varepsilon^1{}_{it} - \varepsilon^0{}_{it}$).

Assume now that $\varepsilon^1{}_{it} - \varepsilon^0{}_{it} = v_{it}$ and $\delta_i = \overline{\delta}_i + \mu_i$, then equation [40] becomes:

[41]
$$\begin{aligned} R_{it} &= x'_{it}\beta + w_{it}\overline{\delta}_i + \varepsilon^0{}_{it} + w_{it}(\eta_i + v_{it}) = \\ &= x'_{it}\beta + w_{it}\tilde{\delta} + \varepsilon^0{}_{it} + w_{it}[\eta_i + v_{it} - E(\eta_i + v_{it} \mid x, w_{it} = 1, w_{i,t-1} = 0)] = \\ &= x'_{it}\beta + w_{it}\tilde{\delta} + \varepsilon^0{}_{it} + \omega_{it} \end{aligned}$$

where:

$$\begin{aligned} &\tilde{\delta} = \overline{\delta} + E(\eta_i + v_{it} \mid x, w_{it} = 1, w_{i,t-1} = 0) \\ &\omega_{it} = w_{it}[\eta_i + v_{it} - E(\eta_i + v_{it} \mid x, w_{it} = 1, w_{i,t-1} = 0)]. \end{aligned}$$

If we hold $\varepsilon^0{}_{it} + \omega_{it} = u_{it}$ into [41], we get:

[42]
$$R_{it} = x'_{it}\beta + w_{it}\tilde{\delta} + u_{it}$$

that is, the standard regression used to measure the impact of *w* on *R*. In particular we need to know what $\tilde{\delta}$ measures exactly. We have seen that the *average treatment effect on treated* (ATET) in this context is:

$$\alpha = E(R^1{}_{it} - R^0{}_{it} \mid x, w_{it} = 1, w_{i,t-1} = 0).$$

Now, by taking expectations on [39], we obtain:

$$\begin{aligned} &\alpha = E(R^1{}_{it} - R^0{}_{it} \mid x, w_{it} = 1, w_{i,t-1} = 0) = E(\delta_i + v_{it} \mid x, w_{it} = 1, w_{i,t-1} = 0) = \\ &= \overline{\delta} + E(\delta_i + \eta_{it} \mid x, w_{it} = 1, w_{i,t-1} = 0) = \tilde{\delta} \end{aligned}$$

showing that $\tilde{\delta}$ is measuring the treatment effect on treated (ATET), conditional on *x*. Observe that, by construction, ω_{it} in [41] is mean independent of w_{it}, so the only possible correlation between the error term and the subsidy can be due to the correlation between $\varepsilon^0{}_{it}$ and w_{it}. Therefore a sufficient condition for OLS consistency in equation [41] is that, conditional on *x* and $w_{i,t-1} = 1$, $\varepsilon^0{}_{it}$ and w_{it} have to be mean independent.

Given these premises, we could assume that the potential correlation between $\varepsilon^0{}_{it}$ and w_{it} is due to firm specific characteristics, as well as a time specific component; it leads to the following error component specification in [42]:

$$u_{it} = \theta_i + \lambda_t + \eta_{it} \quad \text{with} \quad \mathrm{E}(\eta_{it}) = 0$$

that yields the following (system) fixed effects specification:

[43]
$$\begin{cases} R_{it} = x'_{it}\beta + w_{it}\alpha + \theta_i + \lambda_t + \eta_{it} \\ w_{i,t-1} = 0 \end{cases}$$

where the essential difference with customary fixed effects models is that here we condition on $w_{i,t-1} = 0$, i.e., we only consider, for estimation, the sub-sample of firms not receiving any support in *t-1*.

To estimate consistently α in system [43] we can take *first differences* of this equation, getting:

[44] $$\Delta R_{it} = \Delta x'_{it}\beta + w_{it}\alpha + \Delta\lambda_t + \Delta\eta_{it}$$

where (a) the firm specific effect has been dropped out by differencing, and (b) $\Delta w_{it} = w_{it} - w_{i,t-1} = w_{it}$. By taking expectations on equation [44], it follows that:

[45] $$\begin{aligned} &E(\Delta R_{it} \mid \Delta x_{it}, w_{it} = 1, w_{it-1} = 0) - E(\Delta R_{it} \mid \Delta x_{it}, w_{it} = 0, w_{it-1} = 0) = \\ &\alpha + E(\Delta\eta_{it} \mid \Delta x_{it}, w_{it} = 1, w_{it-1} = 0) - E(\Delta\eta_{it} \mid \Delta x_{it}, w_{it} = 0, w_{it-1} = 0) \end{aligned}$$

that is, we can consistently estimate *α* by taking the difference of the difference between *t* and *t-1* in the R&D performance of treated and non-treated units, *as long as*:

$$E(\Delta\eta_{it} \mid \Delta x_{it}, w_{it} = 1, w_{it-1} = 0) = E(\Delta\eta_{it} \mid \Delta x_{it}, w_{it} = 0, w_{it-1} = 0)$$

occurring when $\Delta\eta_{it}$ is mean independent of w_{it} (conditional on the observable Δx_{it}). If this condition is supposed to hold, then:

$$\alpha = E(\Delta R_{it} \mid \Delta x_{it}, w_{it} = 1, w_{it-1} = 0) - E(\Delta R_{it} \mid \Delta x_{it}, w_{it} = 0, w_{it-1} = 0)$$

whose "sample version" is the exactly the so called *difference-in-differences* (DID) estimator:

$$\hat{\alpha}_{DID} = \left[\frac{1}{N^1} \sum_{i=1}^{N^1} [R_{it}{}^1(x_t)] - \frac{1}{N^1} \sum_{i=1}^{N^1} [R^1{}_{i,t-1}(x_{t-1})] \right] - \left[\frac{1}{N^0} \sum_{i=1}^{N^0} [R_{it}{}^0(x_t)] - \frac{1}{N^0} \sum_{i=1}^{N^0} [R^0{}_{i,t-1}(x_{t-1})] \right]$$

or, more compactly:

$$\hat{\alpha}_{DID} = [\overline{R}_t^1\ (x_t) - \overline{R}_{t-1}^1(x_{t-1})] - [\overline{R}_t^0\ (x_t) - \overline{R}_{t-1}^0(x_{t-1})]$$

that is equivalent to:

$$\begin{aligned} \hat{\alpha}_{DID} &= \left[\frac{1}{N^1} \sum_{i=1}^{N^1} [R_{it}{}^1(x_t) - R^1{}_{i,t-1}(x_{t-1})] \right] - \left[\frac{1}{N^0} \sum_{i=1}^{N^0} [R_{it}{}^0(x_t) - R^0{}_{i,t-1}(x_{t-1})] \right] = \\ &\left[\frac{1}{N^1} \sum_{i=1}^{N^1} [\Delta R_{it}{}^1] \right] - \left[\frac{1}{N^0} \sum_{i=1}^{N^0} [\Delta R_{it}{}^0] \right] = \overline{\Delta R_{it}{}^1} - \overline{\Delta R_{it}{}^0}. \end{aligned}$$

From this last relation we get that:

$$\hat{\alpha}_{DID} > 0 \quad \Leftrightarrow \quad \overline{\Delta R_{it}{}^1} > \overline{\Delta R_{it}{}^0}$$

i.e., we have additionality when the average difference in the treated R&D performance between t and t-1 (that is, *after* and *before* subsidy) is greater than that of non-treated firms.

6.2 Extending the difference-in-differences (DID) in a dynamic treatment setting[34]

So far, we have dealt with a very simplified setting in which the event "being treated" for the firm *i* corresponds to the following condition:

$$\{w_{it} = 1 \mid w_{it-1} = 0\} .$$

Nevertheless, the fact to have access to a longitudinal structure of data allows us for inquiring into more complex *treatment designs*; for example, the event "being treated" can be generalized to this event:

$$\{w_{it} = 1, w_{i,t-1} = 1, w_{i,t-2} = 1, ..., w_{i,t-q} = 1 \mid w_{i,t-q-1} = 0\}$$

in which a firm is treated if it receive a subsidy in *t, t-1, ..., t-q*, while receiving any supports in *t-q-1*. Suppose, for the sake of simplicity, to fix *q=1*; in this the case the event "being treated" gets $\{w_{it} = 1, w_{it-1} = 1 \mid w_{it-2} = 0\}$ and system [43] can be written as:

[46]
$$\begin{cases} R_{it} = \alpha_1 w_{it} + \alpha_2 w_{i,t-1} + \eta_{it} \\ w_{i,t-2} = 0 \end{cases}$$

where we omit covariates *x* and fixed effects in order to simplify notation (without any lack of generality). If we rewrite [46] delayed of one lag, and by substituting the condition $w_{i,t-2} = 0$, we obtain:

[47]
$$R_{i,t-1} = \alpha_1 w_{it} + \eta_{it}$$

that subtracted to the first equation of system [46] provides:

$$R_{it} - R_{i,t-1} = \alpha_1 w_{it} + (\alpha_2 - \alpha_1) w_{i,t-1} + (\eta_{it} - \eta_{i,t-1})$$

that is tantamount to:

[48]
$$\Delta R_{it} = \alpha_1 w_{it} + (\alpha_2 - \alpha_1) w_{i,t-1} + \Delta \eta_{it} ,$$

that is equivalent to [44], but this time for the treatment event $\{w_{it} = 1, w_{it-1} = 1 \mid w_{it-2} = 0\}$.

Which is the available counterfactual for this model? In other words, how can we generalize the DID estimator for this specific design? In the first setting we saw that the event "being treated" was:

$$\{w_{it} = 1 \mid w_{it-1} = 0\}$$

and the corresponding event "not being treated" was unique and equal to:

$$\{w_{it} = 0 \mid w_{it-1} = 0\}$$

[34] This section and the next ones on dynamic tretment provide author's original analyses.

and the DID estimator was equal to the fixed effect estimate of *α* in equation [43] using only the observations for which $w_{i,t-1} = 0$.

In the new setting, associated to the "being treated" event:

$$\{w_{it} = 1, w_{it-1} = 1 \mid w_{it-2} = 0\}$$

we have now "four" counterfactual events, i.e., four "not-being treated" events:

1. $\{w_{it} = 0, w_{it-1} = 1 \mid w_{it-2} = 0\}$: and in this case $\alpha = \alpha_1$

2. $\{w_{it} = 1, w_{it-1} = 0 \mid w_{it-2} = 0\}$: and in this case $\alpha = \alpha_2 - \alpha_1$

3. $\{w_{it} = 0, w_{it-1} = 0 \mid w_{it-2} = 0\}$: and in this case $\alpha = \alpha_2$

4. $\{w_{it} = 1, w_{it-1} = 1 \mid w_{it-2} = 0\}$: and in this case $\alpha = 0$.

Proving this result is easy, and follow the same procedure used to arrive at equation [45]; only for conciseness, we prove the case 1. (the others can be obtained in the same manner).

First, take the equation [48], and calculate:

$$E(\Delta R_{it} \mid w_{it} = 1, w_{it-1} = 1, w_{it-2} = 0) = \alpha_2 + E(\Delta\eta_{it} \mid w_{it} = 1, w_{it-1} = 1, w_{it-2} = 0)\,;$$

then calculate:

$$E(\Delta R_{it} \mid w_{it} = 0, w_{it-1} = 1, w_{it-2} = 0) = \alpha_2 - \alpha_1 + E(\Delta\eta_{it} \mid w_{it} = 0, w_{it-1} = 1, w_{it-2} = 0)\,.$$

From [45] we saw that:

$$\begin{aligned}
\alpha &= E(\Delta R_{it} \mid w_{it} = 1, w_{it-1} = 1, w_{it-2} = 0) - E(\Delta R_{it} \mid w_{it} = 0, w_{it-1} = 1, w_{it-2} = 0) \\
&= \alpha_2 + E(\Delta\eta_{it} \mid w_{it} = 1, w_{it-1} = 1, w_{it-2} = 0) - [\alpha_2 - \alpha_1 + E(\Delta\eta_{it} \mid w_{it} = 0, w_{it-1} = 1, w_{it-2} = 0)] = \\
&= \alpha_1 + [E(\Delta\eta_{it} \mid w_{it} = 1, w_{it-1} = 1, w_{it-2} = 0) - E(\Delta\eta_{it} \mid w_{it} = 0, w_{it-1} = 1, w_{it-2} = 0)]
\end{aligned}$$

so that, under mean independence between $\Delta\eta_{it}$ and $(w_{it}; w_{it-1})$ and again conditional on covariates (non reported for simplicity), we obtain:

$$\alpha = \alpha_1\,.$$

Observe that, to reach this results, we need the mean independence between $\Delta\eta_{it}$ and $(w_{it}; w_{it-1})$, that is, we need that w_{it} to be *predetermined* for $\Delta\eta_{it}$.

The estimation procedure can follow this scheme:

a) estimate regression $R_{it} = x'_{il}\beta + \alpha_1 w_{it} + \alpha_2 w_{i,t-1} + \theta_i + \lambda_t + \eta_{it}$ by an FE estimation and obtain consistent estimate $\hat{\alpha}_1^{FE}$ and $\hat{\alpha}_2^{FE}$ (and, $\hat{\beta}^{FE}$, of course);
b) calculate the DID estimators according to the three counterfactual settings:

case 1: $\hat{\alpha}^{I}_{DID} = \hat{\alpha}_1$;
case 2: $\hat{\alpha}^{II}_{DID} = \hat{\alpha}_2 - \hat{\alpha}_1$;
case 3: $\hat{\alpha}^{III}_{DID} = \hat{\alpha}_2$.

Apart from the fourth case (that is not interesting since DID is zero by definition), we have now *three* DID estimators (rather than only *one*, as in the first setting) that can be used to test additionality due to treatment with three potential "control" behaviors. For example: $\hat{\alpha}^{I}_{DID}$ measures the additionality of firms receiving a subsidy in *t-1* and *t* (had they received nothing in t-2) with firms receiving a subsidy in *t-1* while receiving no subsidy in *t* (but had they also received nothing in t-2), and so on. Table 2 clarifies all possibilities.

TABLE 2. THE DID ESTIMATORS ACCORDING TO THE FOUR COUNTERFACTUAL SETTINGS OCCURRING WHEN SUBSIDY CAN BE RECEIVED IN *t* AND *t-1*

		w_{it}	
		Non-supported (0)	Supported (1)
$w_{i,t-1}$	Non-supported (0)	α_2	$\alpha_2 - \alpha_1$
	Supported (1)	α_1	*0*

If $q = 2$ the event "being treated" takes the following form:

$$\{w_{it} = 1, w_{it-1} = 1, w_{it-2} = 1 \mid w_{it-3} = 0\}$$

where now we are allowing for three consecutive years of treatment, in the sub-sample of firms without not receiving any subsidy in *t-3*. In this case:

$$\Delta R_{it} = \alpha_1 w_{it} + (\alpha_2 - \alpha_1) w_{i,t-1} + (\alpha_3 - \alpha_2) w_{i,t-2} + \Delta\eta_{it}$$

and we can calculate, by adopting the same procedure as before, the 2^{q+1} (in this case eight) DID estimators corresponding to the 2^{q+1} counterfactual settings we may identifies. Table 3 shows the results.

TABLE 3. THE DID ESTIMATORS ACCORDING TO THE EIGHT COUNTERFACTUAL SETTINGS OCCURRING WHEN SUBSIDY CAN BE RECEIVED IN *t*, *t-1* AND *t-2*

Counterfactual	w_{it}	w_{it-1}	w_{it-2}	α
1	0	1	1	α_1
2	0	0	1	α_2
3	0	1	0	$\alpha_3 - \alpha_2 + \alpha_1$
4	0	0	0	α_3
5	1	1	1	*0*
6	1	0	1	$\alpha_2 - \alpha_1$
7	1	1	0	$\alpha_3 - \alpha_2$
8	1	0	0	$\alpha_3 - \alpha_1$

Also in this case we can get various kinds of additionality according to the different settings and after estimating the various α. Again, for consistency, we have to assume w to be *predetermined* for $\Delta\eta_{it}$.

6.3 Extension to more complex treatment designs: a note

In the foregone section we have assumed the "being treated" event to be identified by:

[49] $$\left\{w_{it}=1, w_{i,t-1}=1, w_{i,t-2}=1,\ldots,w_{i,t-q}=1 \mid w_{i,t-q-1}=0\right\}$$

and we showed how to get consistent estimations of DID associated to the various counterfactual settings.

Nevertheless, event [49] is only one possibility we have to identify treatment dynamically; an other way, indeed, is that of conditioning on more than one year of *absence* of treatment as in the following new "being treated" event:

$$\left\{w_{it}=1, w_{i,t-1}=1, w_{i,t-2}=1,\ldots,w_{i,t-q}=1 \mid w_{i,t-q-1}=0, w_{i,t-q-2}=0,\ldots,w_{i,t-q-k}=0\right\}.$$

Even in this case we can apply the procedure viewed above, although some difference can emerge according to the choice of conditioning events.

6.4 A comparison between the DID and the FE estimator

Many applications trying to explore the occurrence of additionalty in R&D supporting programs in a longitudinal setting, make use of a simple *fixed effects* (FE) estimation (eventually augmented by subsidy lagged variables)[35].

Which is the difference in using the FE instead of the DID estimator? Does it matter in term of estimate precision. Intuitively, the DID estimator should be more robust since, according to its definition, it takes into account a *ceteris paribus* condition that the FE estimator overlooks. To clarify this point we write the two regression for the DID and the FE:

$$\text{DID: } \begin{cases} R_{it} = x'_{it}\beta + w_{it}\alpha + \theta_i + \lambda_t + \eta_{it} \\ w_{i,t-1} = 0 \end{cases}$$

$$\text{FE: } \left\{ R_{it} = x'_{it}\beta + w_{it}\alpha + \theta_i + \lambda_t + \eta_{it} \right.$$

Where, by substitution and differencing we obtain (omitting x and λ):

$$\begin{aligned} \text{DID: } & \Delta R_{it} = w_{it}\alpha + \Delta\eta_{it} \\ \text{FE : } & \Delta R_{it} = \Delta w_{it}\alpha + \Delta\eta_{it}, \end{aligned}$$

so that we get two different conditions for consistency. For the DID equation we need that:

$$Cov(w_{it};\eta_{it}-\eta_{i,t-1}) = Cov(w_{it};\eta_{it}) - Cov(w_{it};\eta_{i,t-1}) = 0,$$

[35] An example is the work of Klette and Møen (1998) and that of Streicher (2007).

that is:

[50] $$Cov(w_{it};\eta_{it}) = Cov(w_{it};\eta_{i,t-1})\ ;$$

and for the FE equation we need correspondingly:

$$\begin{aligned}&Cov(w_{it}-w_{i,t-1};\eta_{it}-\eta_{i,t-1}) = [Cov(w_{it};\eta_{it}) - Cov(w_{it};\eta_{i,t-1})]\\&+[Cov(w_{i,t-1};\eta_{i,t-1}) - Cov(w_{i,t-1};\eta_{it})] = 0\end{aligned}$$

that is:

[51] $$[Cov(w_{it};\eta_{it}) - Cov(w_{it};\eta_{i,t-1})] = [Cov(w_{i,t-1};\eta_{i,t-1}) - Cov(w_{i,t-1};\eta_{it})]\ .$$

We observe immediately that, when DID is consistent (so that, [50] holds), equation [51] becomes:

$$[Cov(w_{i,t-1};\eta_{i,t-1}) - Cov(w_{i,t-1};\eta_{it})] = 0\ ,$$

that is:

[52] $$Cov(w_{i,t-1};\eta_{i,t-1}) = Cov(w_{i,t-1};\eta_{it})\ .$$

It means that a second and more restrictive requirement on correlations between w and η at different time periods is asked by the FE compared to the DID. It indicates that the condition under which the consistency of the DID is achieved are less restrictive of that required by the FE estimator. In this sense, DID is preferable to the FE estimator.

Nevertheless, an other aspect has be taken into consideration; indeed, even if more robust than the FE estimator, the DID estimate reduces the number of observations needed for the estimation of α; if this drop in number of observations in substantial, than the estimation precision of the DID could decrease considerably: in other words, it could be possible to face a sort *of trade-off* between robustness and precision making use of the DID; in particular, if the number of observations drop dramatically when using the DID, it is likely that the FE estimation will produce more precise estimation making the DID less attractive[36] (even if it could remain useful to continue to use the DID to distinguish between the various counterfactual settings).

As a final remark, it is worth to put in evidence that the entire discussion we did so far about DID and FE can be easily applied when w is a continuous rather than binary variable.

7. CONCLUDING REMARKS

Although many studies aimed at measuring the effect of public support on business R&D have been realized and the literature continues to increase to date, much work needs to be done yet. We summarize some aspects that should deserve more attention in future works:

[36] Probably a Monte Carlo experiment could shed some light on this point.

– first, an aspect very little explored in the literature is the study of dynamic subsidization, an essential issue to appreciate the long-term effect of incentives; this is especially important in this field, since R&D activity displays its benefits along time and with substantial delays; government strategy could be more targeted to long-run, rather than short-run benefits so that, without a sound econometric evidence on dynamic subsidization, evaluation works in this field could run the risk to remain very limited in terms of their predictive power and political value;
– second, only few works take into account the complexity of mechanisms laying behind the functioning of an R&D incentive programs: firm R&D and non-R&D investment strategy, market structure, macroeconomic environment, institutional and cultural factors and expectations, are only some of the numerous elements that could condition the effectiveness of an R&D supporting program and that the majority of works seem overlook;
– third, even if the centre of the analysis of the majority of models is that of testing "private R&D additionality" we should not forget that, on the side of society, R&D effort is only a "mean" and not an exclusive "end"; the very end is, more likely, that of increasing national firms' performance such as productivity, profitability and degree of innovativeness (for improving standard living, economic growth and so on); it means that linking R&D additionality due to subsidy programs with firm performances is a necessary step to provide a complete analysis of "subsidy effectiveness"; it remains unclear, however, how to do that without introducing more complex structural models where more than one variable could be potentially endogenous enlarging the estimation problems and reducing the feasibility and reliability of such models;
– forth, the question of R&D support evaluation in presence of spillovers does not seem to have received a satisfactory treatment up to now; one reason is probably tied to the difficulty of measuring spillovers especially those related to the provision of subsidies; for example: do subsidies generate "knowledge" or "rent" spillovers? And to which extent? This is still an open question; furthermore, even if in presence of spillovers the effect of subsidy treatment (in a counterfactual setting) can be seriously underestimated, we cannot rule out to generate an additional bias when a incorrect spillover measure is provided; in this sense what is better? Is it bearing the risk of incurring in a bias due to a lack of a spillover specification, or rather is it better to accept the risk of introducing a spillover proxy hoping this measure is sufficiently appropriate?
– fifth, the problem of data availability is a widespread one in empirical works, ranging from the lack of a sound database structure (such as repeated cross-sections or longitudinal data), towards information on the policy variable ("continuous" versus "binary" form) and knowledge on "projects quality"; the latter is a very important aspect since, as we underscored above, government is likely to choose to support firms according to three criteria: 1. the firm economic soundness, 2. the worth of firm proposals, 3. general *indirect effect* of supported projects on economy and society as a whole (such as, the boosting of employment, the increase in living standards, the promotion of technical progress, the change in industrial specialization and so on). R&D evaluation works, especially those drawing on general survey generally have rich and good information on point 1, while rarely can rely on information on project proposals and their quality, to not say, about the arguments of the "welfare function" adopted by the government in its decision; hence it is quite clear-cut that, although many econometric methods deal with "selection on unobservables", the risk of omitting relevant variable and generating substantial biases cannot be, in any case, totally prevented.

By means of this review we hope to have risen probably old as well as new questions, insights and possible improvements for future econometric works aiming at modelling and measuring the effect of public support on business R&D effort.

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