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Using Innovation Survey Data to Evaluate R&D Policy: The Case of Belgium

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Non-technical Summary

The importance of R&D as a main factor of sustainable growth in highly industrialized economies is undisputable among economists, especially on the background of the structural shift from resource-based economies to modern knowledge-based economies. In this line, countries cannot only rely on public R&D conducted at universities or public research institutions. The role of R&D performed in the business sector is of increasing importance in society. In order to stimulate R&D in the business sector, governments usually offer a wide range of public incentives, like R&D subsidies, tax credits, technological consultancy etc.

In this paper, we study the relationship between R&D subsidies and R&D activities empirically. Conducting a treatment effects analysis, we investigate whether public R&D funding in Belgium crowds-out the private investment in the business sector. We employ the third wave of the Community Innovation Survey (CIS3) for Flanders for our analysis and link the firm level information with other data resources on patents and financial statements.

Our sample covers the Flemish manufacturing sector and computer services, R&D services as well as business related services. In total, the sample consists of 776 firms, of which 180 refer to recipients of R&D subsidies. Using a non-parametric nearest-neighbor matching, we find that firms which received public funding for innovation projects would have invested significantly less in R&D if they had not received the subsidies. This holds true for both the full sample and a subsample including only innovating firms. First, we use the full sample that also includes non-innovative firms in the control group, because there may be subsidized firms that would not have conducted any R&D if they had not been subsidized. The reasons can range from too high cost or too high risk of research projects to the inability to raise sufficient external capital. If the potential control group is restricted to innovating firms only, one thus might underestimate the treatment effect, because the innovation status of a firm can change from non-innovative to innovative due to the receipt of a subsidy and vice versa. In addition to the full sample, however, we also test the hypothesis of crowding out effects in the subsample of innovating firms only, because some readers might argue that the first result is driven by the non-innovative firms only. However, the results confirm the previous estimations. The hypothesis of crowding-out is also rejected in the subsample of innovating firms.

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Abstract

This study focuses on the impact of R&D policies in Flanders. We conduct a treatment effects analysis at the firm level to investigate possible crowding-out effects on the input side of the innovation process. Different specifications of R&D activity are considered as outcome variables in the treatment effects analysis. Applying a non-parametric matching, we conclude that subsidized firms would have invested significantly less in R&D activities, on average, if they had not received public R&D funding. Thus, crowding-out effects can be rejected in this case.

Keywords: R&D, Subsidies, Policy Evaluation, Non-parametric matching
JEL-Classification: C14, C25, H50, O38

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1 Introduction

The importance of R&D as a main factor of sustainable growth in highly industrialized economies is undisputable among economists, especially on the background of the structural shift from resource-based economies to modern knowledge-based economies. In this line, countries cannot only rely on public R&D conducted at universities or public research institutions. The role of R&D performed in the business sector is of increasing importance in society (cf., for example, European Commission, 2003). In order to stimulate R&D in the business sector, governments usually offer a wide range of public incentives, like R&D subsidies, tax credits, technological consultancy etc.

There are clear economic rationales behind supporting private R&D: the level of privately financed R&D activities is lower than socially desired, because R&D has the characteristics of a public good and generates positive external effects, which cannot be internalized (see Arrow, 1962). Thus, there may be projects that would have positive benefits to society, but do not cover the private cost. Therefore, these projects are not carried out and the quantity of innovations is below the socially desirable level. This circumstance is the main reason for governments to subsidize private R&D projects. Public funding reduces the price for private investors and thus more innovation takes place, ideally reaching the social equilibrium.[1]

However, the economic dilemma is that a firm always has an incentive to apply for public R&D support, even if it could perform the R&D projects using its own financial means. If public support is granted, the firm then might simply substitute public for private investment instead of increasing its R&D spending. This possible crowding-out effect between public grants and private investment has been discussed in the economic literature for decades and has to be taken into account when public authorities decide on the level of their engagement in R&D support programs.

In this paper, we study the relationship between R&D subsidies and R&D activities empirically. Conducting a treatment effects analysis, we investigate whether public R&D funding in Flanders crowds-out the private investment in the business sector. Flanders as part of Belgium is a small region that is subjective to external competition and thus needs to innovate on a permanent basis to be able to compete in the global economy. Therefore the Flemish government is highly interested in stimulating innovation activities in the local business sector. Consequently, it is called for empirical evidence on the benefits of such public investments. We employ the third wave of the Community Innovation Survey for Flanders for our analysis and link the firm level information with other data resources on

[1] Another important argument for a governmental market intervention is financial constraints for research and development. See Hall (2002) for a survey on this topic.

patents and financial statements. The following section summarizes the existing literature on related studies and section 3 outlines our econometric approach. Section 4 presents the data and variables used in the analysis as well as the estimation results. The fifth section concludes and outlines further research on this topic.

2 Literature Review

After the US R&D budget was significantly raised during the 1950s, Blank and Stigler (1957) were among the first to question the relationship between publicly funded and private R&D. With a large sample of firms they tested whether a complementary or substitutive relationship between public and private R&D investment existed. In case of a complementary relationship, firms use to extent their innovation activities due to public funding. If full crowding-out effects between public and private funds occur, the private innovation activities remain constant. The implications of such studies are significant for today's R&D politics because a complementary relationship legitimizes public funding whereas substitution is regarded as misallocation.

Over time and along with improved scientific methods it became clear that definite statements regarding the effect of public R&D funding cannot be made. David et al. (2000) surveyed macro- and microeconomic studies, focusing on their „net impacts". Only two out of fourteen of these empirical studies indicated substitutive effects on the aggregate level. On the firm-level the results are less clear, i.e. nine out of nineteen find substitutional effects. In summary, macroeconomic studies usually identify a complementary relationship between public and private R&D expenditure, whereas several micro-studies on the firm-level are not able to confirm this effect. In contrast to macroeconomic studies, the advantage of a microeconomic analysis is that it can allow for detailed influences among several determinants that may have an impact on private R&D activities. Recent microeconometric studies approach the above-mentioned question with firm level or business data provided by ministerial offices, business publishers, statistical offices or own surveys. Based on this data, the impact of the available determinants on private R&D activities is tested by panel or cross-sectional econometric analysis (cf. Klette et al., 2000).

One major result of the survey by David et al. (2000) is the criticism that previous studies largely ignored a possible selection bias in the empirical investigations. This issue linked to the public funding decision had already been described by Lichtenberg (1987) and Klette/Møen (1997). The difficulty of this aspect lies within potential selection bias of the public institution that – depending on the applying firm and the relevant R&D project – decides on the public funding process. For instance, governments usually follow a "picking-the-winner" strategy, that is, firms that are very innovative even in the absence of public incentive schemes are more likely to receive public grants. The reason is that public authorities want to maximize social benefits and reduce the risk of failure of R&D projects. Firms that have been innovative and successful in the past are hence the best candidates for the receipt of subsidies, because they are expected to generate the highest social return from the public investment due to low failure rates and high spill-over effects. "This makes public funding an endogenous variable, and its inclusion in a linear regression will cause inconsistent estimates if it happens to be correlated with the error term" (Busom, 2000: 114). To estimate the "real" effects of public subsidies it is necessary to address the core evaluation question: "how much would the recipients have invested, if they had not participated in a public policy scheme?".

In fact, several recent studies on the impact of R&D subsidies attempt to model this counterfactual situation. Busom (2000) explores this problem by applying an econometric selection model. Based on Heckman's selection model[2], she estimates a probit model on a participation dummy. In a second equation, the R&D activity is regressed on several covariates including a selection correction term which accounts for the different propensities of firms to be publicly funded. The second equation is estimated separately for participants and non-participants including selection correction terms. The difference in expected values of R&D expenditure of participants in case of funding and in case of non-participation is assigned to public funding. Busom concludes that public funding induced more effort for the majority of firms in her sample, but for 30% of the participants, complete crowding-out effects cannot be ruled out. Wallsten (2000) uses a simultaneous equations model on the receipt of public R&D funding from the US Small Business Innovation Research (SBIR) program and on the R&D spending of firms. Applying the 3SLS estimator to a cross-section of funded and non-funded firms, he finds that the SBIR grants crowd out private investment dollar for dollar. However, he points out that the program still could have positive effects: the recipient firms might have been able to keep their innovation activities constant while in the absence of a subsidy they had to reduce them. This hypothesis would require a longitudinal study, though. Lach (2002) investigates the effects of R&D subsidies granted by the Israeli Ministry of Industry and Trade on local manufacturing firms. He applies different estimators, such as the difference-in-difference estimator and dynamic panel data models. Although Lach finds heterogeneous results from different models applied, he finally concludes that subsidies do not crowd out company financed R&D expenditure completely. For small firms, Lach states that one subsidized New Israeli Schekel (NIS) generates eleven additional NIS of privately financed R&D. However, as large firms receive most of the funds, one publicly financed NIS stimulates only 0.23 additional NIS, on average, and this effect is statistically insignificant. Czarnitzki (2001), Czarnitzki and Fier (2002), as well as Almus and Czarnitzki (2003) employ a matching approach – as done in this paper - to investigate the impact of public subsidies in Germany, and they reject full crowding-out in Eastern German manufacturing and in the German service sector; Hussinger (2003) explores semiparametric selection models using German survey data. She largely

[2] Heckman's selection model is extended to the treatment model (see e.g. Maddala, 1983, section 9.2).

confirms the positive results previously identified with German data. Duguet (2004) employs the matching methodology with a large panel of French firms covering the years 1985 to 1997. Controlling for past public support the firms benefited from, he also rejects the crowding out hypothesis for France. Gonzàlez et al. (2004) investigate subsidies in a panel of more than 2,000 Spanish manufacturing firms and employ a simultaneous equation model. They state that subsidies are effective in inducing firms to invest into R&D, but they induce only slight changes in the level of R&D expenditure. They conclude that in the absence of subsidies, publicly supported R&D projects would be carried out, although in smaller size. However, they do not report crowding-out effects or inefficient use of subsidies.

In fact, there exist also a few studies on Belgian data already: Holemans and Sleuwaegen (1988) where the first who considered the relationship between R&D subsidies and R&D spending for this country. They consider a small sample of Belgian firms for a period from 1980 to 1984 and estimate a fixed effects model on the relationship between the log of private R&D expanditure and the log of public subsidies. Holemans and Sleuwaegen report significant additionality effects of the subsidies. More recently, Meeusen and Janssens (2001) employ a biannual R&D survey from Flanders covering the period 1992 to 1997. They perform (dynamic) fixed effects panel estimations to investigate the impact of subsidies on R&D intensity (R&D/sales) and find that grants from the IWT (the institution administrating the R&D funds of the Flemish Regional Government) have a strong positive effect on private R&D spending. However, they do not take into account the possible endogeneity of the subsidies. Consequently, Suetens (2002) extends the analysis to an IV framework. She also uses the combination of IWT subsidies data with the biannual survey data and employs a lagged value of subsidies as an instrument in order to account for endogeneity. While OLS estimations again support the additionality hypothesis, her results on the IV regressions are not informative. All variables except a lagged R&D term are insignificant with respect to R&D expenditure. While Suetens states that one should be very careful in interpreting this result, she actually does not test for the validity of her instruments. Finally, note that all three studies mentioned are restricted to R&D-performing firms only.

In summary, the majority of recent studies report complimentary effects of public R&D, but crowding-out effects, especially partial ones, cannot be ruled out. For Belgium, the first two studies by Holemans/Sleuwaegen (1988) and Meeusen/Janssens (2001) are in favor of the additionality hypothesis and the third study by Suetens (2002) is not conclusive. Neither of the previous studies on Belgium firms explicitely attempt to control for selection bias which is the main issue investigated in this paper. The following section outlines the matching estimator which is used in the subsequent empirical analysis.

3 Estimation of Treatment Effects with the Matching Estimator

The modern econometric evaluation techniques have been developed to identify treatment effects when the available observations on individuals or firms are subject to a selection bias. This typically occurs when participants in public measures differ from non-participants in important characteristics. Popular economic studies are on the benefit of active labor market policies.

The literature on the econometrics of evaluation offers different estimation strategies to correct for selection bias (see Heckman et al., 1997, Heckman et al. 1999 for a survey) including the difference-in-difference estimator, control function approaches (selection models), IV estimation and non-parametric matching. The difference-in-difference method requires panel data with observations before and after/while the treatment (change of subsidy status). As our database (to be described in the following subsection) consists of a cross-section, we cannot apply this estimator. For the application of IV estimators and selection models one needs valid instruments for the treatment variables. It is very difficult in our case to find possible candidates being used as instruments. Hence, the only appropriate choice is the matching estimator. Its main advantage over IV and selection models is that we neither have to assume any functional form for the outcome equation nor is a distributional assumption on the error terms of the selection equation and the outcome equation necessary. The disadvantage is that is does only control for observed heterogeneity among treated and untreated firms. However, as we discuss in the next section, we think that our set of covariates allows us to assume that selection on unobservable effects is unlikely.

Matching estimators have recently been applied and discussed by Angrist (1998), Dehejia and Wahba (1999), Heckman et al. (1998a, 1998b), and Lechner (1999, 2000). The matching is able to address directly the question "What would a treated firm with given characteristics have done if it had not been treated?" A treatment in our context is the receipt of a subsidy for R&D. Those observations on treated firms are compared with non-treated firms, but not with all non-recipients but a selected group with similar characteristics. Our fundamental evaluation question can be illustrated by an equation describing the average treatment effect on the treated individuals or firms, respectively:

$$E\left(\alpha_{TT}\right) = E\left(Y^T \mid S=1\right) - E\left(Y^C \mid S=1\right) \qquad (1)$$

where Y^T is the outcome variable, that is R&D spending in our case. The status S refers to the group: S=1 is the treatment group and S=0 the non-treated firms. Y^C is the potential outcome which would have been realized if the treatment group (S=1) had not been treated. The problem is obvious: while the outcome of the treated individuals in case of treatment, $E(Y^T|S=1)$, is directly observable, it is not the case for the counterpart. What would these firms have realized if they had not received the treatment? $E(Y^C|S=1)$ is a counterfactual situation which is not observable and, therefore, has to be estimated. In the case of matching, this potential outcome of treated firms is constructed from a control group of non-participants. The matching relies on the intuitively attracting idea to balance the sample of program participants and comparable non-participants. Remaining differences in the outcome variable between both groups are then attributed to the treatment (Heckman et al., 1997).

Initially the counterfactual cannot simply be estimated as average outcome of the non-participants, because $E(Y^C|S=1) \neq E(Y^C|S=0)$ due to the possible selection bias. The participant group and non-participant group are expected to differ, except in cases of randomly assigned measures in experimental settings. Rubin (1977) introduced the conditional independence assumption (CIA) to overcome the selection problem, that is, participation and potential outcome are independent for individuals with the same set of exogenous characteristics X. If this assumption is valid, it follows that

$$E\left(Y^C \mid S=1, X\right) = E\left(Y^C \mid S=0, X\right) \tag{2}$$

The outcome of the non-participants can be used to estimate the counterfactual outcome of the participants in case of non-participation provided that there are no systematic differences between both groups. The treatment effect can be written as

$$E\left(\alpha_{TT}\right) = E\left(Y^T \mid S=1, X=x\right) - E\left(Y^C \mid S=0, X=x\right) \tag{3}$$

Conditioning on X takes account of the selection bias due to observable differences between participants and non-participants. In our case, we conduct a Nearest Neighbor matching, that is, for each treated firm we pick the most similar firm from the potential control group of non-subsidized firms.[3] In addition to the CIA, another important precondition for consistency of the matching estimator is common support. It is necessary that the control group contains at least one sufficiently similar observation for each treated firm. In practice, the sample to be evaluated is restricted to common support. However, if the overlap between the samples is too small the matching estimator is not applicable.

As one often wants to consider more than one matching argument, one has to deal with the "curse of dimensionality". If we employ a lot of variables in the matching function, it will become difficult to find appropriate controls. Rosenbaum/Rubin (1983) suggested to use a propensity score as a single index and thus to reduce the number of variables included in the matching function to just one. Therefore a probit model is estimated on the dummy indicating the receipt of subsidies S. The estimated propensity scores are subsequently used as matching argument. Lechner (1998) introduced a modification of the propensity score matching ("hybrid matching") as one often wants to include additional variables, e.g. like firm size, directly in the matching function. In this case, instead of a

[3] Other matching estimators are, for example, caliper matching or kernel matching (see Heckman et al., 1999).

single X (the propensity score), other important characteristics may be employed in the matching function. The following matching protocol summarizes the empirical implementation of the matching procedure used in this paper.

Table 1: The matching protocol

Step 1	Specify and estimate a probit model to obtain the propensity scores $\hat{P}(X)$.
Step 2	Restrict the sample to common support: delete all observations on treated firms with probabilities larger than the maximum and smaller than the minimum in the potential control group. (This step is also performed for other covariates that are possibly used in addition to the propensity score as matching arguments.)
Step 3	Choose one observation from the subsample of treated firms and delete it from that pool.
Step 4	Calculate the Mahalanobis distance between this firm and all non-subsidized firms in order to find the most similar control observation. $MD_{ij} = \left(Z_j - Z_i\right)' \Omega^{-1} \left(Z_j - Z_i\right)$ In our case, Z contains the estimated propensity score and the firm size (number of employees) as additional argument in the matching function. Ω is the empirical covariance matrix of these arguments based on the sample of potential controls.
Step 5	Select the observation with the minimum distance from the remaining sample. (Do not remove the selected controls from the pool of potential controls, so that it can be used again.)
Step 6	Repeat steps 3 to 5 for all observations on subsidized firms.
Step 7	Using the matched comparison group, the average effect on the treated can thus be calculated as the mean difference of the matched samples: $\hat{\alpha}_{TT} = \frac{1}{n^T}\left(\sum_i Y_i^T - \sum_i \widehat{Y_i^C}\right)$ with $\widehat{Y_i^C}$ being the counterfactual for i and n^T is the sample size (of treated firms). Note that the same observation may appear more than once in that group.
Step 8	As we perform sampling with replacement to estimate the counterfactual situation, an ordinary t-statistic on mean differences is biased, because it does not take the appearance of repeated observations into account. Therefore, we have to correct the standard errors in order to draw conclusions on statistical inference. We follow Lechner (2001) and calculate his estimator for an asymptotic approximation of the standard errors.

4 Data and Estimations

The data used in this paper stem from various sources. The firm level data are the Flemish part of the third Community Innovation Survey (CIS3) and refers to the years from 1998 to 2000. The CIS covers most EU countries, Norway and Iceland using a largely harmonized questionnaire over countries. Eurostat (2004) presents detailed descriptive survey results for all countries and aggregate statistics. Our sample covers the Flemish manufacturing sector and computer services, R&D services as well as business related services. In total, our sample consists of 776 observations, of them 180 refer to recipients of R&D subsidies. The CIS data are supplemented with information from the Belfirst database which contains the annual account data of Belgian firms. Furthermore, the firms are linked to patent data from the European Patent Office. This source contains all patent applications filed at the EPO since 1978.

The receipt of subsidies is denoted by a dummy variable indicating firms that received public R&D funding in the period from 1998 to 2000. The indicator covers subsidies from the local Flemish Government, the Belgian national Government as well as the EU. As this variable covers a three year period, we use lagged values of the covariates measured before 1998 whenever possible, in order to avoid endogeneity problems in the selection equation. As outcome variables, we consider the R&D expenditure at the firm level in 2000, *R&D*.[4] However, as the distribution of this indicator is very skewed in the economy, we also investigate the R&D intensity, *R&DINT* (R&D expenditure / turnover * 100), and the logs of *R&D* and *R&DINT* to check various specifications (see the appendix for the empirical distribution the variables).[5] Due to the skewness of the original variables, some extreme values might affect the mean of the distribution significantly, so that a few observations may determine the estimation results. Using the logarithmic transformation scales down the large values and reduces the problem with these skewed distributions. All R&D indicators mentioned above refer to the year 2000.

As a further outcome variable, we consider patent applications at the European Patent Office (EPO) in the year 2000. In contrast to R&D, patents focus on the outcome of the innovation process and thus on successful R&D projects. However, the number of observations with patent applications at the EPO in our sample is very low: only 43 firms filed EPO patents from 1998 to 2000. Once again, due to the skewness of the distribution we consider the number of patent applications per employee (*PATENT/EMP*) and a dummy variable indicating patenting firms *(D(PATENT>0)).*

We use several control variables in our analysis that might affect both the probability to receive subsidies and R&D or innovation expenditure, respectively: the inclusion of the number of employees at the beginning of the period controls for size effects (EMP_{1998}). Another important variable in our analysis is the firms' patent stock. Where our main data source is only a cross-section of the CIS and no time-series information is included, the patent stock enables us to control for previous (successful) R&D activities. We use all patent information in the EPO database and generate the stock of patents per firm as the depreciated sum of all patents filed at the EPO from 1979 until 1997:

$$PS_{it} = \left(1-\delta\right) PS_{i,t-1} + PA_{it} \, ,$$

where *PS* is the patent stock of firm i in period *t*, *PA* are the number of patent applications filed at the EPO and δ is a constant depreciation rate of knowledge which is set to 0.15, as common in the literature (see e.g. Hall, 1990). On the one hand, firms that exhibit previous successful innovation

[4] The CIS definition of R&D expenditure follows the Frascati Manual (OECD, 1993).
[5] In case of the logarithmic specification, we replace zero values of *R&D* and *R&DINT* with the minimum observed value in order to generate the log of the variables.

projects indicated by patents, are more likely to receive public R&D funding, because the public authorities follow the "picking-the-winner" principle in order to minimize the expected failure rates of the innovation projects, and hence, to maximize the expected benefit for the society. On the other hand, the patent stock controls for the past average innovation engagement of the firms, because it is expected that firms that were highly innovative in the past will continue this strategy. Although our data is basically cross-sectional, the patent stock allows us to control for the time-series dimension of the innovation activities.[6] The patents are counted only until 1997, to ensure that the stock definitely refers to past innovation activities, in order to avoid a simultaneous equation bias in the regression analysis. The patent stock enters into the regression as patent stock per employee (*PS/EMP*) to reduce multicollinearity with firm size.

Furthermore, the export quota from 1998 ($EXQU_{1998}$ = exports / turnover) measures the degree of international competition a firm faces. Firms that engage in foreign markets may be more innovative than others and, hence, are more likely to apply for subsidies. Another indicator considered is the capital intensity. This variable is taken from the BELFIRST database and refers to the year 1997 ($KAPINT_{1997}$ = fixed assets / EMP). Companies with a capital-intensive production process supposedly rely more on innovation activities than more labor-intensive firms. In addition, we include two financial measures that could influence both the public funding probability and the outcome variables. According to the standard financing hierarchy, firms tend to finance risky undertakings like R&D with internal resources. If these are exhausted they turn to external resources which are subject to increasing marginal cost of capital due to the increasing default risk (cf. Fazzari et al., 2000, or Carpenter and Petersen, 2002). Moreover, a highly leveraged company may not be able to raise additional external capital for risky R&D projects which would result in financial constraints and less R&D activities. We take these hypotheses into account by using the cash flow per employee (CF_{1997}/EMP_{1997}) which approximates the internal resources for R&D (see, for example, Harhoff, 1998). In order to take financial constraints due to external resources into account, we include the debt in relation to total assets ($DEBT_{1997}$ = debt / total assets). Both financial measures have been extracted from the Belfirst database.

Finally, a dummy variable indicating whether a firm belongs to a group controls for different governance structures (*GROUP*). Firms that belong to a group may be more likely to receive subsidies because they presumably have better access to information about governmental actions due to their network linkages. In contrast, if firms belong to a group with a foreign parent company (*FOREIGN*), it

[6] Of course, not all innovation efforts lead to patents, and the propensity to patent may be heterogeneous among firms, but as there are no data on previous R&D expenditure available, the patent stock is the best available approximation of past innovation activities.

may be the case that the group tends to file applications in its home country or that, due to the foreign ownership, a Flemish subsidiary does not qualify for local or national technology programs.

4.1 Full sample

This subsection presents the empirics using the full sample, i.e. the potential control group also contains innovators. As Czarnitzki (2002) shows, a subsidy may not only affect the amount of R&D expenditure, but also the R&D status of firms. The smaller a firm is or less collaterals it can offer, the less it will be able to raise external capital for risky projects, which results in cash-constraints and finally in the termination of any innovation activity. A subsidy could alleviate those constraints significantly as the receipt of subsidies may serve as a certification of the firms' activities, and may hence enable it to convince possible financiers. Lerner (1999) pointed that out in the case of subsidies from the US SBIR program. Therefore, we first consider the full sample allowing for non-innovating firms in the potential control group that could be selected as matching partner. This procedure takes into account firms' R&D status. If one would only consider innovating companies, one might underestimate the treatment effects, because the model does not allow firms to drop out of any innovation activity in case they did not receive public R&D funding. However, we consider this scenario as a further robustness check in the following subsection. Table 2 shows the descriptive statistics of the variables in the full sample.

Table 2: Descriptive Statistics (full sample, N = 776)[a)]

	subsidized firms N_1 = 180		potential control group N_0 = 596		p-value of two-sided t-test on mean equality
	Mean	Std. Dev.	Mean	Std. Dev.	
ln*EMP*	4.415	1.439	3.785	1.248	p < 0.0001
PSTOCK/EMP	0.717	2.405	0.127	0.936	p = 0.0015
GROUP	0.683	0.466	0.540	0.499	p = 0.0004
FOREIGN	0.300	0.460	0.257	0.437	p = 0.2637
EXPORT	0.530	0.333	0.354	0.341	p < 0.0001
KAPINT	0.039	0.056	0.052	0.308	p = 0.3571
DEBT	0.660	0.205	0.673	0.398	p = 0.5700
CF/EMP	0.018	0.020	0.015	0.036	p = 0.1488
$\hat{P}(X)$	0.321	0.172	0.204	0.123	p < 0.0001
R&D	1.968	6.510	0.390	2.526	p = 0.0017
R&DINT	4.664	8.427	1.727	5.114	p < 0.0000
D(PATENT>0)	0.072	0.260	0.017	0.129	p = 0.0062
PATENT/EMP	0.092	0.506	0.015	0.139	p = 0.0455

a) 12 industry dummies not reported.

As the t-tests show there are some significant differences among the group of subsidized firms and the potential control group. Subsidized firms are larger, have a higher patent stock, achieve a higher export quota and are more likely to belong to a group. More important, the recipients of public R&D

funding show higher R&D expenditure, on average. There is also a significant difference in patenting activity. However, it is questionable whether these differences can be assigned to the treatment, at least partially, or whether they are determined by the other differing firm characteristics among the two groups.

In order to investigate this, we apply the matching methodology as outlined in the previous section. First, a probit model on the receipt of subsidies is estimated (see Table 3). As already indicated in the descriptive statistics, the most important variables driving the selection are firm size, the patent stock and the export activity. The estimates additionally show that a firm with a foreign parent company is less likely to receive subsidies than other firms. On the one hand, it may be the parent companies which mainly file applications for public R&D funding in their home country. On the other hand, foreignly owned companies may not be eligible for a number of Flemish technology policies. Based on the estimated propensity scores (see the significant difference in average scores in Table 2) and firm size, a nearest neighbor is selected out of the potential control group for each treated firm. The marginal effects on subsidies are calculated at the sample means and their standard errors are obtained by the delta method. As shown in Table 3, the conclusions on statistical inference are the same for the estimated coefficients and their marginal effects.

Table 3: Probit estimation on the receipt of subsidies

	Probit estimates		Marginal effects	
	Coef.	Std. err.	dy/dx	Std. err
ln*EMP*	0.184 ***	0.047	0.053 ***	0.013
PSTOCK/EMP	0.106 ***	0.038	0.031 ***	0.011
GROUP	0.181	0.133	0.052	0.037
FOREIGN	-0.337 **	0.142	-0.091 ***	0.035
EXPORT	0.725 ***	0.171	0.209 ***	0.049
KAPINT	-0.108	0.280	-0.031	0.081
DEBT	-0.117	0.206	-0.034	0.060
CF/EMP	1.156	1.501	0.333	0.433
Constant term	-1.926 ***	0.319		
Test on joint significance on industry dummies	$\chi^2(11) = 16.49$			
Log-Likelihood	-378.1717			
Pseudo R-squared	0.1002			
Number of obs.	776			

*** (**, *) indicate a significance level of 1% (5, 10%). The regression includes 11 industry dummies.

Figure 1: Kernel density estimates of the propensity score and the size distribution

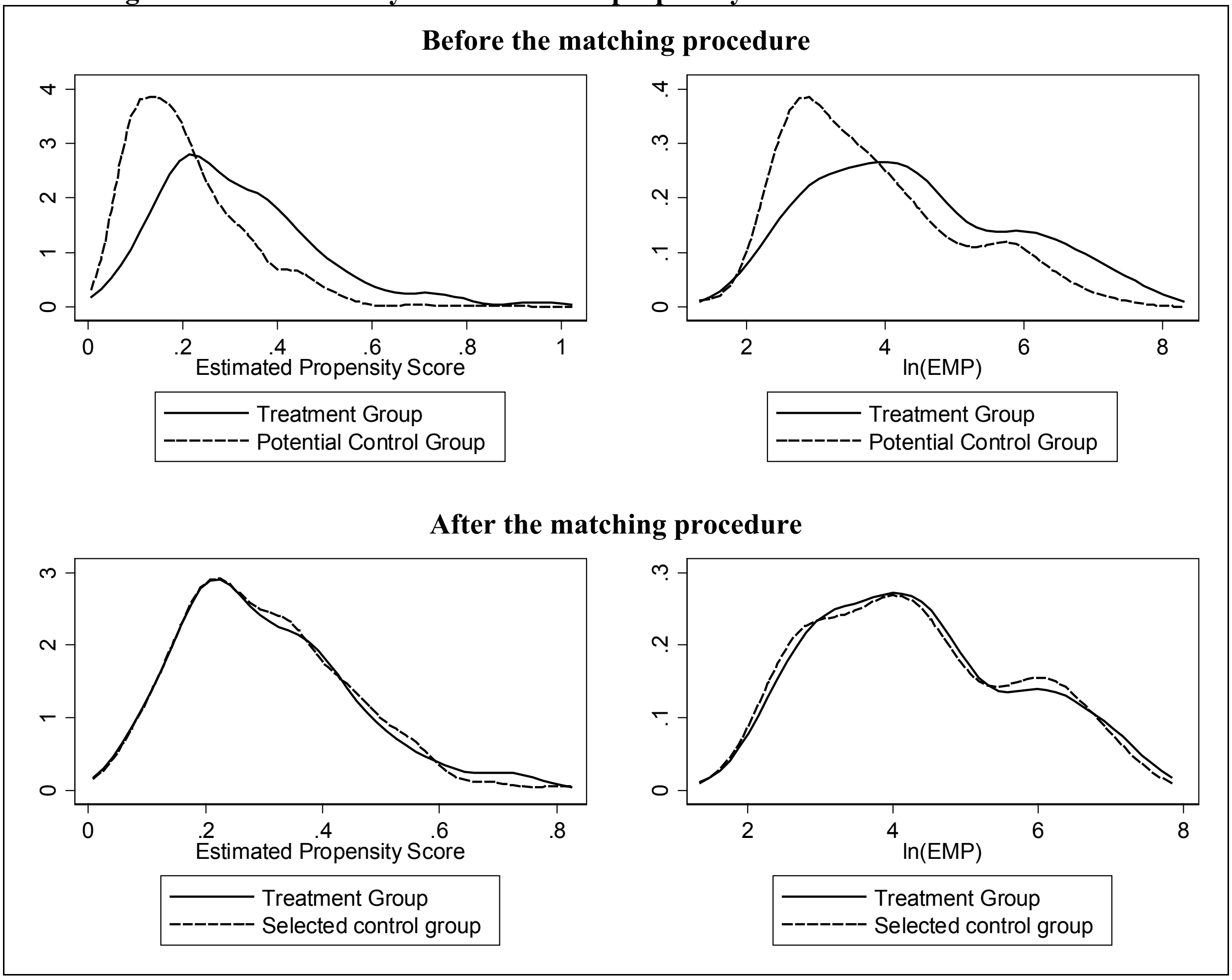

Figure 1 shows Kernel density estimations of the matching arguments, the propensity scores and firm size, before and after the matching. Although the distributions are quite different before the matching, there is a sufficient overlap of the distributions between both groups to apply the matching methodology. The samples are restricted to common support in order to estimate the treatment effect on the treated. As a result, only five observations on subsidized firms are lost which does not noticeably affects the sample. As the graphs on the distributions of the propensity scores and firm size after the matching already demonstrate, both groups are well balanced with respect to the matching arguments after performing the estimation.

Table 4 presents the results of the matching by the propensity score and firm size in more detail. As the means and corresponding t-tests show, both samples are well balanced according to all employed covariates after the matching. There are no statistically significant differences in the exogenous variables, and especially in the propensity score at the 5% level. However, there are still mean differences in both *R&D* and *R&DINT,* and these can be assigned to the treatment. Hence, a crowding-out effect can be rejected with regard to the public R&D funding. On average, the subsidized firms would have significantly invested less in R&D if they had not been subsidized. For instance, with respect to the R&D intensity, the average treatment effect on the treated amounts to

$\hat{\alpha}_{TT} = 4.621 - 1.746 = 2.875$ and is statistically significant below the 5% level. Table 8 in the appendix displays OLS regressions of the different R&D variables on the covariates and the subsidy dummy. It turns out that the OLS estimates are quite similar to the matching in our case. This points to the conclusion that the selection bias is not too serious in our application for Flanders.

Table 4: Matching results (based on full sample, *N* = 776)[a)]

	subsidized firms N_1 = 175		potential control group N_0 = 175		p-value of two-sided t-test on mean equality[b)]
	Mean	Std. Dev.	Mean	Std. Dev.	
ln*EMP*	4.355	1.386	4.309	1.381	p = 0.783
PSTOCK/EMP	0.512	1.664	0.471	1.761	p = 0.844
GROUP	0.680	0.468	0.623	0.486	p = 0.331
FOREIGN	0.291	0.456	0.297	0.458	p = 0919
EXPORT	0.522	0.332	0.528	0.365	p = 0.899
KAPINT	0.040	0.057	0.034	0.059	p = 0.466
DEBT	0.664	0.204	0.615	0.224	p = 0.066
CF/EMP	0.017	0.020	0.025	0.085	p = 0.385
$\hat{P}(X)$	0.304	0.148	0.299	0.138	p = 0.763
R&D	1.700	6.267	0.589	1.520	p = 0.025
ln*R&D*	-2.113	3.096	-4.552	4.196	p < 0.001
R&DINT	4.621	8.473	1.746	3.627	p < 0.001
ln*R&DINT*	-0.108	2.454	-2.104	3.020	p < 0.001
D(PATENT>0*)*	0.109	0.312	0.091	0.289	p = 0.639
PATENT/EMP	0.224	0.942	0.179	0.712	p = 0.647

a) 12 industry dummies not reported.
b) t-statistics are based on Lechner's (2001) asymptotic approximation of the standard errors that accounts for sampling with replacement in the selected control group.

However, as Table 4 also shows there are no significant differences in patenting behavior after the matching. The main reason for this result is possibly the small sample of firms filing EPO patents from 1998 to 2000. The matched samples contain only 35 EPO-patenting firms in total. For an appropriate analysis of the output side of the innovation process it would perhaps be more useful to investigate sales with new products or cost reductions due to process innovations. In this case, however, a longitudinal database would be desirable, as the market result or cost reduction is expected to occur with a considerable time-lag after completion of the corresponding R&D projects. As we currently use only the cross-section of the CIS3 database, a longitudinal analysis is obviously beyond the scope of this paper.

4.2 Subsample of innovating firms

In the full sample, crowding-out effects for R&D can be rejected. As a further test, we consider only the subsample of innovating firms in this subsection. In our opinion, this underestimates the treatment effects, because some firms, especially smaller firms, may be completely deterred from doing R&D if they do not receive a subsidy. On the one hand, the risk of failure, and thus the expected cost, might be too high for small firms if there is no public support. On the other hand, small firms might just not be able to raise external capital for conducting R&D in the absence of public R&D funding. However, some readers may argue that the previous results are simply due to several non-innovators in the selected control group. For this reason, we now focus on innovating firms.

Table 5: Descriptive Statistics (subsample of innovating firms, *N* = 570)[a)]

	subsidized firms $N_1 = 180$		potential control group $N_0 = 390$		p-value of two-sided t-test on mean equality
	Mean	Std. Dev.	Mean	Std. Dev.	
ln*EMP*	4.415	1.439	4.038	1.321	p = 0.0031
PSTOCK/EMP	0.717	2.405	0.159	1.033	p = 0.0032
GROUP	0.683	0.466	0.608	0.489	p = 0.0772
FOREIGN	0.300	0.460	0.308	0.462	p = 0.8530
EXPORT	0.530	0.333	0.400	0.349	p < 0.0001
KAPINT	0.039	0.056	0.046	0.194	p = 0.5581
DEBT	0.660	0.205	0.679	0.459	p = 0.5022
CF/EMP	0.018	0.020	0.016	0.042	p = 0.5513
$\hat{P}(X)$	0.384	0.179	0.284	0.121	p < 0.0001
R&D	1.968	6.510	0.596	3.104	p = 0.0077
R&DINT	4.664	8.427	2.638	6.132	p = 0.0041
D(PATENT>0*)*	0.072	0.260	0.023	0.150	p = 0.0189
PATENT/EMP	0.092	0.506	0.022	0.171	p = 0.0727

a) 12 industry dummies not reported.

Table 5 displays the descriptive statistics of both groups. Like in the foregoing analysis using the full sample, we find similar differences in both exogenous and endogenous variables. Even the subsequent probit estimation is very similar to the full sample. The size, the patent stock, international competition and the governance structure determine the differences between publicly funded and non-funded Flemish companies (see Table 6).

Again, the matched samples are well balanced with respect to the propensity score and the covariates after the matching procedure. In contrast to the full sample results, the estimated treatment effects are – as expected – somewhat smaller in this case. In the case of R&D activities, we can still reject crowding-out effects in most cases. Except in levels of *R&D* where the t-test is not significant, the mean equality can be rejected for the logarithmic R&D variables and the R&D intensity. Similar to the case using the full sample, the average R&D intensity amounts to 4,7% in the sample of subsidized firms. In the absence of subsidies these firms would have exhibited a significantly smaller R&D intensity: 2,2% on average. For comparison, we also include OLS estimates in the appendix (see Table 9). It turns out that they are again quite similar to the matching results which points to the conclusion that the selection bias is not too serious in our sample.

Table 6: Probit estimation on the receipt of subsidies (innovating firms only)

	Probit estimates		Marginal effects at sample means	
	Coef.	Std. err.	dy/dx	Std. err
ln*EMP*	0.108 ***	0.051	0.038 ***	0.018
PSTOCK/EMP	0.105 ***	0.040	0.037 ***	0.014
GROUP	0.135	0.146	0.047	0.050
FOREIGN	-0.346 **	0.150	-0.116 **	0.048
EXPORT	0.592 ***	0.184	0.208 ***	0.064
KAPINT	-0.072	0.433	-0.025	0.152
DEBT	-0.079	0.188	-0.028	0.066
CF/EMP	0.588	1.574	0.206	0.552
Constant term	-1.230 ***	0.345		
Test on joint significance on industry dummies	$\chi^2(11) = 13.46$			
Log-Likelihood	-331.027			
Pseudo *R*-squared	0.0688			
Number of obs.	570			

*** (**, *) indicate a significance level of 1% (5, 10%). The regression includes 11 industry dummies.

Table 7: Matching results (based on subsample of innovating firms, *N* = 570)[a)]

	subsidized firms $N_1 = 173$		potential control group $N_0 = 173$		p-value of two-sided t-test on mean equality [b)]
	Mean	Std. Dev.	Mean	Std. Dev.	
ln*EMP*	4.342	1.381	4.398	1.380	p = 0.745
PSTOCK/EMP	0.503	1.665	0.464	2.297	p = 0.879
GROUP	0.676	0.469	0.618	0.487	p = 0.331
FOREIGN	0.289	0.455	0.277	0.449	p = 0.836
EXPORT	0.528	0.329	0.550	0.359	p = 0.618
KAPINT	0.035	0.039	0.028	0.027	p = 0.074
DEBT	0.664	0.204	0.669	0.229	p = 0.852
CF/EMP	0.017	0.018	0.016	0.014	p = 0.619
$\hat{P}(X)$	0.364	0.136	0.365	0.144	p = 0.957
R&D	1.718	6.301	1.075	4.591	p = 0.327
ln*R&D*	-2.075	3.062	-3.430	3.862	p = 0.050
R&DINT	4.673	8.508	2.204	3.728	p = 0.001
ln*R&DINT*	-0.071	2.432	-1.183	2.779	p = 0.001
D(PATENT>0*)*	0.110	0.314	0.098	0.299	p = 0.759
PATENT/EMP	0.227	0.947	0.191	0.841	p = 0.742

a) 12 industry dummies not reported.
b) t-statistics are based on Lechner's (2001) asymptotic approximation of the standard errors that accounts for sampling with replacement in the selected control group.

5 Conclusion

In this paper, we investigate potential crowding-out effects of public R&D policies in Flanders empirically. Using the Flemish sample of the third Community Innovation Survey from 2001, we conduct a treatment effects analysis to address the question whether public R&D funding stimulates private innovation activities or whether firms just substitute public means for private investment. The treatment effects analysis amounts to an estimation of the counterfactual situation: "How much innovative activity had the recipients of public R&D funding shown if they were not subsidized?" We use a non-parametric matching estimator to estimate the counterfactual situation.

It turns out that firms which received public funding for innovation projects would have invested significantly less in R&D if they would not have received the subsidies. This holds true for both the full sample and a subsample of innovating firms only. First, we use the full sample that also includes non-innovative firms in the control group, because there may be subsidized firms that would not have conducted any R&D if they had not been subsidized. The reasons can range from too costly or too risky research projects to the inability to raise sufficient external capital. If the potential control group is restricted to innovating firms only, one thus might underestimate the treatment effect, because the innovation status of a firm can change from non-innovative to innovative due to the receipt of a subsidy and vice versa. In addition to the full sample, however, we also test the hypothesis of crowding out effects in the subsample of innovating firms only, because some readers might argue that the first result is driven by the non-innovative firms only. However, the results confirm the previous estimations. The hypothesis of crowding-out is also rejected in the subsample of innovating firms.

Further research in this field calls for an analysis that accounts for the time-series dimension of the innovation and subsidy process more in detail than has been possible in this study. It would be useful to check the robustness of our results, controling for previous R&D activities, e.g. in a difference-in-difference setting. Our analysis uses only a binary choice variable on the receipt of a subsidy. It would be desirable to take the amount of subsidies into account in further studies. Moreover, R&D spending only considers the input side of the innovation process. Although crowding-out effects can be rejected, we cannot assume that this increased R&D spending results in new products and processes and thus in increased welfare in society. On the one hand, it may be possible that mainly wages of R&D personnel increased, but that their productivity remained constant. On the other hand, the subsidized projects might be associated with higher failure risk than other innovation projects. Hence, the increased R&D spending could possibly not lead to more innovation in the economy, if many subsidized projects fail. Again, such desirable studies on innovation outcome require long time-series data to allow for a reasonable lag structure between the date of market introduction of new products or the implementation of new processes and the time period when the corresponding R&D projects were actually performed.

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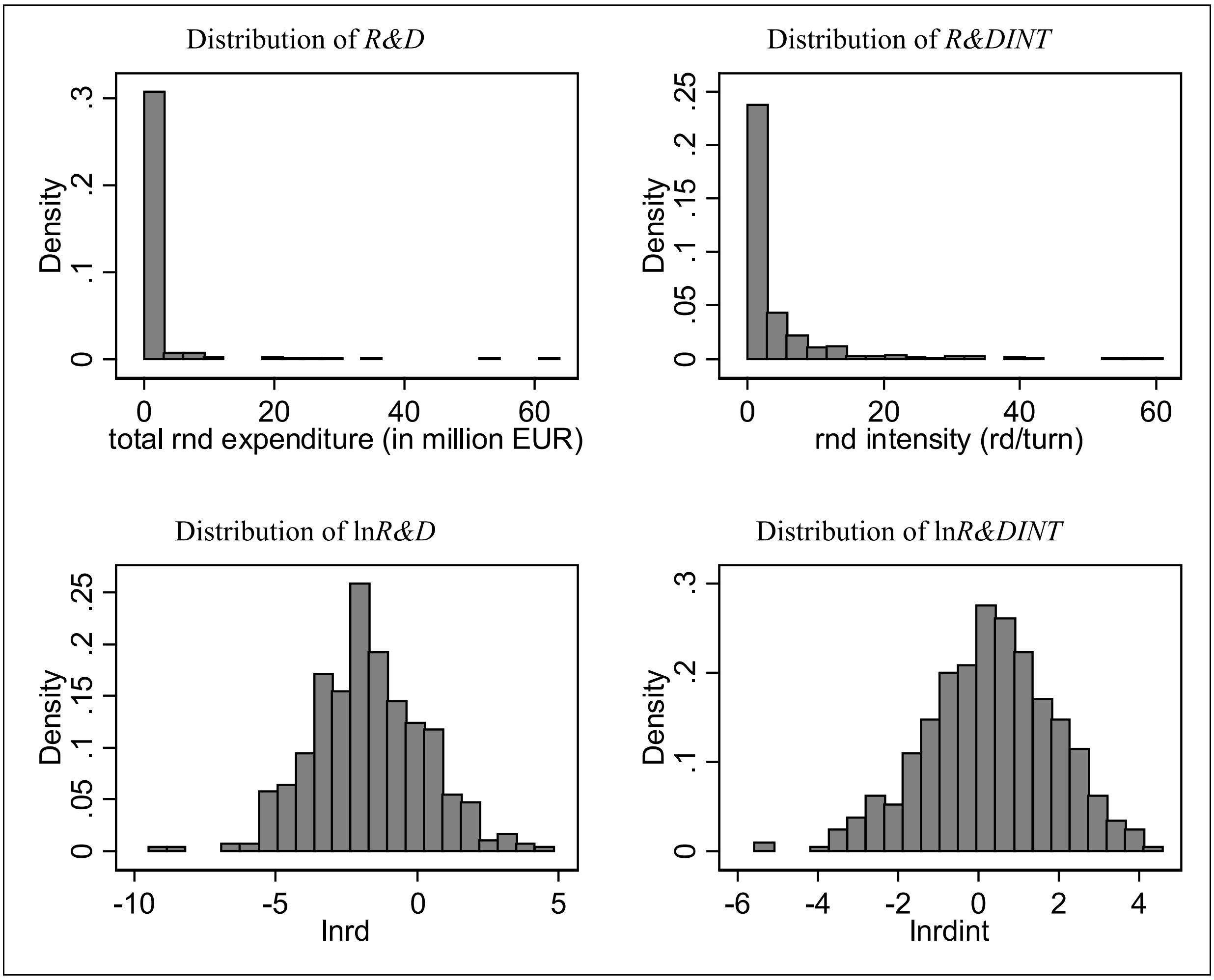
Appendix

Figure 2: Distributions of different R&D variables



Note: R&D performing firms only

Table 8: OLS estimates on different R&D variables (full sample)[a]

	Dependent variable:			
	R&D	*R&DINT*	ln*R&D*	ln*R&DINT*
	Coef.	Coef.	Coef.	Coef.
	Std. err.	*Std. err.*	*Std. err.*	*Std. err.*
ln*EMP*	1.05 **	-0.20	1.18 ***	0.38 ***
	0.26	*0.16*	*0.12*	*0.09*
PSTOCK/EMP	0.37 **	0.50 **	0.25 ***	0.19 ***
	0.15	*0.21*	*0.06*	*0.05*
GROUP	-0.52 ***	0.17	0.45	0.34
	0.25	*0.51*	*0.30*	*0.26*
FOREIGN	0.45	-1.49 ***	-0.45	-0.49 *
	0.41	*0.50*	*0.34*	*0.27*
EXPORT	-0.53	2.13 ***	1.74 ***	1.24 ***
	0.73	*0.78*	*0.43*	*0.34*
CF/EMP	8.79	3.60	3.66	2.16
	6.60	*3.38*	*2.30*	*2.72*
DEBT	0.05	-0.38	0.29	0.23
	0.19	*0.48*	*0.21*	*0.19*
KAPINT	0.40 *	0.62	0.25	0.14
	0.22	*0.77*	*0.58*	*0.46*
SUBSIDY DUMMY	0.76	2.41 ***	2.50 ***	2.01 ***
	0.49	*0.54*	*0.27*	*0.23*
Constant term	-4.32	0.45	-11.79	-5.65
	1.17	*0.81*	*0.65*	*0.52*
Test on joint significance on industry dummies [F(11,755)]	1.87 **	7.07 ***	4.32 ***	5.59 ***
Number of obs.	776	776	776	776
R-squared	0.18	0.19	0.38	0.24

*** (**, *) indicate a significance level of 1% (5, 10%). The regression includes 11 industry dummies.

a) We also run Tobit regressions with this sample as the distribution of R&D is left censored in this case. However, the estimations lead basically to the same interpretion and therefore we ommit a detailed presentation.

Table 9: OLS estimates on different R&D variables (innovating firms only)

	Dependent variable:			
	R&D	*R&DINT*	ln*R&D*	ln*R&DINT*
	Coef.	Coef.	Coef.	Coef.
	Std. err.	*Std. err.*	*Std. err.*	*Std. err.*
ln*EMP*	1.30 ***	-0.33	1.01 ***	0.13
	0.32	0.20	0.12	0.10
PSTOCK/EMP	0.38 **	0.51 **	0.26 ***	0.19 ***
	0.16	0.21	0.04	0.04
GROUP	-0.63 **	-0.25	0.37	0.16
	0.30	0.71	0.33	0.29
FOREIGN	0.34	-1.56 ***	-0.65 *	-0.56 *
	0.45	0.58	0.33	0.27
EXPORT	-0.69	2.27 **	1.44 ***	0.89 **
	0.96	0.99	0.46	0.37
CF/EMP	9.79	2.58	1.73	0.33
	7.36	3.60	2.09	2.86
DEBT	0.09	-0.55	0.26	0.19
	0.22	0.54	0.20	0.17
KAPINT	1.12 *	1.83	1.21 *	0.80
	0.59	1.78	0.65	0.56
SUBSIDY DUMMY	0.75	1.74 ***	1.11 ***	0.87 ***
	0.53	0.57	0.27	0.23
Constant term	-5.75	1.82	-8.92	-2.86
	1.56	1.12	0.72	0.60
Test on joint significance on industry dummies [F(11,549)]	1.93 **	7.20 ***	3.42 ***	4.57 ***
Number of obs.	570	570	570	570
R-squared	0.20	0.21	0.30	0.15

*** (**, *) indicate a significance level of 1% (5, 10%). The regression includes 11 industry dummies.