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Abstract

This paper establishes theoretically a link between investments in economic growth and investments in R&D. This raises the importance of innovation policies as they are designed to narrow the gap between the socially optimal and the privately optimal levels of R&D. As innovation policies may be subject to crowding-out effects, we, second, empirically test whether R&D subsidies stimulate private investment in two countries of the European economic area. We employ Belgian and German firm level data for estimating treatment effects models. It turns out that public R&D grants stimulate private investments in both countries. Furthermore, the estimated treatment effects vary with the innovation experience of firms and their past labor productivity.

JEL-Classification: G38, H25, L59, O31, O38

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1 Introduction

The current economic crisis offers the temptation to become shortsighted and look only for immediate remedies for the crisis. As important as such measures are, there is a real need to look for structural reforms that would enhance European growth in the long run. Policy tools addressing these long run concerns are the various ways in which governments support private sector R&D. They include a wide variety of policies, starting from education and intellectual property rights, and ending with tools that reduce the cost of R&D to companies (see Takalo 2012 for a recent survey). These policies – R&D subsidies and R&D tax incentives – are used by most OECD countries and an increasing number of emerging economies such as India. While the theoretical motivations for the use of such policies are well understood, the understanding on how exactly these policy tools affect individual firms are less well grounded in theory. Our objective in this chapter is to provide a link between such micro-level policies and the future of the monetary union.

We aim to achieve that objective by starting with a discussion of the relationship between R&D and economic growth. We then proceed to a discussion of government policies that would enhance R&D investments. We discuss both the theoretical underpinning of such government policies as well as the empirical evidence on their effectiveness. To complement this literature survey, we provide a new analysis of the effects of R&D subsidies on Flemish and German firms. We especially concentrate on the potentially heterogeneous effects of subsidies on exporting firms and firms that engage in international collaboration. We find additionality in both countries, and that the effect of subsidies depends on both past innovation performance (positively) and labor productivity (negatively) but that it does not depend on whether the R&D early stage (basic) research or more applied research. Anticipating our literature survey, we find that the connection between the monetary union and innovation policies comes through at least two channels: First, the monetary union is about achieving economic growth, and there is a link between economic growth, productivity and R&D, and naturally between R&D and public support to private R&D. Second, the monetary union is also about deeper integration along several dimensions, and there is a link between the lowering of trade barriers and investments in R&D and new technologies, again providing eventually a link between the monetary union and innovation policies.

The rest of the chapter is organized as follows: in the next section we provide a literature review of first, the literature on growth and R&D; second, of the literature on the relationship between productivity, exporting and R&D; and finally, of the literature on the effects and effectiveness of R&D subsidies and R&D tax credits. In section three we provide a new analysis the heterogeneity of the treatment effect of R&D subsidies before drawing policy conclusions in section four.

2 Literature review

R&D subsidies and R&D tax credits – innovation policy – may seem to be separate from monetary policy and the monetary union. In this section we seek to demonstrate that the opposite is true. This we do by first discussing how economic growth and R&D are related. We then turn to the relationship between productivity, exporting and R&D, and finally to the relationship between private R&D and government support. These different literatures are not well connected, and therefore we now provide a brief and selective literature review on each these different strands of research. We will come back to the link between them, and them and the monetary union in section 4 where we discuss policy implications.

2.1 Growth theory and R&D

We do not seek to be comprehensive in this subsection, as several excellent surveys on economic growth exist (see e.g. Jones 2005 and Aghion and Howitt 1998, 2009). A key observation is that modern economic growth theory places a heavy emphasis on the role of ideas in generating growth.[1] As has been noted e.g. by Aghion and Howitt (2006), the neoclassical growth theories cannot easily explain empirical facts such as the divergence in the European and US growth experiences from the mid-1990s onwards. The Schumpeterian growth model advocated by the authors emphasizes competition through innovation. As we discuss below, various implications of the Schumpeterian model have found empirical support, especially the effects of entry and exit on productivity. Such empirical findings have implications on how to structure government support to private sector R&D.

One such empirical fact is the effect of entry on incumbent innovation. Aghion et al. (2009a) establish with UK data that the threat of entry spurs innovation in those sectors that are close to the technology frontier, but hampers innovation in sectors that are further away from the technology frontier. The explanation for this result is that in the former sectors, innovation allows incumbents to "escape" competition through innovation whereas in the latter, threat of entry reduces the rents available to the incumbents from innovation. In line with these results, Aghion et al. (2005) document that the relationship between competition and innovation is likely to be of an inverse-U-shape. In intensely competitive industries, the firms have the choice of innovating and thereby escaping fierce competition, or being left behind. Aghion et al. (2005) also find evidence, again in line with the predictions of the theory, that innovation is more intense in industries where competition is more intense (more "neck-and-neck" to use their phrase). More recent evidence by Hashmi (2013), using U.S. data, suggests however that the relationship between competition and innovation is positive.

A final feature of the Schumpeterian model that is worth pointing out in our context is the importance of the level of education. Somewhat surprisingly, the empirical evidence on the causal impact of education on growth is limited. Aghion et al. (2009b) use US state level data to provide some evidence of a positive causal impact of education on growth. Somewhat

[1] For example, Jones (2005, pp. 1107) writes: "The more inventors we have, the more ideas we discover, and the richer we all are".

more indirectly, Toivanen and Väänänen (2010) establish a causal effect of engineering education on the propensity to patent (and to the quality of the patents). Their finding links to the earlier quote from Jones (2005, see fn. 1) as it suggest that one can indeed educate inventors. Indeed, one could and should view education policy as part of innovation policy.

2.2 R&D, productivity and exporting

One of the oldest strands of the economics of innovation literature attempts to estimate the returns to R&D. Griffith (2000) summarizes the older literature that uses estimates of the effect of R&D in other sectors to infer social rates of return. She reports that the social rate of return could be as high as 100%. Jones and Williams (1998) argue that these numbers are a lower bound on the true social rate of return because the estimates do not capture the general equilibrium effects that are present in endogenous growth models. In a recent paper, Doraszelski and Jaumandreu (2013) study a model where R&D investments endogenously affect firm productivity. They find that the private returns to R&D are of the order of 40%. The link between R&D and productivity is important because it is well established that the main source of economic growth is through improvements in productivity.

A more recent literature studies the link between productivity, exporting and R&D. Early papers such as Clerides, Lach and Tybout (1998) and Bernard and Jensen (1999) established a link between firm productivity and exporting, with more productive firms being more like to become exporters. The more recent research has shown that there is a clear link between the export decision and productivity (Van Biesebrock 2005 and De Loecker 2007) and between the export decision and productivity enhancing investments, be they R&D (Aw, Roberts and Xu 2008) or technology adoption (Bustos 2011). Van Beveren and Vandenbussche (2010) find that firms self-select into R&D in anticipation of entry into exporting. Further, recent research (Lileeva and Trefler 2010) has also shown how important market size effects are in inducing firms to invest in productivity improvements. Different papers agree in finding heterogenous effects across firms, but disagree in which firms benefit the most: Lileeva and Trefler find that it is the least productive new entrants in the export market for whom the productivity gains are the largest while Bustos find that it is the most productive firms (and new and old equally) that have the largest productivity gains.

2.3 Public support to private R&D - theory

The main motivation for using public money to support private R&D is comes from Arrow (1962) and Nelson (1959) – to quote Arrow (1962, pp. 615):

“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.”

This appropriability problem drives a wedge between the socially and the privately optimal amount of R&D for a given R&D project. The second most commonly mentioned market failure has to do with imperfect and incomplete information in financial markets. These informational problems may lead to rationing and/or to higher cost of finance, again lowering the levels of R&D from what would be socially optimal.[2]

These motivations have not gone unnoticed by governments: All OECD countries use R&D subsidies, and a growing number of them use some form of R&D tax incentives to enhance private sector investments in R&D. Given the prominence of these innovation policies, it is not surprising that a relatively sizeable literature studies the effectiveness of these policies.

[2] The theoretical literature has of course made advances after these seminal contributions. Takalo (2012) provides a review of the modern theoretical literature on different innovation policy tools which include intellectual property, prizes and contests, and public production and procurement in addition to subsidies and tax incentives.

What is more remarkable is that the theoretical literature that studies the allocation and effectiveness of these policy tools is rather thin.

The early theoretical literature (Brander and Spencer 1983) studied subsidies in an environment where there were no financial constraints. Takalo and Tanayama (2010) study an adverse selection model where firms first apply to the public agency for a subsidy, and then for additional financing from private financiers. In their model, the agency screens the project of an applicant and finds out its quality. They show that subsidies have two effects: First, they lower the cost of external finance. Second, a subsidy is a signal to private financiers about the quality of the project, thus generating a "certification" effect suggested by Lerner (1999).

Keuschnigg and Ribi (2010) study a moral hazard model where firms may invest in R&D or more conventionally, and those investing in R&D are credit constrained. They study the effects of taxes and public support and find that subsidies not only encourage innovation directly but also relax financial constraints, but taxes on profits may hamper investments in R&D.

Takalo, Tanayama and Toivanen (2010, 2011, 2013) build and estimate a model of the R&D subsidy allocation process. In their model, the firm gets an R&D idea. It then has to decide whether or not to apply for a subsidy; applying is costly. If it applies, the government agency learns the type of the firm, and has to decide whether or not to subsidize the firm, and if, by how much. After this decision the firm raises the rest of financing from private financiers. Finally the firm has to decide whether or not to invest in R&D, and how much to invest. Takalo, Tanayama and Toivanen show that the subsidy decision is more intricate than what has been thought. For example, the cost of financing actually decreases the optimal subsidy at the intensive margin (where the agency knows the project will be executed, and the subsidy only affects the size of the R&D investment at the margin), but increases it at the extensive margin where the firm decides whether or not to execute the project in the first place.

Takalo, Tanayama and Toivanen (2013) also analyse the circumstances under which additionality emerges, i.e., the public support leads to more private investment. They show that whether or not there is additionality depends on the R&D production technology, i.e., how much an extra euro of R&D boosts expected discounted profits, as well as on the cost of finance. A higher cost of finance makes it harder to obtain additionality. They also show that it is harder to obtain additionality with a higher level of support than with a lower level of support.[3] The reason for this potentially counterintuitive finding is that there necessarily are at some point decreasing returns to scale in R&D, but the support, measured in euros, grows faster. Finally, they point out that the mapping from additionality to social benefits of an R&D support scheme is not obvious. It could well be that of two countries with similar R&D production technologies, the country with the higher social benefits from R&D subsidies exhibits no additionality while the country with the lower social benefits would have additionality.[4]

This (possible) discrepancy between additionality and social benefits of public support to private R&D is naturally somewhat disturbing given that the empirical literature has very much focused on measuring additionality. The roots of the discrepancy lie in the different objectives of the firm deciding on how much to invest in R&D given some level of support, and the objectives of the agency (social planner). The firm only takes into account its own expected discounted profits and will increase R&D in response to an increase in the subsidy rate[5] up to the point where marginal benefits in terms of expected discounted profits are equal to marginal cost, i.e., the out-of-pocket cost of R&D net of the subsidy rate. For the

[3] One can also formulate this more precisely in terms of exogenous parameters of their model: an increase in the spillover rate (spillovers per euro of R&D) makes it harder to obtain additionality.
[4] Mohnen and Lokshin (2010) make the same point in their discussion of the evaluation of R&D tax credits.
[5] By subsidy rate we mean the fraction of R&D cost that the government pays / reimburses to the firm.

government, the optimal point would be where marginal social benefits, i.e., expected discounted profits of the firm plus spillovers, are equal to the marginal social cost of R&D. Thus, at the margin the government takes into account more on the benefit side (spillovers), and more on the cost side (the unsubsidized cost of R&D plus the shadow cost of public funds for the subsidized part). Takalo, Tanayama and Toivanen assume, in line with a long theory literature, that spillovers are linear in R&D. It is therefore possible that the government would want to push those R&D projects with high spillovers deep down the decreasing returns part of the R&D production technology, meaning that each extra R&D euro gained becomes progressively more and more expensive to the government as the firm needs to get a higher compensation on the cost side to invest one additional euro in R&D (because of diminishing marginal returns).

Lack of (observed) additionality does thus not mean that the government would not want to subsidize a given R&D project. What the government cares about is that the amount invested in a given R&D project is as close to the social optimum as possible, whether the payer is the firm or the government. The government may prefer private funding if the shadow cost of public funding is higher than the (social) cost of private funding. Only total crowding out, i.e., that the total amount of R&D investment is not changed at all when a project is subsidized, is something that for sure makes public support unattractive.

The theoretical literature on the effects of R&D tax credits is rather scant, too. Most of the theory is developed within a standard model of firm investment (e.g. Bloom, Griffith, and Van Reenen 2002) for empirical purposes. Takalo, Tanayama and Toivanen (2010) provide a counterfactual analysis of R&D subsidies and tax credits, and also a discussion of their relative strengths and weaknesses. We return to these issues in the next subsection when we discuss the available empirical evidence on the uptake of these two forms of public support.

Finally, it is worth mentioning that the endogenous growth theory has quite naturally studied the effects of R&D subsidies on growth. While the results are mostly positive in the sense that R&D subsidies lead to higher growth, there are exceptions. Segerstrom (2002) studies a slightly modified model of Schumpeterian growth and finds that for a surprisingly large range of parameter values, R&D subsidies can actually retard growth. Segerstrom goes on to show that targeted R&D subsidies (towards the "right" kind of innovation) will increase growth.

2.4 Public support to private R&D – empirics

The empirical literature on the effects of public support to private R&D has grown rapidly in the last ten years. The main methodological issue hampering progress and interpretation of the results is that support is not randomly allocated. Firms apply for support, and governments give support based on information that is unlikely to be directly available to the researcher. This creates an endogeneity problem that needs to be solved through a judicious use of econometric methods. We will not go into these technical details; they are discussed e.g. in the survey by Cerulli (2010). The essence of this problem is that one may obtain overly optimistic results on the effects of the policy because either the civil servants running the program play safe and choose "good" projects that are very likely to have a high level of R&D spending, or because firms submit such projects in order to maximize their chances of getting a subsidy. If these effects are not cleaned out, they might show up as a "causal" effect of getting a subsidy when in fact they are not.

The main object of interest in the empirical literature on both R&D subsidies and on R&D tax credits has been the so-called additionality: does a government subsidy lead to an increase in private R&D (additionality or crowding in), or to a reduction in private R&D spending (crowding out)? Additionality has been at the center of interest because it has been thought that a full-blown welfare analysis is beyond reach. In a recent paper, Takalo, Tanayama and

Toivanen (2011) provide a framework for a welfare analysis. Nonetheless, it is a fact that the vast majority of the empirical studies concentrates on additionality in one form or another.

The literature has been surveyed recently by David, Hall and Toole (2000), Garcia-Quevedo (2004), Cerulli (2010), and Zúñica-Vicente et al. (2012) with respect to R&D subsidies and by Hall and van Reenen (2000) and Parsons and Phillips (2007), and Mohnen and Lokshin (2010) wrt. to R&D tax incentives. For example, Zúñica-Vicente et al. (2012) list 77 studies, 43 of which have been produced in the last 12 years. There is wide variation both in the quality of data used and in the quality of the econometric modeling, making a simple summary of the conclusions difficult. Generally speaking one sees a trend towards higher quality data at more disaggregate level and spanning more years, and more sophisticated analytical methods. Taking these difficulties in interpretation into account, we here reproduce Table 2 of Zúñica-Vicente et al. (2012) (see Zúñica-Vicente et al. 2012 pp. 18).

In Table 2, the studies are grouped in the rows by the level of disaggregation (firm, industry, country) and by the origin of the country, and in the columns by the results (crowding in, i.e., additionality, crowding out and neither). What is clear is that a majority of studies, regardless of the level of disaggregation, find evidence for additionality. Of those papers using firm level data, 63% find evidence for additionality, while the remaining 37% either find no effect (17%) or evidence for crowding out (20%). While this is indeed promising, a word of caution is in order: The endogeneity problem mentioned above, when not properly taken care of, may lead to overoptimistic estimates of additionality.

Table 2. Summary Distribution of Econometric Studies of the Effect of R&D Subsidies on Private R&D Spending According to the Aggregation Level and Data Source.

Aggregation level	'crowding-in hypothesis'	'crowding-out hypothesis'	Non-significant effects	Total number of studies
Firm or lower[a]	48 (63.15%)	15 (19.74%)	13 (17.11%)	76[b] (64.41%)
Based only on US data	10	8	4	22
Based only on EU	33	6	7	46
Based on R. of the W. data	5	1	2	8
Industry	10 (50%)	5 (25%)	5 (25%)	20 (16.95%)
Based only on US data	4	3	2	9
Based only on EU data	4	1	1	6
Based on R. of the W. data	2	1	2	5
Country	13 (59.09%)	3 (13.63%)	6 (27.28%)	22 (18.64%)
Based only on US data	6	0	2	8
Based only on EU data	2	1	2	5
Based on R. of the W. data	5	2	2	9
Total number of studies	71 (60.17%)	23 (19.49%)	24 (20.34%)	118[b]

Notes:[a]Lower refers to studies performed at business unit- and plant-level or below.
[b]The total number of studies finding crowding-in, crowding-out or non-significant effects is 118. This number is greater than the number of reviewed studies (77) because there are several studies that find divergent results depending on the different assumptions and/or methods considered. Moreover, in most studies performed at country-level it is possible to confirm the crowding-in hypothesis in the case of some countries, but also the crowding-out hypothesis in the case of other countries, or even non-significant effects in some countries.

Source: Zúñica-Vicente et al. 2012 pp. 18.

One should not be surprised that the estimated effects vary from one study to the next. This would most likely be the case even if there were no differences in methods, but only data. As discussed above (see section 2.3) Takalo, Tanayama and Toivanen (2013) show how the existence (or not) of additionality depends on the production technology "turning" R&D investments into expected discounted profits, the cost of finance, and the level of support given. Thus it is most likely the case that there is variation in the degree of additionality in any given sample of firms, too.

In their recent survey of the empirical literature on the effects of R&D tax credits, Mohnen and Lokshin (2010) report that it is typically the case that there is additionality[6] for incremental R&D tax credits, and crowding out for level-based R&D tax credits.

Finally, it is worth mentioning the study by Busom, Corchuelo and Martinez Ros (2011) who, using Spanish data, study the determinants of using one or the other form of public support, both forms of support, or neither. Of course one needs to keep in mind that while the firm may decide relatively independently whether or not it exploits R&D tax credits (conditional on investing in R&D), the subsidy decision is made by the agency giving out subsidies after the firm first has applied for one. Busom, Corchuelo and Martinez Ros (2011) report that only 12% of SMEs and 20% of large firms investing R&D use both subsidies and tax credits. The rest of the firms invest in R&D with help of either only subsidies (23% of SMEs and 17% of large firms) or only tax credits (17% and 26%), and 47% and 36% invest without neither form of support. In the data used by Takalo, Tanayama and Toivanen only some 10% of firms apply for a subsidy in a given year.

There are very few studies allowing for the effect of public support to vary across firms. Lach (2002) finds that Israeli R&D subsidies lead to additionality for small firms, but had no effect, or a negative effect on the private R&D of large firms. Görg and Ströbl (2007) study R&D subsidies in Ireland and find that for domestic firms, small subsidies lead to additionality while large ones may crowd out private investment. Subsidies to foreign firms cause neither additionality nor crowding out.

2.5 Subsidies versus tax incentives

Important policy implications that relate to subsidies and tax incentives can be drawn based on both the way they are usually designed, and on the type of theoretical work surveyed above. An important distinction between subsidies and tax incentives could be called

[6] They call additionality "bang-for-the-buck", or BFTB.

"tailoring versus accessibility". R&D subsidies can be tailored to match the specific project for which the firm seeks public support. A benevolent social planner would choose the subsidy rate (the percentage of R&D cost paid by the government) to maximize social surplus, taking consumer welfare and spillovers to other (domestic) firms into account. The downside of R&D subsidies is that for reasons that are still not well understood, firms seem reluctant to apply for them, as the numbers in the previous section show. Thus, governments are in practice only able to utilize this potentially important ability to tailor the subsidy only to the relatively few firms who apply for support.

Tax incentives, on the other hand, by definition almost, cannot be firm-specific. While nothing prevents a government from designing them to vary across observable characteristics of firms, this is still a much lower dimension of tailoring than the possibility offered by subsidies, where the subsidy rate can be decided project by project. A tax incentive scheme therefore naturally is a compromise across some firm (sub)population. It could therefore e.g. happen that the government chooses a tax incentive rate that leaves a substantial number of firms just short of satisfying their participation constraint, i.e., making their R&D projects worthwhile to execute.

The other side of the coin is that the uptake of R&D tax credits is usually much higher than that of R&D subsidies. One would imagine that almost all R&D performing firms would utilize R&D tax incentives, making them much more accessible than subsidies. As data from e.g. Spain shows, the difference in accessibility is not as high as one might have thought: Of Spanish SMEs, 43% use subsidies, but only 37% tax incentives. Spanish large firms use tax incentives more often (46% versus 37% using subsidies).

An issue that is more of a concern with R&D tax credits than R&D subsidies is so-called relabeling, i.e, the practice of claiming that some expenses are R&D related when they in reality are not. There is little systematic evidence on this, but a recent study of the Norwegian

R&D tax credit scheme (Haegeland and Moen 2007) offers a window. They compare figures obtained from the tax scheme to those obtained from the Norwegian R&D survey and find that the R&D expenses are on average 34% higher, and R&D personnel costs, on which the Norwegian R&D tax credit are based, 74% higher in the figures reported to the tax credit scheme than in the figures reported in the survey.[7] Cappelen et al (2010) report evidence that especially small firms seem to substantially inflate the R&D expenses they report in relation to the tax credit.

2.6 Summary of the literature review

While there is overwhelming circumstantial and descriptive evidence that innovation is the main driving force behind economic growth, it has proven surprisingly difficult to pin down both the causal effects of R&D investments, as well as their determinants. One of the proven channels works through the link between R&D, productivity, and exporting.

The strong descriptive evidence, combined with the solid theoretical basis for public support for private R&D has lead a large number of countries to resort to active measures to induce higher private sector R&D investments.

The existing empirical research on the efficacy of public support to private R&D has largely concentrated on attempting to measure whether private sector R&D investments increase as a result of public support (additionality), or whether public money crowds out private money in R&D, paying little attention to the effects possibly varying across firms. As any literature that has developed over several decades, this literature has gone through different phases, and today is increasingly using both more detailed and larger data sets, and more credible econometric techniques. A somewhat conservative way of summarizing the literature is to

[7] They also provide information that may through some light on the surprising finding that such a small proportion of R&D performing Spanish firms report having used the R&D tax credit. In the Norwegian data, less than half of the firms that received R&D tax credits report in the survey that they have received a tax credit. The reported sums also deviate from the actual sums.

say that most of the evidence suggests that there is no crowding out, though there may not be much additionality.

Recent theoretical work has however pointed out that the effects of both subsidies and R&D tax credits are most likely heterogenous across firms; this point has been largely neglected in the empirical literature. It also appears that the link between additionality and welfare improvements is more complicated than has been thought heretofore.

3 An empirical study: R&D subsidies, firm size and exporting

In this empirical study we demonstrate that R&D subsidies have a positive effect on R&D and innovation investment of German and Flemish firms, and thus that technology policy has an indirect effect on growth, because as outlined above, it is a commonly accepted view that R&D and innovation are an important driver for economic development.

3.1 Data

The data used in this paper stem from the Community Innovation Survey (CIS) of Germany and Flanders.[8] More precisely, they stem from the CIS surveys of the years 2005, 2007, 2009 and 2011. Each wave of the survey asked firms about their innovation activities in the recent 3 years prior to the data collection, i.e. the CIS 2005 e.g. asked firms about their activities between 2002 and 2004.

Our sample concerns innovative as well as non-innovative firms and covers manufacturing as well as business related services sectors.[9] The German CIS usually covers firms from 5 employees onwards in these sectors. As, however, the Flemish CIS is limited to firms that

[8] The CIS covers all of the EU member states, Norway and Iceland using a largely harmonized questionnaire throughout participating countries. The CIS databases contain information on a cross-section of firms active in the manufacturing sector and in selected business services.

[9] According to the 3rd edition of the Oslo Manual – which is the definition followed by the CIS - an innovative firm is one that has implemented an innovation during the period under review. An innovation is defined as the implementation of a new or significantly improved product (good or service) or process or service (see Eurostat and OECD, 2005).

have 10 employees or more, we restrict our analysis to the latter population of firms in both countries: firms in manufacturing and business related services with 10 or more employees.

In total, the Flemish sample consists of 5,494 firm-year observations, out of which 2,666 refer to innovative firms and 818 received a public R&D subsidy either from the local government, the federal government or from the EU. For the German sample these numbers amount to 13,503 firm-year observations including 7,922 innovative firm-year observations. 2,360 of these are of firms that receive a subsidy.

In the Flemish sample, the 5,494 firm-year observations refer to 3,611 different firms, and 63% are only observed in one of the four survey years. Similarly, the German 13,503 firm-year observations are based on 8,860 firms and 64% of the firms are only observed once. This rules out panel estimators such as the (conditional) difference-in-difference methods, as the loss of observations would be tremendous. Therefore, we use our data as pooled cross-sections.

The receipt of a subsidy is denoted by a dummy variable, *SUBS*, equal to one for firms that received public R&D funding and zero otherwise.

Outcome variables

As outcome variable, we consider the internal R&D investment, *RDINT*, being the ratio of internal R&D expenditures[10] to sales. In addition, we also analyse total innovation expenditure which includes, in addition to internal R&D, also other expenses undertaken because of innovation projects. These are external R&D, the purchase of machinery, expenses for market introductions incl. prototyping of new products, expenses for training of employees coming along with process innovations, and the acquisition of external knowledge such as licenses. This is also specified as intensity relative to sales and called *INNOINT*.

[10] The CIS definition of R&D expenditure follows the Frascati Manual (OECD, 1993).

Control variables

We use several control variables in our analysis that are likely to have an impact on whether or not a firm applies and receives public support for its R&D activities. The number of employees (*EMP*) takes into account possible size effects. As the firm size distribution is skewed, the variable enters in logarithms (*lnEMP*). In addition, we include a dummy variable capturing whether or not a firm is part of a group (*GP*), and if so, whether it has its headquarters on foreign territory (*FOREIGN*). Firms that belong to a group may have a lower incentive to apply for subsidies as small firms that have a large majority shareholder do not qualify for the SME program where larger subsidy rates are granted. In contrast, however, firms belonging to a group may be preferred by a funding agency as group membership possibly promises knowledge spillovers and thus economies of scope from the R&D process. In addition, firms belonging to a network of firms may benefit from better communication structures and thus are better informed about possible funding sources including public technology policy programs. Subsidiaries having a foreign parent, however, may be less likely to receive subsidies, as the parent may prefer to apply in its home country or because the funding agency gives preference to local firms. Furthermore, foreign parents having Flemish subsidiaries are typically large multinational companies and thus the local subsidiary does not qualify for special SME-support which could reduce its likelihood to apply.

The log of the firm's age (ln*AGE*) is included in the analysis as it is often claimed that older firms are more reluctant to pursue innovation, and would thus be less likely to apply, all else constant. However, many policy programs are targeted at younger innovators, and thus age may well have also a positive effect on the subsidy receipt as young firms have more opportunities to apply for funding and may also be more dependent on public sources as it might be more difficult for those firms to raise external capital on the private credit market (See e.g. Hall and Lerner, 2010, for a survey on financial constraints).

Previous experience in successful R&D activities plays a vital role when applying for public support, as governments may adopt a picking-the-winner strategy and hence might favor firms with previous success stories. Therefore, we include the patent stock (*PS*) in our regression. The patent stock enters into the regression as patent stock per employee to avoid potential multicollinearity with firm size (*PS/EMP*). Even though "not all inventions are patentable" and "not all inventions are patented" (Griliches, 1990, p.10), the patent stock is the best approximation we have for past (successful) innovation activities as data on previous R&D expenditures are not available. The patent stock information stem from the EPO database and are computed as a time series of patent applications with a 15% rate of obsolescence of knowledge capital, as is common in the literature (see e.g. Jaffe, 1986; Griliches and Mairesse, 1984): $PS_{i,t} = PS_{i,t-1} \times 0.85 + \text{patent applications}_{i,t}$.

In addition, we include a control measuring the labor productivity of firms. The variable *LPROD* is measured as sales per employee. This variable might also capture past successful business conduct of the firm and is thus more general than the patent stock accounting for past innovation success. It may also serve as proxy for the internal financial resources of the firm as a firm with higher sales per employee might have stocked up more cash over the years.

We also control for the export behaviour of firms by including a dummy variable indicating whether the firm exported in the respective period (*EXPORT*). Firms that engage more heavily in foreign markets may be more innovative than others and, hence, more likely to apply for subsidies. In addition, they may benefit from new R&D results as they reach larger markets. Thus exporters are possibly more likely to realize economies of scale when introducing new products or processes.

Last but not least, industry dummies control for unobserved heterogeneity across sectors and time dummies capture macroeconomic shocks. Regional dummies (in Flanders we differentiate among the five provinces, and in Germany we use 13 regions[11]) control for unobserved geographical differences.

Timing of variables

As mentioned above, each wave of the survey covers a three-year period. In order to avoid endogeneity between the dependent variables and the covariates to the largest possible extent, we employ lagged values wherever possible. For instance, suppose the dependent variables are measured in period t. Then *EMP*, and *PS/EMP*, are measured at the beginning of the survey period, i.e. in t-2. Also the sales variable used to calculate the R&D and innovation intensities are measures in t-2, that is, $RDINT_{it}$ is generated as internal R&D expenditure of firm *i* in period *t* divided by sales of firm *i* in period *t*-2.

The information on *GP*, *FOREIGN*, and *EXPORT* is only available such that the question covers the whole 3-year period, i.e. t-2 to t. For instance, "Did your firm belong to a group during the period 2004-2006?" We consider *AGE* as truly exogenous and hence it is measured in period t.

Descriptive statistics

Table 2 shows the descriptive statistics of the variables of our samples. As we can see, several means of the variables are significantly different between the subsidized and non-subsidized firms. Larger firms that are members of a group, with foreign ownership and exports, are more likely to be subsidized. Less productive firms, measured by sales per employee, are more likely to receive a subsidy. The differences are otherwise similar for the Flemish and German data, but in Germany, the subsidized firms are younger. This is most likely explained by the twin facts that Eastern German firms are younger on average, and they are more likely to receive subsidies,

[11] This is based on the 16 German Länder. We have aggregated some small regions because of low numbers of observations in the sample. The Saarland is grouped together with Rhineland-Palatinate. Hamburg, Bremen and Schleswig-Holstein are aggregated as well as Berlin and Brandenburg.

Table 2: Descriptive statistics

Flanders

	Non-subsidized firms, N=4676		**Subsidized firms, N=818**		**Results of t-tests on mean differences**
Variables	Mean	Std. dev.	Mean	Std. dev.	
Covariates					
PS/EMP	0.002	0.020	0.015	0.043	*
lnEMP	3.771	1.106	4.569	1.519	*
FOREIGN	0.279	0.448	0.285	0.452	
EXPORT	0.610	0.488	0.848	0.359	*
GP	0.533	0.499	0.661	0.474	*
lnAGE	3.145	0.760	3.207	0.787	*
LPROD	0.309	0.430	0.288	0.307	
Outcome variables					
RDINT	0.007	0.038	0.059	0.117	*
INNOINT	0.016	0.056	0.092	0.150	*

Germany

	Non-subsidized firms, N=11143		**Subsidized firms, N=2360**		**Results of t-tests on mean differences**
Covariates					
PS/EMP	0.007	0.035	0.033	0.086	*
lnEMP	4.146	1.363	4.480	1.605	*
FOREIGN	0.079	0.269	0.094	0.292	*
EXPORT	0.465	0.499	0.801	0.399	*
GP	0.348	0.476	0.409	0.492	*
lnAGE	3.145	0.921	2.919	0.867	*
LPROD	0.183	0.230	0.162	0.165	*
Outcome variables					
RDINT	0.008	0.033	0.075	0.123	*
INNOINT	0.023	0.062	0.123	0.158	*

Note: * indicates a significance level of 5%. Note that the distribution across sectors and regions also differs between subsidized and non-subsidized firms. A detailed presentation is omitted here.

For instance, in both countries firms receiving subsidies are on average larger, more export oriented and belong to a group more often. Furthermore, they have a higher patent stock per employee. Regarding the outcome variables, funded firms have on average higher R&D and innovation intensities. Interestingly, the funded firms have lower sales per employee then non-funded firms, on average, in both countries. The econometric analysis in the next section will reveal to which extent these differences can be attributed to the receipt of subsidies.

3.2 Econometric results

In order to evaluate the effects of public subsidies on innovation input at the firm level, we cannot simply compare e.g. R&D spending of subsidized firms and non-subsidized firms. As outlined above, governments may follow a picking-the-winner strategy which makes the subsidy receipt an endogenous regressor to R&D input. In other words this means that the assignment of a treatment does not happen randomly, but may depend on certain firm characteristics most likely including its R&D capacities and R&D capabilities. There are multiple econometric tools on how to deal with this problem. For this study we choose to apply a matching estimator that compares subsidized firms not to all non-subsidized firms, but instead only to those that have the most similar characteristics to subsidized firms based on our observed firm characteristics described above. This means that we construct a control group of firms that are similar to the subsidized firms. If the assumptions of this so-called matching estimator hold, any remaining difference in innovation input between these balanced samples can then be attributed to the fact of the subsidy receipt.[12] The key identifying assumption is called conditional independence: Conditional on the observed characteristics, the treatment is randomly assigned. If we did not observe important characteristics driving the incidence of a receiving a subsidy, the matching estimator would be biased as selection would then happen partly based on "unobservables". Then, the application of IV regressions, parametric treatment effects models, or panel data estimators such as conditional difference-in-difference would become necessary. Unfortunately, our data is not rich enough to apply either of those methods. Therefore, we apply caliper matching implemented as propensity score nearest neighbor matching.

In order to apply the matching estimator, we first have to estimate a Probit model to obtain the predicted probability of receiving a subsidy. We can see in the following table that with the exception of the coefficient of the group dummy, all the other coefficients are significantly different from zero and hence are driving the selection into the funding scheme.

[12] See the methodological appendix for technical details on the method.

The coefficient of ln*EMP* is positive and significant in both countries. Thus, we find that the probability of being subsidized is increasing in firm size. In addition, prior patenting as proxy for past successful R&D activities shows a highly significant positive effect. Also firms engaging in export activities are more likely to receive subsidies. Firms with a foreign parent company are less likely to be publicly funded.

Finally, we also find a significant effect of age in Germany but not in Flanders. Older firms are less likely to receive subsidies in Germany. Another small difference between Germany and Flanders is the effect of lagged labor productivity; it has a negative coefficient in both countries but the coefficient is only significant in the German data. Thus a “laggard” firm is more likely to receive subsidies in Germany than a high-productivity firm. Note that there is also heterogeneity across industries. Firms in hi-tech industries are, for instance, more likely to obtain subsidies than firms in low-tech industries. In addition there is heterogeneity across regions. In Germany, for instance, firms located in the “new” (Eastern) Länder are more likely to receive subsidies than firms in the Western German states. This is reflecting the still on-going efforts concerning the transformation of the Eastern German economy after the re-unification in 1990.

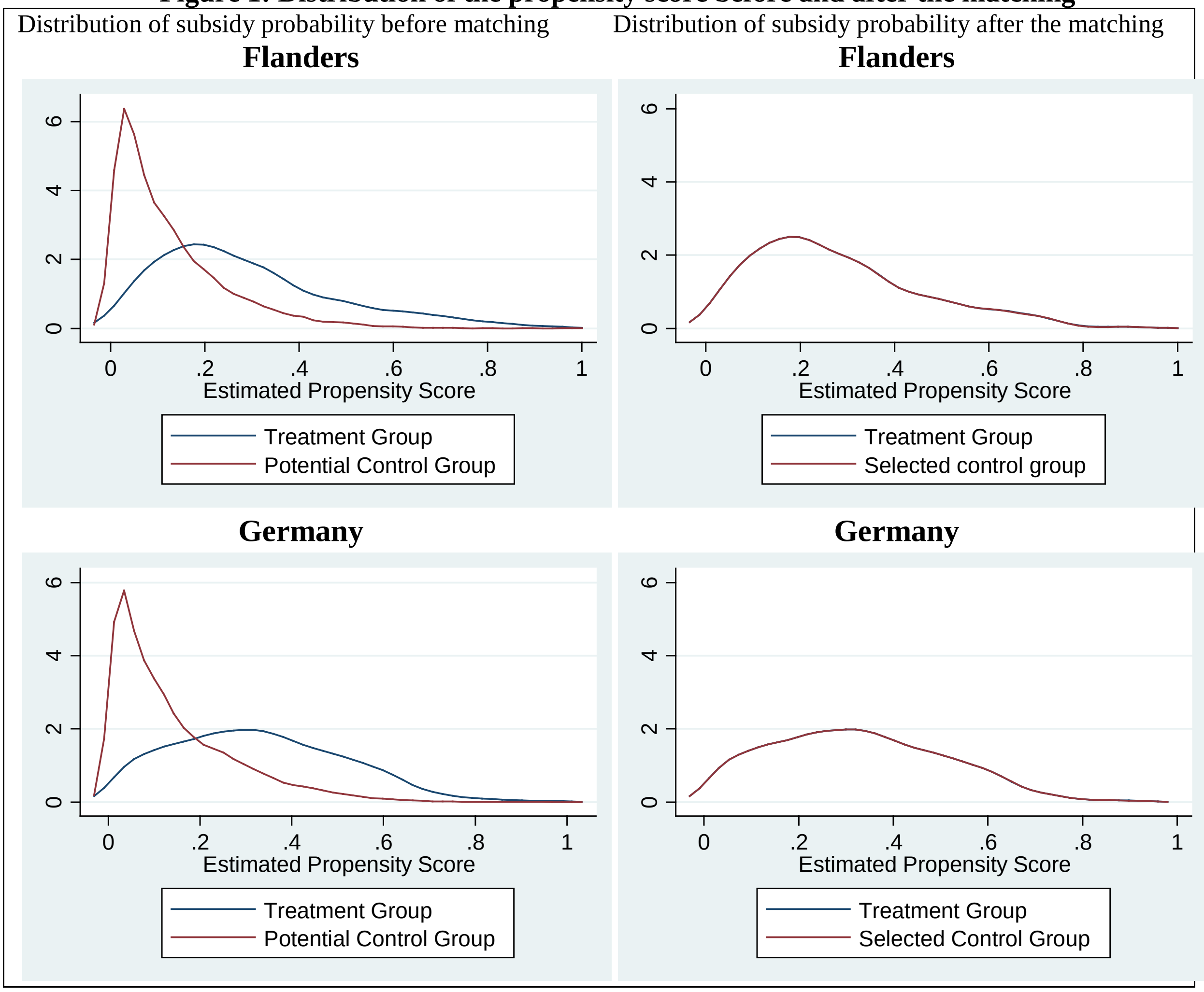
Table 3: Probit regression on subsidy receipt

Variables	Flanders		Germany	
	Coeff.	Std. err.	Coeff.	Std. err.
PS/EMP	6.260***	0.698	3.174***	0.273
lnEMP	0.299***	0.021	0.126***	0.012
FOREIGN	-0.351***	0.062	-0.221***	0.054
EXPORT	0.546***	0.057	0.703***	0.036
GP	0.051	0.058	-0.043	0.036
lnAGE	-0.038	0.032	-0.102***	0.017
LPROD	-0.051	0.070	-0.137*	0.083
Log-Likelihood	-1893.1		-5069.3	
McFadden R^2	0.18		0.19	
Number of obs.	5494		13503	

Note: Regressions include intercepts and full sets of time, region and industry dummies.
*** (**, *) indicate significance at the 1% (5%, 10%) level.

We now use these Probit models to predict for each firm the probability to receive funding. Subsequently we search for each subsidy recipient the nearest neighbour in the group of non-subsidized firms in terms of the estimated probability of receiving subsidies. Figure 1 shows the distribution of the estimated propensity scores before (left hand side) and after (right hand side) matching. As can be seen, the distributions differ significantly between subsidized firms and non-subsidized firms before matching. After the matching, however, the two distributions are basically equal for both groups. This leads to the two groups being balanced after matching, meaning that the means of the control variables are insignificantly different for the two groups (see Table 4).

Figure 1: Distribution of the propensity score before and after the matching



As Table 4 shows, the samples of treated firms and selected controls are well balanced in all covariates after the matching. However, as can be seen at the bottom of the table, the innovation input variables differ significantly between the two groups. If the assumptions of the matching estimator hold, this difference can be assigned to the fact that the treated firms received a subsidy. For Flanders, we find that the internal R&D intensity is 4.7% points larger for subsidized firms compared to the counterfactual situation where they would not have been subsidized (as measured by the average R&D intensity of the selected nearest neighbors, i.e. 5.8% - 1.1%).[13] The corresponding effect for the innovation intensity amounts to 7% points. For Germany, the effects are similar: 5.6% points for R&D intensity and 8.4% points for innovation intensity. Thus we can conclude, as has been found in several recent studies, that technology policy contributes significantly to R&D (i.e., there is not complete crowding out) and innovation investments at the firm level and thus can be expected to contribute to productivity growth in the long run. A key concern regarding the validity of our results is whether matching appropriately controls for differences between firms that receive and firms that do not receive subsidies. If it does not, our treatment effect could be biased. The traditional worry is that the estimated treatment effect could be biased upwards either because the firms that apply are likely to invest more in R&D even without a subsidy than firms in the control group, or because of the decisions of the civil servants deciding on the subsidies. For example, they could prefer "safe" projects that would be carried out anyway. The results of Takalo, Tanayama and Toivanen (2013) suggest that the bias could be downwards: they find that firms apply for subsidies for projects that are not as good as those for which they do not seek subsidies.

[13] Notice how this difference is smaller than that in the raw data where, for Flanders the difference is 0.059 – 0.007 = 0.052 or 5.2% and for Germany, 0.075 – 0.008 = 0.068 or 6.8%.

Table 4: Matching results

Flanders					
	Non-subsidized firms, N=801		**Subsidized firms, N=801**		**Results of t-tests on mean differences**
Variables	Mean	Std. dev.	Mean	Std. dev.	
Covariates					
PS/EMP	0.011	0.053	0.014	0.043	
lnEMP	4.493	1.323	4.507	1.461	
FOREIGN	0.300	0.458	0.287	0.453	
EXPORT	0.844	0.363	0.846	0.361	
GP	0.683	0.466	0.654	0.476	
lnAGE	3.184	0.811	3.190	0.781	
LPROD	0.275	0.329	0.289	0.309	
Outcome variables					
RDINT	0.011	0.035	0.058	0.116	*
INNOINT	0.021	0.056	0.091	0.149	*

Germany					
	Non-subsidized firms, N=2349		**Subsidized firms, N=2349**		**Results of t-tests on mean differences**
Covariates					
PS/EMP	0.029	0.086	0.028	0.067	
lnEMP	4.546	1.471	4.481	1.601	
FOREIGN	0.096	0.294	0.094	0.293	
EXPORT	0.800	0.400	0.792	0.406	
GP	0.443	0.497	0.410	0.498	
lnAGE	2.922	0.851	2.919	0.868	
LPROD	0.164	0.169	0.162	0.165	
Outcome variables					
RDINT	0.018	0.048	0.074	0.121	*
INNOINT	0.037	0.075	0.121	0.157	*

Note: * indicates a significance level of 5%. Note that the distribution across sectors, regions and years is also balanced after the matching. A detailed presentation is omitted here.

3.2.1 Discussion of the magnitude of the effect

The identified treatment effects appear to be large. For instance, the average firm more than doubles its R&D intensity in case of a subsidy receipt. A simple back-of-the-envelope calculation suggests that this result is not implausible. The median firm[14] would in the counterfactual situation have sales of about 15 million EUR in the Flemish case (we restrict this discussion to the Flemish case, as we lack information on the average size of a subsidy in Germany; a number that is used below to interpret the economic significance of the effect). With an R&D intensity of 1.1% (see Table 4), it would mean that the firm spends about

[14] We use the median concept here as averages might be somewhat misleading due to the skewness of distributions, i.e. most firms in both the sample and the economy are small or medium-sized but averages are to a large extent determined by a few very large firms.

165,000 EUR on R&D. In case of a subsidy receipt it would spend 870,000 EUR on R&D. The difference in EUR, i.e. the treatment effect, thus amounts to 705,000 EUR. In order to interpret the economic magnitude of this amount, we have to be compare it with the average grant a subsidized firm receives. In our data, the average size of a subsidy to a firm is 312,000 EUR[15]. If a subsidized project had an average subsidy rate of 50%, the firm would be expected to invest about 624,000 EUR when receiving a subsidy. This is fairly close to the estimated 705,000 EUR. The reason why the estimated effect is larger can be explained by the fact that, on average, the subsidy rate is in reality smaller than 50%.

This calculation, however, is based on the assumption that the subsidized project would not have been implemented without the subsidy (i.e., that the firm would only have conducted the counterfactual R&D amounting to 165,000 EUR. In order to test the robustness of our finding, we exclude the non-innovators from the (control) sample. This change in the control group raises the average R&D in the control group and makes it less likely that our calculation is affected by some firms not investing at all in R&D if they do not receive a subsidy. The treated firms show an R&D intensity of 5.7% [16] while the R&D intensity of the firms in the control group is 2.5% rather than 1.1%. Therefore, a treated firm in the counterfactual situation would now spend 375,000 EUR on R&D (15 million EUR sales times 0.025). In case of subsidy receipt, it would invest 855,000 EUR. The treatment effect thus amounts to 480,000 EUR. Against the background of the average size of the subsidy of 312,000 EUR (and co-financing of 50% of the total project cost), this scenario would still lead to significant additionality of technology policy The result also suggests that the subsidized project replaces to some extent other R&D, as if this was not the case, the treatment effect would be 624,000 EUR instead of 480,000 EUR. Note, however, that we are

[15] Source: authors' own calculation from the ICAROS database of the IWT Flanders; see the discussion in Czarnitzki and Lopes-Bento (2013) for further information

[16] This is slightly different from the 5.8% shown in the table above as the matching estimator could only match 792 firms appropriately rather than 801 as before. The reason is that the set of potential control firms changed.

at risk of underestimating the true treatment effect in this scenario as it might well be that especially smaller firms would not conduct R&D if they would not get subsidized, and this option is by construction ruled out in this scenario where only innovating firms are used in the control group.

To put these figures into a macroeconomic perspective, we perform the following calculation: In our second scenario where we only use the firms investing in R&D whether or not they receive a subsidy, we estimated that the median firm would invest 480,000 EUR more in R&D in response to a subsidy. In Flanders, a total of 2012 firms were awarded a subsidy during the period 2004 to 2010[17]. We therefore can extrapolate that the additionality effect of 480,000 EUR has been generated 2012 times. At the macro level this sums up to 966 million EUR of extra R&D. In total the government invested about 628 million EUR (i.e. 2012 times the average subsidy of 312,000 EUR). We thus find that privately funded R&D increased by 338 million EUR in 2004-2010 in Flanders as a results of the government subsidy program.

The 966 million EUR R&D invested by the Flemish firms as response to a subsidy amount to about 5.4% of total R&D conducted in Flanders during the years 2004 to 2010 (the total R&D is about 18 billion EUR; see Andries et al., 2011).

3.2.2 Heterogeneous treatment effects

We next investigate whether the subsidy generates heterogeneous treatment effects for different types of firms, We regress the estimated treatment effect, α, for each firm *i* in period *t*[18] on firm characteristics.

[17] See Czarnitzki and Lopes-Bento (2013) for more details on these numbers. In total, 3019 projects were funded but on average a firm had 1.5 projects at the same time. We thus use 2012 as number of funded firms here: 3019/1.5

[18] This is equal to the difference of the actual value of the variable of interest, e.g. R&D intensity of firm *i* in period *t*, minus its counterfactual value, that is, the R&D intensity of the selected control observation in period *t*.

For both countries (see Table 5), we find that firms with a larger patent stock havehigher treatment effects. In Germany, the coefficient of the patent stock is smaller than in Flanders, however. This may be explained by the fact that in Flanders less firms hold patents. Out of the Flemish subsidy recipients, 38% hold at least one patent. In Germany, this share is 47%. If we only compare the size of the patent stock among firms that have at least one patent, we find that the German firms hold, on average, about as twice as many patents as Flemish firms.

Another very interesting result is that the coefficient of labor productivity is negative and significant . Thus, the lower the labor productivity, the more the firms' R&D and innovation investments benefit from subsidies. This might support the view that constrained firms respond more positively to a subsidy.[19]

These two results may emphasize the need for customized subsidies for different types of firms. Compared to R&D tax credits, such case-by-case decision-making is a strength of R&D subsidies. One might conclude that a one-policy-fits-all strategy might not be the most efficient way of supporting private R&D.[20]

In addition to the results mentioned above, we also find some other, weakly significant effects, which are however not common to both countries, and not even the same in the R&D and the innovation intensity equations. Therefore we refrain from interpreting these in more depth. For instance, we find very weak evidence that group membership has some small positive effect on the treatment effect in Flanders, but in Germany the *GP* variable has the opposite sign and is insignificant. It is noteworthy that we find in both countries that firm size has a negative coefficient (but it is only significant at the 5% level in Germany). However,

[19] *LPROD* is measured as sales per employee and if this measure is low it might simply proxy low internal financial resources and thus financial constraints.

[20] Admittedly, a proper comparison of R&D subsidies to R&D tax credits would necessitate a full welfare comparison. Takalo, Tanayama and Toivanen (2010) provide a framework for such a comparison, and preliminary results.

we suggest that this is a consequence of us measuring the treatment effect of a subsidy on the R&D intensity as opposed to the level or R&D.

Table 5: OLS Regressions of estimated treatment effects on firm characteristics

	Flanders		Germany	
Variable	*α_RDINT*	*α_INNOINT*	*α_RDINT*	*α_INNOINT*
PS/EMP	0.502**	0.535**	0.181***	0.152**
	(0.206)	(0.228)	(0.053)	(0.066)
lnEMP	-0.004	-0.010*	-0.009***	-0.016***
	(0.004)	(0.005)	(0.002)	(0.003)
FOREIGN	0.022	0.028*	-0.002	0.002
	(0.014)	(0.016)	(0.010)	(0.012)
EXPORT	0.010	0.010	0.019**	-0.001
	(0.012)	(0.018)	(0.009)	(0.012)
GP	0.017*	0.014	-0.003	-0.008
	(0.010)	(0.014)	(0.008)	(0.009)
lnAGE	-0.012**	-0.014*	-0.001	-0.004
	(0.005)	(0.008)	(0.003)	(0.004)
LPROD	-0.052***	-0.059***	-0.054***	-0.086***
	(0.014)	(0.017)	(0.012)	(0.018)
Number of obs.	801	801	2349	2349
R^2	0.269	0.196	0.173	0.152

Note: The table shows the coefficient estimates and their standard errors (clustered at the firm level) in parentheses. All regressions include intercepts, and full sets of time, industry and region dummies (results not presented in detail). *** (**, *) denote a significance level at the 1% (5%, 10%) level.

3.2.3 Heterogeneous treatment effects with respect to the timing of the invention

Last but not least, we present a robustness test with respect to the “optimal” timing of the government intervention. Timing here refers to the stage of the innovation process. Traditionally, the innovation process is sketched as follows: first basic research is conducted, i.e. research without a particular goal for industrial applications; second, applied research may turn knowledge into industrial applications which in the third phase lead to the development of new products and/or processes. Of course there might be feedback from later stages of the innovation process to earlier ones, and these three different stages do not necessarily have to be conducted by the same economic agent. For instance, it might well be that a firm picks up knowledge from outside, and that it can use that to directly develop a new product. Thus, it might not be necessary for each innovator to maintain activities in all stages of the innovation process to eventually innovate.

We can address this question with the Flemish data, as the IWT categorizes the subsidized projects into basic research projects, mixed projects, and experimental development projects that are closest to the market. We use these three categories, that are not mutually exclusive, to describe the timing of the intervention in the innovation process.

Table 6: OLS regressions of estimated treatment effects on firm and project characteristics (Flemish data)

	Model I	Model II	Model III	Model IV
	Dependent variable:			
Variable	*α_RDINT*	*α_RDINT*	*α_INNOINT*	*α_INNOINT*
Basic	0.023	0.016	0.031	0.024
	(0.015)	(0.013)	(0.020)	(0.018)
Mixed	0.042***	0.016	0.056***	0.032
	(0.015)	(0.017)	(0.021)	(0.021)
Development	0.045***	0.026**	0.039**	0.017
	(0.014)	(0.012)	(0.018)	(0.016)
PS/EMP		0.407*		0.482*
		(0.238)		(0.274)
lnEMP		-0.001		-0.002
		(0.005)		(0.008)
FOREIGN		0.020		0.033
		(0.017)		(0.022)
EXPORT		0.010		0.007
		(0.016)		(0.022)
GP		0.016		0.009
		(0.012)		(0.017)
lnAGE		-0.012		-0.015
		(0.007)		(0.011)
LPROD		-0.068***		-0.075**
		(0.023)		(0.031)
Intercept	-0.004	0.029	0.007	0.050
	(0.016)	(0.041)	(0.020)	(0.060)
Number of obs.	566	566	566	566
R^2	0.029	0.287	0.021	0.217

Note: The table shows the coefficient estimates and their standard errors (clustered at the firm level) in parentheses. All regressions include full sets of time, industry and region dummies (results not presented in detail). *** (**, *) denote a significance level at the 1% (5%, 10%) level.

When regressing the treatment effect on just the variables characterizing the nature of the R&D project, we find (see Table 6, models I and III) that projects categorized as mixed and development yield highest additionality although the differences between the coefficients are not statistically significant: the only difference that is marginally significant (p-value 0.101) is the difference between basic research and development. However, when we add the firm characteristics used in the specifications reported in Table 6 (models II and IV), these effects disappear (all the more so when we test for differences between the coefficients). This suggests that the effects found in models I and II were driven by firm characteristics that are correlated with the nature of the R&D project(s). Our previous results on the effects of firm characteristics on the size of the treatment effect are intact.

4 Conclusions and policy implications

What are the implications one should draw regarding the monetary union and innovation policy? In our view it is clear that there is a link that comes about through two channels. The first link is between the monetary union and economic growth. The second link is between deeper economic integration and the effectiveness of innovation policy. Let us elaborate these next.

Our literature survey shows that there is link between investments in economic growth and investments in R&D. This raises the importance of innovation policies as they are designed to narrow the gap between the socially optimal and the privately optimal levels of R&D. Raising the level of R&D is going to lead to more product improvements in a Schumpeterian model of growth, and thereby to higher growth, though one may want to take into account the results of Segerstrom (2002) which show that this need not automatically happen.

A further motivation for the link between growth and R&D comes from the studies showing that R&D increases productivity, which is linked to innovation in the Schumpeterian growth models. Again, innovation policies help close the gap between the socially and privately optimal levels of R&D.

The second link comes through the observation that the monetary union is not only about monetary policy, but more generally about deepening integration within the union. The literature on exporting, productivity and R&D has shown that a lowering of trade barriers leads to productivity increasing investments, among them R&D and technology adoption. Innovation policy is naturally linked to these effects through R&D.

Our empirical investigation shows that the subsidies indeed foster R&D and innovation investment in recipient firms. We find positive treatment effects for internal R&D investment and total innovation expenditure.

As further analyses show, these positive treatment effects are heterogeneous over firms. We find that firms having more experience in inventing and thus innovating show larger treatment effects. Furthermore, firms' productivity is negatively associated with the size of the treatment effect. This could be interpreted as evidence that firms with lower labor productivity benefit more from subsidies than firms that are closer to the technological frontier. However, this does not rule out that subsidized R&D projects actually push the technological frontier upwards in the economy. We find that basic research, experimental development and prototyping projects have similar treatment effects. Thus, the stage of R&D seems not to affect additionality.

A link between innovation policy and macroeconomic policy that is often not getting enough attention is the main motivation for government support – spillovers. A national government should only take into account those spillovers, whether consumer surplus, knowledge spillovers to other firms, or anything else, that accrue to its citizens and firms. Such an approach could well lead to levels of R&D that are lower than what would be optimal from the point of view of the Union. The reason for this (see Toivanen 2008), is that it is likely that a large amount of those spillovers generated by an R&D project of a firm in a given EU country that do not flow directly into that country nonetheless flow to another EU country. Hence, at the level of the union, one would want to subsidize such a project more than what is optimal from a purely national point of view.

5 Methodological appendix

Modern econometric techniques addressing selection bias have been studied for many years now (see Heckman et al., 1999, Imbens and Wooldridge, 2009, for surveys). Different estimation strategies include the (conditional) difference-in-difference estimator, control function approaches (selection models), instrumental variable (IV) estimation, non-parametric (matching) techniques based on propensity scores and others such as regression discontinuity designs.

The difference-in-difference method requires panel data with observations before and after (or while) the treatment (change of subsidy status). As our database consists of four pooled cross-sections and not of a panel (most firms are observed only once), we cannot apply this estimator.

For the application of an IV estimator or a selection model, one needs a valid instrument (or an "exclusion restriction" in the selection model case) for the treatment variables. As finding valid instruments (or exclusion restriction) turns out to be very challenging in the present context, we primarily apply matching estimators in this study. Matching has the advantage to require no assumptions about functional forms and error term distributions. The downside, however, is that it only controls for the selection on observables. Hence, we have to maintain the assumption that we observe all important determinants driving the selection into program participation. [21]

Our fundamental evaluation question can be illustrated by an equation describing the average treatment effect on the treated firms, respectively:

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

[21] Matching estimators have been applied and discussed by many scholars. See e.g. Imbens and Wooldridge (2009), Smith and Todd (2005), or Blundell and Costa-Dias (2009) for econometric overviews, and Cerulli (2010) and Zúniga-Vincente (2012) for surveys in the context of innovation studies.

where Y^T is the outcome variable. 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. As previously explained, while $E(Y^T|S=1)$ is directly observable, it is not the case for the counterpart. $E(Y^C|S=1)$ has to be estimated. Because of a potential selection bias due to the fact that the receipt of a subsidy is not randomly assigned, $E(Y^C|S=1) \neq E(Y^C|S=0)$ and the counterfactual situation cannot simply be estimated as average outcome of the non-participants. Rubin (1977) introduced the conditional independence assumption (CIA) to overcome this selection problem, that is, participation and potential outcome are statistically independent for firms with the same set of exogenous characteristics X. In the case of matching, this potential "untreated outcome" of treated firms is constructed from a control group of firms that did not receive subsidies. The matching relies on the intuitively attractive 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. If the CIA holds, it follows that

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

and the average treatment effect on the treated can be written as:

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

In the present analysis, we conduct a variant of the nearest neighbour propensity score matching, namely caliper matching. More precisely, we pair each subsidy recipient with the single closest non-recipient. The pairs are chosen based on the similarity in the estimated probability of receiving such a subsidy, meaning the propensity score stemming from a probit estimation on the dummy indicating the receipt of subsidies S. Matching on the propensity score has the advantage not to run into the "curse of dimensionality" since we use only one single index as matching argument (see Rosenbaum and Rubin, 1983).

Furthermore, it is essential that there is enough overlap between the control and the treated group (common support). In practice, the samples of treated and controls are frequently restricted to common support. We thus calculate the minimum and the maximum of the propensity scores of the potential control group, and delete observations on treated firms with probabilities larger than the maximum and smaller than the minimum in the potential control group.

In order to avoid “bad matches”, we impose a threshold (a “caliper”) to the maximum distance allowed between the treated and the control unit. If the distance is above this pre-defined threshold, the treated observation is dropped from the sample to avoid bias in the estimation (see also Todd and Smith, 2005). The detail of our matching protocol is summarized in Table 5.

Table 1: The matching protocol

Step 1	Specify and estimate a probit model to obtain the propensity score $\hat{P}(X)$.
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 squared difference in propensity scores between this firm and all non-subsidized firms in order to find the most similar control observation. We use caliper matching, first introduced by Cochran and Rubin (1973). The intuition of caliper matching is to avoid "bad" matches (those for which the value of the matching argument Z_j is far from Z_i) by imposing a threshold of the maximum distance allowed between the treated and the control group. That is, a match for firm *i* is only chosen if $\|P_j - P_i\| < \varepsilon$, where ε is a pre-specified tolerance. We choose $\varepsilon = 0.01$.
Step 5	Select the observation with the minimum distance from the remaining control group. (Do not remove the selected controls from the pool of potential controls, so that it can be used again.) If the control group is empty after applying the caliper threshold, the treated firm is dropped from the sample and is not taken into account in the evaluation.
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).
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.

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