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Demand-side vs. supply-side technology policies: Hidden treatment and new empirical evidence on the policy mix

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Abstract

This paper provides new empirical evidence about the impact of various technological policies upon firms' innovative behaviour. We take into consideration the role of policies for innovative activities and we focus on their interaction. While supply-side policies such as R&D subsidies and tax credits 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 the existing results on supply-side policies, surmise fresh empirical evidence on the outcome of innovative public procurement, and address the issue of possible interaction among the various tools. When controlling for the interaction with other policies, supply-side subsidies cease to be as effective as reported in previous studies and innovative public procurement seems to be more effective than other tools. The preliminary evidence suggests that technology policies exert the highest impact when different policies interact.

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 that has been extensively analysed in the literature. One of the most debated issues has been whether R&D subsidies displace private efforts or, on the contrary, favour them due to some form of complementary relationships. The 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 in which the outcome variable is the innovative performance and the treatment is whether firms receive subsidies or not. In order to control for the selection bias, subsidized firms are compared with a control group that has been previously made comparable through the implementation of non-parametric matching techniques. Most of these studies point in the direction of 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 the subsidized firms and the control group, they do not bias the results. However, when the omitted variables change with the level of the subsidies, they can be a possible source of a confounding effect. The literature is very well aware of this problem and in the next section we mention various papers that 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 that 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 the interaction of a policy mix.

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More specifically this paper considers R&D tax credits and innovative public procurement as possible hidden treatments. While R&D tax credits have been extensively analysed (Eisner et al., 1984; Mansfield, 1986; Hall, 1993; Hall and Van Reenen, 2000), 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 supply-side policies (Edler and Georghiou, 2007). In the case of R&D subsidies and tax credits scholars have mostly focused on the impact upon the innovative input; conversely, the literature on innovative public procurement has focused on the effect of innovative public procurement upon both innovative input and innovative output such as innovative turnover. Despite various theoretical accounts, the empirical evidence is still very fragmented. In this paper, we surmise that R&D subsidies, R&D tax credits, and innovative public procurement are tools of the technology policy mix that 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 able to disentangle the various effects.

In this paper we aim to test the contextual impact of R&D subsidies, R&D tax credits, and innovative public procurement upon a firm's private R&D investment. Hence, we make three points. First, by taking into account innovative public procurement and R&D tax credits, we control the past results on R&D subsidies for a possible hidden treatment such as alternative technology policies. Second, we provide empirical evidence on the effectiveness of innovative public procurement. Finally, we discuss the interaction of the various 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 3.2 we present the data and methodology. The empirical results and conclusion follow.

2. Theoretical framework

2.1. Supply-side technology policy: R&D subsidies and tax credits

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 favours their increase. The argument concerning whether substitutability, additionality or complementarity exists between R&D subsidies and private R&D investments has long been 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 others indicate 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 that are idiosyncratic to the specific subsidies programmes such as country and sector of eligibility, to the 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 presenting some evidence of complementarity, and 24 showing a net effect of substitutability, while the remaining 15 do not produce statistically significant results. Moreover, he empirically rejects the hypothesis that the ambiguity in the literature can be due to differences in the methodological tools. Additionally David et al. (2000) discuss the methodological issues and hold the difficulty of dealing with the problem of endogeneity in such a context responsible for this ambiguous empirical support.

> 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 whereby she first estimates the probability of a firm taking part in a public R&D subsidies programme and only thereafter does she estimate the private R&D efforts to test for the presence of the crowding-out effect. Almus and Czarnitzki (2003) address the issue of selection bias as well: the challenge is to make use of a statistical technique that allows for a counter-factual analysis comparing the innovative behaviour of firms that receive R&D subsidies with the hypothetical situation in which the same firms did not receive them. As it is 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 a significant difference in the mean of a particular proxy for innovative behaviour. This procedure requires the two groups to be 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 a 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, in which, although initially the control group cannot be used as a base line because of the lack of randomization, it could be made comparable with the treated group by manipulating it with various techniques. Almus and Czarnitzki (2003) choose to implement propensity score matching to assign each subsidized firm to a control firm exhibiting the greatest similarity in terms of various characteristics. Almus and Czarnitzki (2003) conclude by showing a reinforcing effect between public R&D subsidies and private R&D efforts.

Their result has been corroborated by several empirical studies that 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 the crowding-out effect and, under certain circumstances, the presence of complementarity. Using the same dataset, González et al. (2005) suggest that the lack of R&D subsidies can even restrain firms from investing in R&D at all. Czarnitzki and Licht (2006) show the 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 relating to 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 for 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) reject the hypotheses of the crowding-out effect in a comparisons between firms in Germany and the Flanders.

All in all, although the evidence is not yet conclusive, it seems that when controlling for the selection bias in quasi-experimental settings, the presence of a crowding-out effect has to be rejected and, under certain conditions, there is empirical support for the claim that R&D policies positively impact upon private investments.[1] However, a quasi-experimental framework is not immune to possible flaws. The first shortcoming is the presence of *extraneous variables*, that is, unobserved firm characteristics that 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 in which an extraneous variable varies with the level of the treatment variable in a systematic way induces a serious drawback in the analysis, because it introduces a *confounding factor.* This is precisely the reason why the results vary when additional firm characteristics are introduced, 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 occurs when an extraneous variable varies in a systematic way with the outcome variables. This kind of confounding factor can be seen as a *hidden treatment* (Huston, 1997). 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 that 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 on the eligibility for a subsidies programme depending on specific firm characteristics (Almus and Czarnitzki, 2003), it is reasonable to assume that the same criteria might also be adopted for eligibility for R&D incentives programmes other than R&D subsidies or

[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.

that being selected for a subsidies programme increases a firm's probability of being elected as a recipient of another technology policy. If this is the case, not controlling for the interaction with other technology policies can result in an over-estimation of the impact of R&D subsidies. The candidates for such a confounding policy could be either other supply-side technology policies or demand-side ones. On the supply side, scholars have been analysing the impact of R&D tax credits. Since the literature has extensively discussed the impact of R&D tax credits, in this paper we briefly summarize the results. Scholars have focused on the impact R&D tax credits, considered as a reduction in the price of R&D, upon private R&D investment (Eisner et al., 1984; Mansfield, 1986; Hall, 1993; Hall and Van Reenen, 2000). Since private R&D activity is assumed to be below the optimal level because of appropriability issues, a tax credit should increase the equilibrium private R&D expenses by reducing at the margin the cost of R&D, while the marginal benefit remains unchanged. Hall and Van Reenen (2000) conduct a reviews of various studies and suggest that a unitary elasticity of R&D expenses to R&D tax credits can be a good "ballpark figure" (Hall and Van Reenen, 2000, p.467), although they contextually warn about both the high degree of heterogeneity of these results across sectors and countries and the changing policy conditions over time. Following Hall (1993), most of the literature directly estimates the demand function for R&D. As discussed for R&D subsidies, more recently a quasi-experimental setting has also been employed for assessing the impact of tax credits (Corchuelo Martínez-Azúa and Martínez-Ros, 2009; Czarnitzki et al., 2011). For instance, Czarnitzki et al. (2011) consider the impact of tax credits on firms' innovative performance by looking at a sample of Canadian firms and show that tax credits might lead to additional innovative output. An interesting point made by Corchuelo Martínez-Azúa and Martínez-Ros (2009) is that tax credits are employed by firms that make use of other public R&D support. This evidence corroborates the hypothesis that the effect of a single policy cannot be considered in isolation, but the analysis should take into account possible forms of hidden treatments.

A second source of hidden treatment can arise from demand-side technology policies such as innovative public procurement, that is a government demand for innovative products and services, which might directly or indirectly stimulate private R&D (Edler and Georghiou, 2007). David et al. (2000) have already hypothesized, but not investigated, 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 therefore seems to be a suitable suspect for investigation as a possible demand-side hidden treatment in the test for the presence of complementarity, additionality, and substitutability of R&D subsidies.

2.2. Demand-side technology policy: Innovative public procurement

Innovative public procurement is a growing trend in the debate about technology policy and deserves a detailed discussion. An early work in this area by Lichtenberg (1988) tests the effect of noncompetitive governmental contracts upon company-sponsored R&D expenditures. He estimates that $1 increase in governmental sales induces a 9.3 cent increment in private R&D, while $1 increase in non-governmental sales induces an increment of only 1.7 cent. 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 for other private contracts. Similarly Geroski (1990) points out the role of public procurement in creating demand for new products and process, making an already-existing demand visible, and 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 1960s and in the 1970s, was somehow left aside after the disrupting critique by Mowery and Rosenberg (1979) and Dosi (1982), which point 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; Rogers, 1995; Malerba et al., 2007; Fontana and Guerzoni, 2008) gave a new twist to this literature stream. The central idea of the new wave of demand studies is that the influence of demand upon innovation should be considered as a mixture of two elements (Guerzoni, 2010). On the one hand, the size of a market can be used as a proxy for the demand. In this case, a larger demand will create more incentives for R&D investment, since it increases the expected profits from the innovation. On the other hand, the demand can be considered as a source of information from users, which, by providing producers with knowledge about the market needs, reduce the uncertainty linked with the development of new products. Focusing on a sample of small- and medium-sized enterprises in several industries and European countries (Fontana and Guerzoni, 2008) show that the former effect is especially true for process innovation, while the latter applies for product innovation.

Contextually, the resurrection of the demand-side also took 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 (Aho et al., 2006; Georghiou, 2006; EU, 2010). Edler and Georghiou (2007) set up a very general framework of discussion, which grounds the need for demand-oriented innovation policy in market failures as it is done for supply-oriented policy.

The growing interest in the topic raises the issue of the theoretical definition of innovative public procurement. The literature proposes several taxonomies and different labels defining this concept[2]. The most widespread definition, as introduced by Edquist and Hommen (2000b) and recently further developed by 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 pens and paper, requiring no R&D (Edquist and Hommen, 2000b). Although this definition has the major advantage of neatly distinguishing these two categories of procurement, recent works (Uyarra and Flanagan, 2010; Rolfstam, 2012) both highlight its potential limitations and stress the fact that it constrains the innovative procurement scope to the activities that follow a formal tender process. The reason for considering innovative behaviour after the procurement order lies in the possibility of observing the direct effect of public procurement on firms' innovative behaviour; 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. Innovative public procurement can be seen as a way to enhance both the size and the sophistication of the demand in a given context. Indeed, a procurement agency can create or enlarge a market and provide firms willing to invest in a specific sector with a sufficient level of expected profits. Secondly, in the procurement context, a public agency may be considered as a prospective customer with a general preference for more or less innovative products, services or systems. In many cases, the government has been an early and crucial purchaser in technological intensive industries (Dalpé et al., 1992; Slavtchev and Wiederhold, 2011; Mazzucato, 2011). 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 players in a specific sector such as in the military sector. Along this line of reasoning, Mazzucato (2011) argues that public spending might have a beneficial influence on entrepreneurs' "animal spirits", by raising their expectations about the growth prospect in an economy or specific sector. Considering this variegated set of studies, in this work we refer to a broader definition of innovative public procurement and we consider it as the purchasing activities carried out by public agencies that may lead to innovation, even if indirectly or as a by-product.

Despite the theoretical and policy attention paid to the issue, the empirical evidence about the effect of innovative public procurement on innovation outcome is rather fragmented and mostly limited to case studies (Edquist and Hommen, 2000a; Rolfstam, 2009; Flanagan et al., 2011; Brammer and Walker, 2011; Uyarra and Flanagan, 2010). Notable exceptions are Aschhoff and Sofka (2009) and Slavtchev and Wiederhold (2011). Slavtchev and Wiederhold (2011)'s work is a very sophisticated paper that 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 about the role of the sectoral composition and intensity of public procurement in 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 industries with higher technology opportunities. Aschhoff and Sofka (2009)'s paper is an exemplary work in the tradition of evaluating technological policy at the firm level with survey data in the same spirit as 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 that responded to the survey "Mannheim Innovation Panel" in 2003. Based on self-reported data, they are thus able to compare the impact on the 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

[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).

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. Second, to our knowledge, it is the only analysis that links firms' innovative behavior with different technology policy mixes and not with a single policy only. Indeed, as already pointed out 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 for subsidies and participate in tenders for public procurement.

Summing up, the work on R&D subsidies and tax credits and the latter by Aschhoff and Sofka (2009) each tackle one side of the problem only. The former manages to develop a robust technique to isolate a causal effect of a policy tool on firms' innovative behaviour, it signals the potential risk of a crowding-out effect of public subsidies on private investment, but it succeeds in ruling it out empirically. However, works on the evaluation of R&D subsidies have omitted to consider other policies, which can potentially interact with R&D. Given the quasi-experimental setting of these pieces of research, this omission might lead to an overestimation of the impact of R&D grants on innovation. The positive impact of R&D subsidies on private investment might be partially or even totally due to the contextual influence of other policies such as R&D tax credits and innovative public procurement and, thus, not to the R&D grants only. Aschhoff and Sofka (2009) following the new trend of demand-oriented technology policies have the merit of including both policies in their 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 insights into the impact of innovative public procurement on private investment in R&D.

On this basis, in this paper we try to gain the best of two worlds. We aim to test the impact of technological policies on a firm's innovative behaviour when both supply-side policies, namely R&D subsidies and tax credits, 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) to test the robustness of results on supply-side policies when innovative public procurement are also taken into account, (2) to provide new empirical evidence on the evaluation of innovative public procurement, and finally (3) to pay a special attention to the interaction of those policies. In the next section, we describe the data available to accomplish this task and the methods we apply. The results and conclusions follow.

3. Data and method

3.1. Method

In order to analyse empirically the impact of the different technology policies, the paper exploits the fact that only a small portion of the 5238 firms included in our dataset received subsidies for innovative activities, innovative public procurement, or tax credits(table A.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 different public programmes. However, since we are analysing non-experimental data in an "experimental spirit"(Angrist and Pischke, 2008), two main problems may arise.

In the first place we are aim to evaluate the effect of three different treatments (technology policy tools) that are not assigned to specific subgroups of different individuals (firms), nor are perfect substitutes (Aschhoff and Sofka, 2009) from the individuals' perspective. Hence, in the dataset, we may find firms in distinct conditions: firms receiving subsidies only, firms winning innovative public procurement contracts only, firms benefiting from tax credits only, firms receiving two or more treatments simultaneously and firms that are not involved at all in any of these programmes. Trying to estimate the impact of each of the policies without taking into account the possible interactions with the others, may clearly lead to procedural confounding effects as we discussed above.

In order to tackle this issue, we exploit the information at our disposal in the dataset that will be presented in the next section, and design ten different treatments out of the three innovation policy tools possibly adopted:

1. *Policy_Subsidies*
2. *Policy_Procurement*
3. *Policy_Tax Credits*

4. *Policy_Subsidies_only*
5. *Policy_Procurement_only*
6. *Policy_Taxcredit_only*
7. *Policy_Sub_Tax*
8. *Policy_Sub_IPP*
9. *Policy_IPP_Tax*
10. *Policy_All*

The first three treatments do not take into consideration the potential simultaneity of the programmes and are hence exposed to the procedural confounding problem defined above. Though they might be biased, the reason for recovering estimates of their effect on firms' innovative behavior is dual. On the one hand, the retrieved estimates will be used as terms of comparison to check effectively 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), we will compare the significance, direction and magnitude of the results with the evidence provided so far of the role of different technology policies upon firms' innovative activity.

Treatments 4, 5 and 6 are explicitly designed to eliminate the potential hidden treatment problem, since they consider each policy in isolation but control for potential simultaneous treatments. The existence of a significant difference between the estimates recovered for the latter treatments with respect to treatments 1-3 would imply that the procedural confounding produced by hidden treatments does indeed play a role and that the estimation recovered for the first three treatments, as those obtained in similar settings in the literature, should not be uncritically trusted. Finally treatments 7 to 10 take into account any possible interaction between the three policies. Thus, they will allow us to evaluate the impact of different policy mixes on firms' innovative behaviour and also to gain a better understanding of the sources of the potential difference between estimates achieved for treatments vulnerable to confounding and those addressing the hidden treatment problem.

Secondly, since the treatments are not randomly assigned, we may clearly incur 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 subsidies 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 to apply to public programmes and they will self-select them selves into the application process. 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. For this reason we cannot interpret a between-groups prospective mean difference in innovative behaviour as the causal effect of the technology policies, since the two groups would behave differently even in the absence of the treatment. Formally:

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

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

where ATT is the average treatment effect we are interested in, Y^T is the 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, an alternative solution is required.

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), Aerts and Schmidt (2008) and Czarnitzki and Lopes Bento (2011), who introduce non-parametric matching methods into innovation policy studies. The basic idea of matching is to find a wide group of non-treated individuals that are similar to the treated ones in all the 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 the matching method, two conditions need to be satisfied. The first one is unconfoundedness, or the conditional independence assumption(CIA), which formally states:

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

This condition implies that the 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) show that it is possible to condense the vector of relevant covariates into a single scalar index, called the 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 predict perfectly 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 that, given the relevant observable characteristics, are assigned with certainty to the group receiving subsidies, winning innovative public procurement contracts or using tax credits.

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:

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

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.

However in our framework the common approach to propensity score matching, in which the treatment is binary, can be adopted only for the three treatments vulnerable to procedural confounding. In these cases we purposely do not account for policy interactions to obtain benchmarks estimates for evaluating the relevance of the hidden treatment problem, hence each treatment can be considered separately. When we instead deal with treatments explicitly designed to eliminate the confounding, since we account for policies simultaneity, we have to consider different treatments as mutually exclusive. We therefore have to deal with M+1 treatments, denoted 0,1,.., M, with the 0 representing the absence of treatment. In the multiple treatment context the average treatment effect should be recovered by:

$$ATT = E(Y^m|T = m) - (Y^l|T = m) \tag{6}$$

where m is the treatment that we would like to evaluate and l is the treatment against which we are comparing m. Since in this study we are mainly interested in assessing the importance of the hidden treatment problem, we will always confront different treatments with the absence of treatment and therefore l will always be $T{=}0$.

As in the binary case, the outcome $Y^l|T{=}m$ is not observed and we therefore have to rely on firms receiving treatment l to estimate the counterfactual outcome. To solve the identification problem Imbens (2000) and Lechner (2001) generalized the model developed by Rosenbaum and Rubin (1983), showing that it is possible to extend the use of propensity score matching to the multiple treatment case. The "generalized propensity score"(Imbens, 2000) is hence defined as the conditional probability of receiving a particular level of treatment given the pre-treatment characteristics. As in the binary treatment case, matching on the generalized propensity score allows us to estimate consistently the average treatment effect if the conditional independence assumption (CIA) and the common support condition hold. The CIA in the multi-treatment case can be formalized as follows:

$$(Y^0; Y^1; Y^{..}; Y^M) \perp W|X = x, \forall x \in \chi \tag{7}$$

where $(Y^0, Y^1, Y^{..}, Y^M)$ is the entire set of outcomes and χ is a set of covariates for which the average treatment effect is defined. This condition implies that the researcher should be able to observe all the characteristics that jointly affect the participation in the treatments and the outcomes. The common support condition for the multiple-treatment case is instead:

$$0 < P^m(X) < 1, \forall m = 0, 1, .., M \tag{8}$$

$$P^m(X) = P(T = m|X) \tag{9}$$

requiring, as in the binary case, the probability of participating in one specific treatment to belong strictly to the interval (0,1).

If these two conditions are satisfied the average treatment effect in the multiple treatment context can be consistently estimated by the following generalized propensity score matching estimator:

$$Psm_{ATT} = E(Y^m|T = m) - E_{P^m(X),P^l(X)}[E(Y^l|P^m(X), P^l(X), T = l)|T = m] \tag{10}$$

In the next sections we briefly describe the data and the variables employed to perform the matching procedure. In section 3.6 we illustrate the propensity score specification, discuss matching quality in terms of balancing and the common support assumption and eventually present the results.

3.2. Data

In the analysis, we use 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 in 5238 companies [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), who investigate the role of users in the innovative process, and by Filippetti and Archibugi (2009), Borowiecki and Dziura (2010) and Filippetti and Archibugi (2011) , who focus on the impact of the crisis upon innovation, and cited in various reports (among others in Kaiser and Kripp (2010)).

3.3. Treatment indicators

As described in the previous section in order to take care of the hidden treatment issue we design different treatments out of the technology policy tools possibly adopted by surveyed firms. By exploiting the information at our disposal in the dataset, we construct treatment indicators for the main policies, innovation subsidies, tax credits and innovative public procurement, and then consider their possible interactions. One of the most interesting features of the Innobarometer survey is that the surveyed firms were expressly asked about any public procurement contracts they have been awarded and whether this procurement contract provided them with the opportunity of selling an innovation. We are hence able to create a treatment indicator, defined as *Policy_Procurement*, which takes the value 1 for firms answering "yes" to the question "Did at least one of the public procurement contracts that you have won since 2006

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

[4] A detailed 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.

include the possibility to sell an innovation (i.e. new or significantly improved products or services)?" and 0 for firms that won only general procurement contracts (i.e. no innovation involved) or firms that did not win any procurement contracts.

This indicator represents a major improvement with respect to other quantitative studies dedicated to public procurement. Most innovation surveys such as the Community Innovation Survey or the KNOW survey[6] have not so far included specific questions about procurement and therefore previous works based on CIS-like survey could only construct noisy proxies for innovative public procurement. That was also the case of the above-mentioned work by Aschhoff and Sofka (2009) which considered a firm as receiving the public procurement treatment if the respondent acknowledged customers as an important source of innovation for the firm and if those customers belonged to the NACE sectors 75.1, 75.2 and 75.3 for public administration, defense or compulsory social security. Even if it might be thought of as a second-best proxy, it still does not ensure the engagement of the surveyed firm in any procurement contract with governments or other public agencies. The NACE sector 75 also, in fact, includes several public and private companies[7]. Having those companies as customers inducing innovation in the surveyed firms does not imply a direct or indirect purchasing channel between a public agency and the surveyed firm nor a direct link between public procurement and innovation. This problem could have clearly led to overstatement of the relevance of public procurement in spurring innovation.

Unfortunately, while the Innobarometer dataset allows us to gain some accuracy on the treatment indicator for public procurement with respect to previous studies, this improvement comes at a cost. Concerning supply-side policies the Innobarometer survey did not directly ask firms whether they received R&D grants or benefited from tax credits for R&D but only whether changes in those policies have contributed to innovation[8]. We are therefore forced to construct our treatment indicators as proxies for the receipt of supply-side innovation policies. In particular we build the variable *Policy_Subsidies*, which takes the value 1 if a firm reported that "changes in public financial support" had a positive effect on innovation and 0 (otherwise) if the firm did not report a positive effect of the public financial support or if it stated that this particular question did not apply to its specific situation. In the same way we define a treatment indicator *Policy_Tax Credits* with a value 1 for firms reporting that "changes in tax environment (e.g. R&D or innovation tax credits)" had a positive effect on innovation and 0 otherwise.

We are aware that such variables may generate some concerns. In particular a potential problem may arise from the fact that among the firms that did not report a positive effect of the two supply-side policies, hence replying "no" to the specific question in the survey, there might also be firms receiving public financial support for innovation or using tax credits but experiencing either no effect or negative effects from participating in these policy programs. Nevertheless we believe that our indicators are good proxies for participation in programmes. In order to be more confident about the fact that the questions in the Innobarometer survey could be used to construct good proxies for the receipt of subsidies, we also confronted the data at our disposal with those that can be retrieved from Community Innovation Surveys implemented in different European countries for 2006-2008. Since the CIS survey explicitly asks firms whether they received subsidies or benefited from tax credits[9] it allows us to construct a dummy variable that accurately reports whether a firm received innovation subsidies or not but without disentangling direct and indirect financial support. We therefore build a similar variable exploiting the information from the Innobarometer survey (i.e. a binary that takes the value 1 if a firm answered "yes" to at least one among *Policy_Subsidies Policy_TaxCredits* and *Policy_TaxCredits* variable takes the value 1 and 0 otherwise). We then confront the average participation rates in subsidies programmes for firms in the Innobarometer and the CIS survey accounting for firms' size, activity sector and state and we find no statistically significant difference. To assess further the goodness of our indicator, we also estimate the probability of receiving subsidies both for the Innobarometer and for the CIS data taking into account the

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

[7]Nace sector 84 (sector 75 in Nace Rev. 1.1) includes very large and technological intensive European firms, such as BAE Systems, European Aeronautic Defence and Space Company (EADS), Finmeccanica, Thales, QinetiQ, Babcock International Group, Rheinmetall AG and Patria.

[8]The exact question states: "Have significant changes in the following policy-related areas introduced since 2006 had a positive effect on innovation in your company? a) Changes in tax environment (e.g. R&D or innovation tax credits) b) Changes in public financial support (grants, loans, support for recruiting new staff)". The potential responses for each option were:" Yes, No, Not Applicable".

[9]The CIS question states: "During the 3 years 2006-2008 did your enterprise receive any financial support from the following levels of government? Include financial support via tax credits or deductions, grants, subsided loans and loans guaranteed. Exclude research and other innovation activities conducted entirely for the public sector under contract". The possible answers are "yes" and "no" for three different government levels: regional government, central government and European Union.

size of the firm, its sector and country of origin and the probability of being surveyed in one survey rather than in the other. A detailed description of this comparison can be found in the Appendix. In figure A.1 it is possible to see how the probability of receiving subsidies is distributed in a similar way when we consider the CIS data with the "accurate indicator" and the Innobarometer data together with our proxy variable.

[Figure 1 about here.]

Moreover, as we noticed above, the main issue that may affect our treatment indicators relates to firms that are potential participants in innovation support programmes that did not report a positive effect on innovation. Since our proxy variable would consider them as not receiving any subsidy this could bias the results to some extent. However, we might be able to say something about the direction of the bias. Inasmuch as we would consider as treated only firms that report positive effects on innovation, if any, we may expect the overestimation of the impact of subsidies on R&D expenditures. In the presence of this kind of bias due to the potential inaccuracy of our proxies, we should therefore be less likely to find evidence of severe hidden treatment problems for supply-side policy tools.

Once we have designed the treatment indicators for the three major policies and briefly discussed their effectiveness, we now take into account the hidden treatment problem they could be vulnerable to. In order to do that, we have to create the variables *Policy_Subsidies_only*, *Policy_Procurement_only*, and *Policy_Tax credit_only*, which identify firms that respectively received only innovation subsidies, used only tax credits or won only innovative public procurement. Since, as it emerges in sections 3.1 and 3.2, it is also worth analysing the case when different policy treatments are simultaneously administered to the same firm, we create four different variables taking into account potential synchronous treatments. We hence build the following dummy variables: *Policy_Sub_Tax*, which pinpoints firms receiving innovation subsidies and using tax credits, *Policy_Sub_IPP*, identifying firms receiving subsidies and winning innovative public procurement contracts, *Policy_IPP_Tax*, for firms that obtained procurement contracts and benefited from tax credits and finally *Policy_All*, which identifies firms receiving all three treatments simultaneously. The control group is instead always composed only of firms that are not receiving any of the treatment. Table A.1 recaps the 10 treatment indicators along with the number of firms they are administered to. As stressed in the table the first 3 treatments are the one exposed to the hidden treatment problem while the second and the third group of indicators take into account all the possible interactions between different treatments, therefore controlling for potential confounding effects[10]. As table A.1 reports the size of the control group for the first three treatments changes. This is due to the fact that for each treatment vulnerable to confounding the control group includes firms that are receiving other treatments. When we control for interactions in order to eliminate the hidden treatment problem the control group is instead composed only by firms which do not receive any of the potentially simultaneous treatments.

[Table 1 about here.]

3.4. Outcome indicator

To construct the input indicator, we exploit the following question from the Innobarometer survey: "Compared to 2006, has the amount spent by your firm on all innovation activities in 2008 increased, decreased, or stayed approximately the same (adjust for inflation)?" [11]. As described by Filippetti and Archibugi (2011), this question allows us to grasp trend in firms' innovation spending, hence we use it to create the binary variable *INNO_increase*, which takes the value 1 for firms that increased their total innovation expenditure in 2008 with respect to 2006 and 0 if no increase was reported. A caveat should be made; this outcome variable is dichotomous, that is respondents declared whether

[10] It should be also noted from table A.1 that if we add up the number of firms for the second and the third group of treatments by different policy instrument the total does not reach the number of firms that are receiving the treatment in the first group. This is due to the fact that among the firms reporting to have received a given treatment (for instance innovative public procurement contracts) there are some of them that did not answer to the question about one of the other (or both) potential treatments (tax credits or subsidies). In that case we cannot argue whether those firms benefited from only one policy ore more than one and they are hence not considered as treated for the treatments of the second and the third group.

[11] This is question Q3 in the Innobarometer Survey. All innovation activities are listed in Q1 and include: a) Research and development within your company, b) Research and development performed for your company by other enterprises or by research organisations, c) Acquisition of new or significantly improved machinery, equipment and software, d) Purchase or licensing of patents, inventions, know-how, and other types of knowledge, Training to support innovative activities, f) Design ,g) Application for a patent or registration of a design. Question Q3 of the Innobarometer survey mimics question 5.2 of the CIS survey and follows the definition of 'total innovation expenditure' stated in the Oslo Manual.

they had increased their innovative expenses or not. Therefore, this variable might suffer from an overestimation bias due to its self-reported nature and, thus, the results might be distorted in the direction of favouring additionality or complementarity of technology policy with private innovation efforts. However, this is a common issue in innovation surveys, which can be dealt with only by careful interpretation of the results. Moreover due to the binary nature of our outcome variable, the average outcomes for treated and control firms should be considered as participation rates and therefore the average treatment effect represents the difference in the proportion of firms that increased their innovative spending between the treated and the control group.

Due to its dichotomous nature and to the fact that it collects the increase in all innovation expenditures and not only investment in R&D, this variable may appear less fit to evaluate the effect of the given policies upon innovative input with respect to variables, such as R&D intensity, used in other studies (Almus and Czarnitzki, 2003; Aerts and Schmidt, 2008), nevertheless we believe that it is suitable to analyze the additionality issue for several reasons. First of all, in most cross-sectional studies dealing with the crowding-out in expenditure on innovation inputs the effect of the policy measure is recovered by comparing the R&D expenses (or intensity) for treated firms with those of firms in the control group. The positive (negative) change in the expenditure for innovation input is therefore inferred by comparing investment levels for different firms but no actual change in R&D or innovation expenditure at the firm level is observed. The variable *INNO_increase* reports instead whether a firm actually declared a rise in its innovation effort or not over the surveyed time span[12]. As in Duguet (2004), knowing if a firm that receives a subsidy is (not) incrementing (at all) its innovation investment is then sufficient to test the full crowding-out hypothesis, i.e. if the subsidy fully replaces private money. Our variable allows us to examine whether firms receiving different technology policies are more likely to raise their innovation effort with respect to control firms. No differences in the share of firms rising their innovation effort between the treated and the control group would therefore imply, everything else equal, the presence of full crowding-out among treated firms.

Secondly, it is true that by referring to the total innovation expenditure we are not looking at the sole investment in formal R&D as in Almus and Czarnitzki (2003) and Aerts and Schmidt (2008), nevertheless several studies in this stream of literature (Czarnitzki and Fier, 2002; Czarnitzki and Licht, 2006; Czarnitzki and Bento, 2012) used variables based on total innovation expenditure to assess the relevance of the additionality vs. crowding-out issue. Moreover, as pointed out by Kleinknecht et al. (2002), R&D measurement tends to be 'manufacturing biased' and to severely underestimate small scale and informal R&D activities, mainly conducted in services and in smaller firms. Using total innovation expenditure hence allows to better evaluate the real size of the innovation effort, independently from the sector and the size of the surveyed firms.

The last row of tables A.2, A.4, and A.3 shows some descriptive statistics. Table A.2 reports the descriptive statistics over all the samples, while the remaining two tables show the stratification over various types of policy.

3.5. Control variables

The data at hand provide us with abundant information to account for firms' characteristics[13]. We hence use this information to build several control variables that we will use in the propensity score specification described in section 3.6. As a proxy for the size of the firm, we use 4 categories (SIZE_1-4) for small (20-49, employees), medium (50-249), medium-large (249-500) and large (500+) enterprises. Similarly, we introduce a dummy for young firms (YOUNG_FIRM), which takes the value of 1 if a firm was set up after 2001 and 0 otherwise. We also create binary variables to control for the industrial sector[14] (SECTOR, 37 variables) and for the country of origin of the firm (COUNTRY, 29 dummies). We create 4 dummies that assess whether the firm sells its products or services in its own region(MKT_region), in its own country (MKT_national), in the European Union market (MKT_eu) or in the global market (MKT_global) [15]. Since, as suggested by Almus and Czarnitzki (2003), the engagement in R&D activity can proxy for a firm's absorptive capacity and its endogenous ability to write proposal for R&D subsidies and public procurement we also build the dummy R&D_ACT, which reports whether a firm engaged in internal R&D activities.

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[12] Duguet (2004) used an outcome variable built in a similar way, i.e. increment or not in R&D expenditure, to evaluate the complementarity/substitutability between R&D public subsidies and private investment in France.

[13] The Innobarometer survey begins with a series of basic questions regarding the surveyed company (question D1 to D5).

[14] Aggregation based on the NACE 2-digit sectoral level, revision 1.1.

[15] Notice that these variables are not mutually exclusive since the question in the survey does not ask where the firm's core activity is located but only if it sells products on the different markets.

Table A.2 reports descriptive statistics and figure A.2 tabulates interactions between firm characteristics and policy tools. According to this picture it is not straightforward to determine whether policies have any effect on firms' innovative behavior. At the same time, the picture shows that the distribution of policies is constantly biased towards large firms suggesting a possible source of selection bias. On this ground, the next section discusses both how to use these data to spot statistically a causal effect of different and potentially coexistent policy tools, on firms' innovative activity.

[Table 2 about here.]

[Table 3 about here.]

[Table 4 about here.]

[Figure 2 about here.]

3.6. Propensity score specification

As pointed out in the previous section, the propensity score consists of a measure of the probability of an individual being treated conditional on 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 on tackling the issue of variable 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. Following Almus and Czarnitzki (2003), we include variables collecting firms' characteristics discussed in section 3.5 as covariates: specifically dummies for the sector, country of origin and size of the firm, a binary that reports whether is a young firm or not, four dummies for different markets in which a company may operate and a dummy for the performance of in-house R&D activities.

As mentioned in section 3.2 the dataset exhibits a cross-sectional structure with data for a three 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 that firms' features are 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 activity location, this is not necessarily the case for the size of the firm and the in-house performing of R&D. Nevertheless, the endogeneity issue for the size variable should not represent a severe problem in our context. Even though it is in fact possible that firms receiving specific technology policies might be more prone to increase the number of employees, 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 very limited number of cases would it switch one firm from one class to another.

To a lesser extent, the variable R&D_ACT, revealing whether a firm performed R&D activities within the company, might also present some risk of endogeneity, if a firm performed some internal R&D only as a consequence of the technology policies. While this is clearly not the case for a policy such R&D tax credits, it cannot be entirely ruled out for subsidies and innovative public procurement. However, since it is more likely for a company to compete for subsidies if some in-house R&D has already been implemented, the endogeneity problem should not be a severe one.

Unfortunately, as mentioned in section 3.2, the dataset lacks variables taking into account the economic performance of the firm or proxies for its market share measured before participation in treatments. The dataset contains some information about trends in companies' turnover, but we do not include this information in the analysis as is generally performed 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 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), the sales of which 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 [16].

Once the relevant variables have been identified, we estimate different propensity scores, one for each treatment. As discussed in section 3.2, we have to deal with two kinds of treatments. The first kind includes the *Policy_Subsidies*, *Policy_Tax Credits* and *Policy_Procurement* treatments which are vulnerable to confounding since they do not take into account the potential presence of a simultaneous treatment. The second group consists of those treatments that consider every possible interactions among treatments and are purposely designed to eliminate the hidden treatment problems (*Policy_Subsidies_only*, *Policy_Procurement_only*, *Policy_Taxcredit_only*, *Policy_Sub_Tax*, *Policy_Sub_IPP*, *Policy_IPP_Tax*, *Policy_All*). Since the treatments of the first kind are not mutually exclusive while those of the second are, we need different discrete choice models to estimate the propensity scores. For what concerns the first three cases we therefore run three probit regressions, regarding each treatment as independent from the others in the common binary treatment framework (Treatment=1, no Treatment=0). The results of the probit regressions are presented in table A.5. For the second group we instead have to consider the different treatments as reciprocally exclusive and to frame the treatment model in the multivariate context illustrated by Lechner (2001) and Gerfin and Lechner (2002). To compute the propensity scores for this case we hence implement a multinomial logit model (Larsson, 2003; Brodaty et al., 2001) with eight alternatives. The baseline of the model is always the no-treatment condition (T=0, firms not receiving any of the policy tools) while alternatives 1 to 7 account for the seven treatment conditions. The outcomes of the logistic regression are reported in table A.6.

[Table 5 about here.]

[Table 6 about here.]

The outcomes confirm that several sectoral and country dummies significantly affect the probability of receiving treatments in both the bivariate and the multivariate case. As expected, the variable with the clearest influence on the likelihood of being treated is the one reporting whether a firm performs in-house R&D activities. Table A.6 shows how size appears to have a major impact on the probability of winning an innovative public procurement contract and, as in Almus and Czarnitzki (2003), on the probability of obtaining subsidies; specifically, medium and large enterprises have higher odds of receiving R&D grants than very small companies.

We then use the propensity scores recovered for each treatment to perform a non-parametric matching. As a matching algorithm[17], we implement the nearest-neighbor procedure and a "caliper" threshold which imposes a tolerance level on the maximum propensity score distance to avoid bad matches[18].

As pointed out by Caliendo and Kopeinig (2008), the choice of the algorithm to apply is a matter of a 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 nearest-neighbor matching, using only the closest observation in terms of propensity score as a comparison for a treated individual, allows for smaller biases at the price of higher variance. Since, as table A.7 shows, the control group is much larger than the treated one for every treatment, in order to raise the efficiency of our estimates, we use up

[16] In line with this reasoning, including turnover variables in the probit and multilogit regressions is only slightly modifying the propensity scores estimation and is not changing significantly any of the results in terms of ATT presented in section 4.

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

[18] To implement the matching we used the stata module psmatch2, developed by Leuven and Sianesi (2003). As suggested by the rule of thumb first introduced by Rosenbaum and Rubin (1985) we set the caliper option to .015, a value that corresponds approximately to .25 times the standard deviation of the propensity scores recovered with the probit/multilogit regressions.

to three neighbors to build the counterfactual outcomes. While this kind of oversampling allows us to gain efficiency, the caliper threshold ensures that using more information does not lead to bad matches.

However, to be sure that our results are not sensitive to the algorithm specification, we also perform the matching using the kernel algorithm[19] and a single nearest-neighbor procedure as robustness checks. In section 4 we present a comparison between the different results.

The inclusion of this threshold together with the common support restriction described in section 3.2 leads, as in Gerfin and Lechner (2002), Larsson (2003), Czarnitzki and Lopes Bento (2011) to the loss of a few treated observations for which is not possible to find a close comparison in terms of propensity scores. As table A.7 reports the total loss of observations by treatment is always less than 10 % of the size of the treated group.

[Table 7 about here.]

A key step in the matching 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 the treated and not-treated groups. Generally, a reduction of the mean standardized bias under the 3 % or 5% threshold after matching is usually considered sufficient to support the success of the procedure. Sianesi (2004) proposes to consider the 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 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 8 about here.]

The results reported in table A.8 show how, for all the estimations, the mean standardized bias (Meanbias) falls below the 5% threshold (mostly below 3%) after the matching, the pseudo-R^2 considerably decreases passing from the raw to the matched sample and the likelihood ratio test (LR chi^2) leads us always to reject the hypothesis of joint insignificance before the matching and never to reject it for the matched sample. The overall matching performance hence appears to be good.

Finally, as pointed out by Caliendo and Kopeinig (2008), there is a need to assess the overlapping between sub-samples through a graphic analysis of the propensity score density distribution, in both treated and the control 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 section 3.1, would be violated because the relevant covariates would be able to predict perfectly if a firm is receiving a treatment or not. Intuitively the matching procedure is implemented to “correct” for the difference in the distribution, which can be thought of 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 figures A.3, A.4 and A.5 we report the graphs of the density distribution of the estimated propensity scores for the treated and the control group before and after the matching.

[Figure 3 about here.]

[Figure 4 about here.]

[Figure 5 about here.]

[19] 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.

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

After the pairing procedure is implemented 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 ten different treatments on firms' innovative input. The results of the estimations are reported in table A.9 for each treatment. Figure A.6 graphically depicts some of the results.

[Table 9 about here.]

[Figure 6 about here.]

The table includes the average outcomes for the 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 3.1[20]. Since the outcome variables are dichotomous, the average outcomes in the table represent a participation rate and therefore the average outcomes display the share of the firms that increase their spending in innovation activities both for the treated and for 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 that increase their spending in innovative input after participating in a given technology policy. For instance, in the case of *Policy_Procurement*, the number of firms that increase their spending is 11.2 percentage points higher among firms that won an innovative public procurement contract.

As pointed out in section 3.1 the first three treatments that we take into account, *Policy_Subsidies*, *Policy_Procurement*, and *Policy_Tax Credits* 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 the previous literature which has never discussed the possible interactions or the existence of a hidden treatment. Regarding *Policy_Subsidies* treatment, our results seem to be coherent with those provided by the large body of literature (Almus and Czarnitzki, 2003; Aerts and Schmidt, 2008; Czarnitzki and Bento, 2012; González et al., 2005) that reports no evidence of crowding-out effect (or substitutability) on private investments in innovation inputs due to R&D subsidies. Moreover, our results appear to confirm the reinforcing effect between public subsidies and private efforts found by Almus and Czarnitzki (2003). Receiving subsidies indeed seems to have a positive and significant impact in terms of innovative input since there are 7.8 percentage points more firms that are increasing their total innovation expenditure in the treated group than in the control group. Similarly, firms receiving R&D tax credits are 10 percentage points more likely to declare an increase in innovation spending than those in the control group. A comparison with the previous literature, which finds at the margin a dollar-for-dollar increase in R&D private expenses (Hall and Van Reenen, 2000), is difficult due to the dichotomous nature of our treatment variable. However, the result points in the direction suggested by the literature, that is an overall positive impact of tax credits. Also for the treatment *Policy_Procurement* the results are rather consistent with the evidence delivered by the 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 a positive and significant effect of innovative public procurement on private expenses in innovation activities: there are 11.2 percentage points more firms increasing their private spending in innovative inputs in the treated than in the control group. A first comparison among the two policies seems to support the theoretical hypothesis made by Geroski (1990) that, under some circumstances, innovative public procurement may be more effective than subsidies in stimulating private investments in innovation activities.

[20] The line "unmachted" is reported to appreciate the difference between the unbiased estimations (ATT) and the biased ones where both the magnitude of the effects and their significance levels are inflated.

We now turn to the analysis of the treatments (*Policy_Subsidies_only*, *Policy_Procurement_only*, *Policy_Tax credit_only*) which consider instead each policy in isolation from the others in order to eliminate the potential procedural confounding, as explained in section 3.1. The most interesting result comes from comparing the impact for *Policy_Subsidies_only* and *Policy_Subsidies* treatment: in this case there is no evidence supporting the existence of any additionality and the positive impact of R&D grants on R&D investment ceases to be significant. Our hypothesis that other policies, such as R&D tax credits and innovative public procurement, might represent a crucial confounding factor in evaluating the impact of innovation policies seems therefore to be corroborated. We may hence speculate that at least a portion of the reinforcing effect between public subsidies and private innovation input 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. The previous results on the additionality of subsidies might therefore be carefully reconsidered. A similar conclusion applies to R&D tax credits, which, when considered in isolation without the confounding effect of possible hidden treatments, remain significant at the 10% level but almost halve their effect. While the procedural confounding seems to play a key role in the evaluation of the impact of subsidies and R&D tax credits on firms' innovative behavior, this does not appear to happen for innovative public procurement. Table A.9 reveals that the differences between the treated and the control group are positive and strongly significant also in the case of *Policy_Procurement_only* treatment and not dissimilar from those recovered for *Policy_Procurement*. This result is to some extent coherent with the findings of Aschhoff and Sofka (2009), who implicitly take into consideration the potential complementarity of different policy programmes and report a robust positive effect of procurement; thus, the second indisputable result is that innovative public procurement has a positive impact on the probability of a firm self-declaring an increase in total innovation expenditure.

The last group of treatment concerns the possible interaction among different policies (*Policy_Sub_Tax*, *Policy_Sub_IPP*, *Policy_IPP_Tax*). The effect of the treatment interaction is always significant and remarkably high. It seems that the best-performing policy mix is the interaction of innovative public procurement with both R&D tax credits and subsidies. Once again we should warn readers that a comparison of magnitudes should be a careful one since the treatments are binary variables.

This result offers us a double check as well as an explanation for the relevance of the procedural confounding issue in evaluating supply side programmes for supporting firms' innovative activity. It clearly shows that the effect of financial support retrieved for the treatment vulnerable to confounding (*Policy_Subsidies*) was driven by the policy mix and not by the effect of the subsidies programme alone. If we in fact considered grants and subsidies without taking into account the potential hidden treatments, firms receiving subsidies only and firms both receiving subsidies and winning innovative public procurement contracts would have been thought as qualitatively identical. The comparison between the *Policy_Sub_IPP* and the *Policy_Subsidies_only* programs indicates instead that the two treatments together determine very different innovative behaviours and suggest that, for firms receiving both treatments, the policy mix is pivotal in having a robust effect on private investments in innovation activities, once again in line with Geroski's hypothesis (Geroski, 1990). However, the fact that the *Policy_Sub_IPP* effect on investment in innovation inputs outperforms the impact of the *Policy_Procurement_only* is also noteworthy (22 percentage points more firms than in the control group increase their innovative effort when receiving both policies, while only 8,4 percentage points more firms in the treated than in the non-treated subsample are increasing their innovation expenditures with *Policy_Procurement_only*), therefore confirming a strong complementarity between the two policy tools. We can also reject the hypothesis that innovative public procurement is solely responsible for the results since the policy mix involving both subsidies and tax credit performs rather well, whereas the two policies in isolation do not show a robust contribution to the increase in private expenses in innovation inputs.

However, it is not possible to compare statistically the effect of the interactions of two policies with the effect of the interaction of a different combination of them since we use different matched firms of the control group. Similarly, it would be hard to compare it with the effect on firms receiving one policy only or three of them. Indeed, there might be a problem of selection bias in the number of policies that a firm received overall, as discussed in Czarnitzki and Lopes Bento (2011). Nevertheless, with this caveat in mind, when considering the non-significance of subsidies alone and the small impact of tax credits alone, we cannot reject the hypothesis that the combined effect of any two policies is higher than the sum of those two policies in isolation.

The last treatment of our concern is *Policy_All*, which considers the impact of receiving simultaneously subsidies, and tax credits and winning innovative public procurement contracts. Table A.9 shows how the concurrence of the three policy tools has a positive and robust effect on innovation input. We in fact have 30 percentage points more firms in the subsample receiving the *Policy_All* treatment than in the control subsample, which increase their private expenses in total innovation expenditure. Once again, we should be aware of the possible endogeneity due to the fact that firms able to be recipients of three types of policies might simply be better than the others.

As a robustness check, we report the results obtained by using different matching methods.Table A.10 presents the results using the nearest-neighbor procedure with only one neighbor and table A.11 reports the outcome of a kernel estimation. As we mentioned in section 3.1, the decision about which algorithm to use in implementing the matching procedure is a matter of a trade-off between efficiency and bias. The approach we used so far (nearest neighbor with up to three neighbors) has the advantage of increasing the efficiency of the estimators without increasing the potential bias. The nearest-neighbor with only one neighbor guarantees even lower biases but at the price of lower efficiency as well, while the kernel estimator raises the efficiency but also the potential bias. As the tables show, the sign and the significance of the effect of different treatments on innovative input are not changed by the algorithm shift, confirming the relevance of innovative procurement as a confounding factor and the complementarity between innovative public procurement and supply-side technology policies.

[Table 10 about here.]

[Table 11 about here.]

5. Conclusion

In this paper we analysed the effect of three technology policies on firms' innovative behaviour. While the role of R&D public subsidies and R&D tax credits has been extensively investigated in the literature, innovative public procurement has only recently regained attention and the empirical evidence is still fragmented. Moreover, very limited work has been conducted taking into consideration the relevance of the possible interactions among policies. Our work tries to fill this gap. We especially hypothesized that evaluating the impact of a policy tool in a quasi-experimental setting without controlling for simultaneous public programmes aiming to achieve 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 and total innovation expenditure, not considering R&D tax credits and innovative public procurement as probable candidates for hidden treatment, could typically have incurred in this problem. We therefore suggested that the previous results might have been overestimated.

In order to corroborate our hypothesis, we used data from the Innobarometer on "Strategic Trends in Innovation 2006-2008", a survey conducted with 5238 firms in the 27 member states of the EU, Norway, and Switzerland. To evaluate the impact of each policy tool on firms' innovative behaviour and to assess the existence of a confounding effect, we designed ten quasi-experimental treatments: three of them do not consider possible policy simultaneity, other three of them consider the policy in isolation taking into account the issue of simultaneity, and the latter four cases measure the effect of the policy mix. To reduce the selection bias that typically affects a quasi-experimental setting, we used the 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 the previous literature only when we do not control for policies' interactions; in this latter case, public subsidies positively and significantly affect private effort on innovation activities, ruling out the crowding-out effect hypothesis and confirming the complementarity between public and private investment in innovation inputs. Moreover, as for subsidies, tax credits have a positive and significant effect on the increase in total innovation expenditure. Innovative public procurement has a robust impact on private expenses in innovation activities. In terms of magnitude our results seem to confirm the theoretical hypothesis that innovative public procurement is more effective than R&D grants in stimulating private expenditure in innovation input (Geroski, 1990). Nevertheless, we should be aware that the dichotomous nature of our treatment variable renders a comparison very difficult, but it could be considered as preliminary evidence.

When we consider the possible interactions of other policies, the results show a different picture. The reinforcement effect of public support on private innovation efforts ceases to be significant for firms exclusively participating in subsidy programmes. 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 serious doubts on the causal relationships found in earlier works, which should be reconsidered. We found a similar outcome for the case of R&D tax credits which when controlling for other policies have a much weaker impact. On the contrary, the same effect does not appear to work for innovative public procurement which, even when considered in isolation, still has a positive and significant impact on innovative input, again coherently with Geroski (1990).

We then analysed policy-mix possibilities. The most interesting case is the contextual effect of both supply-side policies and innovative public procurement: the effect upon the increase in total innovation expenditure is significant and higher than the sum of the effects of the three policies considered in isolation. This further corroborates the idea that a balance of the policies should be the best choice.

From a policy point of view, this work contributes to the debate of supply vs. demand technology policies. Supply-side policies reduce the cost of inventive activities, while demand-side ones increase the incentives and reduce the uncertainty of the process of innovation. Whether one kind of policy outperforms the others is not an output of this paper. The evidence undoubtedly shows that supply-side policies have been overestimated and that the role of innovative public procurement is not a mere theoretical hypothesis. Moreover, the preliminary evidence seems to point at a reinforcing effect of the interaction among different tools in the technology policy mix. We carefully suggest that innovative public procurement is not only able by itself to have a positive impact on firms' innovative behaviour, but that it could also represents an effective way to reinforce potential positive effects of supply-side technology policies, stimulating additional private investments in R&D. However, due to data constraints, the latter conclusion is based only on necessary conditions and calls for more empirical evidence in the area.

AppendixA.

As we discussed in section 3.2, the question in the Innobarometer survey that we use to build the treatment indicators *Policy_Subsidies* and *Policy_Tax Credits* does not directly ask whether a firm received subsidies or tax credits but only whether changes in these policies had a positive effect on innovation. These treatment indicators should therefore be considered only as proxies for the receipt of subsidies and tax credits. In order to check whether they are good proxies we confronted our dataset with data retrieved from the Community Innovation Survey 2008 (CIS 8). The CIS 8 survey presents two very helpful features to test the goodness of our indicators. In the first place it covers the very same time span of the Innobarometer survey we use in our study, which is the period 2006-2008. Secondly it includes a question that specifically ask whether a firm received R&D tax credits or subsidies. The latter feature enables us to build a variable that accurately reports the receipt of supply-side innovation policies at the firm level and to compare it with our proxy indicators. In particular we are interested in two elements. The first element of interest is whether there is a significant difference in the share of firms receiving subsidies in the Innobarometer and CIS surveys. If we do not find such a difference it could mean that the accurate and the noisy indicators are actually grasping the same information. To assess further the goodness of our proxy we then look at the distribution of the predicted probability of receiving subsidies given firm characteristics for the firms surveyed in the Innobarometer and the CIS survey. A high degree of overlapping and similarity between the two distributions would once again confirm the close relatedness between the accurate and the "noisy" measure of the treatment.

However, to make a meaningful comparison several elements should be taken into account. Firstly, the CIS 8 question does not discriminate between tax credits and subsidies. Secondly, CIS data are available only for 14 countries [21]. Third, the sampling performed for the two surveys did not necessarily target the very same groups of firms. It may be the case that one survey oversampled (undersampled) a particular sector or firm size category with respect to the other one. To solve the first problem, we create a variable for the CIS data that takes the value 1 if a firm received any of the two supply-side policies and 0 otherwise. We then build a variable that mimics the CIS one for the Innobarometer and takes the value 1 if either *Policy_Subsidies* or *Policy_Tax Credits* (as defined in section 3.2) is 1 and 0 otherwise and we call it the same name, *policy_support*. Concerning the difference in the countries in the sample, we removed

[21] We have data for Bulgaria, Cyprus, Czech Republic, Germany, Estonia, Spain, Hungary, Italy, Lithuania, Latvia, Portugal, Romania, Slovenia, Slovakia. Though available, we also excluded data for Norway and Ireland because the survey for those countries did not include the question about R&D subsidies.

from the Innobarometer data the observations for the 15 countries for which we do not have a CIS counterpart and therefore the scope of this evaluation is limited to 14 out of the 29 countries included in the Innobarometer. The third problem, that is, the different sampling in terms of firms' characteristics, requires a more in-depth discussion. For a proper comparison of firms from the Innobarometer dataset with firms from the CIS dataset, we should only look at companies that have the same probability of being surveyed by one survey or the other. To accomplish this task, we exploit the fact that both surveys followed similar criteria in the sampling procedure since they both randomly selected firms within given stratifications built on the basis of three main characteristics of the firm: country of origin, activity sector, and size. To avoid potential selection problems we therefore perform an exact matching on firms' size, activity sector, and state and compare whether the share of firms receiving subsidies is different for firms surveyed by the Innobarometer Survey with respect to the CIS. Therefore, we vertically join together the two datasets and create a new variable, *Innobar*, taking the value 1 for firms surveyed in the Innobarometer survey and 0 for firms surveyed in the CIS survey, which we consider as a treatment variable. We then implement exact matching on dummy variables for the country of origin of the firm, for the sector to which the firm belongs (NACE 2-digit aggregation level) and for its size[22]. Table A.12 reports the average difference in participation rates in subsidies programmes for firms in the Innobarometer and the CIS survey. As the table shows, after the matching implementation there is only a small and not statistically significant difference between the firms surveyed by the two datasets. This result reinforces the hypothesis that the "noisy" treatment indicators recovered from the Innobarometer survey are indeed grasping information closely related to that collected by the more accurate indicator made available by the CIS.

[Table 12 about here.]

In order to explore the nature of this relationship further, we also look at the distribution of the probability of receiving subsidies conditional on firm characteristics for the matched firms. We run a probit regression of *policy_support* on size, country, and sectoral dummies for matched firms. We then compare the density distribution of the predicted probability of receiving subsidies for firms in the Innobarometer survey and in the CIS survey. As figure A.7 shows there is a high-degree of overlapping between the two distributions. This similarity again appears to confirm that both the accurate indicator, used to build the variable *policy_support* for the CIS firms, and the noisy proxy, used to build *policy_support* for the Innobaromenter firms, collect the same information.

[Figure 7 about here.]

Since the country for which the CIS data are available are mainly(10 out 14) Eastern European countries that have recently joined the European Union, a possible concern could be that those states have specific characteristics driving the previous result. This would be a major problem considering that the countries for which we are not able to make the comparison with the CIS data are mostly Western European states, long involved in European integration processes. In order to rule out this possibility, we replicated the procedure described above on a subsample composed only of Germany, Spain, Portugal and Italy. Table A.13 reports the result. As in the previous case there is no significant difference in participation rates between firms surveyed in the Innobarometer survey and those surveyed in the CIS 8 survey. In A.8, we also reproduce the density distributions of the predicted probability of receiving subsidies and again the two distributions present a high degree of overlapping.

[Table 13 about here.]

[Figure 8 about here.]

[22] In particular we create 3 dummies for small, medium and large enterprises. Small firms: 10 to 49 employees; Medium firms: 50-249; Large firms:250+. It should be noted that there is a difference in the definition of small firm in the Innobarometer and CIS survey. The former questionaire only targets firms with more than 20 employees, while the latter includes firm with as few as 10 employees or more.

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List of Figures

Figure A.l: Density distribution of the Predicted probability of receiving innovation subsidies for firrns surveyed in the Innobarometer 2009 and CIS 8 survey

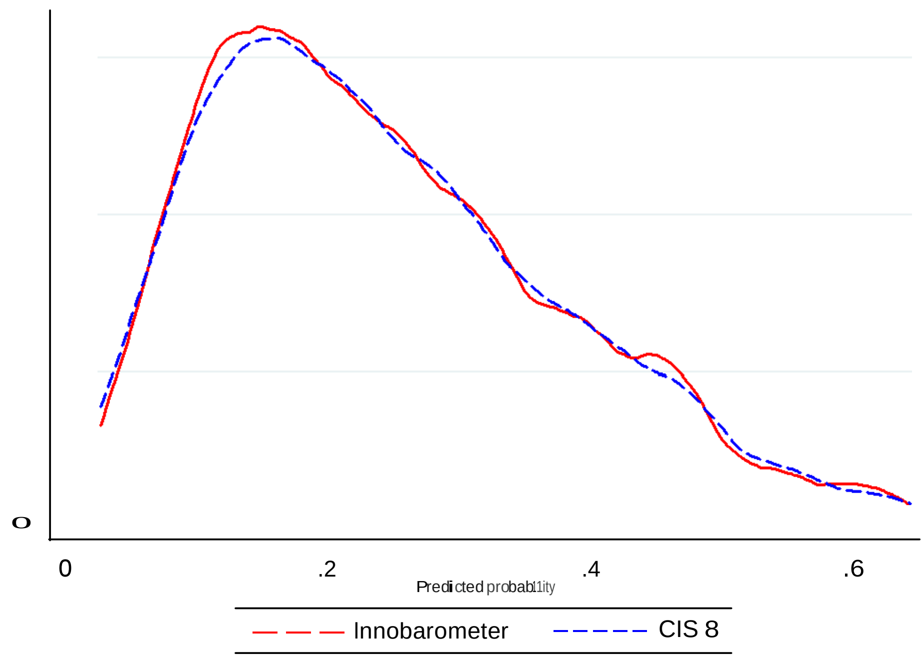


Figure A.2: Descriptive

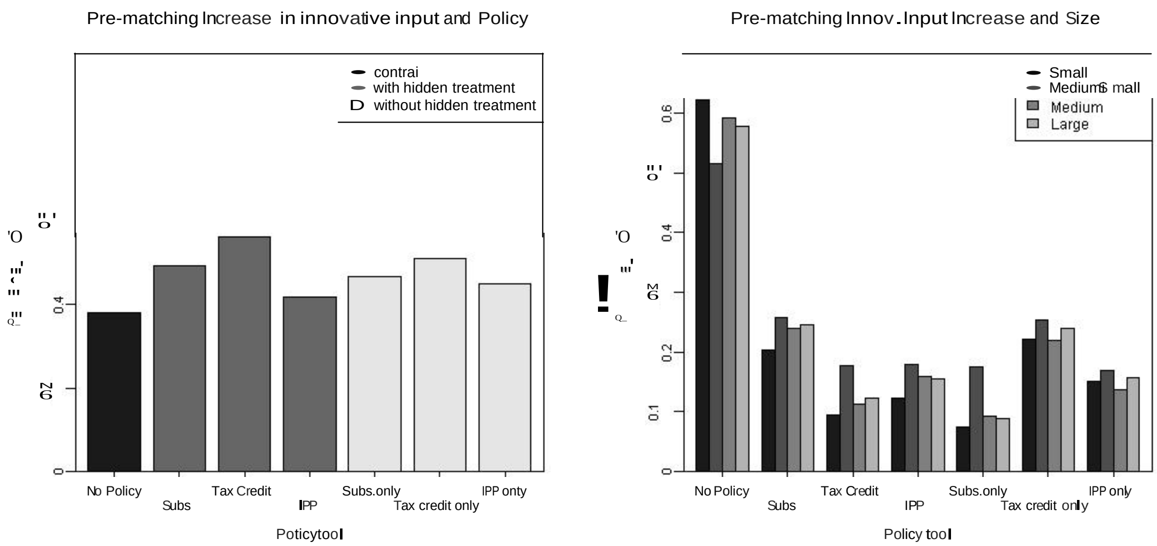


Figure A.3: Distributions of the propensity score for the treated and the not-treated group before and after the matching

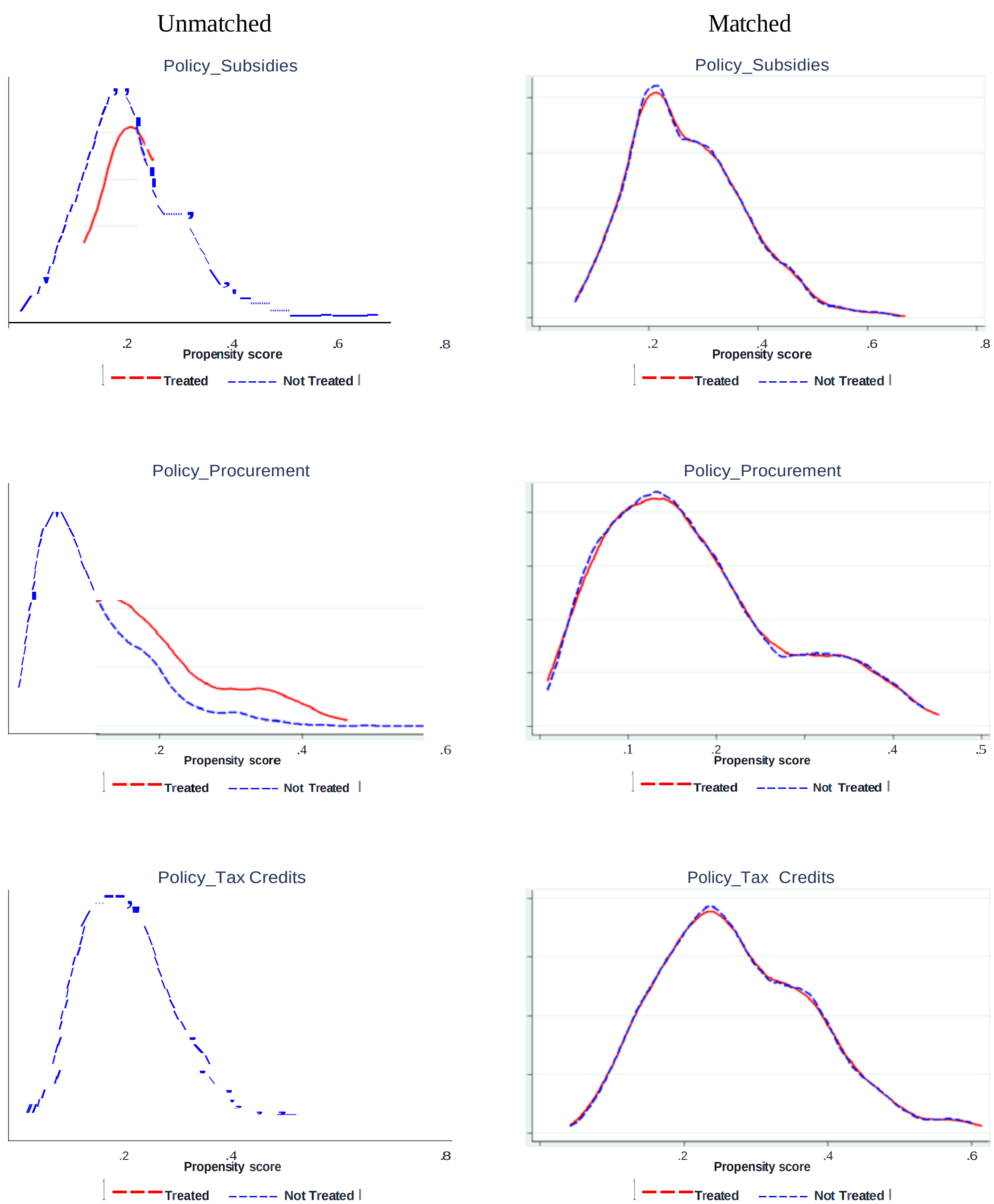


Figure A.4: Distributions of the propensity score for the treated and the not-treated group before and after the matching (cont.)

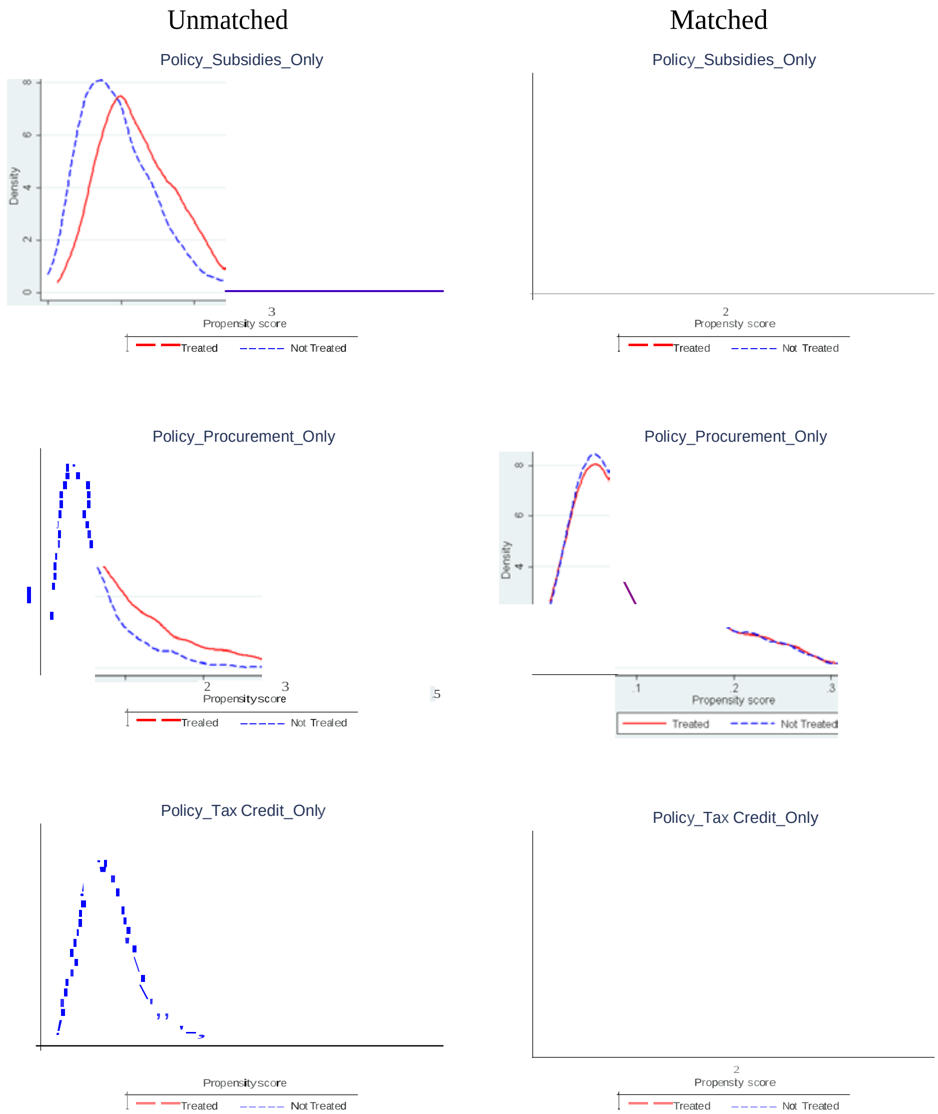


Figure A.5: Distributions of the propensity score for the treated and the not-treated group before and after the matching (cont.)

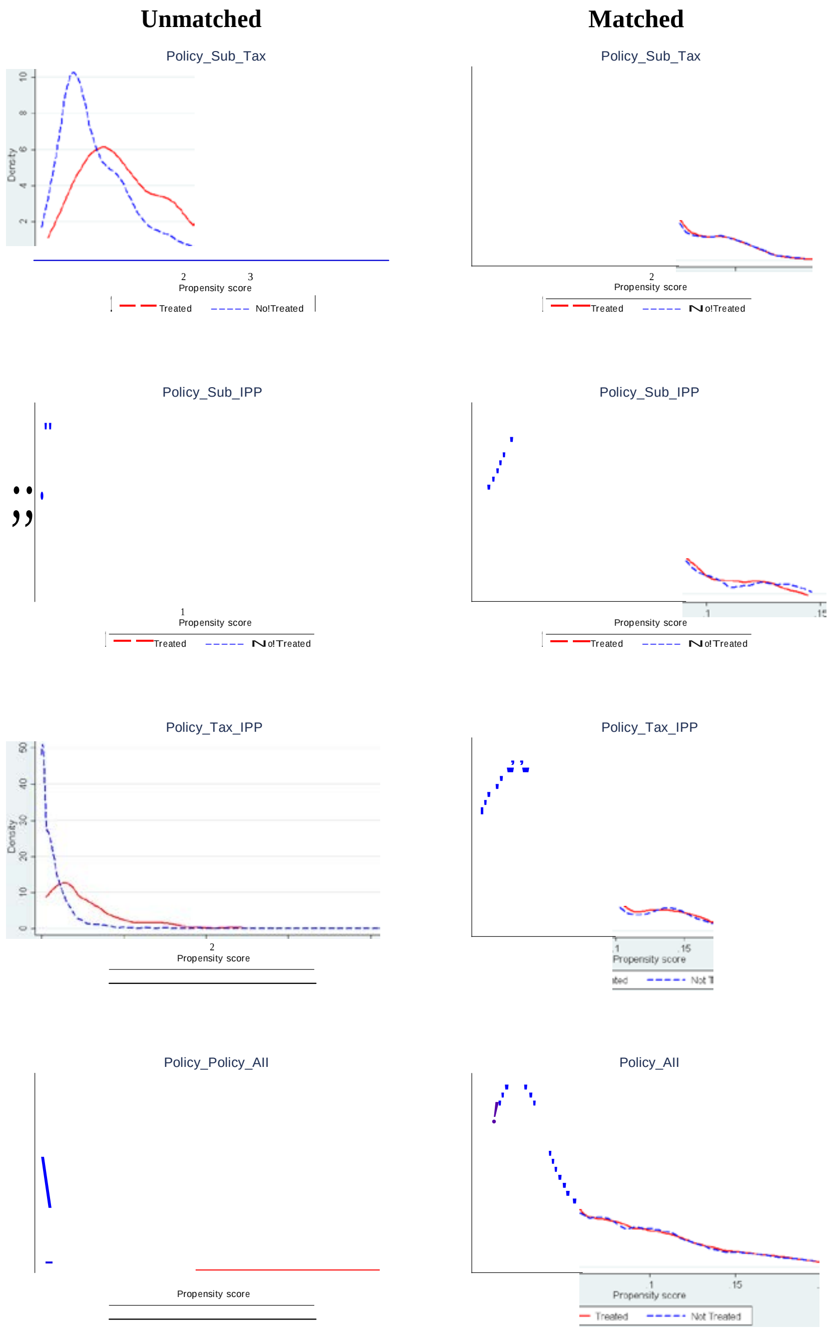


Figure A.6: Results

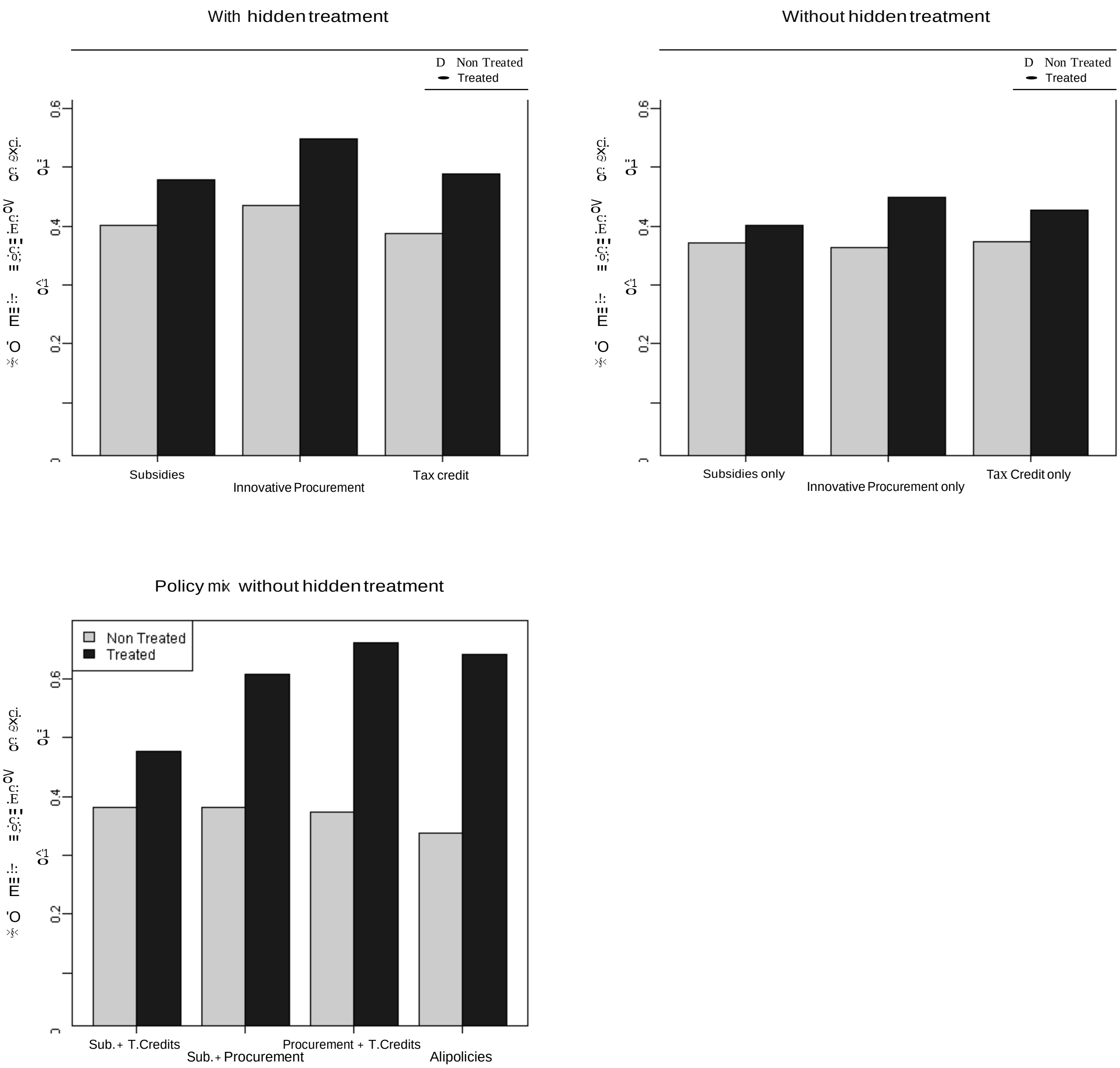


Figure A.7: Density distribution of the Predicted probability of receiving innovation subsidies for firrns surveyed in the Innobarometer 2009 and CIS 8 survey

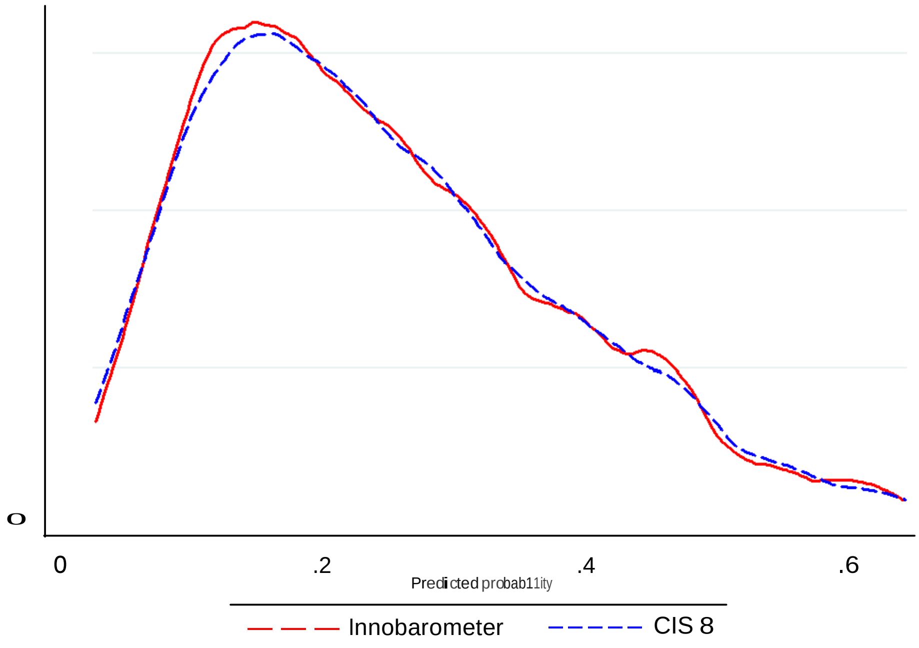
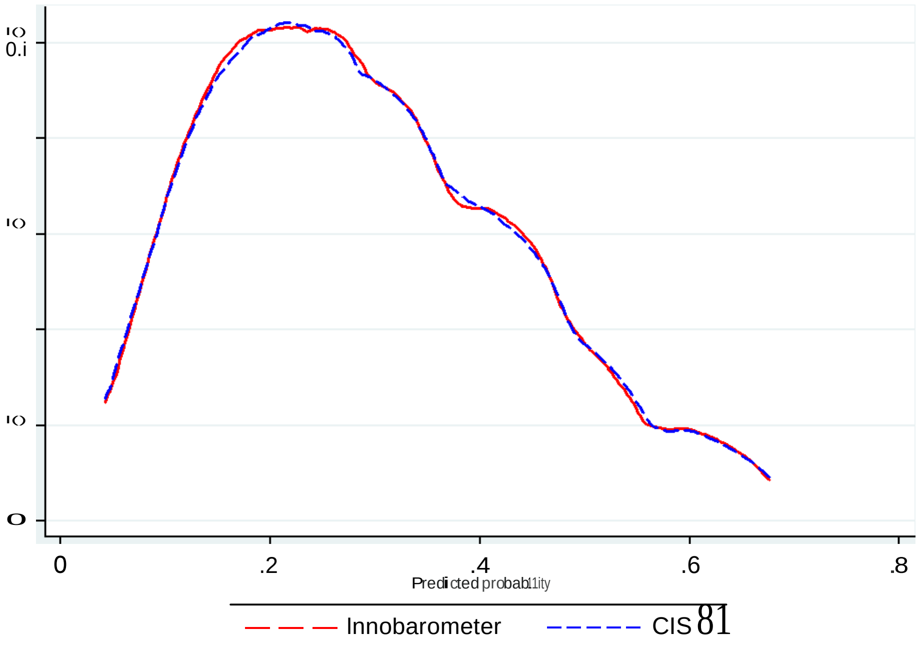


Figure A.8: Density distribution of the Predicted probability of receiving innovation subsidies for firrns surveyed in the Innobarometer 2009 and CIS 8 survey for Germany, Italy, Portugal and Spain



List of Tables

Table A.1: Treated firms

Treatment	Treated	Control	Description
Treatment vulnerable to confounding			
Policy_Subsidies	1108	3723	Firms receiving subsidies
Policy_Procurement	551	4277	Firms receiving innovative public procurement contracts
Policy_Tax Credits	1082	3655	Firms receiving tax credits
Treatment in isolation			
Policy_Subsidies_only	462	2708	Firms receiving only subsidies
Policy_Procurement_only	273	2708	Firms receiving only innovative public procurement contracts
Policy_Tax credit_only	483	2708	Firms receiving only tax credits
Simultaneous treatments			
Policy_Sub_Tax	403	2708	Firms receiving subsidies and tax credits
Policy_Sub_IPP	85	2708	Firms receiving subsidies and innovative public procurement
Policy_IPP_Tax	75	2708	Firms receiving innovative public procurement and tax credits
Policy_All	84	2708	Firms receiving all policies

Table A.2: Summary statistics

	mean	sd	count
RD_ACT	.455	.498	4992
young_firm	.081	.273	4992
SIZE_1	.404	.490	4992
SIZE_2	.323	.467	4992
SIZE_3	.175	.380	4992
SIZE_4	.096	.295	4992
mkt_regional	.904	.293	4982
mkt_national	.669	.470	4980
mkt_eu	.440	.496	4978
mkt_global	.308	.462	4972

Table A.3: Descriptive statistics

Variables	Not treated	Subsidies	Not treated	Procurement	Not treated	Tax credits
RD_ACT	.421	.564	.430	.638	.430	.522
young_firm	.079	.086	.081	.083	.077	.092
SIZE_1	.418	.369	.418	.343	.411	.397
SIZE_2	.321	.336	.321	.313	.328	.313
SIZE_3	.168	.187	.171	.190	.170	.184
SIZE_4	.091	.106	.089	.152	.090	.104
mkt_regional	.901	.913	.900	.934	.904	.903
mkt_national	.662	.688	.654	.774	.654	.708
mkt_eu	.432	.459	.436	.472	.427	.468
mkt_global	.301	.325	.301	.342	.294	.335
INNO_INCREASE	.373	.477	.377	.544	.370	.487
N	3723	1108	4277	551	3655	1082

Table A.4: Descriptive statistics

Variable	Not treated	Subsidies_only	Procurement_only	Tax credits_only	Sub_Tax	IPP_Tax	Sub_IPP	All
RD_ACT	.395	.502	.597	.432	.560	.666	.623	.714
young_firm	.077	.075	.069	.093	.091	.133	.117	.059
SIZE_1	.430	.359	.340	.432	.387	.293	.364	.452
SIZE_2	.322	.352	.318	.287	.339	.346	.317	.250
SIZE_3	.166	.181	.168	.182	.178	.200	.247	.154
SIZE_4	.080	.106	.172	.097	.094	.160	.070	.142
mkt_regional	.898	.917	.930	.893	.898	.960	.929	.928
mkt_national	.640	.652	.761	.694	.684	.800	.670	.833
mkt_eu	.427	.415	.426	.452	.470	.493	.447	.523
mkt_global	.289	.280	.318	.339	.325	.337	.309	.369
INNO_INCREASE	.346	.404	.454	.426	.476	.666	.600	.642
N	2708	462	273	483	403	75	85	84

Table A.5: Probit regression results

	(1) Policy_Subsidies	(2) Policy_Tax Credits	(3) Policy_Procurement
RD_ACT	0.377***	0.234***	0.413***
SIZE_2	0.0362	-0.0687	0.0262
SIZE_3	0.0545	0.00704	0.0595
SIZE_4	0.0701	0.00649	0.292***
mkt_regional	0.0523	0.0223	0.202**
mkt_national	0.0158	0.108**	0.212***
mkt_eu	0.0500	-0.0108	-0.0498
mkt_global	-0.0469	0.0804	-0.0355
young_firm	0.0584	0.0521	-0.00963
Sectoral dummies	Yes	Yes	Yes
Country dummies	Yes	Yes	Yes
_cons	-0.798***	-0.922***	-1.706***

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: Multinomial logit results

	1 Subsidies_Only	2 Procurement_Only	3 Tax_Only	4 Policy_All	5 Tax_IPP	6 Subsidies_IPP	7 Subsidies_Tax
RD_ACT	0.390***	0.700***	0.132	0.704***	1.009***	1.000***	1.345***
SIZE_2	0.214*	0.105	-0.182	-0.0272	0.365	-0.0714	-0.755**
SIZE_3	0.172	0.0956	0.0130	0.0171	0.383	0.350	-0.494
SIZE_4	0.422**	0.891***	-0.0751	0.0288	0.725*	-0.243	0.0140
mkt_regional	0.0708	0.241	0.00266	-0.0204	1.169	0.384	0.321
mkt_national	0.0642	0.361**	0.224*	0.0148	0.756**	-0.255	0.723**
mkt_eu	-0.132	-0.383**	-0.250*	0.0628	-0.141	0.215	0.197
mkt_global	-0.100	-0.0947	0.383***	0.00189	-0.192	-0.0882	-0.00446
young_firm	0.0438	-0.122	0.113	0.125	0.547	0.396	-0.471
Sectoral dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
_cons	-1.719***	-2.990***	-2.613***	-1.702***	-6.468***	-5.698***	-3.791***

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: Loss of observations due to common support and caliper requirement

	Treated group	Control group	Loss to common support & Caliper (%)
Treatment vulnerable to confounding			
Policy_Subsidies	1108	3723	15 (1.35)
Policy_Procurement	551	4277	8 (1.45)
Policy_Tax Credits	1082	3655	11 (1.01)
Treatment in isolation			
Policy_Subsidies_only	462	2708	8 (1.73)
Policy_Procurement_only	273	2708	6 (2.19)
Policy_Tax credit_only	483	2708	10 (2.07)
Simultaneous treatments			
Policy_Sub_Tax	403	2708	8 (1.98)
Policy_Sub_IPP	85	2708	6 (7.05)
Policy_IPP_Tax	75	2708	1 (1.33)
Policy_All	84	2708	6 (7.14)

Table A.8: Balance

Treatment	Sample	Pseudo R2	LR chi2	p chi2	MeanBias	MedBias
Policy_Subsidies	Raw	0.056	287.08	0.000	4.9	4.2
	Matched	0.004	13.58	1.000	1.4	1.5
Policy_Procurement	Raw	0.087	293.85	0.000	7.0	4.7
	Matched	0.009	13.64	1.000	2.1	1.8
Policy_Tax Credits	Raw	0.056	283.65	0.000	5.4	3.9
	Matched	0.004	12.77	1.000	1.5	1.2
Policy_Subsidies_only	Raw	0.062	161.66	0.000	5.9	5.2
	Matched	0.018	23.21	1.000	2.8	2.3
Policy_Procurement_only	Raw	0.099	177.45	0.000	8.5	7.2
	Matched	0.019	13.74	1.000	3.0	1.8
Policy_Tax credit_only	Raw	0.064	172.37	0.000	5.6	4.2
	Matched	0.018	24.03	1.000	2.8	2.4
Policy_Sub_Tax	Raw	0.089	210.54	0.000	7.4	6.2
	Matched	0.022	23.90	1.000	3.1	2.0
Policy_Sub_IPP	Raw	0.134	93.80	0.000	11.8	10.6
	Matched	0.063	13.87	1.000	4.4	2.7
Policy_IPP_Tax	Raw	0.122	76.10	0.020	12.8	9.1
	Matched	0.051	10.38	1.000	3.9	2.8
Policy_All	Raw	0.168	120.63	0.000	12.4	9.0
	Matched	0.051	10.98	1.000	4.6	3.8

Table A.9: Results

Policy	Sample	Treated	Controls	Difference	S.E.	T-stat
Policy_Subsidies	Unmatched	.4777	.3736	.1040***	.0167	6.21
	ATT	.4784	.4004	.0780***	.0201	3.88
Policy_Procurement	Unmatched	.5457	.3775	.1682***	.0221	7.61
	ATT	.5488	.4358	.1129***	.0270	4.17
Policy_Tax Credits	Unmatched	.4883	.3712	.1170***	.0169	6.91
	ATT	.4892	.3879	.1013***	.0206	4.91
Policy_Subsidies_only	Unmatched	.4026	.3473	.0552**	.0242	2.28
	ATT	.4008	.3715	.0293	.0292	1.00
Policy_Procurement_only	Unmatched	.4522	.3473	.1048***	.0304	3.44
	ATT	.4494	.3645	.0848**	.0377	2.25
Policy_Tax credit_only	Unmatched	.4276	.3473	.0802***	.0238	3.37
	ATT	.4270	.3735	.0535*	.0289	1.85
Policy_Sub_Tax	Unmatched	.4763	.3473	.1289***	.0256	5.02
	ATT	.4759	.3822	.0936***	.0318	2.94
Policy_Sub_IPP	Unmatched	.61445	.3473	.26706***	.05312	5.03
	ATT	.6075	.3881	.21940***	.06679	3.28
Policy_IPP_Tax	Unmatched	.6621	.3473	.3147***	.05611	5.61
	ATT	.6621	.3738	.2882***	.0673	4.28
Policy_All	Unmatched	.6428	.3473	.2954***	.0527	5.60
	ATT	.6410	.3376	.3034***	.0661	4.59

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

Table A.10: Results obtained using the nearest neighbor procedure with only 1 neighbor

Policy	Sample	Treated	Controls	Difference	S.E.	T-stat
Policy_Subsidies	Unmatched	.4777	.3736	.1040***	.0167	6.21
	ATT	.4784	.3858	.0926***	.0228	4.05
Policy_Procurement	Unmatched	.5457	.3775	.1682***	.0221	7.61
	ATT	.5488	.4358	.1129***	.0270	4.17
Policy_Tax Credits	Unmatched	.4883	.3712	.1170***	.0169	6.91
	ATT	.4892	.3678	.1213***	.023	5.16
Policy_Subsidies_only	Unmatched	.4026	.3473	.0552**	.0242	2.28
	ATT	.4008	.3810	.0197	.0344	0.57
Policy_Procurement_only	Unmatched	.4522	.3473	.1048***	.0304	3.44
	ATT	.4494	.3592	.0901**	.0436	2.07
Policy_Tax credit_only	Unmatched	.4276	.3473	.0802***	.0238	3.37
	ATT	.4270	.3792	.0478	.03343	1.43
Policy_Sub_Tax	Unmatched	.4763	.3473	.1289***	.0256	5.02
	ATT	.47597	.3370	.1388***	.0361	3.84
Policy_Sub_IPP	Unmatched	.61445	.3473	.26706***	.05312	5.03
	ATT	.6075	.4396	.1679**	.0787	2.13
Policy_IPP_Tax	Unmatched	.6621	.3473	.3147***	.05611	5.61
	ATT	.66216	.4621	.2000**	.08257	2.42
Policy_All	Unmatched	.6428	.3473	.2954***	.0527	5.60
	ATT	.6410	.35961	.2813***	.0757	3.71

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

Table A.11: Results obtained using kernel matching procedure

Policy	Sample	Treated	Controls	Difference	S.E.	T-stat
Policy_Subsidies	Unmatched	.4777	.3736	.1040***	.0167	6.21
	ATT	.4781	.3952	.0828***	.0243	3.40
Policy_Procurement	Unmatched	.5457	.3775	.1682***	.0221	7.61
	ATT	.54576	.3901	.1556***	.0324	4.80
Policy_Tax Credits	Unmatched	.4883	.3712	.1170***	.0169	6.91
	ATT	.4883	.3606	.1276***	.0251	5.08
Policy_Subsidies_only	Unmatched	.4026	.3473	.0552**	.0242	2.28
	ATT	.4026	.3785	.0240	.0360	0.67
Policy_Procurement_only	Unmatched	.4522	.3473	.1048***	.0304	3.44
	ATT	.4522	.3676	.0845*	.0452	1.87
Policy_Tax credit_only	Unmatched	.4276	.3473	.0802***	.0238	3.37
	ATT	.4285	.3613	.0672*	.0355	1.89
Policy_Sub_Tax	Unmatched	.4763	.3473	.1289***	.0256	5.02
	ATT	.4735	.3682	.1052***	.0275	3.83
Policy_Sub_IPP	Unmatched	.61445	.3473	.26706***	.05312	5.03
	ATT	.6075	.3709	.2366***	.0565	4.18
Policy_IPP_Tax	Unmatched	.6621	.3473	.3147***	.05611	5.61
	ATT	.6621	.3767	.2854***	.0569	5.01
Policy_All	Unmatched	.6428	.3473	.2954***	.0527	5.60
	ATT	.6410	.3926	.2483***	.0560	4.43

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

Table A.12: Difference in the participation rate in subsidies program for Inuobarometer and CIS 8 firms

Sample	**Innobarometer**	**CIS 8**	**Di:fference**	S.E.	**T-stat**
Unmatched	.351921275	.218906998	**.133014276*** **	.009247069	14.38
Matched	.34439834	.294605809	.049792531	.041094489	1.21

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

Table A.13: Difference in the participation rate in subsidies program for Innobarometer and CIS 8 firms, for Germany, Italy, Spain and Portugal

Sample	Innobarometer	CIS 8	Difference	S.E.	T-stat
Unmatched	.323489933	.222461757	.101028176***	.015448447	6.54
Matched	.304093567	.328947368	-.024853801	.044974859	-0.55

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