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The Effects of R&D Subsidies to Small and Medium-Sized Enterprises. Evidence from a Regional Program

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Abstract This article evaluates a small-business program implemented in an Italian region, Tuscany, providing small and medium-sized firms with R&D subsidies. To establish whether the subsidy has encouraged non-transitory R&D, enhanced the propensity to intellectual property protection and to collaborative R&D with other firms or research centers, or improved firm performance in general, we estimate a number of potential input, output and behavioral effects that the program might have induced shortly after the completion of the subsidized project. In order to do so, we perform a careful application of matching techniques, using a wide set of pre-subsidy characteristics. We find that the program has been ineffective with respect to the innovation and commercial outputs of small and medium-sized firms, but has encouraged a non-transitory practice of R&D by former non-R&D-performers and contributed to firm upskilling, which may be seen as prerequisites for the creation or the consolidation of absorptive capacity.

Keywords R&D · Subsidies · SMEs · Program evaluation

JEL Classification C21 · L53 · O38

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1 Introduction

Public subsidies to private R&D constitute in most countries one of the main instruments for supporting innovation. The most common argument justifying these programs is based on the presence of market failures, which would lead to insufficient incentives or finance for firms to innovate. The presence of market failures is believed to be particularly important for small and medium-sized enterprises (SMEs) (Peneder 2008). This has greatly encouraged public support in this area through supranational (EU), national and regional programs, with these three levels often interplaying within a multilevel governance framework (OECD 2011).

Although programs in favor of private R&D have been implemented for a long time, the literature devoted to evaluating the effects of these programs has a much shorter history. However, during the last decade or so, this literature has benefitted from the methodological developments achieved in the econometrics of program evaluation (Imbens and Wooldridge 2009; Imbens and Rubin 2015). As a result, the number of works trying to establish whether subsidies are effective has increased rapidly, using both general survey data and ad hoc program data. Although findings are often mixed, some recent surveys have highlighted that the majority of works support the effectiveness of R&D subsidies (García-Quevedo 2004; Zúñiga-Vicente et al. 2014; Caloffi et al 2016).

Small-business programs, which are very common, but are perhaps more likely to be implemented at the regional policy-making level (Blanes and Busom 2004), have received only limited attention in the applied microeconomic literature, although there are some notable exceptions. One of these is Wallsten (2000): focusing on the well known SBIR program in the US, the author finds that subsidies had no effects on R&D expenditure. On the other hand, more recently, both Lee and Cin (2010) and Czarnitzki and Lopes-Bento (2013) find that small-business R&D programs, respectively in South Korea and Belgium, had relevant effects on some major input indicators, such as R&D investment and/or R&D employees. Other contributions, although not specifically focusing on a small-business program, account for possible effects arising for SMEs. For example, both Lach (2002) and Bronzini and Iachini (2014) find that subsidies had a positive effect on the R&D expenditures only of small firms, while González et al. (2005) and González and Pazó (2008) report relevant R&D-inducement effects for smaller non-R&D-performing firms. Other works consider a wider range of outcome variables: for instance, Merito et al. (2010) find that subsidies shifted the employment structure of SMEs towards more skilled workers and raised their employment levels, Bronzini and Piselli (2016) report positive effects on their probability of applying for patents, while Criscuolo et al (2012) report positive effects on employment and investments limited to smaller firms. Most of these studies have focused on effects that are contemporaneous to subsidy receipt (Zúñiga-Vicente et al. 2014). We instead focus on what happens to firms shortly after the completion of the subsidized projects, in order to establish whether the subsidy has encouraged non-transitory R&D, enhanced the propensity to intellectual property protection or to collaborative R&D with other firms or research centers. As will be explained later in this article, these are aspects of major importance with small-business programs, which often aim to promote behaviors that SMEs are reluctant to adopt. We will use and combine elements derived from the more traditional economic approaches, in which effects are expressed in terms of the inputs and outputs of a “black-box” innovation process, with some elements inspired by evolutionary and managerial approaches that stress the importance of considering outcomes related to capacity development and learning that may result into the firm changing its usual innovative behavior. This combination of elements represents, in our view, an advancement with respect to previous works.

We analyze data from a small-business R&D program aimed at product innovation implemented in an Italian region—Tuscany—which is far from being a depressed area but is characterized by a relatively low aggregate level of private R&D expenditure and a very high presence of micro and small firms. This situation can be summarized by the “innovation without R&D” catchphrase that applies not only to Italy but also to an important part of the economy of other countries that are considered the forefront of industrial innovation (Gottardi 1996; Hervas-Oliver et al. 2011; Som 2012).

We opt for an identification strategy that rests on the unconfoundedness assumption (Rosenbaum and Rubin 1983b), which is made plausible by using a vast number of pre-treatment variables, most of which are time-varying. Moreover, our analysis is made robust by the choice of a bias-adjusted matching estimator (Abadie and Imbens 2011), a doubly robust procedure that helps overcome the problem, highlighted in previous studies (e.g. Cerulli and Potì 2012), of the extreme sensitivity of the results with respect to the matching technique employed. We also provide some hints on how this strategy may be performed in a small-sample context, which is typical of small-scale programs. In particular, we will show how precision of estimates may be improved, while keeping bias under control, by establishing an appropriate number of matches in the presence of many matching variables but relatively few observations. This situation is quite common in practice, but the literature provides very little guidance (Imbens and Rubin 2015). Finally, we assess how credible unconfoundedness is in our specific application by implementing the analysis on pseudo-outcomes (Imbens and Wooldridge 2009).

Our results show that subsidies stimulated the upskilling of SMEs and boosted a non-transitory practice of R&D activities. This latter outcome occurred especially in the presence of former nonperformers of R&D, for which we find a clear evidence of an effect. Although no other significant positive effect was found, we believe that upskilling and R&D inducement are effects of no minor importance.

The paper proceeds as follows. Section 2 briefly recalls the market failures that justify public intervention in favor of private R&D of SMEs, puts this paper in the context of the wide body of applied literature on input, output and behavioral effects. Section 3 defines the set of outcome variables of interest. Sections 4 and 5 are devoted to the illustration of data and of the empirical strategy, while Sect. 6 presents the empirical application and its main results. Section 7 concludes.

2 Public Support to Private R&D in the Case of SMEs

It is widely known that the rationale of public intervention in support of private R&D mainly resides in two distinct sources of market failure, which are particularly relevant in the presence of R&D activities: the first source is connected to the presence of externalities and public goods, the second one to capital market imperfections (Martin and Scott 2000; Peneder 2008; Hall and Lerner 2010; Haapanen et al. 2014).

It is also widely accepted that these failures may affect SMEs in particular, preventing them from investing and discouraging risk assumption (Trajtenberg 2001; Hyytinen and Toivanen 2005; Czarnitzki 2006; Lee and Cin 2010).

With reference to externalities, SMEs seem to face greater difficulties in the internalization of technological spillovers (Gans and Stern 2000). This happens because it is relatively infrequent that they make use of secrecy or of expensive tools of intellectual property protection, such as patents (Kitching and Blackburn 1999).

With respect to information asymmetries and related financing constraints, it is well known that these problems seriously affect innovative start-ups and SMEs, as these are relatively lacking in track records (having for example, as in the Italian case, financial statements in simplified form, or none at all) and collaterals, or also, from the financier's viewpoint, the volume of finance they require may not be worth the start of a costly risk-assessment procedure (Peneder 2008). In addition, as for innovative start-ups, constraints may be even stronger as they lack also reputation; moreover new entrepreneurs might be reluctant to disclose confidential information about the characteristics and the potential of their innovative projects, thereby making it even more difficult to assess risk by the lender (Hall 2002; Carpenter and Petersen 2002; Takalo and Tanayama 2010).

For the reasons mentioned above linked to the presence of information asymmetries, SMEs strongly rely on internal finance for their R&D investments (Himmelberg and Petersen 1994; Hall and Lerner 2010; Czarnitzki and Hottenrott 2011).

In view of the approaches discussed so far, tied to a static conception of market failure, the economic literature of evolutionary inspiration adds a focus on internal and inter-organizational learning processes, which strongly rely on the availability of internal skills and competencies, as well as on the firm's capability to absorb external knowledge and match it to its own. Therefore a further major obstacle may be identified in the relatively limited set of skills and capabilities of SMEs. In particular, as noted by Ortega-Argilés et al. (2009), SMEs generally tend to invest little in R&D due to a lack of knowledge about how and where to acquire the necessary skills to do so. In addition, as Trajtenberg argues (2001, p. 433), "younger/smaller firms are disadvantaged relative to large firms in terms of a wide range of competencies and experience that are complementary to R&D, be it in marketing, pure management, access to complementary know how, etc.". For these reasons, SMEs often encounter many difficulties in establishing R&D partnerships with external parties, be they other companies, universities, research centers or other technology providers; such relations are, however, theoretically desirable (Bozeman 2000; Hagedoorn et al. 2000). Even the empirical literature finds that R&D partnerships are beneficial for SMEs (Audretsch et al. 2002; Busom and Fernandez-Ribas 2008), especially when the technology providers are able to understand and adapt to the skill needs of SMEs.

Finally, evolutionary literature points out that SMEs tend to carry out, if any, informal R&D activities (Kleinknecht and Reijnen 1991), often relying on non-permanent departments, or entrusting this task to unspecialized personnel that is allocated also to other activities in the enterprise. The fact that SMEs do not perform R&D, or do so only intermittently and in a semi-structured way (Rammer et al. 2009), entails that the barriers to the absorption of knowledge and inward technological spillovers remain high (Cohen and Levinthal 1990). It is also emphasized that the SMEs' propensity to R&D, be it formal or informal, often depends on the characteristics of the sector and on the related technological dynamics (Acs and Audretsch 1990; Breschi et al. 2000). Especially in lower technology industries it is common to find widespread practices of innovation without R&D (Hervas-Oliver et al. 2011; Som 2012). This issue has been object of a long-lasting debate in Italy, a country characterized by a vast prevalence of SMEs, many of which belonging to low or medium technology sectors (Gottardi 1996).

3 Different Types of Effects and the Choice of Outcome Variables

The peculiarities of SMEs require defining an approach for the evaluation of R&D programs that specifically targets this type of firms. The typical goals of small-business R&D programs do not usually refer to one-dimensional outcomes; instead these programs often pursue a variety of goals. For this reason, we will employ, in this study, combination of input, output and behavioral outcomes. All these are relevant aspects with small-business programs, which often aim to promote behaviors that SMEs are reluctant to adopt for the reasons mentioned in the previous Section. The choice of the outcomes of interest is also motivated by the fact that the program under analysis was aimed at product innovation.

The literature usually distinguishes three types of firm-level outcomes that might descend from an R&D subsidy: effects on the input or on the output of the innovation process (David et al. 2000; Klette et al. 2000; Cerulli 2010), and behavioral effects (Buisseret et al. 1995). Studies inspired by industrial organization literature usually focus on input effects, which are related to R&D expenditures, directly descend from theoretical models and may be empirically identified by means of both structural (Wallsten 2000; Busom 2000; González et al. 2005; Takalo et al. 2013) and reduced-form approaches (Lach 2002; Almus and Czarnitzki 2003; Görg and Strobl 2007; González and Pazó 2008; Bronzini and Iachini 2014; Bocci and Mariani 2015 to cite just a few). A considerable number of studies focus on (or also on) the side of outputs, and take into consideration a variable set of innovation outcomes (Hujer and Radic 2005; Czarnitzki et al. 2007; Hussinger 2008; Bérubé and Mohnen 2009; Arvanitis et al. 2010) or also more generic proxies of firm performance (e.g., Merito et al. 2010; Cerqua and Pellegrini 2014), mostly relying on non-structural approaches. Finally, a third strand of literature puts forward the concept of behavioral effects, based on theoretical arguments mostly drawn from evolutionary, management and organizational literatures. According to this approach, a program is successful if it is able to foster organizational and inter-organizational learning (Clarysse et al. 2009) and to raise permanently the firms' capacities that are essential for innovation activity (Busom and Fernandez-Ribas 2008; Gök and Edler 2012; Antonioli et al. 2014; Marzucchi et al. 2015).

As shown in Caloffi et al (2016), who perform a comprehensive meta-analysis on the recent Italian program evaluation literature related to enterprise and innovation policies, public intervention in this area is quite likely to have positive effects especially on outcomes that are directly targeted by the schemes themselves, much less on other outcomes. With respect to an R&D program, this means that the probability of finding positive effects on the inputs of the innovation process is higher than that of having such positive effects on outputs, performance and other behaviors.

In fact, connected to the issue of what effects should be searched, another important topic is when these effects are expected to arise. Whereas the subsidy may be expected to change inputs immediately, it may take some time before a change in outputs (e.g. patents, turnover, etc.) and behaviors (e.g. the propensity to engage in R&D collaboration with other firms, Universities or other technology suppliers) takes place, so the choice of an appropriate timing is an important issue.

So far, much attention has been devoted to the evaluation of effects that are contemporaneous to the receipt of R&D subsidies, i.e. to establish if subsidies are more likely to substitute or complement private R&D expenditure. However, even effects on R&D inputs may be distributed over time for the following reasons. First, as for example Klette and Møen (2012) have argued, positive effects may be expected to arise later due to the fact that the implementation of the program induces learning-by-doing in R&D activities and thus changes the firms' profit opportunities in favor of more R&D-intensive products. Second, one should not forget that firms could face adjustment costs (Lucas 1967) in the implementation of R&D investments. In the case of SMEs, these costs can be due to the hiring of new skilled R&D personnel, to the reallocation of personnel from production to R&D activities (Zúñiga-Vicente et al. 2014) or also, in case the firm is a non-R&D performer, to the setup of R&D facilities from scratch. For empirical evidence supporting the idea of non-immediate input effects see for example Lichtenberg (1984), Lach (2002) or Klette and Møen (2012).

With respect to outputs and behaviors, one should obviously allow for a time lag and extend the analysis over a longer time horizon, which is often made impossible, unfortunately, due to constraints in the available data.

In this article we will focus on the effects of an R&D small-business program, as measured by the first available yearly data after the completion of the subsidized projects,[1] on a number of simple outcome variables on which the subsidy might reasonably induce change. On the side of effects on inputs, this timing is adequate to see whether the program increases the innovation effort of SMEs also beyond the relatively short time dedicated to the subsidized project. To this end, we will analyze if the subsidy has encouraged SMEs to continue (or increase) R&D investment, to establish proper R&D departments or, more in general, to hire a better educated labor force. These are signals of an increased awareness of SMEs about the importance of fuelling the innovation process with adequate ingredients. All these elements refer to knowledge and competence accumulation and may also be seen as prerequisites for the enhancement of the firm's absorptive capacity. We prefer not to consider in this analysis other generic input indicators that could be observed through balance-sheet

[1] This corresponds to a time range of 1–1.5 years after the subsidized project was closed out, when the subsidy could no longer be part of the firm's R&D investment (if any). As will be explained in Sect. 4, we expunged from the analysis all firms that took more than one subsidy throughout the period in question. It is true that the timing of project outcomes can differ across industries, depending on the technologies employed, and so on. Of course, it could be interesting to explore all timings of effects, but such a comprehensive analysis is beyond the scope of our paper and is infeasible with the available data.

data, such as the stock of tangibles or intangibles, as these are less directly informative about the characteristics of the R&D process undertaken after the program.

On the side of the effects on outputs that might stem from product innovation, one year after the completion of the subsidized project is perhaps a short time to expect considerable improvements, although we may argue that some of these improvements—especially those related to the firms' economic performance—should not necessarily require a long wait if the subsidized innovation projects are relatively small. At any rate, this is the best we can do with the available data. To this regard, we will evaluate not only if participation in the program has increased some generic outcomes, such as turnover, but also if it has encouraged the filing of intellectual property rights (IPR) applications, such as patents, industrial designs or copyrights. Being focused not only on patents, this latter outcome indicator is intended to capture forms of protection that are quite common also in lower technology industries. Again, we prefer not to consider in this analysis other generic outputs that could be observed through balance-sheet data, such as for instance firm productivity, profitability, as the logical linkage between a small R&D subsidy aimed at product innovation and productivity or profitability seems to us rather uncertain (if not passing through turnover, which is considered already).

Finally, in line with the previous literature on behavioral effects, we will verify if the grant has pushed beneficiary SMEs to start (or increase) R&D partnerships with academia or with other firms, i.e. to adopt a (more) cooperative behavior in the area of innovation.

4 Data from a Small-Business Program Supporting R&D

The regional government of Tuscany (one of the Italian regions) implemented, in 2003 and 2004, a program consisting of the delivery of public subsidies to single firms in favor of private intramural R&D investments aimed at product innovation. This program, corresponding to measure 1.1.1b "Aids to pre-competitive development" of the Single Programming Document 2000–2006, was exclusively directed to SMEs, including low or medium technology sectors, as these represent the overwhelming majority of the Tuscan manufacturing. The program was funded by the combination of financial resources from both the European Regional Development Fund (ERDF) and Italian National Law 598/94. The meta-goal of this policy, as documented in the related regional programming documents, was to promote the expansion and the upgrading of business activities and to facilitate an approach to market segments characterized by the presence of more innovative products. The program posed only a few restrictions in terms of sectoral affiliation of possible applicants: all manufacturing sectors and business services were eligible for funding. Nor were there specific technological goals specified, except for the fact that companies had to develop product innovation. This approach was consistent with the philosophy of a bottom-up action, at that time very common among Italian policymakers (Rolfo and Calabrese 2003). Nonetheless, the incentives were not granted automatically, but through a selection procedure based on the evaluation of submitted projects by a committee of experts. As a result of this, a few projects were not admitted to funding (30.8% of applications filed by eligible firms were rejected).

Eligible projects had to last no longer than 18 months and had to be of small or medium size: their total value could not exceed 750 thousand Euros. The aid could cover 35–40% of the total project value, depending on a number of conditions specified in the call for tender. One half of the aid given to each firm consisted of a non-repayable contribution, while the remaining 50% was repayable—in installments—within three years. According to the call for tender, eligible investments included costs for R&D personnel, R&D materials and equipment, specific consultancies or acquisition of intangibles connected to the implementation of the R&D project, and a limited amount of other general R&D costs.

The data we primarily focus on refer to beneficiary firms that incepted and completed the co-funded R&D project. Now, the possibility to identify a causal effect attributable to the incentive requires that no beneficiary firm received more than one subsidy. If this does not hold, the identification of a causal effect becomes very difficult, as the effects of different aids may be additive. We thus expunged firms which, although having completed the co-funded project, participated in other public R&D programs (19 firms)—throughout the period in question—both at the national and at the regional levels. The implication of this choice is that our inference is valid for all participants—the overwhelming majority—who took only the R&D subsidy under investigation. We gathered the balance statements of all these firms and of all eligible non beneficiaries from the Aida–Bureau Van Dijk commercial dataset, starting from two years prior to the first call for tenders. We also reconstructed the amount of individual exports from trade archives held by the Italian National Institute for Statistics (ISTAT) based on customs declarations. This information allowed us to define a first set of potential control firms (henceforth these firms will sometimes be referred to as *controls*), which were roughly similar to the former—before the program was put in place—at least in terms of some basic business characteristics (age, sector, legal form, etc.), key balance sheet data and export behavior. The definition of a set of potential control firms rested on a matched sampling of the control reservoir, whose goal was that of obtaining a manageable set of controls for further analysis (Rosenbaum and Rubin 1985): after estimating a propensity score based on available pre-treatment values of covariates, we selected a pool of potential control firms by matching each beneficiary to its five nearest neighbors, without replacement.

We then launched a direct survey to this population of 754 companies (120 of which beneficiaries, the set of 634 matched potential controls also includes the 62 firms whose application for the subsidy was rejected).[2] Structured telephone interviews were based on a questionnaire aimed at collecting original data on the past R&D and innovation strategies of firms, as well as on their production and marketing strategies. The questionnaire allowed us to obtain some longitudinal information on R&D and other innovation inputs, outputs and behaviors, otherwise unavailable in the datasets previously at our disposal.

[2] The full population of 120 beneficiaries was interviewed, also thanks to a written invitation by local authorities administering R&D programs we could send to firms accompanying our request. Therefore we have no problems of non-response and, thus, of representativeness for ATT estimation purposes.

Table 1 presents some descriptive statistics on selected pre-subsidy variables, acquired also through the interviews, in the two groups of beneficiary and matched potential control firms. All variables are described in the “Appendix”. Where present, subscripts indicate how many years before treatment the variable refers to.

The table shows that, before the implementation of the program, beneficiary firms invested more in R&D and had more stable R&D personnel than potential controls. In addition, they were more experienced in (and thus oriented to) product innovation, they were more used to carry out innovation in collaboration with other firms or universities. They were also relatively more oriented to intellectual property rights (IPRs in the table). These elements highlight that the two groups are still far apart from each other, in spite of the fact that we have preliminary matched a set of potential controls based on balance-sheet and export data. We will address this selection problem by means of a very careful application of matching techniques aimed at selecting, within the wide set of potential controls, a subset of unsubsidized firms that are really comparable to the subsidized ones.

5 The Empirical Strategy

The evaluation of public incentives to R&D should determine what part of the performance of recipient firms is attributable to the aid provided (treatment), net of other factors characterizing beneficiaries, regardless of their exposure to treatment. The starting point of the program evaluation literature is the definition of the causal effect(s) of interest. A causal effect is, in general, a contrast of what we observe after the intervention has been implemented and what we would have observed, at the same point in time and on the same subjects, if the intervention had not taken place. Postulating the existence of only two potential situations for each unit (firm) reflects the acceptance of the Stable Unit Treatment Value Assignment assumption (SUTVA, Rubin 1980), which assures that units do not interfere with one another and that there are no hidden variations of the treatments. With respect to an enterprise program, the hypothesis of non interference rules out that the intervention on some firms may affect the result of firms that do not participate in the program, which is rather credible when the size of the intervention, as in our case (see Sect. 4) is sufficiently small (Arpino and Mattei 2016), much less in the presence of subsidies of considerable size (Cerqua and Pellegrini 2017). In economic terms, impact evaluation based on the SUTVA refers to a partial equilibrium perspective, neglecting or assuming away possible general equilibrium effects.

Assuming a binary treatment, we use the potential outcome approach (see Holland 1986) to define a causal effect as the comparison of the potential outcomes on the same unit measured at the same time: $Y(0)$ = the value of the outcome variable Y if the unit is exposed to treatment $T = 0$, and $Y(1)$ = the value of Y if exposed to treatment $T = 1$. Only one of these two potential outcomes can be observed, $Y^{obs} = Y(1)*T + Y(0)*(1-T)$, yet causal effects are defined by their comparison $Y(1) - Y(0)$. The econometric literature has largely focused on average effects of the treatment defined over an underlying population. Here, we focus on the population of treated units: the causal effect of interest is the average effect on the treated (ATT): $E(Y(1) - Y(0)| = 1$.

Table 1 Selected descriptive statistics of firms prior to treatment

Variable	Treated firms (120 obs.)		Matched potential control firms (634 obs.)		Standardized mean difference	Variance ratio
	Mean	Std. dev.	Mean	Std. dev.		
Employees$_{-1}$	34.682	34.997	24.181	51.814	0.238	0.456
Employees$_{-2}$	34.389	35.157	24.041	50.715	0.237	0.481
Graduated employees$_{-1}$	2.300	4.226	2.274	14.798	0.002	0.082
Graduated employees$_{-2}$	2.175	3.867	2.189	13.459	− 0.001	0.083
R&D personnel$_{-1}$	3.992	5.278	1.774	6.517	0.374	0.656
R&D personnel$_{-2}$	3.842	5.148	1.707	6.105	0.378	0.711
R&D investment$_{-1}$(th. of Euros)	193.617	243.905	84.487	861.042	0.172	0.080
R&D investment$_{-2}$ (th. of Euros)	192.467	244.538	100.898	1218.533	0.104	0.040
Turnover$_{-1}$ (th. of Euros)	6751.429	7628.501	4681.078	11900.000	0.207	0.410
Turnover$_{-2}$ (th. of Euros)	6550.784	6937.218	4823.556	11800.000	0.179	0.348
Value added p.e. $_{-1}$ (th. of Euros)	39.589	16.797	38.886	31.315	0.028	0.288
Value added p.e.$_{-2}$ (th. of Euros)	40.046	18.081	37.912	48.894	0.058	0.137
Previous IPRs$_{-1,-2,-3}$	0.292	0.456	0.219	0.414	0.166	1.215
Collaborative inter-firm innovation$_{-1}$(1/0)	0.200	0.402	0.106	0.308	0.264	1.704
Collaborative inter-firm innovation$_{-2}$(1/0)	0.192	0.395	0.109	0.312	0.233	1.608
Technology transfer $_{-1}$(1/0)	0.192	0.395	0.074	0.262	0.350	2.273
Technology transfer$_{-2}$(1/0)	0.183	0.389	0.077	0.267	0.318	2.114
Rating$_{-1}$ (prob. of default)	0.009	0.006	0.007	0.006	0.294	1.128

Table 1 continued

Variable	Treated firms (120 obs.)		Matched potential control firms (634 obs.)		Standardized mean difference	Variance ratio
	Mean	Std. dev.	Mean	Std. dev.		
Rating_{-2} (prob. of default)	0.008	0.004	0.007	0.005	0.257	0.562
$\text{Cashflow/turnover}_{-1}$	0.046	0.058	0.051	0.158	– 0.046	0.136
$\text{Cashflow/turnover}_{-2}$	0.041	0.052	0.035	0.099	0.077	0.274
$\text{Export/turnover}_{-1}$	0.218	0.293	0.238	0.323	– 0.066	0.825
$\text{Export/turnover}_{-2}$	0.231	0.315	0.234	0.313	– 0.011	1.019
In medium-high to high tech sectors or KIBS (1/0)	0.317	0.467	0.374	0.484	– 0.120	0.931
With product innovation experience (1/0)	0.708	0.456	0.468	0.499	0.501	0.835
With R&D investment history (1/0)	0.792	0.408	0.421	0.494	0.818	0.681
With a own tradmark (1/0)	0.742	0.440	0.700	0.458	0.092	0.919

The standardized mean difference of a given variable X is defined as the difference of means in the two groups divided by the pooled standard deviation: $SMD_X = \left(\overline{X}_T - \overline{X}_C\right) / \sqrt{\text{var}(X)_T + \text{var}(X)_C/2}$ (as in Abadie and Imbens 2011). The variance ratio is defined as $VR_X = \text{var}(X)_T / \text{var}(X)_C$
IPRs intellectual property rights, *KIBS* knowledge-intensive business services

ATT is often a more interesting estimand than the overall average effect, as it refers to the subjects who actually, for various reasons, took the treatment and for whom the treatment was intended.

Identifying and estimating causal effects from observational (i.e. non experimental) studies requires the introduction of some assumptions. Let us first define the assignment mechanism, a stochastic rule for assigning treatments to units and thereby for revealing $Y(0)$ or $Y(1)$ for each unit, $P(T = 1|Y(0), Y(1), X)$. This assignment mechanism can depend on measurements $Y(0)$, $Y(1)$, X. If these measurements are all observed values, then the assignment mechanism is ignorable (Rubin 1974); if, given observed values, it involves missing values, possibly even missing Y's or X's, then it is non-ignorable. Unconfoundedness is a special case of ignorable missing mechanisms and holds when $P(T{=}1 \mid Y(0), Y(1), X) = P(T{=}1 \mid X)$ and X is fully observed. Unconfoundedness is similar to the so called "selection on observables" assumption and amounts to assuming that exposure to treatment is random within the cells defined by the variables X. The plausibility of this assumption relies heavily on the amount and on the quality of the information on the unit contained in X. As it will be detailed in the next section, we can use an high number of pre-treatment covariates and their lagged values, which provide a detailed description of the firms: it should also be emphasized that using a large number of covariates may increase the chance to intercept, at least indirectly, the role that any unobservable variables may have played in determining participation in the program. This chance is even higher if time-varying covariates are used, so as to account for similarity in trends, and not only in levels. Under unconfoundedness one can identify the average treatment effect within subpopulations defined by the values of X:

$$\begin{aligned} E(Y(1) - Y(0) \mid X = x) &= E(Y(1) \mid X = x) - E(Y(0) \mid X = x) \\ &= E(Y(1) \mid T = 1, X = x) - E(Y(0) \mid T = 0, X = x) \\ &= E(Y^{obs} | T = 1, X = x) - E(Y^{obs} | T = 0, X = x) \end{aligned}$$

and also the overall ATT as :

$$E(Y(1) - Y(0) | T = 1) = E(E(Y(1) - Y(0) \mid T = 1, X = x))$$

where the outer expectation is over the distribution of X in the population of treated units ($T = 1$). When the covariates are more or less continuous, so some smoothing techniques are in order: under unconfoundedness several estimation strategy can serve this purpose. Regression models have some pitfalls: unless there is a substantial overlap of the covariates' distributions in the two groups, with a regression model one relies heavily on model specification, i.e. on extrapolation, for the estimation of treatment effects (Rubin 2008). Therefore it is crucial to check the extent of the overlap between the two distributions (Crump et al. 2009). An approach that can be followed is to reduce the problem to a one-dimensional one by using the propensity score, that is, the individual probability of receiving the treatment given the observed covariates $p(X) = P(T = |X)$. In fact, under unconfoundedness the following results hold (Rosenbaum and Rubin 1983b):

1. T is independent of X given the propensity score $p(X)$;
2. $Y(0)$ and $Y(1)$ are independent of T given the propensity score.

From (1) we can see that the propensity score has the so-called balancing property, i.e., observations with the same value of the propensity score have the same distribution of observable (and possibly unobservable) characteristics independently of the treatment status; from (2), exposure to treatment and control is random for a given value of the propensity score. These two properties allow us to (a) use the propensity score as a univariate summary of all the X to check the overlap of the distributions of X, and (b) use the propensity score in the ATT (or ATE) estimation procedure as the single covariate that needs to be adjusted for. In this paper we will use the estimated propensity score to serve purpose (a), and then use it as in (b) as a distance measure in the bias-adjusted matching estimator proposed by Abadie and Imbens (2011).

The assumption that the treatment assignment is unconfoundeded underlies much of the recent economic policy intervention evaluation strategies, so that one might have the impression that researchers no longer pay much attention to unobservables. The problem of the analyses involving adjustments for unobserved covariates, such as the Heckman's type corrections (Heckman and Hotz 1989), is that they tend to be quite subjective and very sensitive to distributional and functional specification. This has been shown in a series of theoretical and applied papers (e.g., LaLonde 1986; Dehejia and Wahba 1999; Copas and Li 1997).

Thus, despite the strength of the unconfoundedness assumption that, nevertheless, cannot be tested, it is very hard not to use it in observational studies: it is then crucial to adjust the "best" possible way for all observed covariates. The issue of unobserved covariates should then be addressed using sensitivity analyses (e.g. Rosenbaum and Rubin 1983a; Ichino et al. 2008), or by performing the analysis on pseudo-outcomes (Imbens and Wooldridge 2009) as will be illustrated and implemented in Sect. 6.

The estimated propensity score (once correctly specified and analyzed) can be used in the estimation methods that rely on matching: each unit is matched to one or more untreated units with the same (or a close) value of the propensity score. The process of matching is unconstrained by any parametric assumptions regarding the relationship between Y and T, and highly reduces the risk of obtaining estimates of the causal effect by comparing non-comparable subjects.

As for specific method of estimation of the ATT, our choice fell on the bias-corrected matching estimator proposed by Abadie and Imbens (2011) that combines the matching (based in our case on the propensity score as distance metric) with a correction factor which reduces the bias due to the fact that matching is not exact. The correction is calculated using a regression model for the outcome variable in the control group.[3] In general, the literature suggests to use such robust methods, which combine in various ways matching techniques with model-based techniques (Abadie and Imbens 2011; see also Robins and Rotnitzky 1995).

[3] Variability estimation occurred using the analytic asymptotic variance estimator by Abadie and Imbens (2006), which focuses on cases, like ours, where matching occurs with replacement and with a fixed number of matches. This approach for estimating variability is incorporated in the bias-adjusted matching estimator later put forward by the same authors (Abadie and Imbens 2011). In the presence of ties, the bias-adjusted matching estimator takes all tied controls (Abadie et al. 2004).

There are a number of issues that need to be addressed when implementing the bias-corrected matching estimator (and matching estimators in general) to a case, as ours, with a relatively small sample size.

First, the choice of the number of matches is relevant here. In the choice of the number of matches, bias-precision trade-off issues are invoked, with a usual focus on reducing bias rather than variance. This usually leads to using a single match in order to have the least bias at the price of smaller precision. However, with a relatively large number of potential controls, the bias induced by multiple matches appear to be less severe, whereas if the number of treated units is relatively small (and so also the number of single-matched controls) trying to reduce the variance may be a sensible goal. It seems thus relevant to optimize the number of matches, although until now very little is known about ways to achieve this optimality and about data-dependent ways of choosing the optimal number of matches (see Chapters 15 and 17 of Imbens and Rubin 2015). Because variance reduction is limited, typically a small number of matches, between 1 and 4, is recommended. We propose an empirical solution to this problem, based on comparing pre-matching and post-matching differences in covariates' distribution under different number of matched controls. The method is illustrated and applied in in the next section.

Second, in principle the estimation strategy proposed by Abadie and Imbens (2011) allows the researcher to correct the bias due to the imperfect matching of all covariates. This strategy, although correct in principle, may be very unpractical in its implementation phase, especially in the context of a limited sample size. Linear regression-based adjustment of all matching covariates is in fact not unlikely to bring to situations of over-fit that may lead to extreme adjustments. We propose to include in the regression adjustment only the lagged values (pre-treatment, two lags) of the outcome variable, as these are likely to be the best pre-treatment predictors of the outcome for which it is particularly important that any residual bias is eliminated.

6 Empirical Application

In this section, we deal firstly with the issues related to propensity score estimation. Secondly, before presenting the results of our application, we assess the plausibility of the identification assumption of *unconfoundedness* by performing an analysis on pseudo-outcomes. Then, we report and discuss the main results of the study, consisting of the ATTs estimated on all firms that received one subsidy from the program under analysis. Finally, we show how these ATTs can be decomposed into different components based on the values of one pre-treatment variable of particular interest. A characteristic frequently used for analyzing heterogeneity of treatment effects is the size of the firm, as smaller firms are believed to suffer from constraints with respect to finance, available competencies, and so on. As this study focuses on a small-business program, a distinction based on firm size makes little sense. For the reasons discussed in Sect. 2, it may be more interesting to decompose the ATTs depending on whether the firms did or did not perform R&D prior to the program, in order to unveil which subgroup of SMEs benefits more from participation.

Estimation and Analysis of the Propensity Score, Common Support and Balance Checks

In the estimation of the propensity score we used a very wide set of pre-treatment characteristics, whose full detail is provided in the "Appendix". They include some general features of the firm, as well as other characteristics related to the following aspects: the firm's recent innovation strategy; its situation in terms of availability of internal cashflows and accessibility of credit market finance; its capital structure and other productivity and performance indicators. In addition, we have included a number of descriptors of sectors and territories to which firms belong. Most of the variables are observed during the two years prior to the incentive: this has allowed the estimation of a propensity-score that takes into account both static and dynamic features of the firms. In general, it is maintained in the literature that the higher the number of pre-treatment covariates considered, the more credible unconfoundedness is (see discussion in Sect. 5). It has also to be stressed that the set of covariates used for estimating the propensity score includes the lagged values of outcome variables, an issue whose importance is highly emphasized in the methodological literature (Imbens 2004) and, more recently, also in the economic one (e.g., González and Pazó 2008).

The propensity score was specified as a logit model;[4] the correct specification was assessed by checking its balancing property. Despite the high number of variables and the relatively small size of our sample, the balance obtained is satisfactory. The estimated propensity score was used to verify the common support assumption and exclude firms out of the support. There are 173 controls that are out of this region, as their estimated propensity score is below the minimum propensity score estimated for firms belonging to the treated group. These controls are therefore excluded from the analysis. The region of common support includes all treated firms and the remaining 461 controls. The fact that controls tend to be much scarcer at high values of the propensity score suggests allowing replacement in the matching procedure (i.e. controls can be used multiple times), in order to ensure that treated firms with high propensity score may find appropriate matches.

Balance requires that the post-matching differences in the distributions of all covariates in the treated and controls groups are close to zero. The extent of the improvements due to matching may of course vary with the number of controls we choose to match to each treated unit. As discussed in Sect. 5, one might be tempted, on the one hand, to add as many controls as possible in order to reduce sampling variability and increase the precision of estimates. Unfortunately, the potential drawback of this is to increase the bias. The literature provides no guidance about the choice of the number of matches. A reasonable empirical solution that we propose and implement, motivated by Imbens and Wooldridge (2009), consists in comparing the pre-matching standardized mean differences and variance ratios to the standardized mean differences and the variance ratios obtained after matching, so as to find out and assess improvements with different number of matches.

Using propensity score as a distance (matching) metrics, Table 2 shows that the two-match solution assures on average positive and considerable improvements on all

[4] Results are not reported here but are available upon request.

Table 2 Assessing balance improvements due to propensity score matching, with respect to the initial non-matching situation

	% of variables whose balance improves		Average improvement on all variables (difference between pre- and post-matching values)	
	Standardized mean diff.	Variance ratio	Standardized mean diff.	Variance ratio
On common support	55.00	63.80	0.00	0.15
1 match	48.80	61.30	− 0.01	0.16
2 matches	52.50	63.80	0.04	0.15
3 matches	51.30	60.00	0.04	0.01
	Average improvement on 1st lagged value of outcomes (−1) (difference between pre- and post-matching values)		average improvement on 2nd lagged value of outcomes (−2) (difference between pre- and post-matching values)	
	Standardized mean diff.	Variance ratio	Standardized mean diff.	Variance ratio
On common support	−0.06	0.16	− 0.06	0.14
1 match	0.07	0.31	0.08	0.39
2 matches	0.15	0.36	0.15	0.40
3 matches	0.17	0.35	0.14	0.39

covariates, both in terms of mean differences and variance ratios, as well as on the lagged values of outcome variables. It also assures that improvements occur on a high number of covariates.[5] For these reasons, we will use two controls for each treated firm.

How Credible is the Unconfoundedness Assumption?

The unconfoundedness assumption is not directly testable. Nevertheless, the literature has put forward some approaches to assess its plausibility. The approach we take is based on the idea of testing the null hypothesis that an average effect is zero, when it is known that this average effect is indeed equal to zero. If the null hypothesis is rejected, this may suggest weak support for the unconfoundedness assumption (Imbens and Wooldridge 2009). The effect should definitely be zero if we try to estimate it on a variable known to be unaffected by the treatment, because its value is determined prior to the treatment itself. If we find a non-zero effect, this must be due to the fact that the observations under treatment are (still) different from (matched) controls prior to treatment, probably because of the action of some unobserved (and therefore omitted) variables that play a role in the assignment to treatment. Instead, statistical evidence of a zero effect makes the unconfoundedness assumption more credible. This kind of test has more power if the variables on which we estimate the pseudo effect are clearly related to the outcome of interest, such as values of lagged outcomes. We will implement this strategy by using as pseudo-outcome the first lagged value of the outcome variables on which we will also estimate the effects of the program. First, we estimate the propensity score conditional on the second lagged value of the outcome variable (but of course not on its first lagged value), as well as on all remaining covariates observed both one and two years prior to treatment. For the reasons already discussed in Sect. 5, we then employ the bias-corrected matching estimator using the propensity score as a metrics and implementing the bias adjustment using the second lagged value.

Results are shown in Table 3a. No evidence of a non-zero effect is found, so that we can conclude that it is very plausible that the pseudo-outcomes are independent of treatment, given the set of remaining covariates. The set of covariates we have adjusted for appears to be sufficient to make the two groups of treated and controls comparable, because an effect known to be zero is effectively found to be zero.

Results

The main results of our analysis are displayed in Table 3b. They were estimated using the bias-corrected matching estimator (Abadie and Imbens 2011), adjusting—case by case—for the first and second lagged pre-treatment values of the outcome variable.[6] Exact matching was forced for three binary variables which are particularly relevant in the following analysis: the fact that firms had performed or not any R&D in the years preceding the program, in order to account for the inducement effect

[5] Detailed results of this analysis are available upon request to the authors. Table 6 in the "Appendix" shows that, as expected, the standard errors of the ATT estimates slightly decrease as the number of matches grows. This occurs at the cost—however—of inducing more bias in the ATT estimates.

[6] Note that when the outcome is continuous the adjustment is carried out by means of a linear regression model; when the outcome is binary by means of a linear probability model.

discussed for example in González et al. (2005), Czarnitzki and Licht (2006) and González and Pazó (2008) (based on this variable we will henceforth distinguish between R&D performers and nonperformers); the fact that firms had already (or had not) previous experience in product innovation, and, finally, whether the industry to which the firm belongs is a R&D or knowledge intensive one according to the OECD classification (the dummy takes the value of 1 if the firm belongs to medium-to-high or high tech manufacturing, or to KIBS, and zero otherwise).

As can be seen in Table 3b, the program has led to overall positive effects (significant at 10% level) for beneficiary firms only in terms of three outcomes, but these are not of minor importance. Treated SMEs have, about 1.5–2 years after the completion of the subsidized project: 1.2 graduated employees; 0.6 stable R&D employees; and invest 28 thousand Euros in R&D more than controls. With respect to the pre-treatment situation (i.e. with respect to the average level of outcomes as measured immediately before the treatment), the program caused a 54% increase in the graduated employees, a 16% increase in the R&D personnel, and a 14.5% increase in the R&D investment of treated firms (these figures are not reported in the Table). This finding suggests that the program has effectively contributed to the upskilling of firms, and to the shaping of some important prerequisites for absorptive capacity. Moreover, it has raised the firms' propensity to R&D over time. The effects on graduated and on R&D employees occur without a parallel effect on the total number of employees. This might have happened for the following reason: despite treated firms and controls have experienced a similar variation of employees, the firms that have benefited from the subsidy have replaced some unskilled or R&D-unspecialized employees (for example when these latter have retired) with more skilled and specialized ones.

The program has not boosted some of the outputs that could derive from a product innovation process, such as IPRs or turnover. Nor has the program enhanced the propensity of SMEs to conduct their innovation processes in cooperation with other firms or with universities. In summary, the analyzed program for SMEs has increased their human capital endowments and encouraged the non-transitory practice of intramural R&D.

To account for the issues related to performing multiple tests on the same data, we take the approach by Benjamini and Hochberg (1995) based on false discovery rates (FDR). The statistical significance (at 10%) of our estimated treatment effects on graduated employees, R&D personnel and R&D investment is preserved by setting the maximum proportion of false positives that one is willing to accept at 25%. Note that a FDR of 25% entails that, out of three results that are statistically significant (Table 3), less than one is likely to be a false positive.

Let us now decompose the ATTs that we found to be statistically significant into two components, depending on whether the firms did or did not perform R&D prior to the program.

Table 4 shows what effects can be found in each of the two resulting subpopulations. For each outcome variable, the weighted average of these "local" ATTs returns the "global" ATTs reported in Table 3.

These results clearly suggest that inducement to R&D has mostly occurred for nonperformers. This result adds to a handful of previous works on inducement effects

Table 3 Pseudo ATTs (a) and ATTs (b) on all treated firms

	(a) Prior to treatment				(b) After treatment			
	Pseudo ATT_{-1}	Std. err.	90% CI	*p* value	ATT	Std. err.	90% CI	*p* value
total Employees (O)	0.978	0.814	− 360; 2.317	0.229	1.589	2.16	− 1.963; 5.141	0.462
Graduated Employees (I)	− 0.099	0.211	− 0.445; 0.247	0.638	1.237	0.737	0.025; 2.449	0.093
R&D personnel (I)	− 0.07	0.229	− 0.447; 0.308	0.762	0.638	0.379	0.016; 1.261	0.092
R&D investment (th. of Euros) (I)	0.107	8.695	− 14.194; 14.409	0.99	28.118	16.604	0.806; 55.430	0.09
Turnover (th. of Euros) (O)	98.48	481.157	− 692.952; 889.912	0.838	597.804	645.994	− 464.762; 1,660.370	0.355
IPRs (O)	− 0.013	0.07	− 0.128; 0.103	0.858	0.038	0.081	− 0.094;0.171	0.636
Collaborative inter-firm innovation (1/0) (B)	− 0.02	0.024	− 0.059;0.019	0.394	0.012	0.029	− 0.035; 0.059	0.667
Technology transfer (1/0) (B)	− 0.028	0.031	− 0.078; 0.023	0.365	0.042	0.039	− 0.021; 0.106	0.274
N. of treated firms	120				120			
Pct. of exact matches	100%				100%			

No. of controls matched to each treated firm: 2 (with replacement). Exact matching by individual R&D history (1/0), individual experience on product innovation (1/0), industry R&D intensity (1/0). Bias adjustment implemented on the 1st (in a and b) and 2nd (in b) lagged values of outcomes. Based on the discussion in Sect. 3, (I) denotes outcome variables related to innovation inputs; (O) denotes possible outcomes of the innovation process; (B) denotes behavioral outcomes

Table 4 ATTs on sub-populations of treated firms identified on the basis of their previous R&D history

	Firms with R&D history				Firms without R&D history			
	ATT	Std. err.	90% CI	*p* value	ATT	Std. err.	90% CI	*p* value
Graduated employees (I)	0.822	0.436	0.104; 1.539	0.060	2.939	2.684	− 1.476; 7.353	0.274
R&D personnel (I)	0.297	0.441	− 0.428; 1.022	0.501	2.080	0.685	0.953; 3.207	0.002
R&D investment (th. of Euros) (I)	30.901	22.961	− 6.865; 68.668	0.178	48.649	21.680	12.988; 84.309	0.025
N. of treated firms	95				25			
Pct of exact matches	100%				100%			

No. of controls matched to each treated firm: 2 (with replacement). Exact matching by individual experience on product innovation (1/0), industry R&D intensity (1/0). Bias adjustment implemented on the 1st and 2nd lagged values of outcomes. Based on the discussion in Sect. 3, (I) denotes outcome variables related to innovation inputs; (O) denotes possible outcomes of the innovation process; (B) denotes behavioral outcomes (including González et al. 2005; González and Pazó 2008) which, however,, do not focus on small-business programs.

From the application of the Benjamini and Hochberg (1995) procedure, we learn that a FDR of 25% is more than sufficient to preserve the statistical significance (at 5%) of our estimated treatment effects on the R&D investment and personnel of firms without an R&D history. In fact, a FDR of 5% would be sufficient to this end.

On the contrary we find no evidence of positive effects for former R&D performers, with the only exception of some increase in the number of graduated employees. If we look at FDRs, we see that a FDR of 25% is sufficient to preserve the statistical significance of the estimated treatment effect on the graduated employees of firms with an R&D history. In fact, a FDR of 20% would be high enough.

7 Concluding Remarks

In this paper, we have evaluated a program which provides SMEs with small-size R&D incentives to carry out product innovation. The program had no specific sectoral or technological target and did not reflect any mission-oriented strategy, which means that it did not adapt by design to local specificities. Instead, it responded to a very inclusive strategy, which is far from being uncommon in many small-business programs around the world that try to encourage SMEs to approach R&D or to do it in a more continuous and organized way, in order to upgrade their competitiveness in the medium or long run. Therefore, we believe that our analysis is quite general and can be interesting well beyond local boundaries.

Our findings suggest that small-size R&D subsidies may bring some interesting effects. We find that subsidies induce former nonperformers of R&D to approach this practice and pursue an innovation model that more enduringly relies on R&D. Further positive effects refer to the upskilling of SMEs connected to the hiring of a better educated labor force. In our view, these are effects of no minor importance, as they reinforce prerequisites for the development of absorptive capacity. The latter is a key aspect for smaller firms that wish to take advantage from the currently prevailing innovation policy frameworks, where inter-firm and university-industry collaborations are strongly encouraged, as exemplified by the well-known Smart Specialization concept that underlies recent EU Cohesion Policy (McCann and Ortega-Argilés 2015). Therefore, we argue that the main implication of this study for policymakers is that small subsidization programs, like the one analyzed here, are not to be viewed as alternative to collaboration policies but rather as complementary to the latter, in that they pave the way to the involvement of SMEs in more complex collaborative projects.

In parallel, we find no further systematic effect on innovation outputs or other firm performance indicators. This suggests that a longer-term perspective should be taken in order to verify whether the R&D induced by the program is able to bring to a better economic performance or to firm growth. The impossibility of looking at a wider time horizon with the available data constitutes in our view the main limitation of this study.

The causal effects of the small-business program evaluated in this study have been estimated by means of propensity-score matching techniques, under the usual unconfoundedness assumption. The tenability of this assumption largely depends on controlling for a vast number of pretreatment variables, therefore we used a wide set of pre-subsidy firm characteristics. Scholars familiar with empirical program evaluation know that, in the presence of relatively small samples and many matching variables, it can be not straightforward to strike a balance between bias reduction and precision of estimates. This work suggests how this balance can be pursued by establishing an appropriate number of matches.

Appendix

See Tables 5 and 6.

Table 5 The variables used in the analysis

Variable	Type of variable	Variable description	Source	Used in matched sampling phase	Used in final *p* score and matching	Used as outcome variable
Firm characteristics						
Age, age squared	Continuous, observed	Age of firm one year prior to the subsidy	Statistical archive of active firms	p score, year (−1)	*p* score, year (−1)	
Legal form	categorical, observed	Ltd/LLC; plc/corporation; other	Statistical archive of active firms	Exact matching	*p* score, year (−1)	
Group	Dichotomous, observed	Dummy that takes a value of 1 if the firm participates in a business group	Interview		*p* score, year (−1)	
Employees	Continuous, observed	Number of employees	Statistical archive of active firms	*p* score, years (−2) and (−1)	*p* score, years (−2) and (−1)	Yes
Graduated employees	Continuous, observed	Number of graduated employees	Interview		*p* score, years (−2) and (−1)	Yes
R&D personnel	Continuous, observed	Number of permanent R&D employees	Interview		*p* score, years (−2) and (−1)	Yes
R&D investment, also squared and cubed	Continuous, observed	investment level in Euros	Interview		*p* score, years (−2) and (−1)	Yes
R&D history	dichotomous, observed	Dummy that takes a value of 1 if the firm has never performed R&D before	Interview		exact matching	

Table 5 continued

Variable	Type of variable	Variable description	Source	Used in matched sampling phase	Used in final p score and matching	Used as outcome variable
Previous IPRs	continuous, observed	Number of applications for patents, copyrights and industrial designs filed to any Authority	Interview		p score, from year (-3) to year (-1)	
IPRs	Continuous, observed	Yearly average of applications for patents, copyrights and industrial designs filed to any Authority in the three years following the completion of the project	Interview			Yes
Innovation experience	Categorical, observed	= 1 if firm has previous experience mainly on product innovation; 2= process innovation; 3= no innovation experience			p score, up to year (-1)	
Product innovation	Dichotomous, observed	dummy that takes a value of 1 if the firm's previous experience is mainly focused on product innovation	Interview		Exact matching	
Collaborative inter-firm innovation	dichotomous, observed	Dummy that takes a value of 1 if the firm carries out innovation activity in collaboration with other firms	Interview		p score, years (-2) and (-1)	Yes

Table 5 continued

Variable	Type of variable	Variable description	Source	Used in matched sampling phase	Used in final p score and matching	Used as outcome variable
Technology transfer	dichotomous, observed	Dummy that takes a value of 1 if the firm carries out innovation activity in collaboration with university	interview		p score, years (−2) and (−1)	Yes
Own trademark	Dichotomous, observed	Dummy that takes a value of 1 if the firm has its own trademark	Interview		p score, year (−1)	
Tangibles	Continua (oss)	Tangible assets/total assets, in Euros	Balance sheet (AIDA)	p score, years (−2) and (−1)	p score, years (−2) and (−1)	
Intangibles	Continuous, observed	Intangible assets/total assets, in Euros	Balance sheet (AIDA)	p score, years (−2) and (−1)	p score, years (−2) and (−1)	
Labor cost	Continuous, observed	Labor cost per employee, in Euros	Balance sheet (AIDA)	p score, years (−2) and (−1)	p score, years (−2) and (−1)	
Labor productivity	Continuous, observed	Value added per employee, in Euros	Balance sheet (AIDA)	p score, years (−2) and (−1)	p score, years (−2) and (−1)	
TFP	continuous, estimated	Total factor productivity, estimated using all Tuscany's firms but separately for each two-digit NACE sector, according to the methodology proposed by Levinsohn and Petrin (2003)	Balance sheet (AIDA)	p score, years (−2) and (−1)	p score, years (−2) and (−1)	

Table 5 continued

Variable	Type of variable	Variable description	Source	Used in matched sampling phase	Used in final p score and matching	Used as outcome variable
Export share	Continuous, observed	export/turnover, pct share	Customs files and balance sheet (AIDA)	p score, years (−2) and (−1)	p score, years (−2) and (−1)	
Turnover	Continuous, observed	Turnover in Euros	Balance sheet (AIDA)	p score, years (−2) and (−1)	p score, years (−2) and (−1)	Yes
ROI	Continuous, observed	Return on investments	Balance sheet (AIDA)	p score, years (−2) and (−1)	p score, years (−2) and (−1)	
ROS	Continuous, observed	Return on sales	Balance sheet (AIDA)	p score, years (−2) and (−1)	p score, years (−2) and (−1)	
ROA	Continuous, observed	Return on assets	Balance sheet (AIDA)	p score, years (−2) and (−1)	p score, years (−2) and (−1)	
Default index	Continuous, estimated	Probability of default, estimated using all Tuscany's firms but separately for manufacturing and service sectors, according to the methodology proposed by Altman and Sabato (2007) for SMEs	Business registers held by Chambers of Commerce, balance sheets (AIDA)	p score, years (−2) and (−1)	p score, years (−2) and (−1)	
Cash flow	Continuous, observed	Cash flow/turnover	Balance sheet (AIDA)	p score, years (−2) and (−1)	p score, years (−2) and (−1)	
Sector characteristics						
Sector	Categorical, observed	Two-digit NACE sector to which the firm belongs	Balance sheet (AIDA)	Exact matching	p score	

Table 5 continued

Variable	Type of variable	Variable description	Source	Used in matched sampling phase	Used in final p score and matching	Used as outcome variable
Competition	Continuous, estimated	Average 2002–2004 Lerner index relative to each three-digit NACE sector, estimated using all Italian firms	Balance sheets (AIDA) and OECD classification	p score	p score	
Medium-high to high tech	dichotomous, observed	Dummy that takes a value of 1 if the three-digit NACE sector is medium-high to high tech manufacturing, or KIBS, according the OECD classification	Balance sheets (AIDA) and OECD classification	p score	exact matching	
Territorial characteristics						
MHtech share	continuous, observed	Share of employees in medium-high to high tech manufacturing in the Local Labor System (LLS) to which the firm belongs	Italian National Institute of Statistics data	p score	p score, year (-1)	
KIBS	Continuous, observed	Share of employees in knowledge-intensive business services (KIBS) in the Local Labor System (LLS) to which the firm belongs	Italian National Institute of Statistics data	p score	p score, year (-1)	

Table 5 continued

Variable	Type of variable	Variable description	Source	Used in matched sampling phase	Used in final p score and matching	Used as outcome variable
Ob2_tot	Dichotomous, observed	Dummy that takes a value of 1 if the municipality to which the firm belongs is totally "Objective 2" according to EU 2000–2006 legislation	EU legislation	p score	p score, year (−1)	
Ob2_part	Dichotomous, observed	Dummy that takes a value of 1 if the municipality to which the firm belongs is partially "Objective 2" according to EU 2000–2006 legislation	EU legislation	p score	p score, year (−1)	

Table 6 ATT estimates and their standard errors under different numbers of matches

	1 Match		2 Matches	
	ATT	Std. err.	ATT	Std. err.
Total employees	3.214	2.485	1.589	2.16
graduated employees	1.266	0.860	1.237	0.737
R&D personnel	0.677	0.403	0.638	0.379
R&D investment (th. of Euros)	4.575	18.409	28.118	16.604
Turnover (th. of Euros)	755.483	1109.628	597.804	645.994
IPRs	0.049	0.081	0.038	0.081
Collaborative inter-firm innovation (1/0)	− 0.011	0.038	0.012	0.029
Technology transfer (1/0)	0.050	0.044	0.042	0.039
Pct of exact matches	100%		100%	
	3 Matches		4 Matches	
	ATT	Std. err.	ATT	Std. err.
Total employees	1.384	2.037	1.162	2.052
Graduated employees	1.174	0.704	1.120	0.675
R&D personnel	0.564	0.370	0.562	0.354
R&D investment (th. of Euros)	19.884	21.134	17.448	18.444
Turnover (th. of Euros)	314.074	528.307	234.270	506.657
IPRs	− 0.570	0.755	−0.403	0.636
Collaborative inter-firm innovation (1/0)	0.016	0.026	0.019	0.025
Technology transfer (1/0)	0.028	0.039	0.022	0.038
Pct of exact matches	100%		100%	

Controls are matched with replacement. Exact matching by individual experience on product innovation (1/0), industry R&D intensity (1/0). Bias adjustment implemented on the 1st and 2nd lagged values of outcomes

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