Discussion Paper No. 06-063

Two for the Price of One?

On Additionality Effects of R&D Subsidies: A Comparison Between Flanders and Germany

Kris Aerts and Tobias Schmidt

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Non-technical summary

Schumpeter was the first to acknowledge the importance of continuous innovation in an economy. Especially against the background of the knowledge economy, innovation nowadays is deemed to be the main driving force of a country's competitive strength. Statistics show that there still is a big gap between the EU25 on the one hand and Japan and the US on the other hand when the R&D expenditure relative to the GDP is considered. As a result the European Commission very recently launched an integrated innovation/research action plan, which calls for a major upgrade of the research and innovation conditions in Europe. Mobilizing EU funds and instruments to support research and innovation is one of the objectives.

Government intervention in the domain of private R&D activities is justified by the argument of market imperfection and has since long been common practice in most industrialized countries. R&D entails the non-excludability characteristic of a public good. Private investments in R&D can never be fully appropriated because other companies have the opportunity to free ride. This leads to underinvestment in R&D activities: the level of R&D expenditure will be below the socially desirable optimum. Public funding reduces the price of socially valuable R&D projects for private investors to a level at which it becomes profitable for companies to invest.

The big challenge for governments obviously is to allocate public funding only to those projects that are socially beneficial and would not be carried out in the absence of a subsidy. This is however not straightforward as companies always have an incentive to apply for public funding. It might be the case that a subsidy merely replaces –or crowds out– private money and does not engender additional R&D investments.

This paper provides empirical evidence on the relationship between public R&D funding and private R&D efforts in Flanders and Germany. Evaluation studies are inconclusive as some report crowding-out effects while others reject them. This is partly due to the fact that researchers use very different databases and econometric methods resulting from differences in information availability in different countries. Therefore it is useful to compare the impact of funding in different countries using similar methods and datasets.

A comparison between Germany and Flanders seems to be a reasonable choice. First of all, Germany is a large economy, while Flanders is a small economy, which may induce different impacts of R&D funding. Flanders is the largest region in Belgium at the NUTS II level. The Belgian science and technology (S&T) policy is highly regionalized and therefore the impact of R&D funding, which falls entirely under the responsibility of the Flemish government, can be evaluated at the regional, Flemish level. The German S&T policy is conducted at the national level. Second, the Flemish and German funding systems for R&D do not differ substantially. In Germany public R&D funding relies largely on direct R&D funding; fiscal measures, like R&D tax credits, do not exist. In Flanders, accelerated depreciation for R&D capital assets and R&D tax allowances are available through the federal Belgian government. However, very few Belgian companies actually make use of these fiscal measures and direct R&D grants through the Flemish government remain the largest source of public R&D funding in the private sector in Flanders.

The key issue in evaluation econometrics is to correct for a potential selection bias. In this paper we first employ the hybrid nearest neighbor matching estimator: for every funded firm we look for a similar firm that did not receive funding. In this way a valid control group is constructed and the additionality effect can be computed from the mean averages of the outcome variables in the funded and non-funded group. Matching offers the advantage that no assumptions have to be made, neither on the functional form of the outcome equation nor on the distribution of the error terms of the selection and outcome equation. The disadvantage is that it only allows controlling for observed heterogeneity among treated and untreated firms. To counter this problem and control for unobserved heterogeneity, in the second step we employ the conditional difference-in-difference method for repeated cross-sections (CDiDRCS), which combines ordinary difference-in-difference estimation with matching, adding a time series framework to our first results. We use two waves from the Flemish and German Community Innovation Survey (CIS): CIS3 (1998-2000) and CIS4 (2002-2004).

The matching estimator indicates that for both samples the crowding-out hypothesis can be rejected: on average, the R&D intensity of German (Flemish) funded companies is 76% to 100% (64% to 91%) higher than the R&D intensity of non-funded companies. Based on the results of the CDiDRCS method the crowding-out hypothesis can clearly be rejected, too; funded firms are significantly more R&D active than non-funded firms. These results are in line with findings from earlier studies on additionality in Flanders and Germany and also other European countries.

TWO FOR THE PRICE OF ONE?

On additionality effects of R&D subsidies: A comparison between Flanders and Germany

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Abstract

In this paper we empirically test whether public R&D subsidies crowd out private R&D investment in Flanders and Germany, using firm level data from the Flemish and German part of the Community Innovation survey (CIS III and IV). Both the non-parametric matching estimator and the conditional difference-in-difference estimator with repeated cross-sections (CDiDRCS) clearly indicate that the crowding-out hypothesis can be rejected: funded firms are significantly more R&D active than non-funded firms. In the domain of additionality effects of R&D subsidies, this paper is the first to apply the CDiDRCS method.

Keywords: R&D, Subsidies, Policy Evaluation, Conditional Difference-in-Difference
JEL-Classification: C14, C21, H50, O38

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1 Introduction

Schumpeter (1942) was the first to acknowledge the importance of continuous innovation in an economy. Especially against the background of the knowledge economy, innovation nowadays is deemed to be the main driving force of a country's competitive strength (see e.g. Griliches, 1986). The European Union aspires to become the most competitive economy in the world and proclaimed innovation as one of the key pillars in its policy to achieve this (Commission of the European Communities, 2000). In the 2000 Lisbon Strategy the ambitious plan was initiated to leverage the EU R&D expenditure to 3% of the GDP by 2010; of which 2% should be privately financed. However, an intermediate evaluation in 2005 revealed that instead of rising, the EU R&D expenditure is currently more or less stagnant. Recent statistics show that the EU25 spent 1.92% of its GDP on R&D activities in 2003. In the US the R&D expenditure amounted to 2.59% of the GDP and in Japan this number rose to 3.15% (Eurostat, 2005). Therefore, the European Commission very recently launched an integrated innovation/research action plan, which calls for a major upgrade of the research and innovation conditions in Europe. Mobilizing EU funds and instruments to support research and innovation is one of the objectives.

Government intervention in the domain of private R&D activities is justified by the argument of market imperfection and is since long time common practice in most industrialized countries. R&D entails the non-excludability characteristic of a public good (see e.g. Samuelson, 1954). Arrow (1962, p.615) states that

> *"No amount of legal protection can make a thoroughly appropriable commodity of something as intangible as information. The very use of the information in any productive way is bound to reveal it, at least in part. Mobility of personnel among firms provides a way of spreading information. Legally imposed property rights can provide only a partial barrier, since there are obviously enormous difficulties in defining in any sharp way an item of information and differentiating it from similar sounding items."*

Private investments in R&D can never be fully appropriated because other companies have the opportunity to free ride. This leads to underinvestment in R&D activities: the level of R&D expenditure will be below the socially desirable optimum. Public funding reduces the price of socially valuable R&D projects for private investors to a level at which it becomes profitable for companies to invest.

The big challenge for governments obviously is to allocate public funding only to those projects that are socially beneficial and would not be carried out in the absence of a subsidy. This is however not straightforward as companies always have an incentive to apply for public funding. It could be the case that a subsidy merely replaces –or crowds out– private money and does not engender additional R&D investments. The key question in this evaluation problem is: "How much would a firm that has received a subsidy, have spent on R&D if it would not have been subsidized?". Several methods are developed to tackle this question. Examples are the so called "matching estimator" and the conditional difference-in-difference method. These methods will be described in more detail below.

This paper provides empirical evidence on the relationship between public R&D funding and private R&D efforts in Flanders and Germany. In a survey of the literature on additionality effects of R&D subsidies, David et al. (2000) conclude that the results of evaluation studies in this field are inconclusive as some report crowding-out effects while others reject them. They attribute this to the fact that studies use very different databases and econometric methods resulting from differences in information availability in different countries. Therefore it is useful to compare the impact of funding in different countries using similar methods and datasets.

A comparison between Germany and Flanders seems to be a reasonable choice. First of all, Germany is a large economy, while Flanders is a small economy, which may induce different impacts of R&D funding. Flanders is the largest region in Belgium at the NUTS II level. The Belgian science and technology (S&T) policy is highly regionalized and therefore the impact of R&D funding, which falls entirely under the responsibility of the Flemish government, can be evaluated at the regional, Flemish level. The German S&T policy is conducted at the national level. Second, the Flemish and German funding systems for R&D do not differ substantially. In Germany public R&D funding relies largely on direct funding of R&D projects of firms and on institutional funding of more basic research. The main federal government agencies providing public funding are the Federal Ministry of Education and Research (BMBF) and the Federal Ministry of Economics and Labor (BMWA). German firms and research institutions also qualify for European funding programs, of course. Fiscal measures, like R&D tax credits, do not exist in Germany. In Flanders, accelerated depreciation for R&D capital assets and R&D tax allowances are available through the federal Belgian government. In contrast to most countries, the Belgian R&D tax allowances are fixed and not granted as a percentage: for each additional employee employed in scientific research, the company is granted a tax exemption for a fixed amount, in the year of recruitment. However, as Van Pottelsberghe et al. (2003) indicate, very few Belgian companies actually make use of these fiscal measures. Main reasons are a low level of acquaintance with the system, high administration costs and the fact that the measures are not significantly substantial: e.g. the tax exemption is a short term measure while R&D is typically a long term process. Direct R&D funding through the Flemish government (IWT, the Institute for the Promotion of Innovation by Science and Technology in Flanders) remains the largest source of public R&D grants in the private sector in Flanders.

In the second part we expound the evaluation problem that arises from the selection bias in the measurement of additionality effects. Next, the reader is guided through the relevant literature. The methodology that is employed in this paper to tackle the selection bias is explained in the subsequent section. The fifth section provides a data description. In the sixth section the empirical evidence is presented. The last section contains some concluding remarks.

2 Selection bias

In this paper we empirically evaluate public R&D funding. The impact of the subsidy can be computed as follows[1]:

$$\alpha_{TT} = E\left(Y^T \middle| S=1\right) - E\left(Y^C \middle| S=1\right), \tag{1}$$

where Y^T refers to the potential outcome (e.g. R&D expenditure) of subsidized companies and Y^C to the outcome of these companies in the case they would not have received the subsidy. *S* indicates the so-called treatment status. It is equal to 1 for treated (subsidized) firms and zero otherwise. So α_{TT}, the average treatment effect on the treated, results from comparing the actual outcome of subsidized firms with their potential outcome in case of not receiving a grant. The approach of measuring potential outcomes goes back to Roy (1951). The actual outcome $E\left(Y^T \middle| S=1\right)$ can be estimated by the sample mean of *Y* in the group of subsidized firms.

The counterfactual situation $E\left(Y^C \middle| S=1\right)$ can however never be observed and has to be estimated. In a hastily analysis a researcher could compare the average R&D spending of subsidized and non-subsidized companies to compute the treatment effect on the treated, assuming that:

$$E\left(Y^C \middle| S=1\right) = E\left(Y^C \middle| S=0\right). \tag{2}$$

However, it is not unlikely that subsidized companies would have been more R&D active than the non-subsidized companies even without the subsidy program, which would imply a selection bias in the estimation of the treatment effect. Firms that already are innovative and very R&D active may be more likely to receive an R&D subsidy, as governments want to maximize the probability of success and therefore may well cherry-pick proposals of companies with considerable R&D expertise. Moreover, it is also quite possible that only particular companies apply for public R&D grants because they have an information advantage and are acquainted with policy measures they qualify for. Expression (2) only holds in an experimental setting where there would be no selection bias and subsidies are granted randomly to firms. This is most likely not to be the case in current innovation policy practice.

[1] All variables are measured at the firm level *i* (with *i* = 1,...,*N*), but we omit the index *i* for convenience.

As the highest expected success is correlated with current R&D spending, the subsidy receipt (treatment) becomes an endogenous variable. To estimate treatment effects while taking this potential endogeneity problem into account, econometric literature has developed a range of methods (see e.g. the surveys of Heckman et al., 1999; Blundell and Costa-Dias, 2000, 2002). Examples of these methods are selection models, instrumental variable (IV) estimations (including simultaneous equation systems), difference-in-difference estimations and matching. For the application of IV estimators and selection models, valid instruments for the treatment variables are needed. In the case of R&D additionality analysis it is very difficult to find valid instruments. The difference-in-difference method requires panel data with observations before and after (or while) the treatment. The matching estimator offers the advantage over IV and selection models that no assumptions have to be made, neither on the functional form of the outcome equation nor on the distribution of the error terms of the selection and outcome equation. The disadvantage is that it only allows controlling for observed heterogeneity among treated and untreated firms. To counter this problem and control for unobserved heterogeneity, the conditional difference-in-difference method was developed, which combines the ordinary difference-in-difference estimation with matching. In section 4 we will expound the matching estimator, the difference-in-difference estimator and the combination of these two[2].

3 Literature Review

This section presents an overview of the literature on additionality effects of R&D subsidies. This paper is situated in the domain of input additionality and addresses the issue of crowding-out effects of subsidized R&D.

David et al. (2000) conclude in their review of evaluation studies on innovation input that the results on potential crowding-out effects are ambiguous, and they criticize that most existing studies neglect the problem of sample selection bias. Consequently, in more recent research the potential sample selection bias is taken into account through selection models, instrumental variable (IV) estimations (including simultaneous equation systems), difference-in-difference estimations and matching techniques.

Busom (2000) applies an econometric selection model on a cross-sectional sample of Spanish manufacturing firms. Based on Heckman's selection model, 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 receive public funding. 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

[2] See Aerts et al. (2006) for an explanation of other techniques used in evaluation econometrics.

(2000) 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. She finds proof that supports the presence of a selection bias: small firms are more likely to obtain a subsidy than large firms, which may be due to the public agency's goals. 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 might have had to reduce them. Examining this hypothesis would require a longitudinal study, though. Suetens (2002) applies an IV framework on a panel of Flemish firms, but the results are by and large not significant. Full crowding-out cannot be rejected in this case. In contrast, Hussinger (2003) explores parametric and semi-parametric two-step selection models using German data. She confirms the positive results previously identified with German data using different econometric methods. Kaiser (2004) employs a simultaneous probit model for Denmark and does not find significant proof to reject the crowding out hypothesis. González et al. (2006) investigate subsidies in a panel of Spanish manufacturing firms and employ a simultaneous equation model with thresholds. They state that subsidies are effective in inducing firms to invest into R&D, but they induce only slight changes in the level of private R&D expenditure. They conclude that in the absence of subsidies, publicly supported R&D projects would be carried out, but in smaller size. However, they do not report crowding-out effects or inefficient use of subsidies.

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 difference-in-difference 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.

Almus and Czarnitzki (2003), Czarnitzki (2001), Czarnitzki and Fier (2002), and Fier (2002) employ nearest neighbor matching approaches to investigate the impact of public subsidies in Germany. Czarnitzki and Fier (2002) analyze the German Service Sector and Fier (2002) the manufacturing sector. Czarnitzki (2001) and Almus and Czarnitzki (2003) use data on Eastern German manufacturing firms. All studies reject full-crowding out effects. Heijs and Herrera (2004) apply nearest neighbor matching using data on Spanish manufacturing firms. They conclude that the average treatment effect is about 60% for the whole set of firms and even over 100% for small firms. If, however, they take the amount of subsidies into account, they conclude that subsidies induced a scarce additionality effect, which is inferior to the amount granted. Aerts and Czarnitzki (2004) address the additionality issue with the nearest neighbor matching technique on a cross-section of Flemish manufacturing and selected services companies, and find evidence that crowding-out can be rejected. Duguet (2004) employs the matching methodology with a large panel of French firms. Controlling for past public support the firms benefited from, he also rejects the crowding out hypothesis for France. Kaiser (2004) cannot reject crowding out effects for Denmark using the Kernel matching procedure, which confirms his results from the simultaneous probit model. Lööf and Heshmati (2005) evaluate the Swedish subsidy policy with nearest neighbor and Kernel matching and reject crowding out effects. González and Pazó (2006) apply a nearest neighbor matching approach to analyze the effects of public R&D support in Spanish manufacturing firms. Their analysis rejects full crowding-out effects but does not confirm that public R&D subsidies stimulate private R&D expenditure. They also conclude that some firms - mainly small and operating in low technology sectors - might not have engaged in R&D activities in the absence of subsidies.

Görg and Strobl (2005) analyse additionality effects in Ireland. They use the CDiD approach with a rich panel data set of manufacturing plants and distinguish according to ownership. They reject crowding out of small/medium grants and find additionality effects of small grants. However, they cannot reject crowding out for foreign plants.

Although recent studies correcting for a potential selection bias tend to reject full crowding out effects, the results are not unambiguous: Aerts and Czarnitzki (2004), Almus and Czarnitzki (2003), Busom (2000), Czarnitzki (2001), Czarnitzki and Fier (2002), Duguet (2004), Fier (2002), González and Pázo (2006), González et al. (2006), Görg and Strobl (2005), Hussinger (2003) and Lööf and Heshmati (2005) reject full crowding-out effects, while Wallsten (2000) finds that public subsidies crowd-out private investment dollar for dollar in the US SBIR program. Lach (2002) finds large additionality effects in small Israeli manufacturing firms, but none for large firms. The results of Suetens (2002) are inconclusive and she cannot reject crowding-out effects. Kaiser (2004) cannot reject crowding out in Denmark. Although Heijs and Herrera (2004) find positive treatment effects, the overall additionality effect is small when the amount is taken into account.

4 Methodology

As the literature overview shows, a range of econometric methods is available to correct for the selection bias. In this section we expound the methods that are employed in this paper, i.e. the matching estimator and the ordinary and conditional difference-in-difference method.

4.1 Matching estimator

The matching estimator is a non-parametric method and its main advantage is that no particular functional form of equations has to be specified. The disadvantages are strong assumptions and heavy data requirements. The main purpose of the matching estimator is to re-establish the conditions of an experiment. The matching estimator attempts to construct a correct sample counterpart for the treated firms' outcomes if they had not been treated by pairing each treated firm with members of a comparison group. Under the matching assumption, the only remaining difference between the two groups is the actual subsidy receipt. The difference in outcome variables can then be attributed to the subsidy.

Rubin (1977) proved that the receipt of subsidies and potential outcome are independent for firms with the same set of exogenous characteristics

$$Y^T, Y^C \perp S \big| X = x. \tag{3}$$

This conditional independence assumption (CIA) helps to overcome the problem that the counterfactual outcome $E(Y^C \big| S = 1)$ is unobservable. If the CIA holds, the expected outcome $E(Y^C \big| S = 0, X = x)$ can be used as a measure of the potential outcome of the subsidy recipients. However, the CIA is only fulfilled if all variables that influence the outcome and selection status *S* are known and available in the dataset. In that case the equation

$$E\left(Y^C \mid S = 1, X = x\right) = E\left(Y^C \mid S = 0, X = x\right) \tag{4}$$

is valid and the average outcome of subsidized firms in the absence of a subsidy can be calculated from a sample of comparable -matched- firms. In the matching process for all treated firms a valid counterpart should be found in the non-treated population and every firm should represent a possible subsidy recipient. Therefore, we impose a so-called common support restriction. If the samples of treated and non-treated firms would have no or only little overlap in the exogenous characteristics *X*, matching is not applicable to obtain consistent estimates. If the CIA holds and common support is given, the average treatment effect on the treated would consequently amount to

$$\alpha_{TT}^M = E(Y^T \big| S = 1, X = x) - E(Y^C \big| S = 0, X = x) \tag{5}$$

which can be estimated using the sample means of both groups.

In the ideal case, the matching procedure includes as many matching arguments X as possible to find a perfect twin in the control group of non-treated firms for each treated firm. However, the more dimensions that are included, the more difficult it becomes to find a good match: the so-called curse of dimensionality enters. Rosenbaum and Rubin (1983) showed that it is valid to reduce the number of matching dimensions X to a single index: the propensity score Pr(X), which is the probability to receive a subsidy. Lechner (1998) suggested a hybrid matching, where the propensity score Pr(*X*) and a subset of *X* condition the matching procedure.

Having defined the neighborhood of similar non-treated firms for each treated firm, the next issue is the choice of appropriate weights w_{ij} for non-treated observations within the neighborhood, so that $\alpha_{i,TT}$ can be computed as

$$\alpha_{i,TT}^{M} = Y_i^T - \sum_{j=1}^{N} w_{ij} Y_j^C \ . \tag{6}$$

Two commonly used procedures are Kernel-based matching and nearest neighbor. In the Kernel-based matching, a treated firm is matched to all non-treated firms in the control group, but the controles are weighted according to the Mahalanobis distance between the treated firm and each non-treated firm. In the empirical part of this paper we will employ nearest neighbor matching. This technique matches a treated firm to the non-treated firm in the control group that is closest in terms of the Mahalanobis distance between the respective propensity scores and possible other matching arguments. The nearest neighbor can be selected with or without replacement. To obtain the best possible match a large pool of controls is required. Therefore, we employ matching with replacement and allow different treated firms to be matched to the same non-treated firm. Moreover, the ordinary t-statistic on mean differences is biased and has to be corrected (Lechner, 2001). The detailed matching protocol is depicted in Table 1.

Table 1: Matching protocol (Nearest Neighbor matching)

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. $MD_{ij} = (Z_j - Z_i)' \Omega^{-1} (Z_j - Z_i)$ In the Flemish case, *Z* contains the estimated propensity score and the firm size (number of employees) as additional arguments in the matching function. In the German case, also the dummy that indicates location in Eastern Germany is an additional argument. 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 treatment effect on the treated can thus be calculated as the mean difference of the matched samples: $\hat{\alpha}_{TT}^{M} = \frac{1}{n^T}\left(\sum_i Y_i^T - \sum_i \hat{Y}_i^C\right)$ with $\hat{Y}_i^C$ being the counterfactual for firm *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.

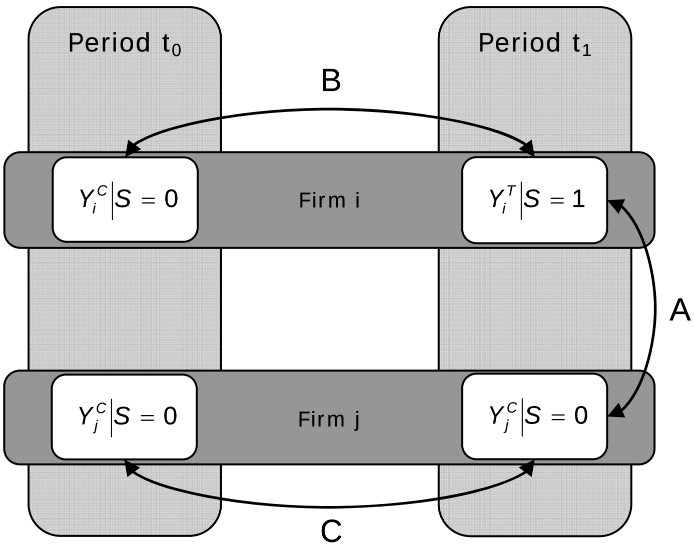
4.2 *Difference-in-difference (DiD) estimator*

In the difference-in-difference (DiD) model the treatment effect is estimated based on the idea that the counterfactual outcome of a subsidized company can be approximated by the outcome of that treated firm in an earlier period where it did not receive a subsidy. To control for macroeconomic changes over time DiD relates the development of subsidized firms i to a control group of non-subsidized firms j and compares them before (t_0) and after (t_1) the treatment moment:

$$\alpha_{TT}^{DiD} = \left(Y_{i,t_1} - Y_{i,t_0} \middle| X_{i,t_0}, X_{i,t_1}, S = 1\right) - \left(Y_{j,t_1} - Y_{j,t_0} \middle| X_{j,t_0}, X_{j,t_1}, S = 0\right) \qquad (7)$$

Figure 1 depicts the DiD methodology. Evolutions B and C are evaluated over time. The DiD technique allows controlling for both common macro-economic trends and constant individual-specific unobserved effects. Besides the outcome and treatment variables, additional covariates X enter equation (7) to account for the possibility that the treated and non-treated samples have systematically different characteristics in t_0 and t_1 (see Wooldridge, 2002). Neither functional form nor regressor is required for the outcome measure. However, a big disadvantage is that panel data is necessary, including observations before and after (or while) the treatment. As subsidies often target longer term research projects, and firms may receive multiple grants over time, it is difficult to construct a database that is suited for an appropriate application of DiD. Another shortcoming of DiD is that strategic behavior of firms to enter the subsidy program would lead to biased estimates. Moreover, if the companies that do and do not receive subsidies react differently on macroeconomic changes, the estimates are biased.

Figure 1: DiD methodology



4.3 Conditional difference-in-difference estimator (CDiD)

The CDiD estimator combines the advantages of matching and DiD and eliminates some of their respective disadvantages. DiD controls for unobserved heterogeneity between treated and non-treated companies and the matching technique controls for potentially different reactions to macroeconomic changes in the treated and the non-treated group. Heckman et al. (1998) show that CDiD based on a non-parametric matching provides an effective tool in controlling for selection on both observables and unobservables.

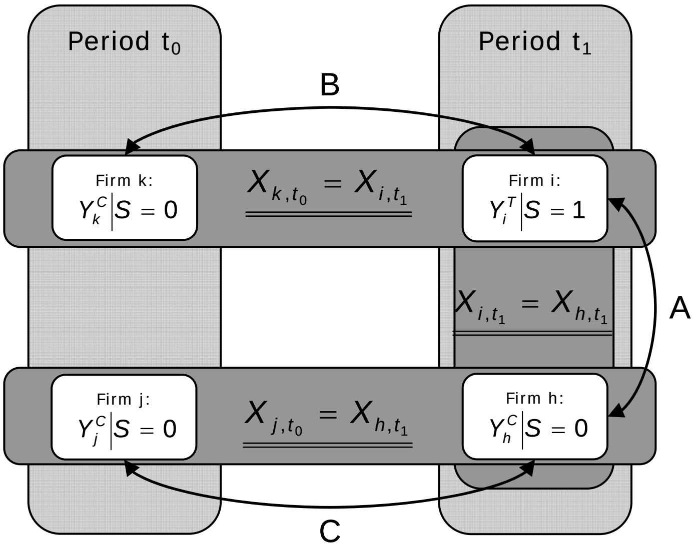
The control group used in the CDiD model is not general as in the ordinary DiD but a sample of non-treated firms that is matched to the treated firms in the period before receiving the treatment. The treatment effect of the treatment on the treated is estimated from the evolution of the two comparable groups over time. Blundell and Costa Dias (2000) suggest employing CDiD[3] for Repeated Cross-sections (CDiDRCS) if panel data are not available. This method will be applied in the empirical part of this paper. Three matching processes are necessary, as depicted in Figure 2. For every treated firm i in period t_1, a non-treated twin firm h has to be found in the same period t_1 (matching A). In the next step, a control group has to be compiled: for each treated firm i and each non-treated firm h in period t_1 a twin firm, i.e. k and j respectively, has to be found in period t_0 (matchings B and C). The average treatment effect on the treated firms then can be estimated as follows:

$$\alpha_{TT}^{CDiDRCS} = \left(E(Y_{i,t_1}^{T} \middle| X, S=1) - E(Y_{k,t_0}^{C} \middle| X, S=0)\right) - \left(E(Y_{j,t_1}^{C} \middle| X, S=0) - E(Y_{h,t_0}^{C} \middle| X, S=0)\right) \quad (8)$$

$$\text{with } X = X_{i,t_1} = X_{k,t_0} = X_{j,t_1} = X_{h,t_0}$$

[3] In Blundell and Costa Dias (2000), CDiD is referred to as MMDiD: method of matching with difference-in-differences.

Figure 2: CDiDRC methodology



5 Data description

The potential crowding-out effect of R&D subsidies is addressed empirically with data from the Flemish and German[4] Community Innovation Survey (CIS). First, only a cross-section of the CIS4, covering the years 2002 to 2004, is used. In a second step, also data from the CIS3, referring to the years 1998 to 2000, is plugged into the analysis. 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 and German manufacturing sector and computer services, R&D services as well as business related services. In accordance with the OECD/Eurostat (1997) guidelines for the CIS survey, the sample is restricted to companies with ten or more employees. The total sample consists of 4565 (1665) German (Flemish) observations on 3902 (1471) companies. Of these companies, 23% (21%) received public R&D funding from the regional, federal or European government. These innovation data are supplemented with patent application data from the European Patent Office since 1978.

The receipt of subsidies is denoted by a dummy variable (FUN) indicating whether the firm, observed in the CIS4 (CIS3), received public R&D funding in the period 2002 to 2004 (1998 to 2000). This funding can come from the regional, national and EU level. We did not distinguish between different funding sources; the funding impact that we analyze in this paper is an average effect over the different funding schemes. We would also like to stress that the restriction to a dummy variable (instead of using full information on the amount of the subsidy) imposes a limitation on the interpretation of the results. We can only analyze whether there is full crowding out, i.e. the subsidy

[4] Note that the German Community Innovation Survey data is part of the Mannheim Innovation Panel, the annual German innovation survey.

fully replaces private money. In this case the actual and counterfactual R&D spending are equal. Partial crowding out would mean that the subsidy partially replaces private money: the funded companies spend more on R&D, but the additional amount of R&D spending is smaller than the amount of the subsidy. In the case of full additionality, funded companies spend their budgeted R&D expenditure and all additional public money or even more. The hypotheses of partial crowding out and full additionality cannot be tested in the framework presented in this paper. The dummy variable also implies a drawback on the comparability between the two countries: it may be the case that the effect of the subsidy is heterogeneous in size.

As the subsidy dummy covers a three year period, we use values of the covariates measured at the beginning of the reference period, 2002 (1998), whenever possible, in order to avoid endogeneity problems in the selection equation.

We test the hypothesis of input additionality on two outcome variables. First, R&D expenditure[5] at the firm level in 2004(2000), *RD*, is evaluated. However, as the distribution of this indicator is very skewed in the economy, we also investigate the R&D intensity, *RDINT* (R&D expenditure / turnover * 100). Also due to the skewness of *RD* and *RDINT*, 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. Therefore, also the logs[6] of *RD* and *RDINT* are evaluated as outcome variables. All outcome variables refer to the year 2004(2000).

We use several control variables in our analysis which may affect both the probability to receive subsidies and R&D expenditure, respectively. Including the number of employees at the beginning of the period allows controlling for size effects. Again, the logarithmic transformation (lnEMP) is used.

Another important variable in our analysis is the firms' patent stock. As we use data from two cross-sectional datasets which do not include time-series information, the patent stock enables us to control for previous (successful) R&D activities[7]. We use all patent information in the EPO database and generate the stock of patents for each firm as the depreciated sum of all patents filed at the EPO from 1978 until 2001(1997):

$$PS_{i,t} = (1-\delta)PS_{i,t-1} + PA_{i,t}, \qquad (9)$$

[5] In the CIS survey, R&D expenditure is defined in accordance with the Frascati Manual (OECD, 1993).

[6] We replaced zero values of *R&D* and *R&DINT* with the minimum observed value, in order to generate the log of the variables.

[7] Of course, not all innovation efforts lead to patents ("not all inventions are patentable, not all inventions are patented" (Griliches, 1990: 1669)), 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 approximation of past innovation activities that is available.

where $PS_{i,t}$ is the patent stock of firm *i* in period *t*, $PA_{i,t}$ 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. Jaffe, 1986; Griliches and Mairesse, 1984). On the one hand, firms that exhibit previous successful innovation projects indicated by patents, are more likely to receive public R&D funding, because the public authorities may 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. The patents are counted only until 2001(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.

A dummy variable indicating whether a firm belongs to a group (*GROUP*) controls for different governance structures. 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 may be the case that the group tends to file subsidy applications in its home country.

The export quota from (2002)1998 (EXQU = 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. In the German analysis we also include the variable EAST, indicating whether the firm is located in Eastern Germany. There are strong indications that the innovation behaviour of Eastern and Western German firms may still be different (see e.g. Aschhoff et al. 2006; Sofka and Schmidt, 2004). Typically, companies in Eastern Germany are younger and smaller. Furthermore, there are special policy programs to foster the transition of this region into a market economy, which is obviously important in the framework of additionality effects of R&D subsidies. Finally, also 12 industry dummies (br2-br12) and their interaction terms with lnEMP (br2_lnemp-br12_lnemp) are included.

6 Estimations

We test the additionality hypothesis with two techniques. First, we employ the matching estimator, as common in the literature on the evaluation of R&D subsidies. In the second step, we control for unobserved heterogeneity effects by using the CDiDRCS estimator. This is new in the domain of R&D additionality research.

6.1 The matching estimator

In this subsection the matching estimator is applied to the data of the CIS4 to estimate the additionality effect of subsidies that were granted to Flemish and German companies between 2002 and 2004. Table 2 presents the descriptive statistics for the samples, which consist of 2374 (883) German (Flemish) companies, of which 503 (171) received public funding.

Table 2: Descriptive statistics of the Flemish and German sample

	Subsidized companies		Potential control group Non-subsidized companies		
Variable	Mean	Std. Dev.	Mean	Std. Dev.	p-values of two-sided t-test on mean equality
Flemish sample					
lnEMP	4.198	1.630	3.645	1.273	p = 0.0000
PS/EMP	0.800	2.592	0.043	0.325	p = 0.0002
GROUP	0.602	0.491	0.449	0.498	p = 0.0003
FOREIGN	0.263	0.442	0.222	0.416	p = 0.2685
EXQU	0.026	0.092	0.018	0.086	p = 0.2916
$\widehat{P}(X)$	0.336	0.241	0.159	0.120	p = 0.0000
RD	2.002	4.972	0.228	1.166	p = 0.0000
RDINT	8.046	14.425	1.096	3.783	p = 0.0000
lnRD	-1.1995	3.513	-7.213	3.694	p = 0.0000
lnRDINT	0.175	2.762	-3.855	2.806	p = 0.0000
Number of obs.	171		712		
German sample					
lnEMP	4.443	1.679	4.206	1.468	p = 0.0041
PS/EMP	0.806	2.127	0.298	1.245	p = 0.0000
GROUP	0.660	0.474	0.569	0.495	p = 0.0002
FOREIGN	0.127	0.334	0.094	0.291	p = 0.0393
EXQU	0.303	0.271	0.166	0.232	p = 0.0000
EAST	0.491	0.500	0.280	0.449	p = 0.0000
$\widehat{P}(X)$	0.351	0.190	0.173	0.145	p = 0.0000
RD	8.062	62.051	1.135	6.756	p = 0.0127
RDINT	7.227	6.710	1.217	3.445	p = 0.000
lnRD	-0.937	2.697	-4.521	3.054	p = 0.0000
lnRDINT	0.376	2.914	-4.278	3.694	p = 0.0000
Number of obs.	503		1871		

Note: the 12 industry dummies and interaction terms with lnEMP are not reported here.

The two-sided t-tests indicate significant differences between the subsidized companies and the potential control group of non-subsidized companies. Flemish and German subsidized firms are larger, have a larger patent stock and are more likely to belong to a group. The dummies for foreign ownership and the export quota do not differ significantly between the Flemish groups. German subsidized firms are more likely to be foreign and have a significantly higher export quota. As expected, also the dummy for companies located in Eastern Germany differs between the two groups. The industry dummies and their interaction terms with the number of employees (not presented in

Table 2) are significantly different both the Flemish and German sample. The outcome variables show that the subsidized companies are significantly more R&D active. However, we cannot simply assign this difference to the subsidy receipt, due to the potential selection bias, which we already described before. Therefore, we have to select a control group that has similar characteristics as the group of funded companies.

This control group is selected in accordance with the matching procedure that was outlined in the methodological section of this paper. The first step consists of estimating a probit model on the receipt of subsidies. The estimation results for the Flemish and German sample in Table 3 shows that the most important variables are -as expected- size, the patent stock, the group and foreign dummy, the export quota and the Eastern Germany dummy. Further tests show that the interaction terms of the industry dummies and size (lnEMP) are jointly significant ($chi^2(11) = 31.51$ and $p = 0.0009$ for the German and $chi^2(11) = 36.50$ and $p = 0.0001$ for the Flemish sample). As a result, these interaction terms are also included in the propensity score (this probit model is not presented in the paper). In the second step, for each subsidized firm a twin-firm is selected from the control group of non-subsidized companies with the hybrid nearest neighbor matching technique. Due to the common support[8] requirement 4 (4) German (Flemish) non-funded firms and 25 (20) funded firms had to be deleted from the sample (CISIII and IV together). The likelihood to receive public funding (propensity score, obtained from the probit model), firm size and for the German sample also the Eastern Germany dummy, are used as arguments in the matching procedure. Table 2 shows that the propensity score is significantly different too between the group of subsidized companies and the potential control group for both samples.

Table 3: Probit estimations and marginal effects

	Flemish sample						**German sample**					
	Probit estimates			Marginal effects			Probit estimates			Marginal effects		
	Coef.		Std.Err.	dy/dx		Std.Err.	Coef.		Std.Err.	dy/dx		Std.Err.
lnEMP	0.168	***	0.046	0.042	***	0.011	0.048	*	0.025	0.012	*	0.006
PS/EMP	0.373	***	0.101	0.092	***	0.025	0.061	***	0.21	0.015	***	0.005
GROUP°	0.089		0.134	0.022		0.033	0.106		0.072	0.027		0.018
FOREIGN°	-0.300	**	0.151	-0.068	**	0.031	-0.130		0.107	-0.031		0.024
EXQU	-0.141		0.623	-0.035		0.154	1.091	***	0.150	0.275	***	0.038
EAST°							0.787	***	0.070	0.223	***	0.021
constant	-1.844	***	0.187				-1.954	***	0.130			
industry dummies br2-br12	$chi^2(11) = 26.66$ p = 0.0052						$chi^2(11) = 103.19$ p = 0.0000					
Log-Likelihood	-379						-1019					
Pseudo R²	0.076						0.147					
Number of obs.	866						2348					

*** (**, *) indicate a significance level of 1% (5, 10%).
The marginal effects on subsidies are calculated at the sample means for continuous variables and for a discrete change of dummy variables (indicated by °) from 0 to 1. Their standard errors are obtained by the delta method.

[8] As this matching procedure within the CIS4 is the starting point for the CDiD in section 6.2 where matches to the CIS3 are added for the treated and selected non-treated firms from this section 6.1, we impose the simultaneous common support requirement for all three matchings already in this first step.

When we only take the selected control group into account in the t-tests (see Table 4) we no longer observe significant differences in the control variables size, patent stock, group, foreign ownership, export quota, location in Eastern Germany, industry dummies and the propensity score. However, the differences in the outcome variables remain significant: the funded companies are more R&D active; they spend more on R&D both in absolute terms and in proportion to the turnover. We can conclude that for both the Flemish and German sample the crowding-out hypothesis can be rejected: the average R&D expenditure and the average R&D intensity have increased due to the public funding of R&D.

Table 4: Descriptive statistics of the Flemish and German matched samples

	Subsidized companies		Selected control group Non-subsidized companies		
Variable	Mean	Std. Dev.	Mean	Std. Dev.	p-values of two-sided t-test on mean equality*
Flemish sample					
lnEMP	4.129	1.517	4.121	1.493	p = 0.969
PS/EMP	0.228	0.788	0.135	0.577	p = 0.283
GROUP	0.573	0.496	0.567	0.497	p = 0.921
FOREIGN	0.248	0.433	0.197	0.399	p = 0.340
EXQU	0.024	0.087	0.015	0.064	p = 0.396
$\widehat{P}(X)$	0.289	0.175	0.285	0.170	p = 0.864
RD	1.287	3.070	0.450	1.184	p = 0.002
RDINT	7.240	13.415	2.534	6.278	p = 0.000
lnRD	-2.283	3.484	-5.211	4.243	p = 0.000
lnRDINT	-0.007	2.792	-2.341	3.265	p = 0.000
Number of obs.	157		157		
German sample					
lnEMP	4.453	1.647	4.451	1.609	p = 0.985
PS/EMP	0.695	1.777	0.522	1.548	p = 0.164
GROUP	0.659	0.475	0.688	0.464	p = 0.418
FOREIGN	0.126	0.332	0.145	0.352	p = 0.480
EXQU	0.291	0.263	0.302	0.300	p = 0.626
EAST	0.486	0.500	0.486	0.500	p = 1.000
$\widehat{P}(X)$	0.338	0.177	0.335	0.174	p = 0.834
RD	4.982	20.587	1.750	7.744	p = 0.002
RDINT	7.033	9.662	1.707	4.002	p = 0.000
lnRD	-0.987	2.686	-3.667	3.457	p = 0.000
lnRDINT	0.312	2.942	-3.486	3.899	p = 0.000
Number of obs.	484		484		

Note: the 12 industry dummies and interaction terms with lnEMP are not reported here.
* t-statistics to test the mean equality between the sample of funded firms and the selected control group are based on Lechner's (2001) asymptotic approximation of the standard errors that accounts for sampling with replacement in the selected control group

The average treatment effects can be calculated from the sample means in Table 4 and are presented in Table 5. The absolute difference in RD in million EUR and RDINT in % is converted into a relative difference, based on the values for RD and RDINT of the treated group. Strictly speaking, the treatment effect that is calculated in the matching procedure can only be evaluated at the averages of the samples. However, as the distribution of both R&D expenditure and intensity is very skewed, we also calculated the median differences. These results should be interpreted cautiously, though. On average, a Flemish company that receives a subsidy, spends 0.837 million EUR (65%) more on R&D, compared to the situation where it would not have received the subsidy. The German subsidized firms spend, on average, 3.232 million EUR (65%) more. The R&D intensity in absolute terms increases with about 5% in Flanders and Germany due to the subsidy. It would be interesting to test the presence of heterogeneous treatment effects: large subsidies could induce other effects than small subsidies. Unfortunately, the data that is available (only a dummy for funding and not the amount) does not allow us to further investigate this issue.

Table 5: Average treatment effects on the treated companies[9]

	Flanders				Germany			
	Absolute		Relative		Absolute		Relative	
	mean	median	mean	median	mean	median	mean	median
RD (in mio EUR)	0.837	0.211	65%	89%	3.232	0.401	65%	100%
RDINT (in %)	4.669	1.484	64%	91%	5.327	3.219	76%	100%

6.2 The CDiDRCS Estimator

The matching estimator indicates that crowding-out effects can be rejected in the Flemish and German case. However, one critique to the matching approach is that it only controls for observed heterogeneity between the subsidized and non-subsidized companies. Therefore, we apply the CDiDRCS estimator, which combines matching with the DiD approach for a set of cross-sectional data. The starting point is the matching result of section 6.1 (A in Figure 2). In the CDiDRCS approach, two additional matchings (B and C in Figure 2) are conducted. For the treated and selected non-treated firms, a twin firm is selected from the firms observed in the CIS3. The treatment effect is then calculated from the mean difference between the treated and non-treated firms over time. In this way, both unobserved heterogeneity and potentially different reactions to macroeconomic changes in the treated and the non-treated group are controlled for.

The two additional matchings entail exactly the same procedure as the one conducted in section 6.1. However, in some cases, the same treated firm in CIS4 was observed being non-treated in CIS3 (26 Flemish and 36 German firms) or the same non-treated firm in CIS4 was observed in the same status in CIS3 (18 Flemish and 82 German firms). Those firms were matched to their own past observation. The same outcome and control variables are analyzed in the same hybrid matching procedure as before. Therefore, the intermediate matching results are not reported in this paper. The t-tests after the matching show that the selected control groups constitute a reliable match.

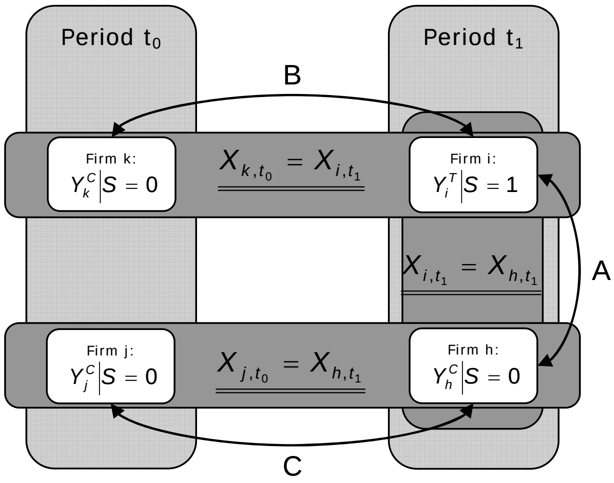
First, the final treatment effect estimations are presented for each matching separately. The estimations of the treatment effects are depicted in Table 6 and can be interpreted as shown in the

[9] Although the econometric analysis that is used here was developed to evaluate average treatment effects, we also calculated the median difference, as an additional approximation of the effect.

accompanying picture. Estimation A is the result of the matching of treated to non-treated firms within CIS4 (period t_1); thus estimation A corresponds to the estimation presented in section 6.1. Estimation B results from matching treated firms in CIS4 to non-treated firms in CIS3 (period t_0). Finally, estimation C indicates the difference in outcome variables between non-treated firms in CIS4 and non-treated firms in CIS3. The treatment effects A and B are always significant. The treatment effect over time is in line with the treatment effect in the same period. The correction for different reactions to macro economics shocks between treated and non-treated firms (estimation C) is never significant. The structure of the results is very similar in the Flemish and German sample.

Table 6: Treatment effect estimations in the three matchings (difference in group means)

	------A------	------B------	------C------
	Flemish sample		
RD	0.838 ***	0.900 ***	0.050
	(0.273)	(0.288)	(0.178)
RDINT	4.669 ***	5.017 ***	0.203
	(1.246)	(1.429)	(1.190)
lnRD	2.923 ***	2.530 ***	-0.480
	(0.512)	(0.832)	(0.854)
lnRDINT	2.334 ***	2.065 ***	-0.242
	(0.400)	(0.635)	(0.646)
	German sample		
RD	3.232 ***	2.432 *	-0.262
	(1.049)	(1.433)	(2.027)
RDINT	5.327 ***	5.717 ***	0.201
	(0.503)	(0.544)	(0.939)
lnRD	2.680 ***	2.956 ***	0.165
	(0.245)	(0.344)	(0.823)
lnRDINT	3.798 ***	4.052 ***	0.125
	(0.274)	(0.386)	(0.935)



*** (**, *) indicate a significance level of 1% (5, 10%)
The standard errors (between brackets) are heteroscedastic consistent and the 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

Second, we use the differences (graphically relation B in Table 6 for the treated and relation C for the non-treated firms) in the variables as input in an OLS regression as we would do in an ordinary DiD approach, with the extra feature that we condition on the exogenous variables mentioned before[10]. The difference in each of the outcome variables over time is regressed on the difference over time in funding (0 for the non-treated/non-treated matched firms and 1 for the treated/non-treated matched firms).

Because the regression is performed on matched samples, the t-statistics may be biased downwards and result in misleading conclusions (see e.g. Heckman et al., 1998). Therefore, we employ the bootstrap methodology in order to obtain unbiased standard errors (see e.g. Efron and Tibshirani, 1993). We used 200 replications of the procedure to estimate the bootstrapped standard errors.

[10] As the coefficients for relationship C are not significant in our first outcome presentation (see Table 6), it is not possible to merely subtract coefficient C from coefficient B for each outcome variable to obtain a corrected coefficient; the difference-in-difference approach allows us to bring the matching procedures B and C together.

The treatment effect (FUNdif) is always significantly positive, with one exception for the R&D expenditure in the Flemish sample; this insignificance however may be due to the skewed distribution of R&D expenditure and the relatively small sample size. When the R&D intensity or the logarithmically rescaled variable is evaluated, the additionality effect is again significantly positive. The coefficients are in line with the results that only take the evolution over time of the treated firms into account (estimate B in Table 6). Taking relationship C into account results in minor corrections. As a further robustness analysis we also include the difference in the other continuous variables into account[11]. For the German sample we can take the EAST dummy into account as well, as this dummy was included in the hybrid matching: only companies with the same value for EAST are matched. These extra variables add to the explanatory power of the model and the impact of public funding remains strongly significant, even if we control on the differenced exogenous variables. In the Flemish sample, the difference in outcome variables is only due to the receipt of a grant. In the German sample, some differenced exogenous variables are significant, but the main impact on outcome variables comes from the strongly significant relationship with the subsidy receipt. Even though the funding systems in Flanders and Germany are slightly different, the additionality effects have the same structure.

Table 7: Treatment effect estimations: OLS in differences

Variable	RDdif		RDINTdif		lnRDdif		lnRDINTdif	
Flemish sample								
FUNdif	0.661	0.571	5.204 ***	5.158 ***	2.574 ***	2.444 ***	2.129 ***	2.144 ***
	(0.588)	(0.600)	(1.170)	(1.224)	(0.528)	(0.525)	(0.415)	(0.498)
lnEMPdif		1.505		-2.461		1.069		0.227
		(2.742)		(4.163)		(1.738)		(1.760)
PS/EMPdif		0.461		0.324		0.786		0.541
		(0.545)		(1.232)		(0.892)		(0.499)
EXQUdif		4.727		12.668		2.185		1.427
		(6.644)		(15.079)		(6.076)		(5.002)
Number of obs.: 314								
R^2	0.064	0.134	0.109	0.132	0.118	0.161	0.124	0.150
German sample								
FUNdiff	2.922 **	3.529 **	5.509 ***	4.871 ***	2.856 ***	2.644 ***	3.877 ***	3.466 ***
	(1.187)	(1.351)	(0.598)	(0.699)	(0.249)	(0.296)	(0.316)	(0.362)
lnEMPdif		8.062		-2.886		0.054		-0.952
		(5.316)		(1.971)		(0.809)		(0.933)
PS/EMPdif		0.483		0.389		0.169		0.134
		(0.399)		(0.478)		(0.158)		(0.192)
EXQUdif		3.258		0.500		0.636		0.425
		(3.797)		(2.117)		(1.342)		(1.778)
EAST		-1.179		1.256 *		0.447		0.812
		(0.733)		(0.574)		(0.442)		(0.668)
Number of obs.: 968								
R^2	0.013	0.040	0.184	0.197	0.233	0.236	0.249	0.246

Bootstrapped standard errors (between brackets) are heteroscedastic consistent
*** (**, *): significant at 1% (5%, 10%)

[11] Through the triple matching procedure, we explicitly condition the selection of non treated firms on their exogenous characteristics. This however does not mean that no differences exist in the differenced exogenous variables.

Czarnitzki (2006) shows that not only the R&D expenditure but also the R&D status may change when a subsidy is granted. Small firms and firms that can offer only limited surety may experience great difficulties in raising external capital for risky projects. Consequently, only a limited budget is available for R&D activities, which may be shut down as a result. As Lerner (1999) argued, the subsidy receipt may serve as a certification of the firm's activities, which could convince potential financiers. Up until now the switch of R&D status was taken into account, as we allowed the possibility that a funded R&D active company is matched to a non-funded non-R&D active company. If we limit the sample to innovating companies only, the treatment effect may be underestimated. However, it provides a robustness check. For both the Flemish and German sample the treatment effect remains significantly positive, but is –as expected– somewhat lower.

7 Conclusions

We empirically tested whether public R&D subsidies crowd out private R&D investment in Flanders and Germany, using data from the CIS3 and CIS4. The main concern in evaluation analysis is to tackle the problem of selection bias. Several methods are available to solve this problem, each with specific advantages and disadvantages. First, hybrid nearest neighbor matching was employed in the CIS4 cross-sectional sample. The sample contains information on the funding status in 2004 and on the other covariates in the period 2002-2004. For both samples the crowding-out hypothesis was rejected: on average, the R&D intensity of German (Flemish) funded companies is 74% to 100% (65% to 100%) higher than the R&D intensity of non-funded companies. The disadvantage of the matching estimator is that it does not control for unobserved heterogeneity. Therefore, we applied a combination of the matching procedure and the difference-in-difference method, i.e. conditional difference-in-difference using the two cross-sections of CIS3 and CIS4. This estimator allows correcting for both observed and unobserved heterogeneity. Also in this case, the crowding-out hypothesis can clearly be rejected; funded firms are significantly more R&D active than non-funded firms. These results are in line with results from earlier studies on additionality in Flanders and Germany and also other European countries. Although the funding systems in Flanders and Germany are different, the additionality effects have the same structure.

In this paper only the funding status of firms is analyzed. Therefore it is not possible to indicate how much R&D expenditure is leveraged with 1 Euro extra funding. This has been tested for a cross-section of Flemish data. It would be interesting to employ continuous treatment analysis in a time series framework for both countries and in this way test for heterogeneous treatment effects of subsidies. Another appealing research question is the output additionality. Input additionality is not necessarily translated into innovative output and economic welfare. Very recently, studies have been conducted on output additionality, measured in terms of patents, in German firms (Czarnitzki and Hussinger (2004) as well as Czarnitzki and Licht (2006). In addition to these studies, it would be interesting to look at other innovation indicators on the output side of the innovation process, such as the introduction of new products or processes. A first study using a dummy variable on the introduction of an innovation into the market has been conducted by Hujer and Radić (2005). However, long time-series data would give more insight and would allow testing different lag specifications 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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