# Innovative public procurement and R&D subsidies: hidden treatment and new empirical evidence on the technology policy mix

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**Abstract**

This paper provides new empirical evidence about the impact of technological policies upon firms' innovative behavior. We take into consideration the role of R&D subsidies and innovative public procurement. While the R&D subsidies have been both extensively discussed in the literature and empirically investigated, the analysis of innovative public procurement is a growing trend in the literature, which still lacks robust empirical evidence. In this paper, we replicate existing results on R&D subsidies, we surmise fresh empirical evidence on the outcome of innovative public procurement, and we address the issue of a possible interaction among the two tools. When controlling for the interaction with innovative public procurement, R&D subsidies cease to be as effective as reported in previous studies and innovative public procurement seems to be more effective than R&D subsidies. Evidence suggests that the two policies provide the highest impact when they interact and that they have to be simultaneously considered. Failure in doing so might lead to biased results.

*Keywords:* *R&D Subsidies, Public Procurement, Crowding-out, confounding effect, hidden treatment, propensity score matching*

## 1. Introduction

R&D subsidies are a form of innovation policy which has been extensively analyzed in the literature. One of the most debated issues has been whether R&D subsidies displace private efforts or, on the contrary, favor them due to some form of complementary relationships. More recent literature seems to converge towards a substantial rejection of the presence of a crowding-out effect in R&D subsidies. Since the seminal paper by Almus and Czarnitzki (2003), a widespread empirical method to approach the issue has been the use of a quasi-experimental setting where the outcome variable is the innovative performance and the treatment is whether firms receive subsidies or not. In order to control for selection bias, subsidized firms are compared with a control group which has been previously made statistical identical through the implementation of nonparametric matching techniques. Most of these studies points towards the direction of a substantial complementarity of R&D subsidies and private R&D investment. However, this specific empirical method in use deserves further analysis. In quasi-experimental settings, the researcher runs the risk of omitting non-observable variables which can nevertheless influence the results. When these variables are randomly distributed among subsidized firms and control group, they do not bias the results. However, when omitted variables change with the level of the subsidies, they can be a possible source of confounding effect. The literature is very well aware of this problem and in the next section we mention various papers trying to cover the majority of possible sources of confounding factors. A second possible confounding factor, which has not been discussed at all in the literature, consists of the presence of a potential hidden treatments. In the case of a specific technology policy, a hidden treatment might be represented by a confounding variable which is not a firm's characteristic, but an additional strategic option that can be implemented by the policy maker to obtain the same results. If this event is not taken into account, it is impossible to conclude that the observed innovative outcome is due to the use of R&D subsidies or, by contrast, to the implementation of other non-observed technology policies or to the interaction of a policy mix.

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More specifically the focus of the paper is on the innovative public procurement as a possible hidden treatment. Indeed, there is a growing trend in the literature on technology policy about the role of innovative public procurement as a possible complement or alternative policy to R&D subsidies (Edler and Georghiou, 2007) . While in the case of R&D subsidies scholars have been mostly focusing on the impact upon the innovative input, they analyzed the effect of innovative public procurement both upon innovative input and innovative output such as the innovative turnover. Despite various theoretical accounts, empirical evidence is still very fragmented. In this paper, we surmise that innovative public procurement and R&D subsidies are tools of the technology policy mix which can contextually affect a firm's innovative performance. For this reason, in order to evaluate the effect of either policy a researcher should implement a method apt to disentangle the two effects. Moreover, we insist in the need of considering the impact upon both innovative input and output. On the one hand, the focus on the innovative input is an important control for the biases which a policy can introduce on a firm's behaviour. On the other hand, the change in innovative output provides a sound measure of the final effect of the policy. A policy should aspire to have considerable effect on the innovative output without crowding out the private innovative input.

*In this paper we aim at testing the contextual impact of R&D subsidies and innovative public procurement upon both a firm's private R&D investment and its innovative output.* The relevance of the paper is threefold. First, by taking into account innovative public procurement, we control past results on R&D subsidies for a possible hidden treatment such as an alternative technology policy. Secondly, we provide empirical evidence on the effectiveness of innovative public procurement. Finally, we discuss the interaction of the two policies and call for further research on the policy mix rather than on policy in isolation.

In the next section we discuss the state of the art. In section 3.1 we present the data and methodology. Empirical results and conclusion follow.

## 2. Theoretical framework

### *2.1. R&D subsidies*

The impact of public R&D subsidies upon innovation outcome has been broadly discussed in the literature. Yet, there is still puzzling evidence about the nature of the interaction of R&D subsidies with private investment. The central question is whether public support displaces private efforts, simply adds to them, or even favors their increase. The argument whether it exists a substitutability, additionality or complementarity between R&D subsidies and private R&D investments has been long debated in the literature. David et al. (2000) survey the empirical literature and find mixed evidence for various levels of aggregation of the unit of analysis. On the one hand, some studies at the firm level suggest that public R&D subsidies crowd out private R&D investment (Shrieves, 1978; Carmichael, 1981; Higgins and Link, 1981), while some others point at the existence of a possible reinforcing mechanism between the two of them (Holemans and Sleuwaegen, 1988; Link, 1982; Antonelli, 1989). Capron (1992) and Capron and De La Potterie (1997) show that the effect might depend on various covariates which are idiosyncratic to the specific subsidies programs such as country and sector of eligibility, to firm and market size and to the intensity of the subsidies. Garcia-Quevedo (2004) discusses the studies reviewed in these surveys and counts 37 articles with some evidence of complementarity, 24 showing a net effect of substitutability, while the remaining 15 do not end up with statistically significant results. Moreover, he also empirically rejects the hypothesis that the ambiguity in the literature can be due to differences in the methodological tools. Also David et al. (2000) discuss the methodology issues and they hold responsible for this ambiguous empirical support the difficulty of dealing with the problem of endogeneity in such a context.

> This [mutual interdependence of public and private R&D expenditures] may present an issue for econometric analysis, either because of simultaneity and selection bias in the funding process, or because there are omitted latent variables that are correlated with both the public and private R&D investment decisions (David et al., 2000, p. 509).

Similarly, Busom (2000) suggests the possible endogeneity of R&D subsidies and tries to deal with the issue of selection bias with a structural approach where she first estimates a probability for a firm to take part to a public R&D subsides program and only thereafter she estimates the private R&D efforts to test the presence of crowding-out effect. Almus and Czarnitzki (2003) address the issue of selection bias as well, since most of the literature surveyed by David et al. (2000) does not concern with it. The challenge is to make use of a statistical technique which allows for a counter-factual analysis comparing the innovative behaviour of firms which receive R&D subsidies with the hypothetical situation where the same firm did not receive them. Not being possible to observe the same firm in both states of the world, the first best solution would be to run an experiment on a group of subsidized firms vs. a control group of not subsidized firms and test whether there is significant difference in the mean of some proxy for innovative behaviour. This procedure requires that two groups are perfectly randomized, *i.e.* the innovative behaviour of a firm does not correlate with the probability of the firm to be in a specific group. However, when a real randomized experiment is not at hand and the researcher is forced to use non-experimental data, the existence of selection bias precisely undermines this requirement. In such a case, the solution suggested by Almus and Czarnitzki (2003) consists of dealing with the data as in a quasi-experimental setting, where, although initially the control group cannot be used as a base line because of the lack of randomization, it could be made statistically identical to the treated group by manipulating it with various techniques. Almus and Czarnitzki (2003) choose to implement a propensity score matching to assign each subsidized firm to a control firm exhibiting the highest similarity along various characteristics. Almus and Czarnitzki (2003) end up showing a reinforcing effect between public R&D subsidies and private R&D efforts.

Their result has been corroborated by several empirical studies which control for the selection bias in a quasi experimental setting *á la* Almus and Czarnitzki. Among others, González and Pazó (2008) indicate in a sample of Spanish manufacturing firms both the absence of crowding-out effect and, under certain circumstances, the presence of complementarity. On the same dataset, González et al. (2005) suggest that the lack of R&D subsidies can even restrain firms to invest in R&D at all. Czarnitzki and Licht (2006) show additionality of R&D subsidies for Western- and Eastern Germany. Czarnitzki et al. (2004) conclude that R&D tax credits increase the overall R&D engagement for a sample of Canadian firms. Goerg and Strobl (2007) find that the absence of additionality depends on the size of the R&D grants and on the country of origin: evidence on Irish firms suggests that additionality in R&D subsidies holds for small grants, while large grants might crowd out private investment. These results hold only for Irish firms and not for foreign ones. Czarnitzki et al. (2007) show on a sample of Finnish and German firms that R&D subsidies affect more innovative output measures such as the number of patents rather than R&D expenditure. Aerts and Schmidt (2008) rejects the hypotheses of crowding-out effect in a comparisons between firms in Germany and the Flanders.

All in all, although evidence is not yet conclusive, it seems that when controlling for the selection bias in quasi-experimental settings, the presence for a crowding-out effect has to be rejected and, under certain conditions, there is an empirical support for the claim that R&D policies positively impact upon private investments.[1] However, a quasi-experimental framework is not immune from possible flaws. A first shortcoming is the presence of *extraneous variables*, that is unobserved firms characteristics which influence other independent variables. If they affect both the subsidized firms and the control group, extraneous variables do not usually bias the results although they might create some noise and increase the variance. However, the case when an extraneous variable varies with the level of the treatment variable in a systematic way brings in a serious drawback to the analysis, because it introduces a *confounding factor*. This is precisely the reason why results vary when additional firms characteristics are introduced such as for instance the size of the grants (Goerg and Strobl, 2007), the size of the firm, or the sector of activity (González et al., 2005). An even more serious type of confounding factor is when an extraneous variable varies in a systematic way with the outcome variables. This case of confounding factor can be seen as a *hidden treatment* (Huston, 1997) and the problem is very well known in clinical research, when typically various compounds are administered to patients as a cure for the same disease (Thall et al., 2000), and in the alternative medicine/integrative medicine framework, which studies the interaction of alternative and integrative medicine with the administration of standard compounds (Caspi and Bell, 2004).

In this article, we claim that a crucial confounding factor which has not been taken into account in the previous literature is the presence in a system of innovation of other technology policies designed to stimulate private R&D. If the major source of selection bias derives from public institutions, which decide for the eligibility to a subsidies program depending of specific firms' characteristics (Almus and Czarnitzki, 2003), it is reasonable to assume that

[1] Many other studies can be cited which can corroborate these hypotheses in a non quasi-experimental setting as well, such as Hussinger (2008) and Blanes and Busom (2004), which still control in various ways for selection bias.

the same criteria might be adopted also for eligibility in R&D incentives programs other than R&D subsidies or that being selected for a subsidies program increases the firm's probability to be elected as a recipient of another technology policy. If this is the case, not controlling for the interaction with other technology policies can end up in over-estimating the impact of R&D subsidies. A candidate for such a confounding policy could be the innovative public procurement, a government procurement for innovative products and services which might directly or indirectly stimulate private R&D (Edler and Georghiou, 2007). David et al. (2000) already hypothesized, but not investigated though, the relevance of the possible interaction of these technology policies:

> government-funded industrial R&D projects would be seen as carrying less (private) risk, especially as much of it is devoted to 'product innovation' for 'output' that eventually is to be sold back to the government procurement agency (David et al., 2000, p. 498).

Innovative public procurement seems therefore to be the suitable suspect to be investigated as a possible hidden treatment in the test for the presence of complementarity, additionality, and substitutability of R&D subsidies.

### *2.2. Innovative public procurement*

Innovative public procurement is a growing trend in the debate about technology policy. An early work in this area by Lichtenberg (1988) tested the effect of noncompetitive governmental contracts upon company sponsored R&D expenditures. He estimated that 1$ increase in governmental sales induces 9.3 cents increment in private R&D, while 1$ increase in non governmental sales induces an increment of only 1.7 cents. This result suggests not only that public procurement has a positive effect on a firm's propensity to engage in R&D, but also that the demand pull effect is larger for public procurement than other private contracts. Similarly Geroski (1990) pointed at the role of public procurement in creating demand for new products and process, for making visible an already existing demand, and for providing a minimal market size in the early stage of an innovation. It clearly emerges that the discussion of innovative public procurement is intrinsically linked with the debate about the role and magnitude of demand as a source of innovation. The demand pull-hypotheses, extensively studied in the Sixties and in the Seventies of the last century, was somehow left aside after the disrupting critique by Mowery and Rosenberg (1979) and Dosi (1982), which pointed at both theoretical and empirical flaws of the study in the area. A slow, but over time consistent work about the demand side approach (Von Hippel, 1988; Malerba et al., 2007; Rogers, 1995; Fontana and Guerzoni, 2008) gave a new twist to this literature stream. Contextually, the resurrection of the demand side took also place both in the literature about industrial policy with the work by Edler and Georghiou (2007) "Public procurement and innovation. Resurrecting the demand side" and at the policy level (Georghiou, 2006; Aho et al., 2006; EU, 2010). Edler and Georghiou (2007) set up a very general framework of discussion, which grounds the need of demand oriented innovation policy in market failures as it is done for supply-oriented ones.

The growing interest on the topic raised the issue of the theoretical definition of innovative public procurement. In the literature we can track several taxonomies and different labels defining this concept[2] has been proposed. The most widespread definition, as introduced in Edquist and Hommen (2000b) and recently further developed in Edquist and Zabala-Iturriagagoitia (2012), considers public procurement of innovation as occurring when 'a public agency places an order for a product or a system which does not exist at the time, but which could probably be developed within a reasonable period'. This form of purchasing is usually opposed to 'regular public procurement' which occurs when a public agency buys ready made simple products such as pen and papers, where no R&D is involved (Edquist and Hommen, 2000b). Although this definition has the major advantage to neatly distinguish these two categories of procurement, recent works (Uyarra and Flanagan, 2010; Rolfstam, 2012) both highlighted its potential limitations and stressed the fact that it constrains the innovative procurement scope to the activities that follow a formal tender process. The reason of considering innovative behavior after the procurement order lies in the possibility to observe the direct effect of public procurement on firms' innovative behavior; however, ignoring the phase before the order could hide the indirect impact of the procurement policy. As the demand-pull literature suggests (Guerzoni, 2010), a firm's decision to introduce a new product or service rather than a standardized one is affected by the size and the degree of sophistication of the potential demand. In the procurement context, a public agency may be considered

[2]Expressions like 'public technology procurement' and 'public procurement of innovation' are used to refer to very similar phenomena. For further discussion see Rolfstam (2012)

as a prospective customer with a general preference towards more or less innovative products, services or systems. The firm's innovative activity might therefore be crucially influenced by the nature of public demand even if a formal order is yet to come; this is especially true if the public agency is one of the key player in a specific sector such as in the military sector. For this reason, in this work we refer to a broader definition of innovative public procurement and, in line with Rolfstam (2012), we consider it as 'purchasing activities carried out by public agencies that lead to innovation", even if indirectly or as a by-product (ibid.).

Despite the theoretical and policy attention on the issue, the empirical evidence about the effect of innovative public procurement on innovation output is rather fragmented and mostly limited to case studies (Edquist and Hommen, 2000a; Rolfstam, 2009; Uyarra and Flanagan, 2010; Flanagan et al., 2011; Brammer and Walker, 2011). Notable exceptions are Aschhoff and Sofka (2009) and Slavtchev and Wiederhold (2011). Slavtchev and Wiederhold (2011) is a very sophisticated paper which departs from the traditional test of public policy at the firm level. Indeed they develop a Schumpeterian model of growth in order to make predictions on the role of the sectoral composition and intensity of public procurement upon the economy growth path. An empirical test with panel data at the sectoral level of the US economy suggests that the model predictions are correct and public procurement leads to higher returns in industry with higher technology opportunities. Aschhoff and Sofka (2009) is an exemplary paper in the tradition of evaluating technological policy at the firm level with survey data in the same spirit of the articles mentioned above about R&D subsidies. Aschhoff and Sofka (2009) test the role of various policies on a cross-section of 1149 German firms which respond to the survey 'Mannheim Innovation Panel' in 2003. Based on self-reported data, they are thus able to compare the impact on innovative output of firms proxied by their innovative turnover, which is defined as the share of turnover with market novelties. They find robust evidence for a positive impact of public procurement using a latent class tobit regression, which might partly control for the selection bias of the sample. The value of their paper is twofold; first, it is the only recent empirical work on procurement with a large cross-sectoral dataset. Secondly, to our knowledge, it is the only analysis which links firms' innovative behavior with different technology policy mixes and not with a single policy only. Indeed, as already pointed in the previous section, it might be the case that R&D subsidies are explicitly linked with a subsequent procurement (Lichtenberg, 1988; David et al., 2000) or that a firm can both apply to subsidies and participate to tenders for public procurement.

Summing up, the work on R&D subsidies mentioned in the previous section and this one by Aschhoff and Sofka (2009) tackle each one side of the problem only. The former manages to develop a robust technique to isolate a causal effect of policy tool on firms' innovative behavior, it signals the potential risk of a crowding-out effect of public subsidies on the private investment, but it succeeds in ruling it out empirically. However, works on R&D subsidies missed in considering other policies, which can potentially interact with R&D. Given the quasi-experimental setting of these researches, this omission might lead to an overestimation of the impact of R&D grants on innovation. The positive impact of R&D subsidies on the private investment might be partially or even totally due to the contextual influence of innovative public procurement and, thus, not to the result of R&D grants only. Aschhoff and Sofka (2009) following the new trend of demand oriented technology policies has the merit to include both policies in the analysis. However, their econometric approach obliges them to cut from the dataset non-innovative firms which, on the contrary should be the first candidate for an adequate control sample. Moreover they limit their analysis to the output of innovation activities and therefore they do not provide any insight on the impact of innovative public procurement on private investment in R&D

On this basis, in this paper we try to get the best out of two worlds. We aim at testing the impact of technological policies on a firm's innovative behavior when both R&D subsidies and innovative public procurement are taken into account and we perform the analysis in a multi-treatment quasi-experimental setting. Our finals goals are (1) testing the robustness of results on R&D subsidies when also innovative public procurement is taken into account, (2) provide new empirical evidence on the evaluation of the innovative public procurement. A special attention is then given to the interaction of the policies. Moreover, we contextually observe both the impact of a technology policy on firms' innovative input and output. In the next section we describe the available data to accomplish this task. Section 3 describes the methods we apply. Results and conclusions follow.

## 3. Data and Method

### *3.1. Data*

In the analysis, we used data from the Innobarometer on Strategic trends in innovation 2006-2008, which is a survey conducted by the Gallup Organization upon the request of DG Enterprise and Industry in April 2009 in the 27 Member States of the EU, Norway and Switzerland[3]. Gallup interviewed senior company managers responsible for strategic decisions making of 5238 company[4]. The project surveyed companies with more than 20 employees in a large selection of sectors[5].

This survey has already been used by Flowers et al. (2009), which investigate the role of users in the innovative process, by Filippetti and Archibugi (2009) and Borowiecki and Dziura (2010), which focus on the impact of the crisis upon innovation, and cited in various reports (among others in Kaiser and Kripp (2010)).

Beside usual information about firms' innovation activities usually asked in innovation surveys such as the Community Innovation Survey or the KNOW survey[6], in this case firms have been expressly asked about any public procurement contracts they have been awarded and whether this procurement was innovative or not. Specifically, in the sample of firms which declare to have won at least one public procurement contract since 2006, we took the subsample of firm answering YES to the question "Did at least one of the public procurement contracts that you have won since 2006 include the possibility to sell an innovation (i.e. new or significantly improved products or services)?". Usual information about R&D subsidies have also been asked. We are thus able to create the dummy variable *policy_R&D* which identifies firms that have been granted with R&D subsidies; similarly the variable *Policy_Procurement* defines firms which won an innovative public procurement contract. Thereafter, to take into account the hidden treatment of innovative procurement, we created the variables *policy_R&D_only* and *Policy_Procurement_only* which identify firms which won respectively R&D grants or innovative public procurement only. Since, as it emerges in the literature description, it is also worth analyzing the case when R&D and innovative public procurement act simultaneously on the same firm, we created the variable *Policy_Both*, which takes value 1 for firms receiving both R&D subsidies and a public procurement contract and 0 for firms receiving none of them. Tabel 1 shows the sample size of each group of firms .

[Table 1 about here.]

In order to measure firms' innovative behavior we make use of both input and output indicators. The input indicator is the dummy variable *R&D_increase*, which takes the value 1 if a firm declared a positive change in R&D expenditures after the recipe of public funds. By doing so, we are able both to test for a possible crowding out effect of subsidies and procurement and to easily compare our results with previous literature. At the same time, we introduce an innovation input indicator such as the variable *Innovativeness*, which takes value 1 when most of a firm's sales come from innovative products and 0 otherwise. The use of the indicator *Innovativeness* allows a comparison with literature on the innovative output like in Aschhoff and Sofka (2009), which precisely uses innovative turnover. Additional innovative output variables such as the introduction of a new product, process, or service innovation are used for robustness check. A caveat should be made; all of these outcome variables are dichotomous, that is respondents declared whether they introduce an innovation or not and whether they increase their R&D expenses or not. Therefore, this variable might suffer of an overestimation bias due to its self-reported nature and, thus, results might be distorted in the direction of favoring additionality or complementarity of technology policy with private innovation efforts. However, this is a common issue of innovation survey, which can be dealt with only by a careful interpretation of the results.

[Table 2 about here.]

[3]http://cordis.europa.eu/innovation/en/policy/innobarometer.htm . We rae in debt with Antony Arundel who provided us with the data

[4]a detail description of data collection and of the survey can be read at http://www.proinno-europe.eu/page/innobarometer.

[5]Aerospace engines, Aerospace vehicles, Defence, Analyt. Instr., Constr. Equipment, Apparel, Automotive, Build. Fixtures, Equip., Services, Business services, Chemical Products, Communications equipment, Construction / Materials, Distribution services, Energy, Entertainment, Financial services, Fishing and fishing products, Footwear, Furniture, Heavy construction services, Heavy machinery, Hospitality and tourism, Information technology, Jewellery and precious metals, Leather products, Lighting and electrical Equipment, Lumber & Wood Mfrs, Medical devices, Metal Manufacturing, Oil and gas products and services, Other, Paper, (Bio)Pharmaceuticals, Plastics, Power Generation & Transmission, Processed Food, Publishing and Printing, Sport and Child Goods, Textiles, Transportation and Logistics, Utility.

[6]http://epp.eurostat.ec.europa.eu/portal/page/portal/microdata/cis and Caloghirou et al. (2006)

To account for firms' characteristics we make use of several controls. As a proxy for the size of the firm we use dummies related to the size (SIZE) of the firms (small-medium,medium-large, large enterprises and small enterprises which is taken as the reference category) . Similarly, we introduce a dummy for age (AGE), which takes the value of 1 if a firm has been set up before 2001 and 0 otherwise. The dummy INTL reports if the core activity of the firm is located in its domestic market or abroad; 3 dummies assess whether the firm sells its products or services in its own region, in its own country or internationally; a dummy considers if a firm is doing in house R&D or not (R&D_ww). We also control for the industrial sector (SECTOR[7]) and for the country of origin with 28 country dummies (COUNTRY). We control for firms' R&D investment in the past, since the level of R&D activity can proxy a firm's endogenous ability to write proposal for R&D subsidies and public procurement. This is also usually done in the literature (Almus and Czarnitzki, 2003). Table 3 reports descriptive statistics and Figure 1 tabulates interactions between firms characteristics and policy tools. According to this picture it is not straightforward whether policies have any effect on firms'innovative behavior. At the same time, the picture shows that distribution of policies is constantly biased towards small and medium firms suggesting a possible source of selection bias. On this ground, next section discusses how to use this data to statistically spot a causal effect of two different and potentially coexistent policy tools, innovative public procurement and R&D subsidies, on firms' innovative activity, both in terms of input and output variables. Moreover, it also discusses how to escape the selection bias as emerged from figure 1.

[Table 3 about here.]

[Figure 1 about here.]

### *3.2. Method*

In order to empirically analyze the impact of public policies, the paper exploits the fact that only a small portion of the 5238 firms included in our dataset received either R&D subsidies, innovative public procurement or both(table 1). This allows the design of a quasi-experimental framework in which policy tools are considered as treatment variables and firms are assigned to the treatment, rather than to the control group, on the basis of their participation into different public programs. However, since we are analyzing non-experimental data in an "experimental spirit"(Angrist and Pischke, 2008), two main problems may arise.

In the first place we are aiming at evaluating the effect of two different treatments (technology policy tools) that are not assigned to specific subgroups of different individuals (firms), nor perfect substitutes (Aschhoff and Sofka, 2009) from the individuals' perspective. Hence, in the dataset, we may find firms in four distinct conditions: firms receiving R&D grants only, firms winning innovative public procurement contracts only, firms receiving both R&D grants and innovative procurement contracts and, finally, firms that are not involved at all in any of these programs. Trying to estimate the impact of each of the two policies without taking into account the possible interactions with the other one, may clearly lead to procedural confounding effects as we discussed above. In order to tackle this issue, we exploit the information at the disposal in the dataset and design five different treatments out of the two innovation policy tools possibly adopted: *policy_R&D*, *Policy_Procurement*, *policy_R&D_only*, *Policy_Procurement_only*, *Policy_Both*. The first two treatments do not take into consideration the potential simultaneity of the programs and are hence exposed to the procedural confounding problem defined above. Though they might be biased, the reason to recover estimates for their effect on firms' innovative behavior is dual. On the one hand, the retrieved estimates will be used as term of comparison to effectively check for the existence of a confounding effect. On the other hand, since they will be recovered in a similar setting to the one proposed in the literature (Almus and Czarnitzki, 2003; Aerts and Schmidt, 2008), they will tell us if our results are going in the same direction with the evidence provided so far of the role of R&D grants and innovative public procurement upon firms' innovative activity. The other three treatments take into account every possible interaction between the two policies and are therefore explicitly designed to get rid of potential procedural confounding. The existence of a significant difference between the estimates recovered for the latter treatments with respect to the former ones would imply that procedural confounding produced by the hidden treatment is indeed playing a role and that the estimation recovered for the first two treatments should not be uncritically trusted.

[7] Aggregation based on the NACE 2-digit sectoral level

Secondly, since treatments are not randomly assigned, we may clearly incur in biased estimation due to potential selection biases. As stressed by Aerts and Schmidt (2008), the source of these potential biases is twofold. On the one side, firms receiving both R&D grants or innovative public procurement contracts, are always selected by public institutions that might well cherry-pick winners on the basis of some peculiar characteristics. For example it is very likely that governments are willing to maximize the probability of success of their innovation policies and hence tend to select firms that are already more innovative than others. On the other side, firms that are able to apply for R&D grants or to submit a project for an innovative public procurement competition, possibly possess information or search capability advantages over firms that fail to spot opportunities for application to public programs. For instance, larger firms may have specific staff devoted to this purpose while smaller ones may not. These two sources of potential selection biases make the treated groups, for each treatment, intrinsically distinct from the control groups and do not allow us to interpret a prospective mean difference in their innovative behavior as the causal effect of the technology policies, since the two groups would behave differently even in the absence of the treatment. Formally:

$$ATT = E[Y^T - Y^C|T] + E(Y^C|T) - E(Y^C|C) \tag{1}$$

$$E(Y^C|T) - E(Y^C|C) \neq 0 \tag{2}$$

where ATT is the average treatment effect we are interested in, $Y^T$ is our outcome variable representing the innovative behavior if treated, $Y^C$ is the same outcome variable if untreated and T and C define the belonging to treated or control groups. Clearly $Y^C$|T is not observed and, since the second equation is different from zero (i.e. non zero selection bias), the use of the mean outcome of untreated individuals, E($Y^C$|C), as a substitute for the counterfactual mean for treated, is not possible. For a proper identification of the treatment effect there is the need for an alternative solution.

While the hidden treatment has been mostly neglected in the literature, especially in empirical studies intended to evaluate the effect of R&D subsidies on innovative activities, the selection bias issue has been widely acknowledged and effectively tackled in several works. Here we follow the approach applied by Almus and Czarnitzki (2003) and Aerts and Schmidt (2008), who brought in innovation policy studies non-parametric matching methods. The basic idea of matching is to find a wide group of non-treated individuals that are similar to the treated ones in all relevant pre-treatment characteristics and to use this group as a perfect substitute for the non-observable counterfactual group (Caliendo and Kopeinig, 2008).

For an identification and a consistent estimation of the average treatment effect (ATT) through matching method, two conditions need to be satisfied. The first one is unconfoundedness, or conditional independence assumption(CIA), which formally states:

$$(Y^C; Y^T) \perp W|X \tag{3}$$

This condition implies that assignment to treatment is independent of the outcome (W), conditional on a set of observable covariates(X). For the CIA to be valid all the possible variables affecting the probability of being treated should be known and taken into account. Even though this condition is not testable, it is very likely that it requires a high dimension vector of exogenous covariates to hold true. Since, in that case, exact matching on observables is very difficult to implement, Rosenbaum and Rubin (1983) showed that it is possible to condense the vector of relevant covariates in a single scalar index, called propensity score. This measure is the probability of being treated given the relevant covariates. At a given value of the propensity score, the exposure to treatment should be random and therefore both treated and control units should be on average observationally identical.

The second requirement that has to be satisfied is the common support condition. It ensures that the vector of relevant covariates is not by itself able to perfectly predict whether an individual is receiving a treatment or not.

$$0 < P(T|X) < 1 \tag{4}$$

Thus, we should not observe a significant share of firms which, given the relevant observable characteristics, are assigned with certainty to the group receiving R&D subsidies or winning innovative public procurement contracts.

If both conditions hold, propensity score matching produces unbiased estimates of the average treatment effect considering the difference in outcomes over the common support, weighted by the propensity score of individuals (Caliendo and Kopeinig, 2008). Formally:

$$Psm_{ATT} = E_{p(x)}|T\{E[Y^T|T, Pr(X)] - E[Y^C|C, Pr(X)]\} \tag{5}$$

As in the case of Almus and Czarnitzki (2003), given the abundance of information on firms' characteristics available, we implement propensity score matching to mitigate potential selection biases, assuming the CIA condition to hold. In the next sections we illustrate the propensity score specification, discuss matching quality in terms of balancing and common support assumption and eventually present results.

### *3.3. Propensity score specification*

As pointed out in the previous section, the propensity score consists of a measure of the probability for an individual to be treated conditional to a set of relevant characteristics. The first step is therefore the detection of those variables affecting the likelihood of the treatment. Caliendo and Kopeinig (2008) provide some practical guidance to tackle the issue of variables selection:

> Only variables that influence simultaneously the participation decision and the outcome variable should be included. Hence, economic theory, a sound knowledge of previous research and also information about the institutional settings should guide the researcher in building up the model [...]. It should also be clear that only variables that are unaffected by participation should be included in the model. To ensure this, variables should either be fixed over time or measured before participation (Caliendo and Kopeinig, 2008, p. 39 ).

Following their suggestion, we make explicit reference to the literature using propensity score matching applied to innovation policy tools and list the possible candidates as relevant covariates. Thereafter, a probit model will estimate the impact of those variables on the probability for a firm to be treated. Following Almus and Czarnitzki (2003), we include variables collecting firms' characteristics discussed in section 3 as covariates. Specifically dummies for sector, age, country of origin, location of the core market and size of the firm and a dummy for the presence of a R&D department within company's wall.

As mentioned in section 3.1 the dataset exhibits a cross-sectional structure with data for a 3 year time period (2006-2008) and the information on the firms gathered through a survey conducted during April 2009. Firms characteristics are hence recorded after the potential treatment had been administered. We thus have to assume firms' features as fixed over time and, hence, unaffected by any of the treatment. While this assumption is reasonable for variables such as country of origin, industrial sector, age and core activity location, this is not necessary the case for the size of the firm and the in-house performing of R&D. Almus and Czarnitzki (2003) consider the possible endogeneity issue for the size variable but underline that it should not be a severe problem since, at least for R&D subsidies, there are usually only a few programs implemented with the aim of raising R&D personnel. Thus R&D staff is quite stable over time. In the case of innovative public procurement, the problem could be sharper. Nonetheless, even though it is in fact possible that firms winning procurement contracts are more prone to increase the number of employees (both R&D and non-R&D), there is still little evidence in the field literature that public technology procurement increases employment (Slavtchev and Wiederhold, 2011). Furthermore labor economics literature suggests that, due to convexities in the adjustment cost function and to indivisibility, employment adapts in a sticky way to shocks in demand. Moreover, we do not measure size through a continuous variable but by means of 4 dummies for small(20-49, employees), medium (50-249), medium-large (249-500) and large (500+) enterprises. For this reason, a possible change in size should be negligible because only in a small number of cases only would switch one firm from one class to the higher.

To a lesser extent also the variable R&D_ww, capturing the presence of a R&D department within company's wall, might present some risk of endogeneity. Almus and Czarnitzki (2003) discard the hypothesis that R&D grants can be awarded to firms with no R&D department at all since they focus on Eastern Germany, where programs designed to support the founding of an entirely new R&D department did not exist. Since there is no evidence of the existence of programs as such for other EU members, Norway and Switzerland either we also discard the endogeneity of this variable at least for the R&D subsidies treatment. Also in the case of innovative public procurement there is only a minor risk of endogeneity because it is very likely that a company can submit a project to compete for an innovative procurement contract only if some R&D within its walls is already implemented.

Unfortunately, as mentioned in section 3.1, the dataset lacks variables taking into account the economic performance of the firm or proxies for its market share measured before participation into treatments. The dataset contains some information about trends in companies' turnover, but we do not include them in the analysis as it is generally done in similar works (Almus and Czarnitzki, 2003; Aerts and Schmidt, 2008; Aschhoff and Sofka, 2009) since it is possible that increasing (decreasing) revenues are affected by the treatments. This is especially true in the case of innovative public procurement, because winning a tender has an unambiguous impact on a firm's turnover and it is reasonable for R&D subsidies as well. In the three years time span in which the data are collected, a R&D grant received in 2006 might lead to an innovation embodied in a product (service) whose sales determined an increase in revenues, for a specific firm only, in 2008. However, the size of the firm, the country, and the sector may collect some of the aggregate demand fluctuations affecting firms' economic performance and reduce the portion of variation remaining unexplained due to the omission of turnover variables. In line with this reasoning, including turnover variables in the probit regression is only slightly modifying the propensity score model estimation and is not changing significantly any of the results in terms of ATT presented in section 4.

[Table 4 about here.]

[Table 5 about here.]

Once that the relevant variables are identified we estimate five different propensity scores, one for each treatment; the results of the five probit regressions are presented in table 4. The outcomes confirm that several sectoral and country dummies are significantly affecting the probability of receiving treatments. As expected, the variable that has the clearest influence on the likelihood of being treated is the one reporting if a firm do perform *intra muros* R&D or not. While size appears to have a major impact on the probability of winning an innovative public procurement contract, in contrast with Almus and Czarnitzki (2003)'s findings, it exhibits a limited effect on the probability of obtaining R&D subsidies; specifically, medium and medium large enterprises have higher odds of receiving R&D grants than very small and very large companies.

We then use the propensity score for each observation and each treatment to perform a non-parametric matching. Since there are two different outcome variables, one related to innovative output and one related to innovative input, we perform two distinct matchings with the purpose of maximizing the quality of the pairing. In order to capture the average treatment effect both for different treatments and outcomes, we implement 10 pairing procedures. As matching algorithms[8] we implemented kernel matching[9]. As pointed out by Caliendo and Kopeinig (2008), the choice of the algorithm to apply is a matter of trade-off in terms of bias and efficiency of the estimator and this choice abundantly relies on the nature of the data at hand. The Kernel matching estimator calculates the counterfactual outcome for each treated individual using the weighted averages of observations from all individuals in the control group and assigns higher weight to observations closer in terms of propensity score. Thus the kernel matching estimator provides some advantages in terms of lower variance because it uses more information than other algorithms such as nearest neighbor. However, because this raise in efficiency can lead to a higher bias, in section 4 we present a comparison with results obtained via nearest neighbor matching method, which should instead returns results characterized by lower bias and higher variance.

A key step in this procedure is the evaluation of the matching quality, that is the assessment of the ability of the matching procedure to balance the distribution of the relevant variable in the control and the treatment group. The literature puts forward several methods; Rosenbaum and Rubin (1985) suggest a procedure that computes the standardized bias for each of the relevant covariates as a percentage of the square root of sample variance in treated

[8]See Caliendo and Kopeinig (2008) for a discussion.

[9]To implement the matching we used the stata module psmatch2, developed by Leuven and Sianesi (2003)

and not treated groups. Generally a reduction of the mean standardized bias under the 3 % or 5% threshold after matching is usually considered as sufficient to support the success of the procedure. Sianesi (2004) proposes to consider propensity score on the matched sample only and then to compare the pseudo-$R^2$ for treated and non-treated participants, before and after the matching. Since the pseudo-$R^2$ somehow grasps the extent to which the variation in the sample is explained by the vector of the relevant covariates, once that the sample is matched conditioning on this vector, the pseudo-$R^2$ on the matched sample conditioned on this vector should be much lower than in the unmatched case. Moreover, it is possible to perform a likelihood ratio test for the joint insignificance of all the regressors: the test should be rejected before matching and not rejected after the matching procedure. The three methods described above are applied to all the matching performed in the paper.

[Table 6 about here.]

The results reported in table 6 show how, for all the estimations, the mean standardize bias (Meanbias) falls below the 3% threshold after the matching, the pseudo-$R^2$ considerably decreases passing from the raw to the matched sample and how the likelihood ratio test ( LR chi$^2$) leads us to always reject the hypothesis of joint insignificance before the matching and to never reject it for the matched sample. The overall matching performance appears hence to be good.

Finally, as it is pointed out by Caliendo and Kopeinig (2008), there is the need of assessing the overlapping between subsamples through a graphic analysis of the propensity score's density distribution, in both treated and controls group. Before the matching procedure the two distributions should differ but they still need to have a support that partially overlaps. Otherwise the common support condition, presented in the section 3.2, would be violated because the relevant covariates would be able to perfectly predict if a firm is receiving a treatment or not. Intuitively the matching procedure is implemented to "correct" for the difference in the distribution, that can be thought as a visual representation of the selection bias. After the matching, the two distributions should therefore be more similar and have a much larger common support. In figure 2 we report the graphs of the density distribution of the estimated propensity scores for treated and control group before the matching.

[Figure 2 about here.]

As expected, for every treatment there are some differences in the density distributions among the two subsamples, nonetheless, as required, the common support condition appears to hold everywhere.

[Figure 3 about here.]

Figure 3 instead reports the density distribution of the propensity score for each treatment, after the pairing is implemented, for the outcome variables we are interested in. The graphs show that the propensity score matching abundantly reduces the dissimilarities in the distributions. Moreover the high degree of overlapping signals the good quality of the matching procedure.

## 4. Results

Since the goodness of the matching performance appears to hold, we can cautiously interpret the average treatment effects, estimated through multiple propensity score matching procedures, as the causal impact of the five different treatments on firms' innovative behavior. The results of the estimations are reported in table 7 for the two outcome variables and for each treatment. Figure 4 graphically depicts the results.

[Table 7 about here.]

[Figure 4 about here.]

The table includes the average outcomes for treated and control groups, both before and after the matching. We are interested in the ATT value of the column "difference", which is the difference in averages between the two groups after the pairing as discussed in section 3.2[10] . Since the outcome variables are dichotomous, the average outcomes in the table represent a participation rate. For instance, when looking at the *R&D_increase*, the average outcomes display the share of the firms that increase their R&D spending both for the treated and the control group. The average treatment effect in the column "difference" should therefore be interpreted as the change in percentage points of the proportion of firms which increase their R&D spending after participating to a given technology policy. For the case of innovative procurement, the number of firms which increase their R&D spending is 12.9 percentage points higher among firms which won an innovative public procurement contract.

[Table 8 about here.]

As was pointed out in section 3.2 the first two treatments that we are taking into account, *Policy_R&D* and *Policy_Procurement*, are vulnerable to potential confounding effects because there is no control for interactions among them. However, we show the results to lay down a comparison with previous literature which never discussed the possible interactions or the existence of a hidden treatment.

For what concerns *Policy_R&D* treatment our results seem to be coherent with the ones provided by the large body of literature (Almus and Czarnitzki, 2003; Aerts and Schmidt, 2008; González et al., 2005) that reports no evidence of crowding-out effect (or substitutability) on private investments in R&D due to R&D subsidies. Moreover, our results appear to confirm the reinforcing effect between public R&D subsidies and private R&D efforts, found by Almus and Czarnitzki (2003). Receiving R&D grants seems indeed to have a positive and significant impact in terms of innovative inputs since there are 6,5 percentage points more firms that are increasing their R&D expense in the treated group than in the control group. However, no evidence of any effect upon the innovation outcome in term of turnover (*Innovativeness*), in line with findings from Aschhoff and Sofka (2009).

Also for the treatment *Policy_Procurement* results are rather consistent with the evidence delivered by the growing but still limited literature on the role of innovative public procurement as a technology policy tool. As in Lichtenberg (1988) and Slavtchev and Wiederhold (2011), we find positive and significant effect of innovative public procurement on private expenses for R&D (there are 12 percentage points more firms increasing their R&D expenditure in the treated than in the control group), and as in Aschhoff and Sofka (2009) we recover a positive and significant impact for the variable *Innovativeness* (9.3 percentage points more firms in the treated than in the control group report that most of their sales are coming from innovative product or service). Furthermore, a first comparison among the two policies seems to support the theoretical hypothesis made by Geroski (1990) that, under some circumstances, innovative public procurement is more effective than R&D grants both in generating successful innovations and in stimulating private investments in R&D.

The *Policy_R&D_only* and the *Policy_Procurement_only* treatments consider instead each policy in isolation from the other in order to get rid of the potential procedural confounding, as explained in section 3.2. The most interesting result here comes from comparing the impact for *Policy_R&D_only* and *Policy_R&D* treatment. While also in the isolation case there is no evidence supporting the existence of a crowding-out effect, the positive impact of R&D grants on R&D private investment remarkably reduces and ceases to be significant. Our hypothesis that innovative public procurement might represent a crucial confounding factor in evaluating the impact of innovation policies such as R&D subsidy programs, seems therefore to be corroborated. We hence may speculate that at least a portion of the reinforcing effect between public R&D subsidies and private R&D efforts found in earlier works (Almus and Czarnitzki, 2003), might be explained by the fact that those studies have not taken this sort of hidden treatment into account in their estimation. While the procedural confounding seems to play a key role in the evaluation of the impact of R&D subsidies on firms' innovative behavior, this does not appear to happen for innovative public procurement. Table 7 reveals how differences between treated and control groups are positive and significant also in the case of *Policy_Procurement_only* treatment and not dissimilar from the ones recovered for *Policy_Procurement*. Once again this result is coherent with the findings by Aschhoff and Sofka (2009), that implicitly took into consideration the potential complementarity of different policy programs and reported a robust positive effect of procurement and no impact for R&D subsidies on innovative turnover.

[10] The line "unmachted" is reported to appreciated the different between the unbiased estimations (ATT) and the biased ones where the significance levels are inflated.

The last treatment of our concern is *Policy_Both*, which considers the impact of receiving simultaneously R&D subsidies and winning innovative public procurement contracts. Table 7 shows how the concurrence of the two policy tools is having a positive and robust effect both on innovation input and output. We have in fact 20 percentage points more firms in the subsample receiving the *Policy_Both* treatment than in the control subsample which are increasing their private expenses in R&D. This result provides us with solid evidence of complementarity between the two policy programs and also of additionality with private investment in R&D. It is also offering us a double check for the relevance of the procedural confounding issue in evaluating R&D subsidy programs. If we considered R&D grants without taking into account the potential hidden treatment, firms receiving only R&D grants and firms both receiving subsidies and winning innovative public procurement contracts would have been thought as qualitatively identical. The comparison between the *Policy_Both* and the *Policy_R&D_only* programs indicates instead that these treatments determine very different innovative behaviors and suggests that, for firms getting both treatment, innovative public procurement is pivotal to have a robust effect on both R&D private investments and *Innovativeness*, once again in line with Geroski's hypothesis (Geroski, 1990). However, is also noteworthy the fact that the *Policy_Both* effect on R&D private investment outperforms the impact of the *Policy_Procurement_only* in terms of magnitude (20 percentage points more firms than in the control group are increasing R&D investments when receiving *Policy_Both* treatment, while only 8,7 percentage points more firms in the treated than in the non-treated subsample are increasing their R&D expenses with *Policy_Procurement_only*) confirming therefore strong complementarity between the two policy tools.

In table 8 we show the same analysis for other innovative output variables. As discussed in 3.2, these other variables may suffer of overestimation or endogeneity. Therefore, we do not fully trust them for a causal interpretation, but we do report them in the table to show that they are in line with the other results. To further asses the robustness of our estimates in table 9 we also report the results retrieved using nearest neighbor matching algorithm rather than the kernel one. As we mentioned in 3.2 while the latter approach, that we used so far, has the advantage to increase the efficiency of the estimators, the former guarantees lower biases. As the table shows the sign and the significance of the effect of different treatments on innovative input and output are not changed by the algorithm shift, confirming the relevance of innovative procurement as a confounding factor and the strong complementarity between innovative public procurement and R&D subsidies.

[Table 9 about here.]

## 5. Conclusion

In this paper we analyzed the effect of two technology policies, public R&D subsidies and innovative public procurement, on firms' innovative behavior both in terms of innovative input and output. While the role of the former policy tool has been extensively investigated in the literature, the latter has only recently regained attention and empirical evidence is still fragmented. Moreover, very limited work has been conducted taking into consideration the relevance of the possible interactions among the two policies. Our work tries to fill in this gap. We especially hypothesized that evaluating the impact of a policy tool in a quasi-experimental setting without controlling for simultaneous public programs aiming at the same objective, can lead to procedural confounding due to hidden treatments. Previous studies on the effect of R&D funds on firms' private investments in R&D, not considering innovative public procurement as a probable candidate for hidden treatment, could typically have incurred in this problem.

In order to corroborate our hypothesis we used data from the Innobarometer on Strategic trends in innovation 2006-2008, a survey conducted in the 27 Member States of the EU, Norway and Switzerland for 5238 firms. To evaluate the impact of each policy tool on firms' innovative behavior and to assess the potential significance of the confounding effect, we designed five quasi-experimental treatments: two that do not consider possible policies simultaneity and three that take into account programs interactions. We have studied the effect of both an innovation input variable (*R&D_increase*) and an innovation output one (*Innovativeness*). To reduce the selection bias that typically affects a quasi-experimental setting, we used propensity score matching method.

The results of the paper challenge the state of the art in the field and call for a deeper understanding of the technology policy mix. In the first place, findings are coherent with the evidence in previous literature only when there is no control for policies interactions; in this case, public R&D subsidies are positively and significantly affecting private investment in R&D, ruling out the crowding out effect hypothesis and confirming the complementarity between public and private expenses in R&D (Almus and Czarnitzki, 2003). Innovative public procurement has a robust impact both on private expenses on R&D and on innovative turnover. In terms of magnitude our results seem to confirm the theoretical hypothesis that innovative public procurement is more effective than R&D grants both in generating successful innovations and in stimulating private investments in R&D (Geroski, 1990).

When we consider the possible interactions of other policies, results show a different picture. The reinforcement effect of R&D public funds on R&D private investment ceases to be significant for firm exclusively participating in R&D subsidy programs. This is in line with the suggested existence of a procedural confounding effect produced by innovative public procurement as a hidden treatment. This evidence casts some doubts on causal relationships found in earlier works, which should be reconsidered. The same effect does not appear to work in the opposite direction, since innovative public procurement considered in isolation is still having positive and significant impact on innovative input and output, again coherently with Geroski (1990) and Aschhoff and Sofka (2009). The most interesting case is the contextual effect of both R&D subsidies and innovative public procurement: the effect upon both increase in R&D and *Innovativeness* is significant and higher than the sum of the effects of the two policies considered in isolation. This further corroborate the idea that a balance of the two policies should be the best choice.

From a policy point of view our results recommend to carefully consider the interaction among different tools in composing technology policy mixes. Our findings suggest that innovative public procurement is not only able by itself to have a positive impact on firms' innovative behavior both in terms of input and output, but that it could also represent an effective way to reinforce potential positive effects of public R&D subsidies, stimulating additional private investments in R&D.

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**List of Figures**

Figure 1: Descriptive

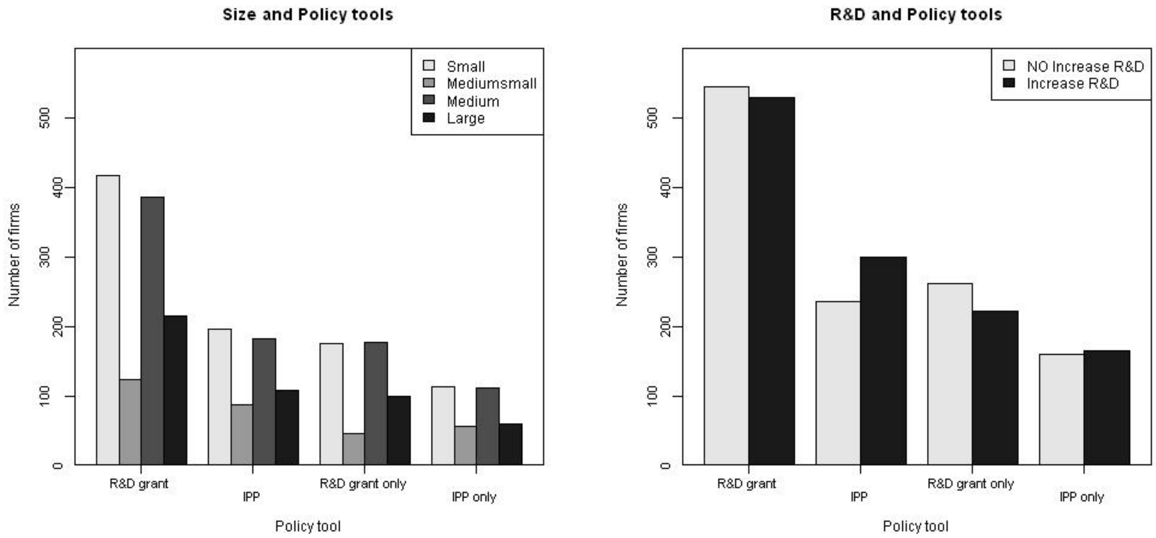


Figure 2: Distributions of the propensity score for treated and not treated before matching

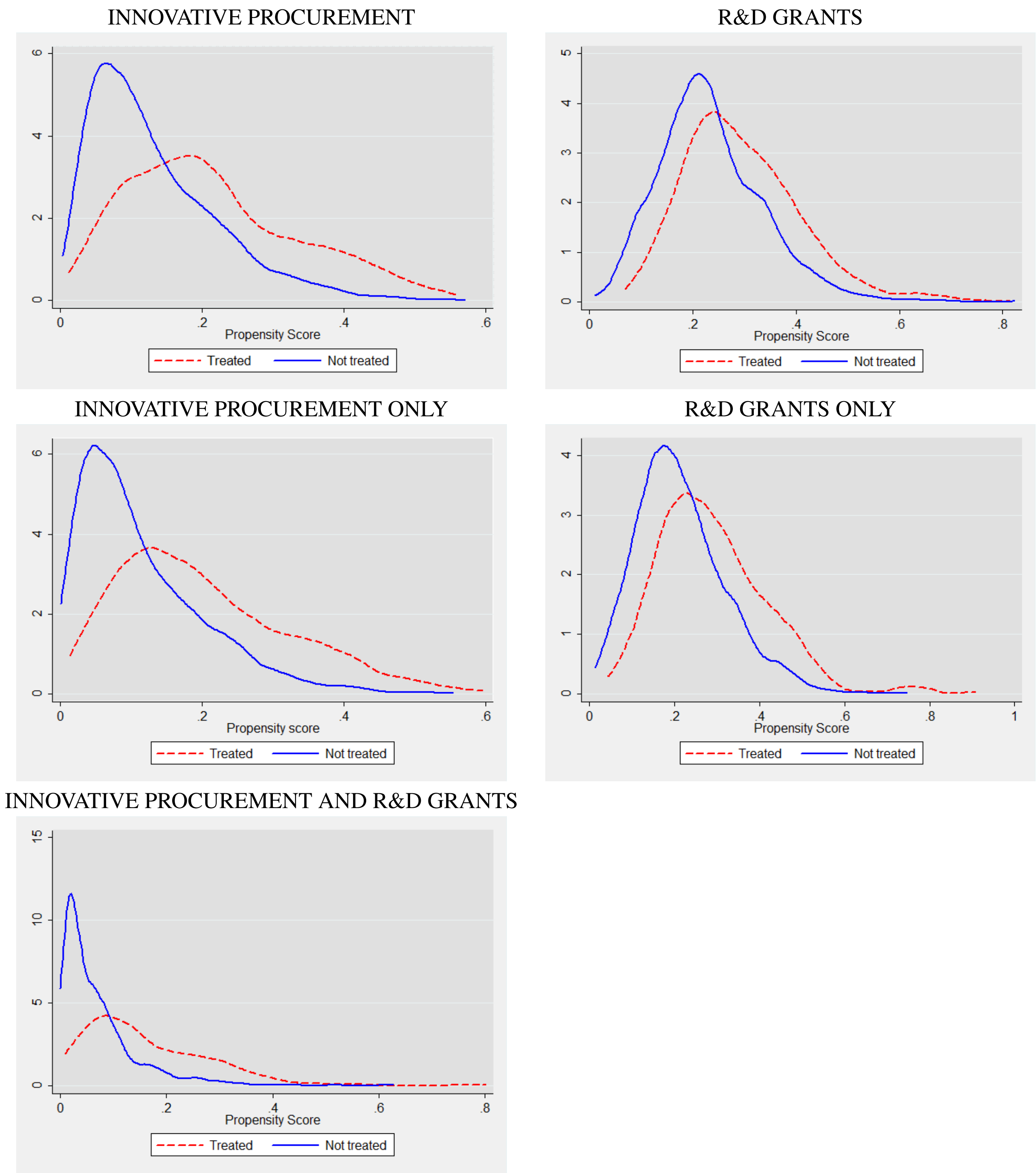
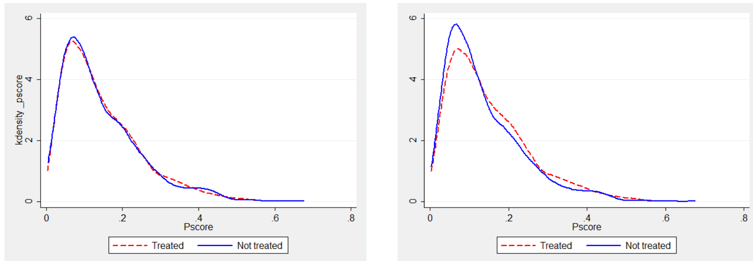


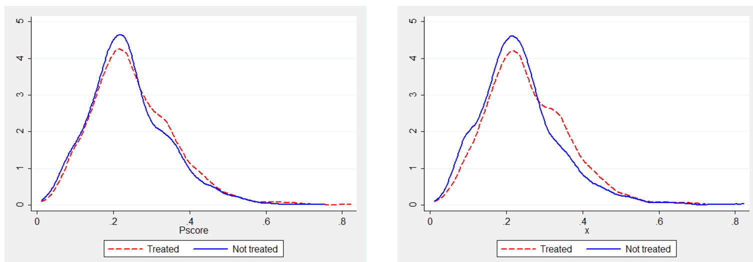
Figure 3: Distributions of the propensity score for treated and not treated after each matching

R&D_increase Innovativeness

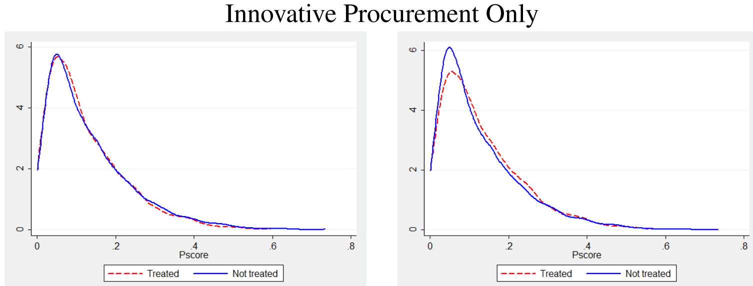
Innovative Procurement



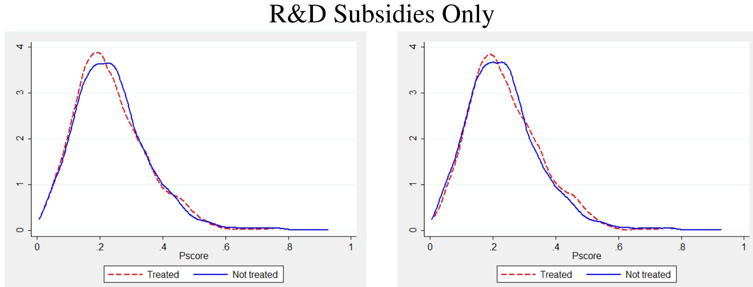

R&D Subsidies




Innovative Procurement Only




R&D Subsidies Only




R&D Subsidies and Innovative Procurement

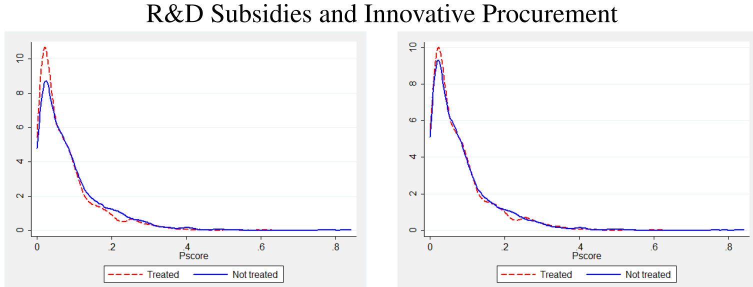
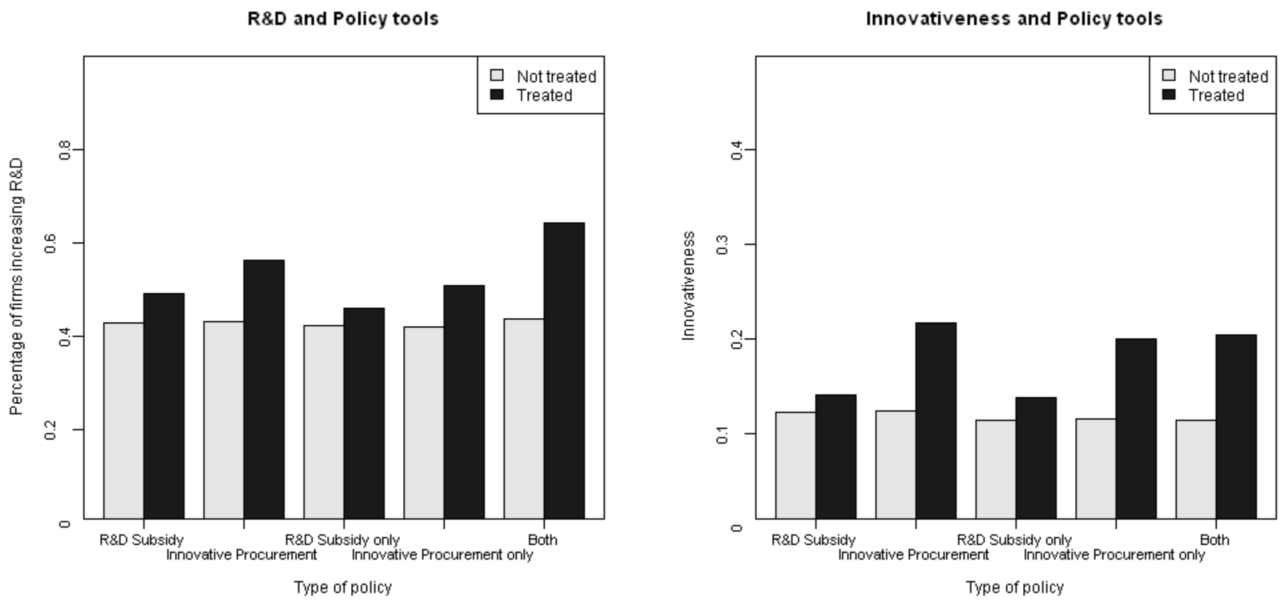


Figure 4: Results



**List of Tables**

Table 1: Treated firms

| **Treatment** | **Number of firms** |
|---|---|
| R&D subsidies | 1140 |
| Innovative Procurement | 573 |
| R&D subsidies only | 500 |
| Innovative procurement only | 341 |
| Innovative Procurement and R&D subsidies | 183 |

Table 2: Summary statistics

| Variable | Mean | Std. Dev. | Min. | Max. | N |
|---|---|---|---|---|---|
| Innoprod | 0.564 | 0.496 | 0 | 1 | 4693 |
| Innoserv | 0.516 | 0.5 | 0 | 1 | 4895 |
| Innoproc | 0.570 | 0.495 | 0 | 1 | 4876 |
| Innovativeness | 0.129 | 0.335 | 0 | 1 | 3946 |
| R&D_increase | 0.426 | 0.494 | 0 | 1 | 4664 |

Table 3: Summary statistics

| Variable | Mean | Std. Dev. | Min. | Max. | N |
|---|---|---|---|---|---|
| R&D_ww | 0.47 | 0.499 | 0 | 1 | 5234 |
| SIZE | 2.273 | 1.165 | 1 | 4 | 5234 |
| INTL | 0.522 | 0.5 | 0 | 1 | 5234 |
| REG1 | 0.903 | 0.296 | 0 | 1 | 5234 |
| REG2 | 0.002 | 0.05 | 0 | 1 | 5234 |
| REG3 | 0.09 | 0.286 | 0 | 1 | 5234 |
| NAT1 | 0.666 | 0.472 | 0 | 1 | 5234 |
| NAT2 | 0.317 | 0.465 | 0 | 1 | 5234 |
| NAT3 | 0.003 | 0.053 | 0 | 1 | 5234 |
| EU1 | 0.542 | 0.498 | 0 | 1 | 5234 |
| EU2 | 0.438 | 0.496 | 0 | 1 | 5234 |
| EU3 | 0.003 | 0.059 | 0 | 1 | 5234 |
| AGE | 0.918 | 0.275 | 0 | 1 | 5234 |

Table 4: Probit regression results

| | (1) INNO Procurement | (2) R&D Subsidies | (3) R&D Subsidies Only | (4) INNO Procurement Only | (5) Both Policies |
|---|---|---|---|---|---|
| R&D_ww | 0.455$^{***}$ | 0.347$^{***}$ | 0.325$^{***}$ | 0.464$^{***}$ | 0.632$^{***}$ |
| SIZE2 | 0.361$^{***}$ | 0.0805 | 0.182 | 0.498$^{***}$ | 0.0765 |
| SIZE3 | 0.0427 | 0.0581 | 0.158$^{*}$ | 0.0866 | -0.0999 |
| SIZE4 | 0.0790 | 0.0512 | 0.202$^{*}$ | 0.0971 | -0.0109 |
| SECTOR2 | -0.226 | -0.0597 | 0.218 | -0.633 | 0.243 |
| SECTOR3 | 0.130 | -0.0567 | 0.0159 | 0.190 | -0.196 |
| SECTOR4 | -0.584$^{**}$ | -0.0146 | -0.0137 | -0.671$^{*}$ | -0.512 |
| SECTOR5 | -0.304 | 0.384$^{**}$ | 0.390$^{*}$ | -0.0651 | |
| SECTOR6 | -0.123 | 0.0642 | 0.195 | -0.138 | 0.0114 |
| SECTOR7 | -0.0389 | 0.272$^{**}$ | 0.307$^{*}$ | -0.0820 | 0.286 |
| SECTOR8 | -0.363$^{*}$ | 0.150 | 0.333$^{*}$ | -0.0837 | -0.790$^{*}$ |
| SECTOR9 | -0.547$^{*}$ | 0.00672 | 0.0232 | -0.356 | -0.641 |
| SECTOR10 | -0.526 | 0.124 | 0.207 | | -0.0763 |
| SECTOR11 | 0.0640 | -0.113 | -0.648 | 0.153 | 0.106 |
| SECTOR12 | -0.102 | 0.0232 | 0.0992 | 0.0899 | |
| SECTOR13 | 0.289 | -0.186 | | 0.288 | 0.271 |
| SECTOR14 | 0.383 | -0.149 | | 0.261 | 0.504 |
| SECTOR15 | 0.0595 | -0.104 | 0.102 | 0.386 | -0.585 |
| SECTOR16 | 0.0223 | 0.167 | 0.251 | 0.222 | 0.0512 |
| SECTOR17 | -0.131 | -0.0730 | 0.000245 | -0.118 | -0.0201 |
| SECTOR18 | 0.0590 | 0.0844 | -0.0348 | 0.0369 | 0.100 |
| SECTOR19 | 0.326 | 0.119 | -0.108 | 0.863 | |
| SECTOR20 | 0.447 | 1.223$^{***}$ | 1.484$^{*}$ | | 1.813$^{***}$ |
| SECTOR21 | 0.349$^{**}$ | 0.182 | -0.0346 | 0.375$^{**}$ | 0.396$^{*}$ |
| SECTOR22 | -0.253 | -0.0704 | -0.220 | -0.191 | -0.377 |
| SECTOR23 | -0.0469 | 0.0695 | -0.124 | -0.0758 | 0.139 |
| SECTOR24 | 0.477$^{***}$ | 0.189$^{*}$ | 0.125 | 0.482$^{***}$ | 0.602$^{***}$ |
| SECTOR25 | -0.0915 | 0.835$^{***}$ | 1.174$^{**}$ | 0.174 | 0.931 |
| SECTOR26 | 0.0717 | -0.889 | -0.590 | 0.331 | |
| SECTOR27 | 0.0839 | 0.187 | 0.531 | -0.0314 | 0.119 |
| SECTOR28 | -0.166 | -0.0123 | 0.328 | -0.0791 | |
| SECTOR29 | 0.728 | -0.0582 | | 0.581 | |
| SECTOR30 | -0.460 | -0.194 | -0.684 | -0.274 | |
| SECTOR31 | 0.602$^{**}$ | 0.315 | 0.203 | 0.503 | 0.847$^{**}$ |
| SECTOR32 | 0.387 | -0.000495 | 0.147 | 0.248 | 0.339 |
| SECTOR33 | -0.539 | -0.546 | -0.264 | -0.200 | |
| SECTOR34 | 0.0253 | 0.0213 | -0.0957 | -0.0139 | 0.193 |
| SECTOR35 | -0.0930 | -0.385 | 0.0983 | 0.313 | |
| SECTOR36 | 0.0913 | 0.673$^{***}$ | 0.245 | 0.273 | 0.346 |
| SECTOR37 | -0.290 | 0.173 | -0.329 | | 0.389 |
| *N* | 3993 | 4552 | 2141 | 2719 | 2405 |

$^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$

CONT.

Table 5: Probit regression results (Cont.)

| | (1) INNO Procurement | (2) R&D Subsidies | (3) R&D Subsidies Only | (4) INNO Procurement Only | (5) Both Policies |
|---|---|---|---|---|---|
| COUNTRY2 | -0.396$^{*}$ | 0.00847 | -0.219 | -0.530$^{*}$ | -0.219 |
| COUNTRY3 | -0.203 | -0.522$^{***}$ | -0.649$^{*}$ | -0.256 | -0.654$^{*}$ |
| COUNTRY4 | -0.628$^{**}$ | -0.140 | -0.426 | -0.684$^{*}$ | -0.830$^{*}$ |
| COUNTRY5 | -0.634$^{**}$ | -0.707$^{***}$ | -1.153$^{***}$ | -0.810$^{**}$ | -0.667$^{*}$ |
| COUNTRY6 | -0.705$^{***}$ | -0.103 | -0.242 | -0.762$^{**}$ | -0.600 |
| COUNTRY7 | -0.629$^{**}$ | -0.0974 | -0.257 | -0.824$^{**}$ | -0.540 |
| COUNTRY8 | -0.395$^{*}$ | -0.265 | -0.321 | -0.528$^{*}$ | -0.720$^{*}$ |
| COUNTRY9 | -0.524$^{**}$ | 0.0761 | -0.207 | -0.822$^{**}$ | -0.229 |
| COUNTRY10 | -0.744$^{***}$ | -0.669$^{***}$ | -0.942$^{***}$ | -0.916$^{***}$ | -0.944$^{**}$ |
| COUNTRY11 | -0.632$^{*}$ | -0.140 | -0.265 | -0.754$^{*}$ | -0.468 |
| COUNTRY12 | -0.352 | -0.497$^{**}$ | -0.603$^{*}$ | -0.478$^{*}$ | -0.447 |
| COUNTRY13 | -0.206 | -0.226 | -0.413 | -0.158 | -0.439 |
| COUNTRY14 | 0.0287 | -0.382 | -0.530 | -0.0227 | -0.537 |
| COUNTRY15 | -0.383 | -0.0666 | -0.154 | -0.574 | -0.177 |
| COUNTRY16 | -0.394 | 0.100 | -0.271 | -0.499 | -0.250 |
| COUNTRY17 | -0.410$^{*}$ | -0.554$^{***}$ | -0.752$^{**}$ | -0.417 | -0.544 |
| COUNTRY18 | -0.380 | -0.195 | -0.462 | -0.943$^{**}$ | -0.226 |
| COUNTRY19 | -0.168 | -0.0593 | -0.261 | -0.301 | 0.00256 |
| COUNTRY20 | -0.173 | -0.256 | -0.375 | -0.465 | -0.165 |
| COUNTRY21 | -0.369$^{*}$ | -0.241 | -0.495$^{*}$ | -0.366 | -0.558 |
| COUNTRY22 | -0.783$^{***}$ | -0.0873 | -0.361 | -0.994$^{***}$ | -0.538 |
| COUNTRY23 | -0.255 | -0.401$^{**}$ | -0.704$^{*}$ | -0.327 | -0.449 |
| COUNTRY24 | -0.232 | -0.377$^{*}$ | -0.506 | -0.312 | -0.570 |
| COUNTRY25 | -0.450$^{*}$ | -0.628$^{***}$ | -0.826$^{**}$ | -0.656$^{**}$ | -0.767$^{*}$ |
| COUNTRY26 | -0.224 | -0.331$^{*}$ | -0.455 | -0.447 | -0.0491 |
| COUNTRY27 | -0.644$^{**}$ | -0.246 | -0.504$^{*}$ | -0.948$^{***}$ | -0.346 |
| COUNTRY28 | -0.511$^{*}$ | -0.851$^{***}$ | -0.657 | -0.575$^{*}$ | -0.960$^{*}$ |
| COUNTRY29 | -0.121 | -1.063$^{***}$ | | -0.156 | -0.681 |
| REG1 | 0.701 | 0.517 | 0.725 | 0.526 | 4.101 |
| REG2 | 0.298 | | | 0.308 | |
| REG3 | 0.543 | 0.476 | 0.713 | 0.346 | 3.952 |
| NAT1 | 0.0582 | 0.0163 | -0.642 | -0.471 | 0.853 |
| NAT2 | -0.116 | 0.0518 | -0.515 | -0.688 | 0.820 |
| NAT3 | -0.230 | 0.596 | -0.0260 | -0.918 | |
| EU1 | -0.153 | -0.344 | 0.308 | 0.398 | -0.922$^{*}$ |
| EU2 | -0.204 | -0.266 | 0.290 | 0.178 | -0.979$^{*}$ |
| EU3 | 0.110 | -0.915 | 0.167 | 0.995 | |
| AGE | 0.0245 | -0.0658 | -0.270$^{*}$ | -0.0575 | -0.0636 |
| INTL | 0.0318 | 0.0635 | -0.00754 | -0.00647 | -0.102 |
| *N* | 3993 | 4552 | 2141 | 2719 | 2405 |

$^{*}$ $p < 0.05$, $^{**}$ $p < 0.01$, $^{***}$ $p < 0.001$

Table 6: Balance

| INNOVATIVE PROCUREMENT | Sample | Pseudo R2 | LR chi2 | p chi2 | MeanBias | MedBias |
|---|---|---|---|---|---|---|
| R&D_increase | Raw | 0.0940 | 287.3 | 0 | 7 | 4.800 |
| | Matched | 0.00500 | 7.440 | 1 | 1.600 | 1.300 |
| Innovativeness | Raw | 0.0920 | 260.4 | 0 | 7 | 4 |
| | Matched | 0.00900 | 13.05 | 1 | 1.900 | 1.300 |

| R&D SUBSIDIES | Sample | Pseudo R2 | LR chi2 | p chi2 | MeanBias | MedBias |
|---|---|---|---|---|---|---|
| R&D_increase | Raw | 0.0550 | 260.0 | 0 | 4.600 | 3.600 |
| | Matched | 0.00200 | 7.150 | 1 | 0.900 | 0.600 |
| Innovativeness | Raw | 0.0530 | 217.2 | 0 | 4.500 | 3.500 |
| | Matched | 0.00700 | 17.07 | 1 | 1.600 | 1.200 |

| R&D SUBSIDIES ONLY | Sample | Pseudo R2 | LR chi2 | p chi2 | MeanBias | MedBias |
|---|---|---|---|---|---|---|
| R&D_increase | Raw | 0.0670 | 148.4 | 0 | 5.500 | 4.300 |
| | Matched | 0.00400 | 5.370 | 1 | 1.300 | 1.100 |
| Innovativeness | Raw | 0.0660 | 127.0 | 0 | 5.900 | 4.600 |
| | Matched | 0.0100 | 12.33 | 1 | 2.300 | 1.800 |

| INNOVATIVE PROCUREMENT ONLY | Sample | Pseudo R2 | LR chi2 | p chi2 | MeanBias | MedBias |
|---|---|---|---|---|---|---|
| R&D_increase | Raw | 0.111 | 213.2 | 0 | 7.900 | 5.700 |
| | Matched | 0.00700 | 5.830 | 1 | 1.900 | 1.400 |
| Innovativeness | Raw | 0.111 | 195.8 | 0 | 7.900 | 4.900 |
| | Matched | 0.0130 | 11.22 | 1 | 2.400 | 1.900 |

| R&D SUBSIDIES and INNOVATIVE PROCUREMENT | Sample | Pseudo R2 | LR chi2 | p chi2 | MeanBias | MedBias |
|---|---|---|---|---|---|---|
| R&D_increase | Raw | 0.141 | 170.8 | 0 | 10 | 8.700 |
| | Matched | 0.0150 | 6.930 | 1 | 2.700 | 2.300 |
| Innovativeness | Raw | 0.134 | 149.0 | 0 | 9 | 7.600 |
| | Matched | 0.0220 | 9.900 | 1 | 3 | 2.800 |

Table 7: Results

| INNOVATIVE PROCUREMENT | Sample | Treated | Controls | Difference | S.E. | T-stat |
|---|---|---|---|---|---|---|
| R&D_increase | Unmatched | 0.562 | 0.400 | 0.162*** | 0.0230 | 7.040 |
| | ATT | 0.562 | 0.432 | 0.129*** | 0.0247 | 5.25 |
| Innovativeness | UnmCatched | 0.217 | 0.110 | 0.108*** | 0.0159 | 6.760 |
| | ATT | 0.217 | 0.124 | 0.0933*** | 0.0199 | 4.68 |

| R&D SUBSIDIES | Sample | Treated | Controls | Difference | S.E. | T-stat |
|---|---|---|---|---|---|---|
| R&D_increase | Unmatched | 0.493 | 0.404 | 0.0892*** | 0.0175 | 5.110 |
| | ATT | 0.493 | 0.428 | 0.0656*** | 0.0183 | 3.58 |
| Innovativeness | Unmatched | 0.142 | 0. 118 | 0.0238* | 0.0125 | 1.900 |
| | ATT | 0.141 | 0.123 | 0.0182 | 0.0134 | 1.350 |

| R&D SUBSIDIES ONLY | Sample | Treated | Controls | Difference | S.E. | T-stat |
|---|---|---|---|---|---|---|
| R&D_increase | Unmatched | 0.459 | 0.387 | 0.0715*** | 0.0256 | 2.790 |
| | ATT | 0.459 | 0.422 | 0.0374 | 0.0276 | 1.350 |
| Innovativeness | Unmatched | 0.141 | 0.110 | 0.0311* | 0.0179 | 1.740 |
| | ATT | 0.139 | 0.114 | 0.0254 | 0.0199 | 1.280 |

| INNOVATIVE PROCUREMENT ONLY | Sample | Treated | Controls | Difference | S.E. | T-stat |
|---|---|---|---|---|---|---|
| R&D_increase | Unmatched | 0.508 | 0.386 | 0.121*** | 0.0291 | 4.170 |
| | ATT | 0.508 | 0.420 | 0.0877*** | 0.0315 | 2.79 |
| Innovativeness | Unmatched | 0.201 | 0.105 | 0.0966*** | 0.0198 | 4.880 |
| | ATT | 0.201 | 0.116 | 0.0851*** | 0.0249 | 3.42 |

| R&D SUBSIDIES and INNOVATIVE PROCUREMENT | Sample | Treated | Controls | Difference | S.E. | T-stat |
|---|---|---|---|---|---|---|
| R&D_increase | Unmatched | 0.645 | 0.384 | 0.262*** | 0.0386 | 6.780 |
| | ATT | 0.643 | 0.437 | 0.206*** | 0.0405 | 5.09 |
| Innovativeness | Unmatched | 0.207 | 0.106 | 0.102*** | 0.0259 | 3.930 |
| | ATT | 0.204 | 0.115 | 0.0886*** | 0.0336 | 2.64 |

* , ** and *** denote significance at the 10%, 5% and 1% level

Table 8: Robustness Check

INNOVATIVE PROCUREMENT

| Variable | Sample | Treated | Controls | Difference | S.E. | T-stat |
|---|---|---|---|---|---|---|
| Innoprod | Unmatched | 0.718 | 0.545 | 0.174*** | 0.0230 | 7.560 |
| | ATT | 0.718 | 0.577 | 0.142*** | 0.0230 | 6.15 |
| Innoproc | Unmatched | 0.685 | 0.556 | 0.128*** | 0.0226 | 5.690 |
| | ATT | 0.685 | 0.595 | 0.0898*** | 0.0230 | 3.91 |
| Innoserv | Unmatched | 0.693 | 0.492 | 0.201*** | 0.0227 | 8.84 |
| | ATT | 0.693 | 0.549 | 0.144*** | 0.0230 | 6.260 |

R&D SUBSIDIES

| Variable | Sample | Treated | Controls | Difference | S.E. | T-stat |
|---|---|---|---|---|---|---|
| Innoprod | Unmatched | 0.631 | 0.540 | 0.0914*** | 0.0178 | 5.150 |
| | ATT | 0.631 | 0.577 | 0.0538*** | 0.0182 | 2.96 |
| Innoproc | Unmatched | 0.632 | 0.553 | 0.0794*** | 0.0173 | 4.580 |
| | ATT | 0.632 | 0.590 | 0.0423** | 0.0178 | 2.38 |
| Innoserv | Unmatched | 0.609 | 0.488 | 0.121*** | 0.0174 | 6.940 |
| | ATT | 0.609 | 0.520 | 0.0891*** | 0.0179 | 4.97 |

R&D SUBSIDIES ONLY

| Variable | Sample | Treated | Controls | Difference | S.E. | T-stat |
|---|---|---|---|---|---|---|
| Innoprod | Unmatched | 0.670 | 0.540 | 0.129*** | 0.0261 | 4.960 |
| | ATT | 0.671 | 0.595 | 0.0762*** | 0.0272 | 2.80 |
| Innoproc | Unmatched | 0.637 | 0.553 | 0.0846*** | 0.0257 | 3.290 |
| | ATT | 0.639 | 0.611 | 0.0278 | 0.0270 | 1.030 |
| Innoserv | Unmatched | 0.552 | 0.469 | 0.0835*** | 0.0260 | 3.210 |
| | ATT | 0.553 | 0.502 | 0.0511* | 0.0278 | 1.84 |

INNOVATIVE PROCUREMENT ONLY

| Variable | Sample | Treated | Controls | Difference | S.E. | T-stat |
|---|---|---|---|---|---|---|
| Innoprod | Unmatched | 0.717 | 0.518 | 0.200*** | 0.0295 | 6.780 |
| | ATT | 0.717 | 0.544 | 0.173*** | 0.0294 | 5.90 |
| Innoproc | Unmatched | 0.647 | 0.542 | 0.104*** | 0.0290 | 3.600 |
| | ATT | 0.647 | 0.590 | 0.0571* | 0.0300 | 1.90 |
| Innoserv | Unmatched | 0.636 | 0.473 | 0.163*** | 0.0291 | 5.600 |
| | ATT | 0.636 | 0.519 | 0.117 *** | 0.0301 | 3.88 |

R&D SUBSIDIES and INNOVATIVE PROCUREMENT

| Variable | Sample | Treated | Controls | Difference | S.E. | T-stat |
|---|---|---|---|---|---|---|
| Innoprod | Unmatched | 0.696 | 0.506 | 0.190*** | 0.0396 | 4.790 |
| | ATT | 0.694 | 0.539 | 0.156*** | 0.0396 | 3.93 |
| Innoproc | Unmatched | 0.739 | 0.538 | 0.201*** | 0.0388 | 5.170 |
| | ATT | 0.737 | 0.580 | 0.157*** | 0.0376 | 4.17 |
| Innoserv | Unmatched | 0.789 | 0.483 | 0.305*** | 0.0388 | 7.860 |
| | ATT | 0.787 | 0.547 | 0.240*** | 0.0355 | 6.76 |

* , ** and *** denote significance at the 10%, 5% and 1% level

Table 9: Nearest-Neighbor Results

| INNOVATIVE PROCUREMENT | Sample | Treated | Controls | Difference | S.E. | T-stat |
|---|---|---|---|---|---|---|
| R&D_increase | Unmatched | .0561 | 0.399 | 0.161*** | 0.022 | 7.040 |
| | ATT | 0.561 | 0.436 | 0.125*** | 0.033 | 3.71 |
| Innovativeness | Unmatched | 0.217 | 0.109 | 0.107*** | 0.0159 | 6.760 |
| | ATT | 0.217 | 0.135 | 0.081*** | 0.0256 | 3.18 |

| R&D SUBSIDIES | Sample | Treated | Controls | Difference | S.E. | T-stat |
|---|---|---|---|---|---|---|
| R&D_increase | Unmatched | 0.493 | 0.404 | 0.0892*** | 0.0174 | 5.110 |
| | ATT | 0.493 | 0.412 | 0.0813*** | 0.0252 | 3.22 |
| Innovativeness | Unmatched | 0.142 | 0. 118 | 0.0238* | 0.0125 | 1.900 |
| | ATT | 0.142 | 0.114 | 0.0275 | 0.0179 | 1.530 |

| R&D SUBSIDIES ONLY | Sample | Treated | Controls | Difference | S.E. | T-stat |
|---|---|---|---|---|---|---|
| R&D_increase | Unmatched | 0.458 | 0.387 | 0.0714*** | 0.0256 | 2.790 |
| | ATT | 0.459 | 0.4042 | 0.0539 | 0.0372 | 1.450 |
| Innovativeness | Unmatched | 0.141 | 0.110 | 0.0311* | 0.0179 | 1.740 |
| | ATT | 0.141 | 0.104 | 0.0370 | 0.0268 | 1.380 |

| INNOVATIVE PROCUREMENT ONLY | Sample | Treated | Controls | Difference | S.E. | T-stat |
|---|---|---|---|---|---|---|
| R&D_increase | Unmatched | 0.508 | 0.386 | 0.121*** | 0.0291 | 4.170 |
| | ATT | 0.508 | 0.3869 | 0.120*** | 0.0430 | 2.81 |
| Innovativeness | Unmatched | 0.201 | 0.105 | 0.0966*** | 0.0198 | 4.880 |
| | ATT | 0.201 | 0.139 | 0.0616* | 0.0323 | 1.91 |

| R&D SUBSIDIES and INNOVATIVE PROCUREMENT | Sample | Treated | Controls | Difference | S.E. | T-stat |
|---|---|---|---|---|---|---|
| R&D_increase | Unmatched | 0.645 | 0.384 | 0.262*** | 0.0386 | 6.780 |
| | ATT | 0.645 | 0.447 | 0.197*** | 0.0571 | 3.46 |
| Innovativeness | Unmatched | 0.207 | 0.106 | 0.102*** | 0.0259 | 3.930 |
| | ATT | 0.207 | 0.128 | 0.0792* | 0.0431 | 1.84 |

* , ** and *** denote significance at the 10%, 5% and 1% level