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R&D INCENTIVES: THE EFFECTIVENESS OF A PLACE-BASED POLICY

Roberto Gabriele
university of Trento
department of management and computer sciences
roberto.gabriele@unitn.it

Marco Corsino
University of Bologna
DEpartment of Management
marco.corsino@unibo.it

Anna Giunta
University of Rome 3
DEpartment of Economics
anna.giunta@uniroma3.it

Abstract

The paper contributes to the debate on the effectiveness of R&D subsidies analyzing a local policy implemented in the Italian province of Trento in the past decade. The empirical analysis draws upon a very detailed and informative database, profiling the population of firms which were awarded at least one R&D grant during the years 2002-2007. The econometric analysis is based on counterfactual models. First, treated firms are matched with ?twins? alongside a series of relevant predetermined variables. Secondly, a matching estimator is employed to estimate the average effect of treatment on treated. We evaluate the achievements of the local policy maker with respect to the following objectives: (i) enhance the overall competitiveness of the business sector in the regional area; (ii) prompt additional investment in innovation. We find that R&D incentives are effective on intangible assets and have a confined to three years impact on

firms? labour productivity, while they have no effect on firms? operating margin.

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SPURRING R&D INVESTMENTS: THE EFFECTIVENESS OF A PLACE-BASED POLICY

Marco Corsino[1], Roberto Gabriele[2] and Anna Giunta[3]

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Abstract. The empirical evidence concerning the impact of R&D subsidies on both sides of the innovation process (input and output) and the overall performance of the firm is mixed. Moreover, while the role of regions in implementing and assessing innovation policies has increased since the last decade, little is known on the effectiveness of public policy implemented at the national level, thus providing no clue on the efficacy of local interventions. This paper analyzes the effectiveness of a local R&D policy implemented in the Italian province of Trento. The empirical analysis draws upon a very detailed database, profiling the population of firms which were awarded at least one R&D grant during the period 2002-2007. The econometric analysis is based on counterfactual models. We evaluate the achievements of the local policy maker with respect to the following objectives: *(i)* prompt additional investment in innovation; (*ii*) enhance the overall competitiveness of the business sector in the regional area. We find that R&D incentives positively affect investments in intangible assets and human capital, while they have no effect on firms' turnover, labor productivity and profitability.

Keywords: Regional Innovation Policy, Ex Post Evaluation, Subsidies, Research and Development, Counterfactual Models

[1] Department of Management, University of Bologna, Italy.
[2] D.I.S.A. Department of Management and Computer Science, University of Trento, Via Inama, 5 Trento, Italy, e-mail: roberto.gabriele@unitn.it.
[3] Department of Economics, University Roma 3, Italy.

1. Introduction

The pursuit of economic growth, firm competitiveness and development of new knowledge represent common justifications for government intervention to support private investments in innovation (Lundvall and Borras, 2005). As declining population in developed countries and diminishing returns from investment in physical capital weaken the prospects of long-term growth, the role of innovation as a key enabler of economic prosperity gains momentum. What is still contended, however, is the ability of policy makers to rectify market failures, provide effective incentives to spur welfare-enhancing innovations, and avoid the introduction of distortions in the economic system (Lerner, 2002).

A review of the empirical literature (Klette and Moen, 1999; Lerner, 1999; Wallsten, 2000; Czarnitzki *et al.*, 2007; Merito *et al.*, 2007; Piekkola, 2007; Hussinger, 2008; Potì and Cerulli, 2010) suggests that findings concerning the effect of R&D subsidies on the innovative effort and the overall performance of the firm are mixed. Moreover, despite the number of government layers intervening in several areas of business soared over time (European Commission, 2004), most of the empirical evidence deals with public policies implemented at the national level, thus providing no clue on the role of a place-based policy (Bronzini and Iachini, 2011) and the ability of host regions to retrieve social benefits from sponsored activities (Roper *et al.*, 2004).

The primary goal of our paper is to provide novel evidence by analyzing the effectiveness of R&D subsidies implemented within a local design of public intervention. Specifically, our investigation evaluates the achievements of the local policy maker with respect to the following objectives: *(i)* prompt additional, private investments in innovative activities; (*ii*) enhance the competitiveness of the business sector in the regional area. As to the local dimension, our paper explores the effectiveness of R&D subsidies in the Italian northern province of Trento in the past decade. In this context, the public initiative emanates from the regional government, draws on resources raised in the region and exploits the administrative and decisional competencies of the local authority.

Since 2001 the local government has played an active role in financing R&D projects carried out by private enterprises. The financial commitment of the policy-maker has been substantial: in years 2007 and 2008 the share of R&D subsidies over the total amount of financial subsidies was 33.1% and 46.7%, against a national average 17.7% and 17.5%, respectively (MET, 2009). The average yearly value of total spending along the time window under analysis was €22.7 million. Despite this effort, to the best of our knowledge, no systematic assessment of the impact of such an impressive intervention has been so far carried out.

We evaluate the effectiveness of the public endeavor relying upon econometric methods commonly used to evaluate technology programs (Klette *et al*., 2000; Hussinger, 2008; Trivellato *et al*., 2009). Such methods recognize the counter factual nature of the analysis and allow the researcher to clean out most of the confounding effects associated with factors like technological opportunities, appropriability conditions, endowment of knowledge capital, other types of incentives granted to enterprises, which may eventually influence a firm's ability to benefit from innovation activities.

The treatment consists of an incentive to co-finance an R&D oriented project. Each treated firm is matched with a set of control units, which are the most similar along a series of predetermined variables such as: sector of activity; size; past innovative activity; capital intensity and financial constraints. The reliability of the estimation depends on the quality of matches: the more control firms are similar to treated ones, the more the estimation is precise. In this respect, the local dimension of the intervention (a firm to be eligible must be located and must carry out the investment in the province of Trento) reduces the level of heterogeneity across firms, as it allows to compare firms that are more similar than in nationwide programs, this, in turn, enhances the performance of matching estimators (Bronzini and Iachini, 2011; Heckman, Ichimura, and Todd, 1997; Heckman, Ichimura, and Todd, 1998; and Lechner, 2001).

Our investigation draws on a very detailed and informative database, profiling the population of companies awarded at least one R&D grant during the years 2002-2007. A distinguishing feature of our data set (as in Mairesse and Mohnen, 2010) is the merge of balance sheet data of limited liability companies operating in the province of Trento with the administrative archives, which allows us to track all the subsidies granted to firms operating in the province under the Law 4/1981 for the period 1991-2004 and the Provincial Law 6/99 for the period 2002-2007.

The empirical analysis suggests that R&D incentives positively affect investments in intangible assets and human capital. The effect lasts for at least two years after the allowance of a grant and it is robust to unobserved heterogeneity at the firm level. Beyond this, we do not find any significant bearing of the R&D policy on the overall performance of the firm.

The paper is organized as follows. Section 2 summarizes the recent empirical literature on the effectiveness of R&D policy and outlines the goals of our paper; section 3 illustrates the main characteristics of the Provincial Law 6/99 implemented in the province of Trento. The description of data, variables and estimation method follow in section 4. Section 5 presents the results of the econometric analysis, while section 6 concludes.

2. Literature review

Government programs that subsidize innovative activities, in general, and R&D spending, in particular, are justified on the grounds that market failures prevent firms from opportunely investing. The theoretical and empirical literature identify two major rationales[1] behind these market failures. The primary line of reasoning states that profit-maximizing firms do not face sufficient incentives to make a socially optimal investment in R&D. This occurs because R&D is likely to generate positive spillovers,[2] that make firms unable to fully appropriate the benefits originating from their innovative effort (Nelson, 1959; Arrow, 1962). An extensive econometric literature (Griliches, 1992; Hall *et al*., 2009) shows that the external economies associated with R&D effort are important as they engender productivity gains at both industry and firm level. R&D spillovers have been found to positively affect the inventive performance of competing units especially if a certain degree of relatedness exists among research programs (Jaffe, 1986; Henderson and Cockburn, 1996). Moreover, external economies are larger in magnitude when the R&D effort is directed towards product rather than process innovation (Ornaghi, 2006). It is then conceivable that, from the social point of view, the gains from private R&D are often higher than private returns. Hence, a number of research projects would be worth undertaking even if they are privately unprofitable. A

[1] Hall (2002) discusses additional arguments in favor of policy intervention: *(i)* the existence of industries that are strategic for national security or to foster technological advances in other industries; *(ii)* the promotion of technological standards.

[2] Nelson (1959) recognizes that, beyond R&D spillovers, two other factors may create a gap between private and social returns to R&D and prevent profit maximizing firms from carrying out the desirable level of investment: *(i)* the often long time elapsing between the inception of a research project and the time when some valuable outcome arises may discourage firms concerned with short-run performance; *(ii)* the increasing variance of the profit distribution as one moves from the basic-research towards the end of the spectrum may cause a risk-averse firm to value projects at that stage significantly less than their expected profitability.

public policy that re-balances the marginal costs and revenues for firms that bring about R&D initiatives can make these projects privately profitable as well.

A second rationale that can explain the under-investment in R&D is related to the presence of capital market imperfections that make costly for firms, especially new ventures, to secure the financing needed to support innovative endeavors. Hall (2002) discusses three types of factors that make the raising of external capital expensive as compared with the internal costs of capital: *(i)* information asymmetries between investors and inventors; *(ii)* moral hazard problems; *(iii)* tax considerations that differently impinge on alternative sources of financing. Although a venture capital industry can ameliorate these problems, drawbacks remain that call for governments' intervention. By granting R&D awards, the latter conveys information to potential investors and certify the quality of start-ups, thus easing the financing constraint that might have otherwise precluded the undertaking of socially valuable projects (Lerner, 1999, 2002).

A review of the empirical literature suggests that unambiguous conclusions on the effectiveness of policy interventions can be hardly derived. Moreover, differences in the characteristics of firms under scrutiny (e.g., the average size of subsidized enterprises (Wallsten, 2000; Czarnitzki *et al*., 2007; Busom, 2000)), of the objective variables (e.g., R&D investment, number of patented inventions, overall firm performance); of the econometric methods adopted (e.g., the way in which control groups are constructed), recommend caution in deriving general implications from the bulk of evidence thus far collected. Bearing in mind these limitations, we provide a tentative summary of results from the recent empirical literature based on firm-level data.

Most of the empirical literature concentrates on the impact of public policy on private R&D spending. This stream of research tests the hypothesis that subsidies prompt additional R&D investments rather than substituting investments that firms might have in any case carried out. David *et al*. (2000) and Garcia-Quevedo (2004) provide a systematic assessment of major results from studies published before year 2000 that address this theme at different levels of analysis. Recent contributions differ from extant studies primarily because they recognize the counter factual nature of the evaluation exercise and rely upon methods that address the endogeneity of the R&D treatment (Klette *et al*., 2000; Cerulli, 2010).

Although the bulk of evidence conveys the idea that public support do not crowd out private investment in R&D activities (Almus and Czarnitzki, 2003; Hyytinen and Toivanen, 2005;

Czarnitzki and Tool, 2007; Czarnitzki *et al*., 2007; Hussinger, 2008; Aerts and Schmidt, 2008), a number of scholars point out contrasting results. Busom (2000) and Potì and Cerulli (2010) find that crowding out exists for a non negligible share (i.e., thirty and fifty per cent, respectively) of firms awarded R&D grants in Spain and Italy. Lach (2002) presents evidence on Israeli enterprises according to which additionality only concerns small firms, whereas no significant effect emerges among large companies that are, nonetheless, the more likely to gain access to public funding. Similarly, Görg and Strobl (2007) show that in Ireland only small R&D grants awarded to domestic firms spur additional private investment in innovative activities. On the contrary, no significant effect arises for foreign multinationals and crowding-out is observed when relatively large grants are awarded to domestic firms. Duguet (2004) and Gonzalez *et al*. (2005) do not find any significant relationship between public funding and R&D intensity in France and Spain; in the case of Spanish firms, however, subsidies induce firms to perform research activities. Finally, Wallsten (2000) provides evidence of crowding-out in a sample of US ventures which received awards from the Small Business Administration.

Public R&D subsidies positively affect the propensity to patent of Italian firms (Potì and Cerulli, 2010), although the effect is significant only in the short run (Merito *et al*., 2007). Positive effects emerge among Finnish companies, whereas the propensity to patent and the actual number of patents per employee are not significantly higher among Western Germany firms that get R&D awards (Czarnitki *et al*., 2007). Alongside, there is scant evidence (Berubè and Mohnen, 2009; Hussinger, 2008) that R&D policy increases the stream of revenues from newly commercialized products.

Even if public funds stimulate private R&D investment, does it follow that firms receiving a grant will achieve higher levels of performance? According to some authors (Klette and Moen 1999; Wallsten, 2000; Merito *et al*., 2007; Piekkola, 2007), R&D subsidies do not generally produce significant effects on firm performance, measured in terms of productivity growth, sales and employment growth, profitability. The only two cases where public support spurs improvements in firm performance reveal that some mediating factors impinge upon the estimated relationship. In particular, Piekkola (2007) finds that R&D grants drive productivity growth only among small and medium sized enterprises in Finland, while Lerner (1999) finds that grants awarded by the Small Business Administration generate growth in sales and employments only for firms located in area with substantial venture capital activity.

The present study contributes to the existing literature in two ways: *(i)* it provides original evidence on the effectiveness of an innovation policy implemented at the regional level; *(ii)* it deals with the distortions that may render the identification of the causal effect of an R&D policy cumbersome.

The first issue we address concerns the increased role that regional governments play in several areas of business. Across European countries, for instance, the role of regions in designing, implementing and evaluating innovation policies, targeted in particular to small and medium-sized firms, soared since the early 2000 (European Commission, 2004). The academic debate, however, has only recently focused on the rationales and perspectives of regional innovation policy. This debate has emphasized that a regional orientation to innovation policy may help achieve nation-wide goals insofar as it accounts for the uneven distribution of innovation processes across space (Fritsch and Stephan, 2005). Also, it has been argued that, because of differences in the way regional innovation systems function, a one-size-fits-all approach to innovation policy is unlikely to be efficient (Asheim and Coenen, 2005; Todtling and Trippl, 2005; Howell, 2005). Still, the empirical evidence at this level of analysis is scarce. Among the few exceptions, Hyytinen and Toivanen (2005) show that the provision of government funding at the regional level can pin down constraints on R&D investment and the expected growth of firms operating in industries that depend on external finance. Moreover, the effectiveness of a place-based policy cannot be easily inferred from estimated relationships based on national programs.

Diverse and, sometimes, contrasting forces can influence the effectiveness of a place-based policy. In principle, regional policy makers may be able to select projects with high social returns but insufficient private returns because they have a deep knowledge of potential awardees and local market conditions. This preferential stance should help them to ascertain whether submitted projects and firms applying for a grant display those attributes that are more likely to generate knowledge spillovers (Feldman and Kelley, 2006). Moreover a place-based policy should allow the host region to retain a share of the social benefit arising out from the financed projects (Roper *et al*., 2004).

Nevertheless, local authorities may be more easily captured by lobbies and therefore, prone to finance R&D projects that are privately profitable and would be pursued even without R&D subsidies (Wallsten, 2000). As underlined by Lerner (2002), there is an extensive political economy literature that has emphasised this kind of distortions and the several ways these distortions can manifest themselves (Eisinger, 1988; Cohen and Noll, 1991; Lerner, 1999; Wallsten, 2000).

The second concern that we try to address in this paper is a methodological one. Government typically deploys a range of industrial policies to support business activities where R&D policies represent just one measure in this broader policy set. Moreover, an R&D program can rely on multiple instruments to channel finance towards enterprises (Potì and Cerulli, 2010). To the extent that firms apply for various subsidies that, contemporaneously, affect their performance, an identification problem may arise that prevents the researcher from isolating the causal effect associated with each subsidy. The identification problem can become even more severe any time the central and the regional governments share competences on the funds promoting innovation activities (Cook *et al*., 1997; Fritsch and Stephan, 2005). Such an occurrence makes it difficult to isolate confounding factors. In these circumstances a proper evaluation of the effectiveness of R&D subsidies requires comprehensive information on measures that agencies, at different levels of government, undertake and firms, eventually, exploit.

The present study tackles this methodological problem by analyzing a specific case where the resources firms receive for R&D investments are entirely raised in the host region. Moreover we have complete information on all types of grants that firms have been awarded. An in depth discussion of the technology program under scrutiny and the data used to carry out the analysis unfold in the following sections.

3. The Law 6/99 in the province of Trento

Trentino is an Alpine province in north-east Italy, with nearly 500,000 inhabitants and a gross domestic product per inhabitant of € 30,400 in 2007, one of the top 50 richest NUTS2 regions in Europe. The manufacturing sector accounted for the 24.6% of total employment in 2007 and, within this sector, the three major activities were: basic metals and fabricated metal product (4.1%), food products; beverages and tobacco (3.3%), machinery and equipment (3%). The rate of unemployment averaged 3.14% during the period 2002-2007, while labor productivity averaged € 48771 during the same period and declined of about 1 percentage point along this time horizon.

A distinguishing feature of the institutional setting under scrutiny is that firms operating in the province of Trento can apply only for subsidies awarded by the local government. In this setting the Provincial Law 6/99 (hereafter, PL6)[3] provides the guidelines on the allowance of economic incentives to firms operating in the province. It comprises a large set of incentive schemes that are meant to foster fixed investments; research and development expenditures; firm restructuring; the adoption of production processes to safeguard the environment; the re-localization of firms within the province. The incentive scheme concerning the promotion of innovation activities has the following objectives. On the one side, it aims at stimulating additional expenditures in research activities by firms operating in the region. On the other side, it aims at stabilizing the employment rate, increasing the share of employees involved in research activities and enhancing the competitiveness of the local economic system.

The financial commitment of the local policy-maker was by far the highest compared to other Italian regions: in years 2007 and 2008 the share of R&D subsidies over the total amount of financial subsidies was 33.1% and 46.7%, against a national average 17.7% and 17.5%, respectively (MET, 2009). This notwithstanding, an overview of science and technology indicators (Eurostat, 2009) for the EU-27 states reveals some weaknesses of the province of Trento that the local government intervention can tackle. These indicators show that total R&D expenditures as a percentage of GDP amounted to1.11% in 2005, a value that is above the Italian average (0.89%), but lower than the average value in the EU-27 club (1.28%) and the 3% target specified in the Lisbon strategy for the EU as a whole. Moreover, the percentage of researchers as a share of total employment was 0.65%, better than the average Italian region (0.5%), but worse than the EU-27 average (0.9%).

In line with the guidelines in the Oslo Manual (OECD, 2005), the PL6 identifies two types of commercial research activities worth to be financed: (i) industrial research and (ii) experimental development.[4] All firms operating in the province of Trento can apply for grants within the framework of the PL6 submitting a project to the local authority. There is no

[3] The PL6 substitutes in all respects the previous Provincial Law 4/1981 and subsequent modifications. Nonetheless the two laws present a period of overlapping in the years going from 2002 to 2004, due to pending procedures of requests for grants presented before the date of 31 December 2001 which was the last term for presenting such requests within the PL 4/81. Note that starting from 2002 the PL6 was still in force for all firms.

[4] Industrial research is defined as a planned activity aiming at acquiring new knowledge that is used to introduce new products, new processes and services. Activities that can improve the quality of existing products, processes and services also fall into such category. The creation and the construction of prototypes is excluded from this category. Experimental development is defined as the acquisition, recombination and utilization of existing scientific, technological and commercial knowledge in order to produce projects, products, processes new to the firm or enhanced. This category includes the development of prototypes and their testing.

deadline to submit a project during the calendar year; however, since a first-in-first-out criterion is used to assign financial resources (provided that a panel of expert gives a positive assessment of the project), some firms might get a refusal once the budget for financing R&D activities is exhausted.[5] Once a firm applies for a grant, its research project is examined by a technical committee (evaluation procedure). If the application receives a positive evaluation, its economic viability and its financial sustainability are examined in a second stage. Only if the project gets a positive assessment at both stages, it can be co-financed by the local government.

Firms can ask for co-finance projects of different magnitudes ranging from € 25,000 to € 3 miolions. Projects can entail expenses referred to a period going from the date of concession to the following three years. The expenses fall in the following categories: (1) employment costs; (2) patenting costs and contractual costs of license acquisition; (3) general additional costs related to the project (overhead up to 60% of costs declared at point 1); (4) costs related to the use of the tools and machines employed within the project. Once a firm is awarded a grant, it must fulfill some constraints in order to get financed: (a) the results of the research have to be used/exploited in the province of Trento; (b) when the subsidy is bigger than € 500,000 or when the firm asks for further financing beyond the amount granted, it must guarantee a level of employment declared in the projects for at least two years after the grant is awarded.

4. Data, variables and estimation method

4.1 Data

We relied upon several sources to construct the database. Administrative archives, held by the local government, are the primary source we used to gather information on firms receiving the R&D grants and on firms that received any type of grants throughout the period of analysis. Alongside, data from the profit and loss account together with balance sheet data of limited liability companies operating in the province of Trento were retrieved from the Bureau Van Dijk's AIDA database and the Cerved Group's Pitagora database. Although both databases collect data from a common source, the Italian Chambers of Commerce, they differ

[5] Nonetheless, this situation never occurred in the period of analysis (2002-2007): the, so called, take up rate was quite low, even if it was increasing along time.

as for the number of companies surveyed. Thereafter, the joint use of both databases allowed us to cover a wider set of firms as well as the opportunity to run a double check on the quality of data at hand. The final collection of data comprises about eight thousand companies observed over the period 1998-2008.

One typical concern with data from secondary sources is the low quality and often the lack of employment figures. To deal with this problem we recovered data on the working force of firms in our sample from the Archivio Statistico delle Imprese Attive (ASIA), constructed and managed by the National Statistical Office (ISTAT). The ASIA database represents the most comprehensive and reliable collection of information on the localization, sector of economic activity, legal form and employment figures for business firms operating in Italy.

Table 1 summarizes the major characteristics of subsidized and control firms during the period 2001-2008, along with basic statistics on the amounts of co-financed projects in each year. It shows that during the analysis period the number of research projects financed increased from 7 in 2002 to 25 in 2007. The distribution of interventions is biased towards medium-high and high tech firms, which account for 28 and 33 grants, respectively. The average value of subsidized projects ranges from €600.000 to €1m. In this respect, the year 2005 is an exception (the average value of a grant is around €2m) due to political decisions.

4.2 Variables

The treatment indicator used in this study is a dummy variable that equals 1 if a firm has a co-financed R&D project in the time window 2002-2007.

In constructing the objective variables to evaluate the effectiveness of the public policy, we exploit the fact that expenses subsidized by the PL6 can be associated with specific items in the balance sheet and the profit and loss account. As mentioned above, the overall grant is partitioned in the following items: (i) expenses for intangible assets (i.e., costs for "genuine" R&D activities, the acquisition of licenses and patents etc.); *(ii)* expenses for tangible assets; *(iii)* expenses for human resources. Knowing the amount of financing that subsidized firms receive for each item, and knowing that more than 50% of the overall subsidy is made available as soon as a grant is awarded, we compute[6] the net value of these items for the year

[6] Net intangible assets(t_1) = intangible assets (t_1) - financed expenses classified into intangible assets for the period t_1; Net tangible assets(t_1) = tangible asset (t_1) - financed expenses classified into tangible assets for the period t_1; Net labor costs(t_1) = labor costs(t_1) – financed expenses for human resources for the period t_1.

of concession (t_1). Finally, using these net values we consider the following objective variables to study additionality:

1. Intangibles intensity *(Inta_int)* - the ratio between net intangible assets and turnover, as a proxy for R&D capitalization. Although R&D expenses would be the preferred indicator to assess the research effort, it is worth noting that the Italian accounting laws do not require business enterprises to single out R&D costs in their balance sheets. This shortcoming is common to many studies because details on R&D expenses are typically available only for publicly traded firms (Wallsten, 2000).
2. Capital intensity (*Capint*) - the ratio between net tangible assets and turnover
3. Unit labor costs - the ratio between net labor costs and the total number of employees
4. Employment dynamics.

The effect of receiving a grant on the overall performances of the firm is evaluated through the following variables:

1. dynamics of sales at year end (*TS*)
2. labor productivity computed as value added per employee *(VAxempl)*;
3. the return on investment (*ROI*) as a measure of profitability.

The control variables included in our models comprise a set of dummy variables for the technological sectors[7] (OECD, 2003)*:* low technology sectors (DUtech_sech=1), low-mid technology sectors (DUtech_sech=2), mid-high technology sectors (DUtech_sech=3), high technology sectors (DUtech_sech=4). A set of dummy variables measuring firm size and constructed by jointly considering the number of employees and the total sales of the firm: micro firms (DUsizeEU=1), small firms (DUsizeEU=2), medium firms (DUsizeEU=3), large firms (DUsizeEU=4). The firm age *(Age)* measured as the number of years since firm's foundation that is meant to gauge experience effects, such as managerial skills and the ability to obtain external resources (Almus and Czarnitzki, 2003; Görg and Strobl, 2006; Hussinger, 2008). The level of firm sales at the year end *(TS)*. The rescaled cashflow *(cashflow)* as a proxy of the financial constraints that firms face: it is expressed as the ratio between cash flow and total sales. The level of intangible assets of the firm *(Inta)*. Finally, a control variable *(year)* for business cycle effects.

[7] We extended the OECD classification in order to take into account enterprises that operate in service sectors.

4.3 Estimation method

Estimating the effect of an R&D policy is problematic because grants are not randomly assigned to firms. In this context the usual OLS regression leads to biased estimates (Heckman *et al*, 1998; Rubin, 1977). The selection bias can arise from the behavior of the local government that picks the best preforming firms in terms of past R&D activity in order to maximize the probability of success of the policy. But it can also arise from self-selection of more innovative firms that can be more active in applying for and, consequently, in receiving the grants (Aerts and Schmidt, 2008).

The standard way to solve the endogeneity problem –i.e. the subsidy receipt is correlated with the past R&D activity- and to correct for the bias involves the search of instrumental variables for the treatment. However, the task is not easy and it suffers from the major shortcoming of arbitrary choices in selecting instruments and functional forms (Heckman *et al*, 1998). Matching estimators are a viable alternative and have the advantage of being free from any parametric assumption related with the selection process. In these models each firm receiving an R&D grant is compared with its counterfactual situation i.e., the hypothetical situation that it does not receive the grant. Under a series of assumptions, the counterfactual situation is built looking at the set of non treated firms (Rosenbaum and Rubin, 1983; Heckman *et al.* 1998).

Although valuable, standard matching does not solve the problem of unobserved heterogeneity. Indeed, while one can control for a series of observed covariates in order to neutralize the effect of selection, nothing can be done to correct for unobserved characteristics that can affect the selection of firms into the treatment (e.g., differential ability of entrepreneurs in capturing grants). When panel data are available, however, the conditional difference-indifferences (CDID) methodology can be used to properly account for the existence of invariant over time unobserved heterogeneity.

Let Y be an objective variable on which we want to estimate the effect of the policy. T is the binary treatment and signals the concession of an R&D grant in a particular year. Following the notation of Rubin (1973, 1977) we denotes with $Y_i(0)$ the potential outcome for a firm i in the case that it is not included into the treatment and $Y_i(1)$ the potential outcome of the same firm in the case of inclusion into the treatment. Obviously, we can observe each firm i only in one of these two states. Formally, we have:

$$Y_i = \begin{cases} Y_i\,(0(\; if\; T_i = 0 \\ Y_i(1(\; if\; T_i = 1 \end{cases} \qquad (1)$$

We define the average treatment effect on the treated (ATT), which represents the average impact of the R&D program on the subset of subsidized firms, as follows:

$$ATT = E[Y_i\,(1) - Y_i\,(0(|T_i = 1] \qquad (2)$$

Unfortunately, the quantities in the second member of equation (2) are not both observable, because each firm will either receive the treatment or not. The unobserved potential outcome of a treated firm is substituted with the outcome of a non treated firm whose characteristics are as close as possible to the treated one. Doing so, we build two groups of firms - the treated firm group and the control firm groups - and we can compare their performance so as to evaluate the impact of the program.

The choice of the control firm(s) to match with each treated is made using the so called matching estimator that uses sample observations to find adequate substitutes of the quantities we cannot observe. Given that we are not in the case of a randomized experiment in which the assignment to treatment is random, we need to model the assignment mechanism in order to select relevant dimensions along which firms included into the control group can be considered as similar as possible to those in the treated group.

The identification of the treatment effect is conditioned on two assumptions. First, unconfoundedness according to which the treatment T is independent of the potential outcomes $Y(0)$ and $Y(1)$, conditional on a set of predetermined variables (X). Such an assumption is not directly testable given that we cannot observe the potential outcomes but only their realizations. Second, the probability of being included into the treatment is greater than zero given any set of covariates (overlapping): $Prob(T=1|X=x) \in (0,1)$.

An additional assumption is the Stable Unit Treatment Value Assumption (SUTVA; Rubin, 1973), which states that the outcomes of one firm is not affected by treatment assignment of any other firm. This is a subtle assumption to make in our context because spillover effects might exist and cause its violation. Nonetheless, if the objective variable is the additional innovative activity we can make two considerations: *(i)* in the short run, the level of investment in innovation activities in one firm does not influence the investment behavior of other firms; *(ii)* while there is evidence of partnerships between private firms and public institutions – university and research centers - we do not have any evidence of private firms collaborating in innovation activity. In other words, the behavior of firms seems to support more a competition like mindset than a coopetition one.

Under these assumptions we can identify the average treatment effect on treated firms, which is the effect for the subsample of treated firms (*ATT*), as follows:

$$ATT=E[ATT(x)=E[Y(1)-Y(0)|T=1,X=x]]. \quad (3)$$

ATT is obtained comparing the actual outcome of subsidized firms with their potential outcome in the case of not receiving the R&D subsidy.

4.4 Empirical strategy: implementation of the model

We define a firm as treated if it is awarded a grant to carry out an R&D project. The year of treatment (t_1) corresponds to the period in which the firm receives a notification of allowance from the local government. From this moment through the following three years, the firm is co-financed for costs entailed in the project.

The definition of control firm is crucial to correctly identify the impact of the policy. Because of the wide range of activities that the law under scrutiny promotes and because of its non-competitive design, it is likely that a large number of firms in our sample received at least one grant during the period of analysis. Thereafter, we classify a firm as eligible in the control group only if it did not receive any grant in the three years before the period under investigation. More precisely, we compare a treated firm in period t, $t=2002,\ldots,2008$ with a set of control firms which: a) did not receive any grant in the past periods $t-1$, $t-2$, $t-3$; b) did not receive any grant in the subsequent periods $t+1$, $t+2$, $t+3$. A further condition we impose to include a firm either in the group of treated or in the control group is that it was a business organization active in at least one year before the notification of the grant.[8] The exact knowledge of which firm received what grant for the population of companies operating in the province allows us to neutralize the bias arising from a wrong choice of units to be included in the control group.

Our estimation strategy comprises a pre-filtering stage (Ho *et al.*, 2007), in which we exclude from the sample those firms belonging to the three digit Ateco 2002 sectors where no treated

[8] The conditions cause the exclusion of two research centers and three business organizations from the set of awardees.

firm operates. Moreover, a preliminary analysis revealed that the degree of innovation activity of subsidized and potential control firms is not comparable. Hence, we decided to restrict the sample of potential control firms only to units with an innovative performance above the median of the distribution of the initial sample of not subsidized firms. Such procedure guarantees that the two groups of treated and control firms are comparable.

The choice of the control group is done using the propensity score technique. Such methodology allows to consider several control variables as matching arguments without incurring in the problem of curse of dimensionality: the more dimensions are included, the more difficult it becomes to find a good match for each treated firm. The propensity score, i.e., the probability to receive a subsidy, is a valid tool to reduce all the dimensions considered to a single index (Rosenbaum and Rubin, 1983).

Specifically, the methodology we adopt unfolds as follow (Dehejia and Wahba, 2002). We start with a parsimonious specification to estimate the score; we stratify observations according to the value of their propensity score separately for the treated and the control group; we test whether the average values of all control variables for the two groups in each stratum are not statistically different, thus satisfying the *balancing property*. If all the covariates are "balanced", i.e. no differences are found, we stop and use the form of propensity score assumed in the first step; if in some strata covariates are not balanced the algorithm divide strata into finer ones and test again. If the balance is not reached after several attempts we should introduce high order terms and/or interaction effects among controls and start again with the procedure until the balance is reached. One further advantage of the procedure is that the homogeneity within strata can be considered as an indirect test of unconfoundedness (Stuart, 2010).

In selecting the initial set of variables to include into the propensity score two methodological concerns are worth noting. First, in order to satisfy the assumption of ignorable treatment assignment, all variables that are alleged to influence the participation decision and the outcome variable according to that the economic theory, previous empirical findings and/or information on the institutional setting should be included in the matching procedure (Rubin and Thomas, 1996; Heckman, Ichimura and Todd, 1998; Glazerman, Levy and Myers, 2003; Caliendo and Kopeinig, 2008; Stuart, 2010). Second, only variables that are unaffected by participation into the treatment should be included into the model (Caliendo and Kopeinig, 2008). In order to address this concern, all time-variant control variables are lagged one period (t_0) with respect to the year of treatment (t_1), thus making them predetermined with respect to the treatment[9].

Once matched the treated observations with control ones, we adopt a conditional-difference-in-differences (CDID) matching estimator (Smith and Todd, 2005; Blundell and Costa Dias, 2000). Heckman *et al*. (1998) show that CDID based on a non-parametric matching provides an effective tool in controlling for selection on both observables and unobservables. In particular, it allows us to control for temporally invariant differences in outcomes between participants and nonparticipants (Smith and Todd, 2005). The control group used in the CDID is a sample of non-treated firms j *in* I_0 which is matched to the treated firms i in the period (t_0) before receiving the treatment. The differences in performance before (t_0) and after the treatment (t_1) of the two groups are then compared. The effect of the treatment on the treated is estimated from the evolution of the two comparable groups over time.The estimator takes the following analytical form (Smith and Todd , 2005):

$$\hat{\alpha}_{DDM} = \frac{1}{n_1} \sum_{j \epsilon I1t \cap S_P} \{ (Y(t_1, i(- Y(t_0, i() - \sum_{j \epsilon I0t \cap S_P} W(i, j((Y(t_1, j) - Y(t_0, j() \} \quad (4)$$

where n_1 is the number of treated firms; $Y(t_0,i)$, $Y(t_1,i)$ are the value of the objective variable before and after the treatment for firm i in the treated group (I_1); $Y(t_0,j)$, $Y(t_1,j)$ are the value of the objective variable before and after the treatment for firm j in the control group (I_0); $W(i,j)$ represents the weights and depends on the particular cross-sectional matching estimator employed; finally S_p is the region of common support, i.e. the interval of propensity score in which we can find both control and treated firms. In our case the baseline results are obtained using kernel matching estimator with bandwidth equal to 0.01.

Given the small number of subsidies per year (around 13 on average, see Table 1) we pooled the data across years, i.e. we consider the group of treated firms regardless of the calendar year in which they receive the subsidy. Accordingly, a set of time dummies is used to control for time related aggregate shocks. Furthermore, all the monetary variables are deflated using production prices indices.

[9] We should also note that the final number of control variables included into the model is also influenced by statistical properties of the propensity score. There is, indeed, a little cost of including variables that are actually not related to the treatment assignment – they slightly modify the propensity score model estimation – and yield only a small increase in the variance of the model (Stuart, 2010).

5. Results

This section starts discussing the specification and the quality of the propensity score matching used to deal with the selection bias that may affect the evaluation exercise. Then, we present estimates of the effect of R&D grants on innovative investments and the overall performance of the firm. Finally, we show robustness checks and offer further considerations on the adequacy of the unconfoundedness assumption that is needed to identify the *ATT*.

The propensity score specification

Since we are interested in evaluating the effectiveness of the R&D policy after one, two and three years, the sample of treated and control firms changes in each exercise. Accordingly, we estimate three different propensity score models, using a probit estimator, given that each sample needs its own propensity score (Dehejia and Wahba, 2002). Moreover. in each model we consider a specific set of control variables and/or the inclusion of higher order terms and interaction effects to satisfy the balancing property.

Table 2 shows the number of blocks and the number of treated and control firms taken into account for the estimations referred to different time lags. This number ranges from 7, in the case of one year time lag, to 5, in the other two cases. Within these strata we performed the *t-tests* for mean equality with respect to all the control variables and the null hypothesis was never rejected in each stratum for each variable. The result can be interpreted as an indirect test of unconfoundedness (Stuart, 2009): once matched, no significant differences emerge between the samples of treated and control firms.

[Insert Table 2 about here]

We estimate the probability that a firm had to receive the treatment given a set of observables, $Prob(T=1,t_1)$, where t_1 is the treatment period and *Subs* is a dummy variable indicating the concession of an R&D grant in year t_1. The set of variables included in the three models is similar even if there are some differences due to the particular procedure used to satisfy the balancing property. For the same reason some terms are included with a degree higher than one (*Inta_int* and *Cashflow*). Table 3 presents the estimations of the propensity score on sample constructed to account for the different time lags.

[Insert Table 3 about here]

To assess the quality of the matching procedure we adopt the methodology proposed by Imbens (2004). The author argues that the performance of the propensity score before and after matching is informative to evaluate the adequacy of the matching in terms of the functional form used in the propensity score. Hence, we estimate the propensity score models before and after the matching. Then, we evaluate their performance on the full sample (before) and on the restricted sample of treated and matched control firms given by the particular matching estimator we chose (i.e., the nearest neighbor matching estimator). The performance is gauged through the share of explained variability in the sample (pseudo *R* squared) and the significance of control variables (χ^2). A good propensity score specification implies that: (a) the pseudo *R* squared is higher in the before-matching estimation; (b) the χ^2 is significant before matching and not significant in the after-matching estimation. Results in Table 4 show that for the three exercises with different time lags the pseudo r-squared drops when passing from the unmatched to the matched sample, and the χ^2 statistics is always significant before matching, but not significant after matching.

[Insert Table 4 about here]

The degree of overlapping between the samples of treated and control firms is another indicator of the quality of the matching. We study graphically this issue looking at the distributions of the propensity score for treated and control firms before and after the matching. Figure 1 shows the unmatched distributions of treated and control firms in the case of one year lag estimations. Two considerations can be made: (a) the two distributions have a support that partially overlaps, thus making it possible to conduct the evaluation exercises; (b) the two distributions are different, suggesting that the re-selection of the samples is mandatory to avoid bias in the estimation. Put it differently, differences between awardees and controls firms are statistically significant in the unmatched samples. The matching procedure mitigates the estimation bias: after matching the two distributions present a high degree of overlapping (Figure 2). The matching procedure is able to wash away the selection bias in the untreated sample.

[Insert Figure 1 and Figure 2 about here]

The effect of R&D subsidies

Table 5 reports the conditional difference-in-differences estimates of the average treatment effect on the treated for the objective variables that we selected to evaluate the R&D policy: intangible intensity, employment dynamics, unit labor costs and capital intensity.

[Insert Table 5 about here]

The first indicator refers to intangibles intensity i.e., the ratio between intangibles and total sales of the firm to avoid confounding effects due to sheer size. Figures in the upper box of Table 5 reveal that subsidized firms outperformed units in the control group in terms of innovative effort, after the allowance of the R&D grant. After one year the former experienced an increase of 31 percentage points in the intangibles intensity. After two years, we still record a positive and statistically significant upsurge of 15 percentage points as a consequence of grabbing a subsidy. After three years, however, the difference between the two groups seems to vanish: the difference of 6 percentage points is not significant.

To deepen our investigation of input additionality, we consider the bearings of the R&D policy on the employment dynamics and the quality of human resources. Since employment is measured on a logarithmic scale, the CDID estimates of the average treatment effect on the treated reported in the second box of Table 5 can be interpreted as growth rates arising out from the awarding of an R&D grant. We record a statistically significant 13.8% upsurge in the workforce of subsidized firms after two years, and a 29.3% difference in employment dynamics after three years, that although marginally, still distinguishes the behavior of the two groups.

Besides, receiving an R&D grant can positively influence the quality of the labor force. To address this issue we consider unit labor cost as a proxy of the skill level of employees. Results in the third box of Table 5 show that unit labor costs of treated firms increased more than those ones in the control group. After one year the estimated average treatment effect on the treated is around € 26 thousand, after two years € 11 thousand, and after three years € 6.5 thousand. Hence, the investment in innovative activity spurred by the policy intervention seems to bring about a change in labor composition of treated firms.

Finally the lower box in Table 5 indicates that the behavior of treated and control firms in terms of investments in fixed assets is not significantly different at all time lags considered in the analysis.

To summarize: results suggest that the R&D policy had a positive and significant effect on the input side of the innovation process. After receiving the financial support, treated firms recorded a significantly higher variation in their intangible assets, the number and the quality of human resources. These differences appear stable over time and tend to last even three years after the assignment of the R&D grant: Moreover, the results are quite robust even though the number of treated firms on which we can assess the effect of the policy shrinks when we look at a longer time span.

We now move forward to evaluate whether the R&D policy has had an impact also on the overall performance of subsidized firms. The results shown in Table 6 refer to the impact of the R&D program on the performance of awarded firms as measured by total sales, labor productivity and profitability. All three indicators reveal that differences between the two group widen as we consider a longer time span and that subsidized firms tend to fare better than their counterparts. In particular, the dynamics of suggests that awarded firms would experience a 21% increase in total sales after three years, although these ATT is only significant at the 10% level. More generally, the lack of statistical significance for all observed differences and regardless of the measure of performance considered, prevents us from deriving any conclusion on the alleged effect of the policy on the overall firm performance.

[Insert Table 6 about here]

While the reduction in the number of treated firms in our sample can likely account for most of the foregoing evidence, it is worth noting that our results are consistent with earlier findings at the national level (Merito *et al*., 2007) and corroborate the idea that an increased level of investment spurred by the R&D policy not necessarily translate into higher levels of performance at the firm level. Accordingly, even though companies are ready to adopt new knowledge and technologies, they are less able to exploit them efficiently. For example, previous findings on the dynamics and the determinants of labor productivity in the province of Trento (Pedrotti *et al*., 2008), strengthen this idea. In particular, it has been shown that the labor productivity is stagnant over the period 2001-2006 for the entire economic system, and even declining in the manufacturing and construction industries. Moreover, after decomposing the labor productivity indicator into (i) an index that captures the evolution of capital deepening and (ii) an index that gauges the dynamics of the multi-factor productivity, it comes out that a contraction in the total factor productivity entirely accounts for the flattering, or even decreasing, pattern observed in labor productivity. Hence, while the work force has been endowed with renewed capital along the period of analysis, the organization did not succeed in effectively combining capital and labor to enhance their operational performance. There is another factor that may account for the null effect on labor productivity: complementarities between intangible assets, skilled labour force and firms' reorganization. In other words, investments in intangible asset require an adequate level of human capital and firms' reorganization, for example the presence of an R&D function inside the firm, in order to release their full potential.

Robustness checks

In what follows we present some robustness checks concerning the observed additionality in the objective variable intangible intensity.[10] The first two boxes in Table 7 show the CDID estimates of the average treatment effect using the nearest neighbor estimator with one and three neighbors. In line with the evidence discussed above, we obtain positive and significant effects that range from about 30% in the first year to 16% in the second year after a grant is awarded. Moreover, when the nearest neighbor matching estimator with three matches is used, we also find positive bearings of the R&D policy after three years: the average treatment effect in this case is equal to 15%.

In the second robustness check we restrict the sample to medium-high and high technology firms that represent the bulk of subsidized companies in our sample and can be expected to carry out the projects with the high expected returns from a social point of view. Also in this case, results outline that the R&D policy under scrutiny successfully spurred a significant amount of additional investments in intangible assets.

The availability of a panel data allows us to run a final indirect test of unconfoundedness by regressing the treatment effect on a lagged objective variable -$Y_i(t_0)$ - which is by definition not affected by the treatment. If treatment effect is not zero this implies that the distribution of treated units is not comparable to the distribution of control. If the treatment is zero it is more plausible that the assumption holds (Imbens, 2004). Table 4 shows the results of the test where the intangible intensity is the objective variable and the one, two and three years lags are considered. In all the CDID estimations intangible intensity are not statistically different for subsidized and not subsidized firms as confirmed by the small differences between the

[10] Results of the robustness checks involving other variables are available from the authors on request.

two groups and the significance level of *t-tests*. This result provides us, as discussed above, an evidence that the unconfoundedness assumption holds in our context.

[Table 8 about here]

6. Conclusions

This paper empirically investigates whether public R&D funding in the province of Trento fostered private firms R&D investment (intangible assets investments) and improved firms' performance (labour productivity and operating margin) over the period from 2002-2008,. In order to accomplish this task, we build up a very appropriate dataset that combines firms' balance-sheet data with information on the specific projects that firms carried out from administrative archives.

The investigation of the effectiveness of the R&D policy is carried out using counterfactual methods: treated firms (the population amounted to 89) were matched with around 335 control firms each year. The latter were carefully selected, against predetermined variables, in order to guarantee the closest similarity with treated firms.

The paper contributes to the existing literature in several ways. It takes into account the effectiveness of R&D place-based intervention, a topic that has received so far little scrutiny, despite the increasing regionalization of innovation policies. Moreover, confining our ex post R&D policy evaluation to the province of Trento guarantees a much closer similarity among treated and non-treated firms than one can find comparing nationwide firms, thus reducing heterogeneity that could undermine the robustness of counterfactual methods. By analyzing firms that received only R&D subsidies (and no other incentive), we have been able to neutralize the potential confounding effects associated with multiple incentives assigned by different sources.

Unlike most of the empirical studies, we know the amount of financing each firm receive and the detailed expenses such an amount is going to be used for. This implies that we can go beyond a potential crowding out concern and directly address the issue of additionality.

The empirical analysis reveals that an R&D grant has a positive, significant effect on the input side of the innovation process. One year after receiving the financial support, the group of treated firms record significantly higher level of intangible intensity than the control group.

The differential lasts two years after the assignment of the R&D grant. Moreover, subsidies positively impinge on the quality of the workforce leading treated firms to a significantly higher investment in human capital. As for the effect of the policy intervention on the overall performance of the firm, we do not observe any distinctive effect either on labor productivity or on profitability.

While providing a partial positive assessment of the effectiveness of the PL6, we know that there remain a number of issues to deal with so as to achieve a comprehensive investigation of this public intervention. First, the existence of complementarities between the acquisition of knowledge and organizational changes deserves further investigation to shed some light on the reason why firms seem not able to exploit the R&D investments fostered by the PL6. Second, localized spillovers may arise as an indirect effect of the place-based policy under scrutiny. For example, recent contributions (Roper *et al*., 2004) point to the importance of the nature of the R&D project and the surrounding innovation system as two major forces that make it more likely for the host region to appropriate the benefits of private R&D activities.

7. AKNOWLEDGEMENTS

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Table 1. Sample firms by technological sector, year and treatment status (inclusion in the program LP6) - amount of subsidies per year.

	low tech		**low-mid tech**		**high-mid tech**		**high tech**		**total**		**Co-financed amount of subsidies (current values €)**			
	NSF*	SF**	NSF*	SF**	NSF*	SF**	NSF*	SF**	NSF*	SF**	average	std. dev.	min	max
2001	57	0	166	0	77	0	39	0	339	0				
2002	52	1	142	0	69	3	37	3	300	7	706,394.8	1,276,796.0	30,180.0	4,720,260.0
2003	52	2	154	2	54	3	46	8	306	15	993,488.5	617,534.5	46,559.9	1,717,250.0
2004	46	2	121	2	41	5	47	1	255	10	603,212.8	3,376,766.0	133,499.6	12,000,000.0
2005	49	2	134	2	55	3	52	5	290	12	2,068,161.0	864,055.0	47,362.5	2,606,848.0
2006	77	2	200	5	71	6	66	7	414	20	916,883.0	770,804.0	59,187.1	2,746,977.0
2007	81	2	226	6	73	8	72	9	452	25	887,238.8	1,287,379.0	241,817.2	4,666,648.0
2008	166		359		159		142		826					
Total	580	11	1502	17	599	28	501	33	3182	89				

*Notes: * NSF: not subsidized firm; **SF: subsidized firm.*

Table 1 includes both treated firms and all the “potential controls”. Among the latter in each evaluation exercise we choose a subset of controls.

Table 2. Sample structures and block partition according to the Dehejia and Wahba (2002) procedure.

Time lags of the estimated effects	Blocks	on common support Controls	on common support Subsidized	Used obs.	Discarded Treated	Full sample
1 year lag	7	786	87	866	2	1124
2 years lags	5	495	61	551	5	879
3 years lags	6	378	39	417	5	661

Table 3. Propensity score estimations for different time lags. Dependent variable: Prob(Subs=1,t_1).

	One year lag			Two years lags			Three years lags		
control variables:	Coef.	Std. Err.	z	Coef.	Std. Err.	z	Coef.	Std. Err.	z
Capint(t_0)	-0.021	*0.085*	0.250	0.078	*0.122*	0.640	-0.009	*0.184*	-0.050
Inta(t_0)	0.000	*0.000*	0.430				0.000	*0.000*	-1.380
TSxempl(t_0)	-0.001	*0.001*	2.030**	-0.001	*0.001*	-1.410	-0.002	*0.001*	-1.400
TS(t_0)	0.000	*0.000*	2.850**	0.000	*0.000*	2.290**	0.000	*0.000*	2.490**
Inta_int(t_0)	1.089	*0.479*	2.280**	1.004	*0.962*	1.040	2.125	*1.485*	1.430
Inta_int(t_0)^2	-0.351	*0.213*	-1.650*	-0.361	*0.854*	-0.420	-1.075	*1.413*	-0.760
Cashflow(t_0)	0.074	*0.504*	0.150	4.653	*2.568*	1.810*	0.963	*1.062*	0.910
Cashflow(t_0)^2	-0.261	*0.564*	-0.460	-14.969	*9.124*	-1.640	0.305	*0.993*	0.310
Δinta_int(t_0)	0.242	*0.322*	0.750	0.663	*0.802*	0.830			
Age(t_0)	0.005	*0.004*	1.110	-0.003	*0.005*	-0.480	-0.006	*0.007*	-0.810
constant	-2.485	*0.267*	9.320***	-3.097	*0.412*	7.520***	-3.393	*0.505*	6.710***

Notes: Included dummies: technological sector, year, size class (EU definition). Probit specification.

Table 4. Performance of the propensity score specification.

	1 year lag		2 years lags		3 years lags	
	before	after	before	after	before	after
LR χ^2	133.52	8.14	109.42	19.98	80.58	10.64
Prob > χ^2	0.0000	0.9909	0.0000	0.4593	0.0000	0.9091
Pseudo R^2	0.2314	0.0415	0.2626	0.0156	0.2719	0.1229

Table 5. CDID estimations of average treatment effect on treated ($\hat{\alpha}_{DDM}$).

		Intangibles intensity		
	$\hat{\alpha}_{DDM}$	T-stat	Treated	Controls
1 year lag	0.312	2.540	0.288	-0.024
	0.123			
2 years lags	0.152	4.570	0.133	-0.019
	0.033			
3 years lags	0.066	1.270	0.056	-0.010
	0.052			
		Employment (log)		
	$\hat{\alpha}_{DDM}$	T-stat	Treated	Controls
1 year lag	0.048	1.090	0.063	0.015
	0.044			
2 years lags	0.138	2.440	0.144	0.005
	0.057			
3 years lags	0.263	1.760	0.295	0.032
	0.149			
		Unit labor cost		
	$\hat{\alpha}_{DDM}$	T-stat	Treated	Controls
1 year lag	26.429	2.490	26.751	0.322
	10.598			
2 years lags	11.341	3.960	12.508	1.166
	2.862			
3 years lags	6.529	2.190	6.648	0.119
	2.978			
		Capital intensity		
	$\hat{\alpha}_{DDM}$	T-stat	Treated	Controls
1 year lag	0.066	1.120	0.075	0.009
	0.059			
2 years lags	0.028	0.360	0.021	-0.007
	0.078			
3 years lags	-0.061	-0.810	-0.041	0.019
	0.075			

Notes: standard errors in italics.

Table 6. CDID estimations of average treatment effect on treate ($\hat{\alpha}_{DDM}$) –firm performance.

		Total sales (log)		
	$\hat{\alpha}_{DDM}$	T-stat	Treated	Controls
1 year lag	0.023	0.880	0.025	0.002
	0.026			
2 years lags	0.045	1.160	0.068	0.023
	0.038			
3 years lags	0.210	1.830	0.165	-0.045
	0.115			
		Labor productivity (value added per employee)		
	$\hat{\alpha}_{DDM}$	T-stat	Treated	Controls
1 year lag	5.754	0.720	5.389	-0.365
	8.017			
2 years lags	-0.491	-0.090	-1.695	-1.204
	5.549			
3 years lags	16.156	1.380	9.417	-6.738
	11.674			
		Profitability		
	$\hat{\alpha}_{DDM}$	T-stat	Treated	Controls
1 year lag	0.018	1.150	-0.006	-0.024
	0.015			
2 years lags	0.032	1.510	0.014	-0.018
	0.021			
3 years lags	0.040	0.860	0.033	-0.007
	0.046			

Table 7. Robusteness checks: CDID estimations of average treatment effect on treated ($\hat{\alpha}_{DDM}$) - intangibles intensity.

nearest neighbor (3 matches)				
	$\hat{\alpha}_{DDM}$	T-stat	Treated	Controls
1 year lag	0.313	2.690	0.265	-0.048
	0.116			
2 years lags	0.162	4.800	0.131	-0.031
	0.034			
3 years lags	0.152	2.210	0.069	-0.083
	0.069			
nearest neighbor (1 match)				
	$\hat{\alpha}_{DDM}$	T-stat	Treated	Controls
1 year lag	0.268	2.370	0.265	-0.002
	0.113			
2 years lags	0.162	4.320	0.131	-0.031
	0.037			
3 years lags	0.183	1.350	0.069	-0.114
	0.136			
Sample restricted to medium-high and high technology firms				
	$\hat{\alpha}_{DDM}$	T-stat	Treated	Controls
1 year lag	0.350	2.480	0.309	-0.040
	0.141			
2 years lags	0.240	4.200	0.191	-0.050
	0.057			

Notes: standard errors in italics.

Table 8. Imbens (2004) indirect tests of unconfoundedness - intangible intensity.

	$\hat{\alpha}_{DDM}$	T-stat	Treated	Controls
1 year lag	0.005	0.110	0.001	-0.004
	0.045			
2 years lags	0.004	0.170	-0.003	-0.008
	0.026			
3 years lags	-0.003	-0.040	-0.021	-0.018
	0.059			

Notes: standard errors in italics.

Figure 1. Distributions of the propensity score for treated and control subsamples. Not Matched.samples one year time lag.

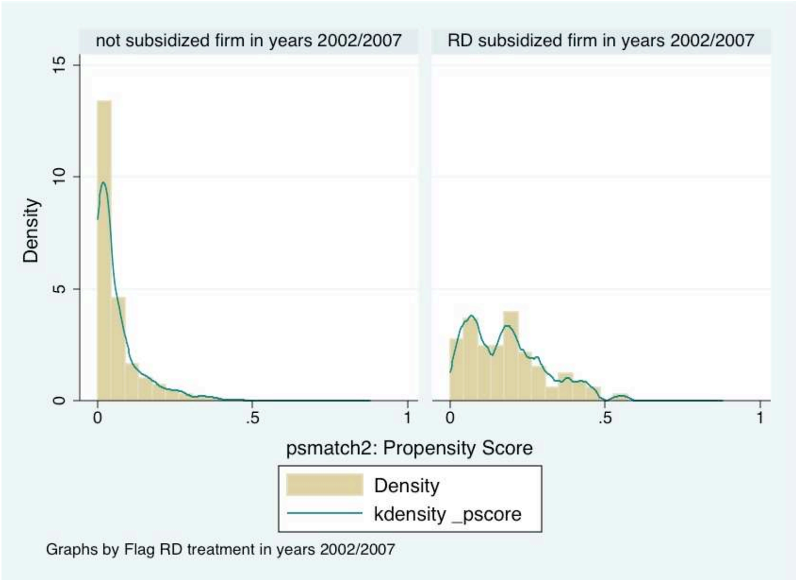

Figure 2. Distributions of the propensity score for treated and control subsamples. Matched.samples one year time lag.