Paper to be presented at the DRUID Academy 2013

on

DRUID Academy 2013
at Comwell Rebild Bakker, Rebild/Aalborg

Innovative Procurement and R&D Subsidies: confounding effect and new empirical evidence on technological policies in a quasi-experimental setting

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Abstract

In this paper we analyze the effect of two technology policies, public R&D subsidies and Innovative Public Procurement (IPP), 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 possible interactions among the two policies. Our work tries to fill this gap. We especially hypothesize that evaluating the impact of a policy tool in a quasi-experimental setting without controlling for simultaneous public programs aiming at the same target, 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 IPP as a probable candidate for hidden treatment, could typically have incurred in this problem. In order to corroborate our hypothesis we use 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 procedural confounding's potential significance, we design five quasi-experimental treatment: two that do not consider possible policies simultaneity and three that take into account programs' interactions. Two outcome variables are defined: one input that reports whether a firm increased its investments in R&D and one output that states if most of a company's turnover is coming from innovative products or services. To reduce the selection bias that typically affects quasi-experimental setting, following an growing body of literature, we use the propensity score matching method. We hence estimated the probability for a firm to receive each of the different treatment on the basis of a vector of relevant covariates and we then exploited the recovered index to match treated firms with a suitable control group for each outcome variable. The results of the paper are the following. In the first place, when we are not controlling for possible policies interactions, our findings are coherent with the evidence in previous literature: 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. IPP 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 IPP is more effective than R&D grants both in generating successful innovations and in stimulating private investments in R&D. When we do take into account possible policy interactions we encounter a quite different situation. The reinforcement effect of R&D public funds on R&D private investment loses its significance for firm exclusively participating in R&D subsidy programs, confirming our hypothesis on the relevance of the procedural confounding effect produced by IPP as a hidden treatment and casting some doubts on causal relationships found in earlier works. The same effect does not appear to work in the opposite direction, since IPP considered in isolation is still having positive and significant impact on innovative input and output. However, the impact on R&D expenses for firms that received both R&D grants and won IPP contracts is higher than the sum of each effect alone. These results corroborates the idea that a balance of the two policies should be the best choice and recommend to carefully consider the interaction among different tools in composing technology policy mixes.

Jelcodes:O31,O38

Innovative Procurement and R&D Subsidies: confounding effect and new empirical evidence on technological policies in a quasi-experimental setting.

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11th December 2012

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 former policy tool has been both extensively discussed in the literature and empirically investigated, the latter is a growing trend, 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 this interaction of public procurement, R&D subsidies cease to be as effective as reported in previous studies. 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, propensity score matching

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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 innovative performance of subsidized firm is compared with a control group which has been previously made statistical identical through the implementation of nonparametric matching techniques. Most of these studies points in 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 various papers try 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 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, on the contrary, to the implementation of other technology policies or to the interaction of a policy mix.

More specifically we focus on the innovative public procurement (IPP). 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. While in the case of R&D subsidies scholars have been mostly focusing on the impact of this technology policy on the innovative input, they analyzed the effect of innovative public procurement both upon private R&D and upon 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 confounding factor 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 favor 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 firms' 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 crowing-out effect. Departing from these observations, Almus and Czarnitzki (2003) address the issue of selection bias since most of the literature surveyed by David et al. (2000) do 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. Being not 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, whenever 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 quasi-experiment setting, where, although initially the control group cannot be used a base line because 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 control firms 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) finds 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 Flanders and Germany firms.

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 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 policy which might confound the outcome is the innovative public procurement, that is a government procurement for innovative products with the declared policy goal of stimulate private R&D (Edler and Georghiou, 2007). This possible interaction of the two technology policy has been already suspected, but not investigated though, by David et al. (2000) when they claim that

> 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 to be the suitable suspect to be investigated as a possible confounding factor 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 technological 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 increase in private R&D, while 1$ increase in non governmental sales induces only a 1.7 cents only. 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

[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.

(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). 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. However, 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 (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 lead 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 set 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 empirically out. 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 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 be 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[2]. Gallup interviewed senior company managers responsible for strategic decisions making of 5238 company[3]. The project surveyed companies with more than 20 employees in a large selection of sectors[4].

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[5], 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 in order to avoid the confounding effect we create 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 of this firms group.

[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 a possible

[2] http://cordis.europa.eu/innovation/en/policy/innobarometer.htm

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

[4] 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.

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

crowding out effect of subsidies and procurement and to easily compare our results with those of previous literature about the existence of the crowding-out effect. 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 sale come from innovative product 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 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 only 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.]

We have also information to account for firms' characteristics. We use the number of employees as proxy for the firm size. Since the variable is a categorical one, we do not add the square term. Similarly, we introduce a dummy for age, which group firms in two categories whether they have been set up before or after 2001.We control if firms did invest in R&D 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). We further take into account the location of a firm's market, that is whether it is a local, an European, or an international one. Descriptive statistics of these variables are in table 3. Figure 1 shows interactions between firms characteristics and policy tools. According to the picture it is not clear whether policies have any effect on firms'innovative behavior. At the same time, the picture show that distribution of policies is constantly biased towards Small and Medium firms. 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 avoid the selection bias which emerge already from figure 1.

[Table 3 about here.]

[Figure 1 about here.]

3.2 Method

In order to carry out the above mentioned estimation the paper exploits the fact that only a small portion of the 5238 firms included in our dataset either received R&D subsidies, IPP 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 in the 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 only R&D grants, firms only winning IPP contracts, firms receiving both R&D grants and IPP 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 estimations will be used as terms 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 IPP 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 hidden treatments is indeed playing a role and that the estimation recovered for the first two treatments should not be uncritically trusted.

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 IPP 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 a IPP 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 innovation policies' implementation 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) we are not allowed to use the mean outcome of untreated individuals, E(Y^C|C), as a substitute for the counterfactual mean for treated. To have identification for the treatment effect we have to recover a better alternative for the latter.

While the procedural confounding effect 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 numerous 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).

In order to have identification and consistently estimate average treatment effect (ATT) through matching method we need two condition to be satisfied. The first one is unconfoundedness, or conditional independence assumption(CIA), which formally states:

$$(Y^0; Y^1) \perp W|X \tag{3}$$

This condition implies that assignment to treatment is independent of the outcomes, conditional on a set of observable covariates. For the CIA to be valid all the possible variable that are 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 represents the probability of being treated given the relevant covariates. At a given value of the propensity score exposure to treatment should be random and therefore treated and control units should be on average observationally identical.

The second requirement that must 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.

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

In our case it states that in the dataset there should not be a significant share of firms for which, given the relevant observable characteristics, it is possible to assess with certainty whether they are receiving R&D subsidies or not or if they are winning IPP contracts or not.

If both these conditions hold propensity score matching is able to produce unbiased estimates of the average treatment effect taking 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 in our dataset that could be included in the vector relevant covariates, we then implement propensity score matching to mitigate potential selection biases, assuming CIA to hold. In the next sections we will 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 we pointed out in the previous section, the propensity score represents a measure of the probability for an individual to be treated given a set of relevant characteristics. To recover this measure we therefore need to detect the significant variables in our dataset that are actually affecting the likelihood of the treatment. Caliendo and Kopeinig (2008) provide some practical guidance to tackle this issue and suggest that:

> 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).

Even though we are aiming at estimating the effect of five different treatments, all of them originate from the deployment and interaction of only two technology policy programs: R&D subsidies and IPP. As suggested by Caliendo and Kopeinig (2008), in order to spot the relevant variables in our dataset, we therefore make explicit reference to the piece of literature that used propensity score matching to study the effect of innovation policy tools. Once those variables are identified we regress them onto the probability of receiving each treatment through a probit estimation.

Following Almus and Czarnitzki (2003) we then include in the first regression a vector of 38 dummies related to the industrial sector to which each firm belongs (aggregation based on the NACE 2-digit sectoral level); a vector of 28 country dummies (COUNTRY) that report each firm's country of origin; 1 dummy for the age (AGE) of the firm, that basically states if the firm was born after January 2001; 3 dummies related to the size (SIZE) of the firms (small-medium, medium-large, large enterprises; the small enterprises dummy is omitted due to collinearity); a dummy that report if the core activity of the firm is located in its domestic market or abroad(INTL); 3 dummies that assess whether the firm sells its products or services in its own region, in its own country or internationally; 1 dummy that considers if a firm is doing research and development within the company's walls(R&D_ww).

As mentioned in section 3.1 we are dealing with a cross-sectional dataset that collects data for a 3 year time period (2006-2008) and the information on the firms are gathered through a survey conducted during April 2009. Firms characteristics are hence recorded after the potential treatment had been administered. In order to comply with the recommendations by Caliendo and Kopeinig (2008), we have to assume those firms' features as fixed over time and, hence, unaffected by any of the treatment. While this assumption is not a strong one for variables such as country of origin, industrial sector, age and core activity location, it may require some discussion for the size of the firm and performing of *intra muros* 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 given that, at least for the case of R&D subsidies, there are usually only a few programs implemented in order to raise R&D staff which is quite stable over time. For what concerns IPP contracts the problem could be sharper. Nonetheless, even though it is in fact possible that firms winning IPP 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 IPP is able to increase employments(Slavtchev and Wiederhold, 2011). Moreover labor economics literature suggests that, due to convexities in the adjustment cost function and to indivisibility, employment adapts in a sticky way to shock in demand. In addition to that 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. Hence, even if the IPP was actually able to change the size of a firm we may think that change as negligible since, only in a minor number of cases, it would be pivotal to move one firm from one size class to the other.

To a lesser extent also the variable related to the presence of a R&D department within the firm might present some risk of endogeneity. Almus and Czarnitzki (2003) discard the hypothesis of R&D grants awarded to firms who do not have a R&D department at all grounding this statement on the fact that, in the administrative region they focused on in their work (Eastern Germany), there were no programs explicitly designed to support the founding of a whole R&D department. We were not able to find any evidence of the existence of programs as such for other EU members, Norway and Switzerland and, therefore, we discard as well the endogeneity issue for the R&D subsidies treatment. Also in the case of IPP there is only a minor risk of endogeneity because it is very likely that a company can submit a project to compete for an IPP contract only if it is already implementing some R&D within its walls.

Unfortunately, as mentioned in section 3.1, we are not able to include variables that take into account the economic performance of the firm or that proxy its market share as it is generally done in similar works (Almus and Czarnitzki, 2003; Aerts and Schmidt, 2008; Aschhoff and Sofka, 2009). Although in our dataset we have some information about trends in companies' turnover, we cannot in fact rule out that increasing (decreasing) revenues are affected by the treatments. This is especially true in the case of IPP, because winning a tender has an unambiguous impact on a firm's turnover, but it is far from unreasonable even for R&D subsidies. Given the three years time span in which the data are collected, it is in fact possible that a R&D grant received in 2006 led to an innovation embodied in a product (service) whose sales determined an increase in revenues, for a specific firm, in 2008. However, the size of the firm, the country, and the sector may collect some of the aggregate demand fluctuations that affect firms' economic performance and reduce the portion of variation that remains unexplained due to turnover variables forced omission. The fact that including the 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 that we will present in section 4, is supporting this view.

[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 estimation 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 IPP contract, in contrast with Almus and Czarnitzki's findings (Almus and Czarnitzki, 2003), we recover only a limited effect on the probability of obtaining R&D subsidies, with medium and medium large enterprises having more odds of receiving only R&D grants than very small and very large companies.

After recovering the propensity score for each observation and each treatment as described above, we use these measures to proceed to a non-parametric matching. Since there are two different outcome variables, one related to innovative outputs and one related to innovative inputs, we perform one distinct matching for each of them to maximize the quality of the pairing. In order to capture the average treatment effect both for different treatments and outcomes, we then implement 10 pairing procedures. Among the several matching algorithms available[6] we decided to use kernel matching[7]. 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 it abundantly relies on the nature of the data at hand. The Kernel matching estimator uses weighted averages of observations from all individuals in the control group to construct the counterfactual outcome for each treated individual, assigning higher weight to observations close in terms of propensity score and lower weight on more distant observations. Since it uses more information than other algorithms such as nearest neighbor, the kernel matching estimator provides some advantages in terms of lower variance. However a raise in efficiency may entail a higher bias. To ensure the robustness of our results in section 4 we will briefly present a comparison with estimations retrieved through nearest neighbor matching, which should instead returns results characterized by lower bias and higher variance.

Once the matching procedure has been implemented and before presenting results we evaluate the quality of the propensity score specified, that is we assess whether the matching procedure is able to balance the distribution of the relevant variables in the control and the treatment group. Several methods has been proposed in the literature; Rosenbaum and Rubin (1985) suggests 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 and not treated groups. Generally a reduction of the mean standardized bias under 3 % or 5% threshold after matching is seen as sufficient to tell the success of the procedure. Sianesi (2004) proposes to consider propensity score only on the matched sample and then to compare the pseudo-R^2 for treated and non-treated participants, before and after the matching. Since the pseudo-R^2 is a measure that provide some information about how much of 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 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 is implemented. The three methods described above are applied to all the matching we performed.

[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.

[6] see Caliendo and Kopeinig (2008) for a discussion.

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

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 distribution 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. As it is clearly visible from the graphs the propensity score matching reduces abundantly the dissimilarities in the distributions, which then present a high degree of overlapping, underlining 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 interest in the column "difference", which is the difference in averages between the two groups after the pairing as discussed in 3.2. 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 potential interactions among them. However, we show the result to lay down a comparison with previous literature which never discussed the possible interactions or the possible 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% 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, 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 (12% firms more increased 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* (increase of 9.3 percentage points in *Innovativeness* for firms granted with an innovative public procurement contract) Furthermore, a first comparison among the two policies seems to support the theoretical hypothesis made by Geroski (1990) that, under some circumstances, IPP 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 IPP 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 IPP. 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 IPP and no impact for R&D subsidies on innovative turnover.

The last treatment of our concern is *Policy_Both*, which considers the impact of receiving simultaneously R&D subsidies and winning IPP 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% more firms which are increasing their innovation expenses in the subsample receiving the *Policy_Both* treatment than in the control subsample. 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 IPP hidden treatment, firms receiving only R&D grants and firms both receiving subsidies and winning IPP 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, IPP 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% more firms than in the control group are increasing R&D investments when receiving *Policy_Both* treatment, while only 8,7% more firms 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 the in table to show that they are in line with the other results. To further asses the robustness of our estimations in table **??** 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 is not changed by the algorithm shift, confirming the relevance of IPP as a confounding factor and the strong complementarity between IPP 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 inputs and outputs. 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 IPP 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 treatment: 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 IPP 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 IPP 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 IPP 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 IPP 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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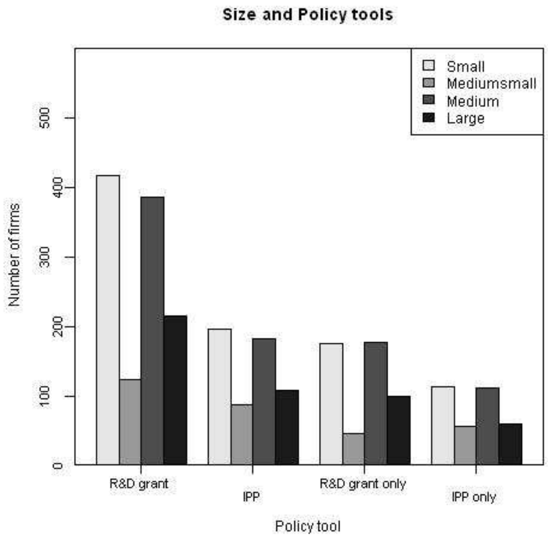
List of Figures

Figure 1: Descriptive



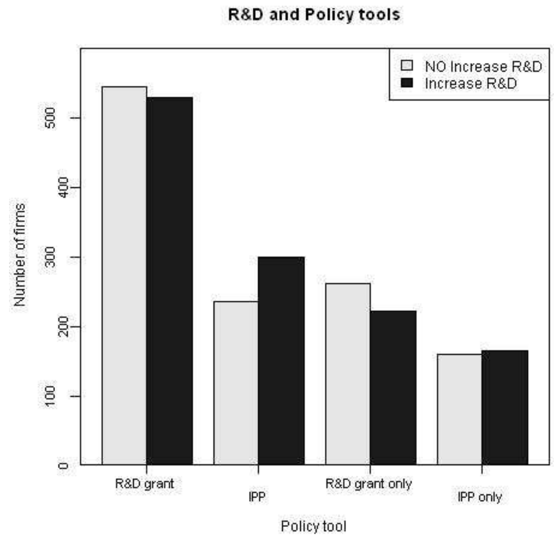


Figure 2: Distributions of the propensity score for treated and not treated before matching

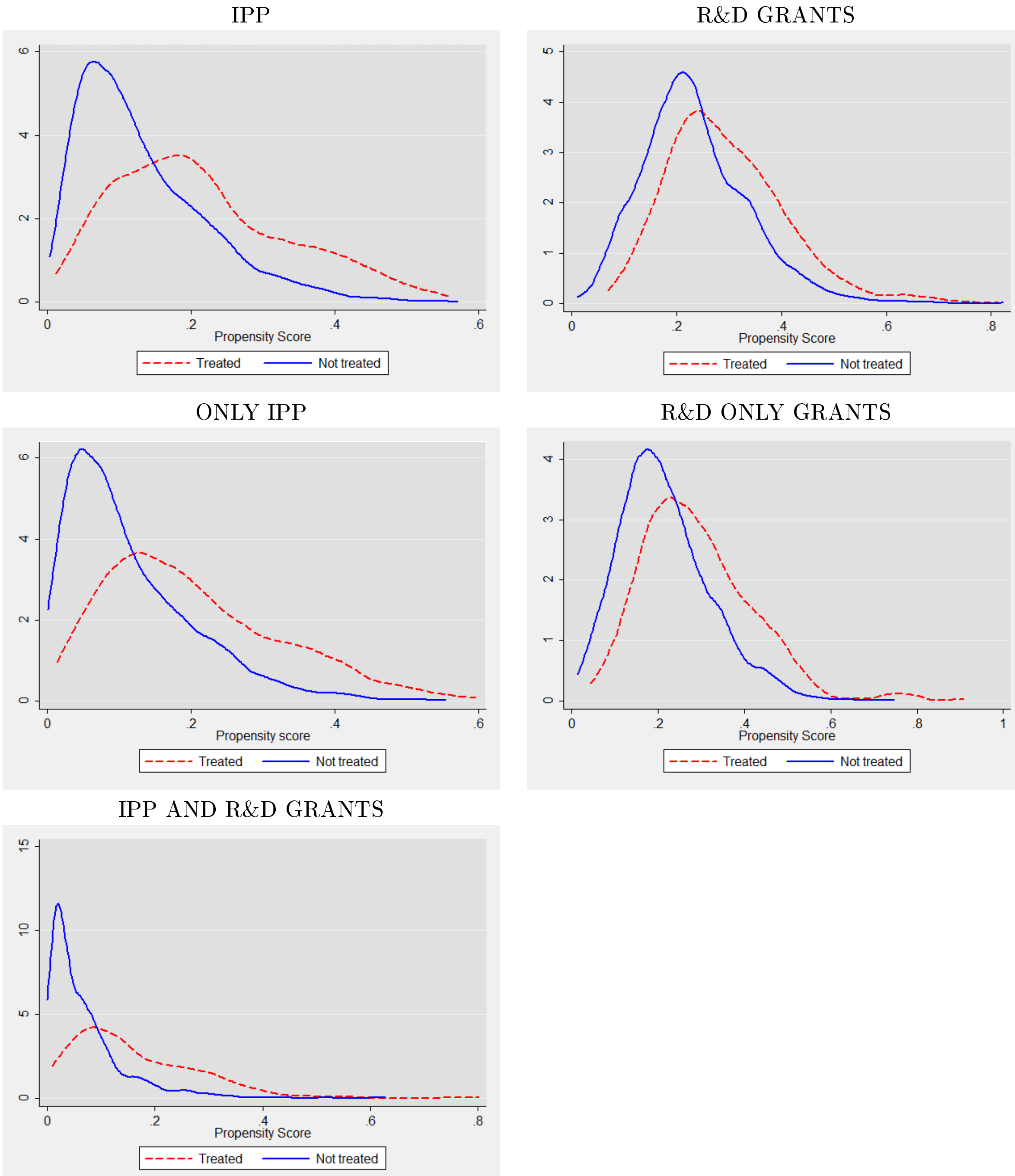
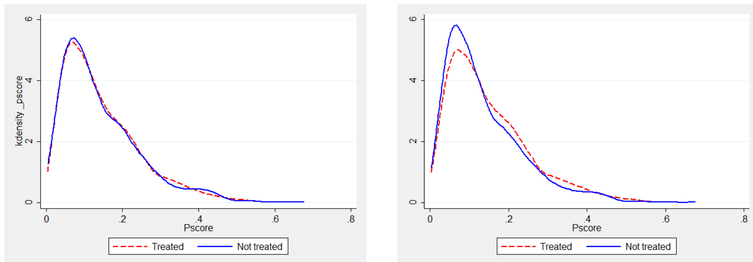


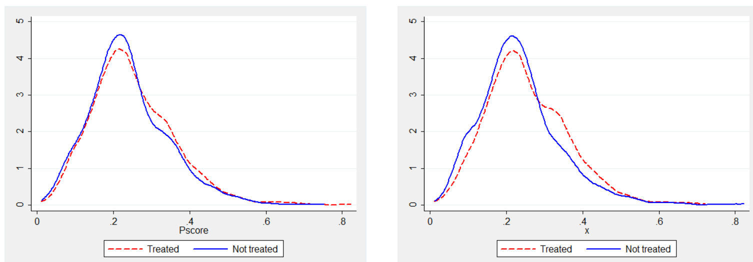
Figure 3: Distributions of the propensity score for treated and not treated after each matching

R&D_increase Innovativeness

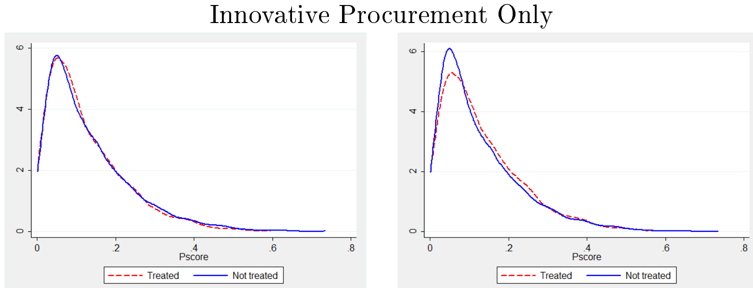
Innovative Procurement



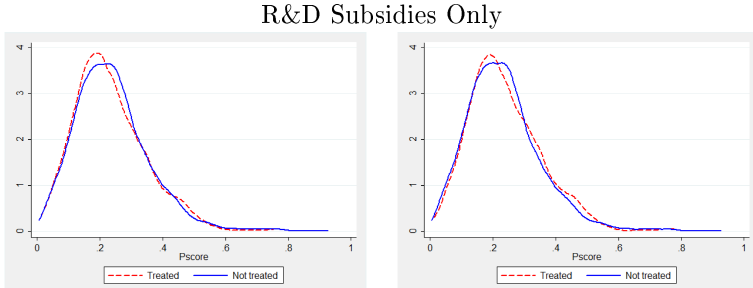
R&D Subsidies



Innovative Procurement Only



R&D Subsidies Only



R&D Subsidies and Innovative Procurement

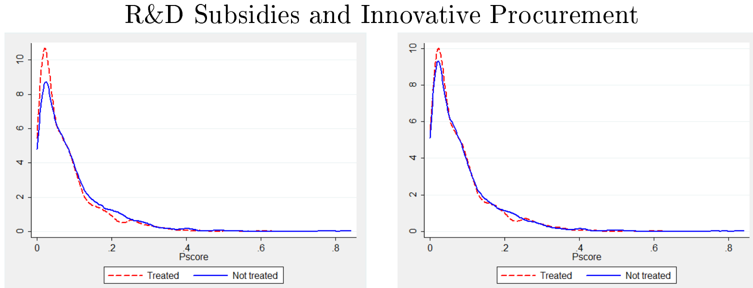
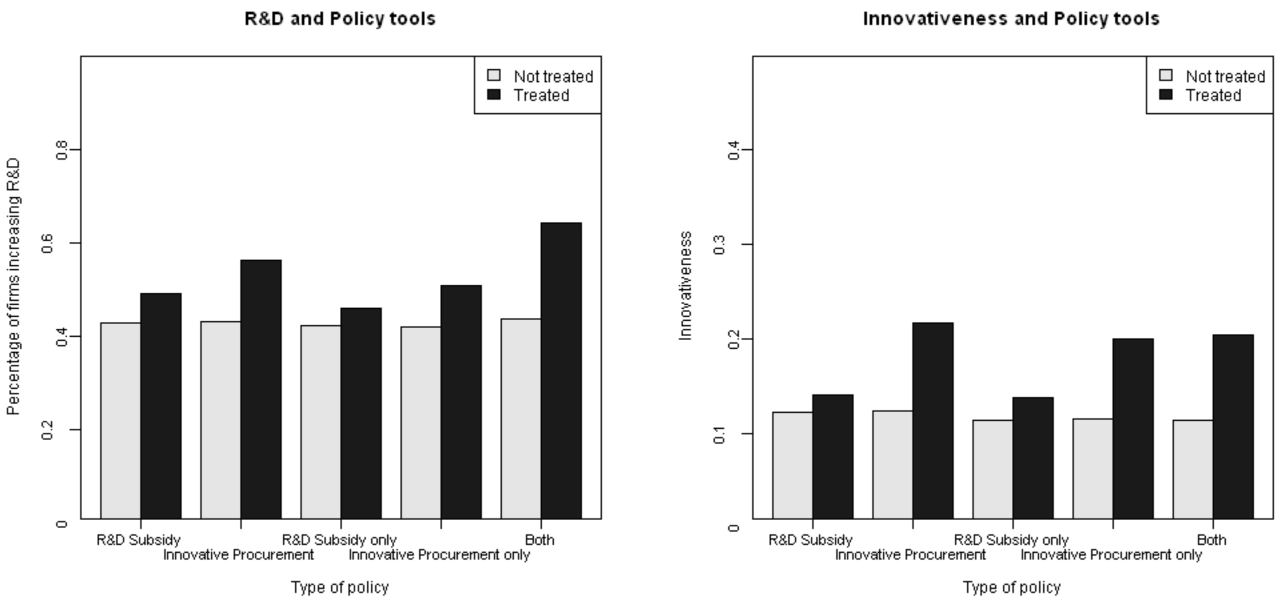


Figure 4: Results



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

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	0.089***1	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

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