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Estimating the Local Average Treatment Effect of R&D Subsidies in a Pan-European Program

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Abstract

Most OECD countries have policy measures in place that aim to support private research and development (R&D) activities. The rationale for these public interventions lies in prevalent forms of market failure associated with the financing of R&D projects (Hall and Lerner, 2010). Limited possibilities to appropriate the returns of R&D investments (Arrow, 1962) and positive economic externalities of knowledge production (Bloom et al, 2013) result in a socially optimal level of R&D investment which exceeds the aggregate investment level provided by the market. Researchers that want to estimate the effect of public R&D subsidy programs on firm performance face a well-understood endogeneity problem (David et al, 2000). Grants are rarely allocated randomly. Firms self-select into applying for a subsidy based on an individual cost-benefit analysis. Moreover, public authorities usually choose the most promising projects among a pool of applicants. These two mechanisms create a positive selection of firms in a R&D subsidy program. Instrumental variable techniques that exploit quasi-random variation in treatment status to address the problem of selection are limited because suitable instruments are difficult to obtain in the context of R&D subsidies (Hussinger, 2008). Other popular econometric methods are nonparametric matching estimators (Almus and Czarnitzki, 2003; Czarnitzki and Lopes-Bento, 2013), differences-in-differences (Lach, 2002), regression discontinuity designs (Bronzini and Iachini, 2014), and estimation based on structural models (Takalo et al, 2013). Partly because of the variety of methodological approaches the findings on the effectiveness of R&D subsidies are mixed. The aims of this study were twofold: (1) to evaluate the effect of a large pan-European subsidy program, which promoted international research collaborations of small and medium-sized enterprises, and (2) to propose a new instrument based on a specific budget allocation rule within the program to overcome the aforementioned endogeneity problems. We used detailed administrative records about successful as well as unsuccessful applicants and complemented them with firm-level information from the Amadeus and Patstat database. We find no significant average treatment effect of R&D grants on firm growth in the complier population to our instrument. However, a subsequent analysis of treatment effect heterogeneity reveals that effects differ substantially according project quality scores. We conducted a counterfactual analysis and show that the program could have been much more effective under a different budget allocation rule which assigns grants to projects with a higher quality.

Jelcodes:O38,C31

Estimating the Local Average Treatment Effect of R&D Subsidies in a Pan-European Program

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We investigate the effect of Europe's largest multilateral subsidy program for R&D-performing small and medium-sized enterprises on firm growth. A specific budget allocation rule serves as an instrument and allows us to identify the local average treatment effect of public R&D grants. This rule, referred to as Virtual Common Pot (VCP), is designed to avoid cross-subsidization between participating countries. We compare the program's effect under the VCP rule with the standard situation of a Real Common Pot (RCP), where project authorities allocate a single budget according to uniform project evaluation criteria. Our estimates suggest no average effect on firm growth but treatment effects are heterogeneous and increase with project quality. Because funded projects are of lower average quality in a VCP, a job created by the program would have cost 27% less under an RCP.
Key words: Joint Programming Iniatives, R&D Policy, Virtual Common Pot, Instrumental Variable Estimation, Treatment Effect Heterogeneity
JEL classification: O38, H25, C31

1. INTRODUCTION

Most OECD countries have policies in place that aim to support private research and development (R&D) activities. The rationale for these public interventions lies in prevalent forms of market failure associated with the financing of R&D projects (Hall and

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Lerner, 2010). Limited possibilities to appropriate the returns of R&D investments (Arrow, 1962) and positive economic externalities of knowledge production (Bloom et al., 2013) result in a socially optimal level of R&D investment which exceeds the aggregate investment level provided by markets.

Researchers that want to estimate the effect of public R&D subsidy programs on firm performance face a well-understood endogeneity problem (David et al., 2000). Grants are rarely allocated randomly. Firms self-select into applying for a subsidy based on an individual cost-benefit analysis. Moreover, public authorities usually choose the most promising projects from a pool of applicants. These two mechanisms create a positive selection of firms with higher growth potential participating in an R&D subsidy program.

Instrumental variable techniques which exploit quasi-random variation in treatment status in order to address the problem of selection are limited because suitable instruments are difficult to obtain in the context of R&D subsidies (Hussinger, 2008). Other popular econometric methods are nonparametric matching estimators (Almus and Czarnitzki, 2003; Czarnitzki and Lopes-Bento, 2013), differences-in-differences (Lach, 2002), regression discontinuity designs (Bronzini and Iachini, 2014), and estimation based on structural models (Takalo et al., 2013). Partly because of this variety of methodological approaches the findings on the effectiveness of R&D subsidies in the literature are mixed.

The aims of this study are twofold: (1) to evaluate the effect of a large pan-European subsidy program, which promoted international research collaborations of small and medium-sized enterprises, and (2) to propose a new instrument based on a specific budget allocation rule within the program to overcome the aforementioned selection bias.

1.1. The Eurostars Joint Programming Initiative

Article 185 of the *Treaty on the Functioning of the European Union* enables the European Commission to coordinate and financially support subsidy programs for pre-commercial R&D that are jointly undertaken by several member states. These *Joint Programming Initiatives* (JPI) constitute a main policy tool to achieve an integration of the national innovation systems towards a *European Research Area* (ERA). They are also an integral part of the *Innovation Union Flagship Initiative*–Europe's 10-year strategy to foster innovation-led growth, competitiveness, and scientific excellence.[1]

The *Eurostars Joint Programme* (Eurostars hereafter) was launched in 2008 to support R&D-performing small and medium-sized enterprises (SME).[2] Until 2013, the program allocated a total estimated budget of EUR 472 million in ten applications rounds (called "cutoffs"). The 33 participating countries contributed financial resources of EUR 372 million and EUR 100 million came from the *7th Framework Programme for Research and Technological Development* on behalf of the European Commission. Eurostars was

[1] `https://ec.europa.eu/jrc/en/research-topic/research-and-innovation-policies`

[2] Eurostars defines an R&D-performing SME as a company with less than 250 employees, Either 10% of the work force (in full time equivalents) must be occupied with R&D activities or 10% of annual turnover must be dedicated to R&D.

coordinated by EUREKA, a research network of European countries[3] which aims at supporting pre-commercial but close-to-market R&D for civilian purposes. Its secretariat (ESE) is based in Brussels.

Projects in Eurostars needed to be conducted by an international consortium with at least two partners from different countries. The main applicant of a consortium was required to be an SME. However, larger companies and research-based partners such as universities and research institutes were allowed to be part of project consortia. In each cutoff, project proposals were subject to a central evaluation process. Applicants had to provide detailed information about the envisaged R&D activities, work packages, and expected costs. Based on this information, ESE carried out a basic eligibility check regarding administrative requirements. Subsequently, an application underwent an in-depth analysis by at least two independent technical experts. These experts rated projects according to three equally important quality criteria

1. Basic assessment: the consortium itself, its participants' capabilities, the project plan, and financial aspects

2. Technology and innovation: the R&D activities to be conducted in the project, the degree of innovation, and the technological profile

3. Market and competitiveness: the market opportunities (size, geography, potential, time and risk), return on investment, and strategic importance of the project

(see Eurostar's Final Evaluation Report, p. 18 f.). Based on the experts' reports projects were given an overall evaluation score, ranging from 0 to 600 (600 being the best score), and the proposals were ranked accordingly. Eurostars applied a general quality threshold of 400 below which proposals were not considered eligible for funding.

Participating countries in Eurostars were committed to allocate their earmarked budgets strictly according to the central evaluation ranking. However, there was no common program budget but rather R&D grants were allocated individually by every participating country only to their respective national applicants. Under this allocation rule, referred to as Virtual Common Pot (VCP), a project could only receive funding if financial resources were sufficiently available in all countries involved in the consortium. If only one country ran out of budget the entire project was rejected. These additional national budget constraints created variation in the funding of projects with nearly equal evaluation scores. For some projects budget constraints were binding whereas for other consortia, because the partners came from different countries, budget constraints were still slack. This stands in contrast to a Real Common Pot (RCP), under which all projects would be funded until a common budget is exhausted. Figure A.2 gives an impression about the variation in funding at different evaluation ranks introduced by a VCP. Funded projects have a lower average rank under a VCP than under an RCP.

We exploit the variation in funding-status for projects of comparable quality as an instrument in a nonparametric instrumental variable estimation. We further investigate

[3] The EU28 and five associated countries: Iceland, Israel, Norway, Switzerland, and Turkey.

treatment effect heterogeneity of R&D grants depending on project evaluation scores. Our results suggest that grants had no significant average effect on firm growth but treatment effects were higher for projects with higher quality. Increasing treatment effects cause a relative inefficiency of a VCP compared to an RCP because projects with lower scores are funded where grants are less effective. According to our estimates, a job created by the program under an RCP would have cost 27% less than under the VCP.

The remainder of the paper is organized as follows. Section 2 describes our empirical methodology and presents the data. Section 3 shows results of the estimation and the counterfactual analysis. Section 4 discusses the implications of our findings and concludes.

2. DATA AND SETUP

2.1. Nonparametric Instrumental Variable Estimation

In the presentation of the empirical model we follow the notation in Frölich and Lechner (2014). Let Y be an outcome variable, D be a binary treatment variable, and Z be an instrumental variable. Consider the general model (see Figure A.1 for a graphical representation)

$$\begin{aligned} Y &= \varphi(D, U_Y, U_{YD}, U_{YZ}, U_{YDZ}) \\ D &= \xi(Z, U_D, U_{YD}, U_{DZ}, U_{YDZ}) \\ Z &= \zeta(U_Z, U_{YZ}, U_{DZ}, U_{YDZ}) \end{aligned} \tag{1}$$

with φ, ξ, ζ being unknown functions and the U-variables being mutually independent random variables. This notation makes explicit that there are some influence factors that only affect one variable, U_Y, U_D, U_Z, some affect two at a time, U_{YD}, U_{DZ}, U_{YZ}, and some factors have an influence on all variables, U_{YDZ}. In the tradition of the potential outcome framework (Rubin, 1974, 1978), we define

$$Y_i^d = \varphi(d, U_{i,Y}, U_{i,YD}, U_{i,YZ}, U_{i,YDZ}), \qquad d = 0, 1$$

as individual i's potential outcome. Y^1 denotes a treated individual ($D = 1$) and Y^0 a non-treated individual ($D = 0$). For ease of notation we will in the following omit the individual's subscript when it causes no confusion.

The notion of Z being an instrumental variable follows from the exclusion restriction that Z is not an argument of the function φ. Consider the case of a binary instrument, $Z \in \{0, 1\}$. Then, there are four possible types T of individuals in the population depending on the state of Z

$$\begin{aligned} \text{Compliers } (T = c): &\quad D^{Z=0} = 0 \text{ and } D^{Z=1} = 1, \\ \text{Defiers } (T = d): &\quad D^{Z=0} = 1 \text{ and } D^{Z=1} = 0, \\ \text{Always-treated } (T = a): &\quad D^{Z=0} = 1 \text{ and } D^{Z=1} = 1, \\ \text{Never-treated } (T = n): &\quad D^{Z=0} = 0 \text{ and } D^{Z=1} = 0. \end{aligned}$$

The type T categorizes how the treatment status of an individual changes when the instrument changes. Compliers are those individuals that have a treatment status of one if the instrument is equal to one and zero otherwise.

The standard literature on instrumental variable estimation assumes U_{YZ}, U_{DZ}, and U_{YDZ} to be empty, i.e., Z is independent of potential outcomes and types

$$(Y^d, T) \perp\!\!\!\perp Z.$$

Under the additional assumption of ξ being monotone increasing in Z, such that there are no defiers[4], Imbens and Angrist (1994) show that the local average treatment effect (LATE) for the subpopulation of compliers is identified as

$$E[Y^1 - Y^0|T = c] = \frac{E[Y|Z = 1] - E[Y|Z = 0]}{E[D|Z = 1] - E[D|Z = 0]}. \tag{2}$$

In the case of Eurostars, we exploit as an instrument the fact that for some firms in a project consortium the respective national budget was exhausted. In this case all firms within the project consortium were denied funding. To follow the usual notation that treatment is monotone increasing in the instrument, we define $Z = 0$ as the case when at least one respective national budget was exhausted and $Z = 1$ when all respective national budget constraints were still slack.

In our setting, we have perfect compliance to our instrument. No project was funded when budget constraints were binding and all applicants took up funding if they were still sufficiently available. Thus, $D = Z$ and we can delete the second equation in (1) together with the variables U_{YD}, U_{DZ} and U_{YDZ} from the model. The denominator in equation (2), the definition of the LATE, becomes equal to one.

We acknowledge the possibility that there might be factors U_{YZ} that appear both in φ and ζ and therefore influence outcomes and the instrument alike. We assume that we observe all these confounding factors and call them X from here on to comply with the standard notation in the literature. Conditional on X Z is a valid instrument

$$Y^d \perp\!\!\!\perp Z \mid X.$$

However, this conditioning (also denoted as "balancing") is only feasible when the distribution of X has the same support for both values of the instrument. This assumption makes sure that the treatment propensity lies strictly between zero and one: $0 < \Pr(Z = 1|X) < 1$ (Heckman et al., 1998). Accordingly, we restrict our analysis to a region of common support defined as

$$S = \text{Supp}(X|Z = 1) \cap \text{Supp}(X|Z = 0)$$

to be able to make reasonable comparisons between treated and non-treated individuals. Integrating the difference in mean outcomes over X in the region of common support identifies the average treatment effect (ATE) for this region

$$E[Y^1 - Y^0|X \in S] = E[Y|Z = 1, X \in S] - E[Y|Z = 0, X \in S].$$

[4]Alternatively, ξ could be monotone decreasing in Z such that there only exist defiers but no compliers.

This quantity can be estimated by nearest-neighbor or propensity score matching. Because of perfect compliance the estimation procedure in the instrumental variable setup is equivalent to a "selection on observables"-approach (Cameron and Trivedi, 2005). However, our instrument is not able to shift treatment in all regions of $\text{Supp}(X)$. Most notably, we can estimate the treatment effect of Eurostars funding only for specific regions of the project evaluation score, where there is sufficient variation in the funding status (see Figure A.2). Because of this local definition of the treatment effect conditional on $X \in S$, for a well-defined subpopulation, we present our estimation strategy in the general framework of the LATE (Imbens, 2010).

We employ propensity score matching to alleviate the dimensionality problem (Heckman et al., 1998; Abadie and Imbens, 2012) of conditioning on multiple covariates. Rosenbaum and Rubin (1983) establish that balancing is feasible conditional only on the one-dimensional propensity score $\Pr(Z = 1|X) = P(X)$

$$Y^d \perp\!\!\!\perp Z \mid P(X).$$

We estimate $P(X)$ by Probit regression and match an observation with its nearest neighbor according to the estimated propensity score. Abadie and Imbens (2012) derive the large sample distribution of the propensity score matching estimator. Note that we assume that potential outcomes do not depend on the actual treatment exposure. Hence, the assumption of causal effect stability or stable unit treatment values (SUTVA) is fulfilled.

2.2. Data

We study the official Eurostars application records provided by the EUREKA secretariat (ESE). To assess the effect of R&D subsidies on firm performance, we combine our data set with Bureau van Dijk's *Amadeus* database which contains employment data until 2013. We restrict the analysis to applications until cutoff round 7 (which took place in September 2011) to allow for sufficient time for positive effects of the program to materialize. We drop from the sample all non-SME such as universities, research institutes or larger companies. In addition, we drop applications from Russia, Ukraine and Malta as they were few and Malta only joined the program in cutoff 6.

We construct a measure of employment growth (*Employment Growth*) between the year before application (t_{-1}) and 2013 divided by the number of elapsed years

$$\frac{Empl_{2013} - Empl_{t_{-1}}}{2013 - t_{-1}}.$$

When data is missing for the respective years we adjust this time window but make sure that at least two years have passed before we compare the difference in employment levels[5]. We motivate our choice of outcome variable by the fact that increasing

[5]We also studied the effect on compound annual growth rates and found qualitatively similar results. For the following counterfactual analysis, however, we found absolute employment growth to be a more conservative measure for out-of-sample predictions.

levels of employment are a general indicator of firm growth and competitiveness. Moreover, stimulating employment was an explicit program goal for public authorities (Final Evaluation Report, p. 10).

As a baseline set of covariates we condition on the technology class of a proposed project (*Technology Class*, see Table 1), the cutoff round (*Cutoff*) in which a project was applied for, and the number of employees one year before application (*Employment Start*). We further control for the cumulative patent stock (*Patent Stock*) of a firm at application date.[6] Most relevant for identification is to include the project evaluation scores (*Score*) in the matching. *Score* directly affects the treatment propensity of a project as well as it has a likely effect on potential outcomes. By comparing direct neighbors in the quality ranking of a cutoff we make sure that funded and non-funded projects are of similar quality.

The probability to receive funding in Eurostars was close to one for proposals that received high project evaluation scores because budgets for them were still slack. This violates the common support assumption introduced in Section 2.1. The same applies for project proposals which did not pass the quality threshold (*Score* below 400) as their treatment propensity was zero by design. We thus restrict our analysis to projects above the threshold that received a *Score* less than or equal to 510. In this range there was sufficient variation in funding status (see Figure A.2).

Our final data set contains 767 observations. Table 1 shows descriptives statistics of the variables in our sample. With an average number of 28 employees firms were relatively small when they applied to Eurostars. On average they hired 1.3 new employees per year which indicates a dynamic growth of these firms. However, *Employment Growth* shows a high variance in the sample. This is most likely the result of the diverging macroeconomic environment within Europe during the years 2008 to 2013. 64% of the firms in the sample received funding by Eurostars.[7] Projects predominantly came from the fields of information and communications technology (ICT) and engineering.

Unlike many other studies on R&D subsidies, our data allow to compare treated and non-treated firms that both applied for funding by Eurostars. Often there is only information about the identity of treated firms available and a control group needs to be drawn randomly from the population of all firms. However, firms select into applying for R&D subsidies based on the prospective gains of receiving a subsidy and thus based on potential outcomes (Takalo et al., 2013). Many firms refrain from ever applying to a program, especially when there are non-negligible application costs involved due to administrative requirements and the need to coordinate a joint proposal. The subpopulation of applicants to a subsidy program might therefore possess substantially different (unobserved) characteristics (Blanes and Busom, 2004). Our study avoids this potential source of confounding. As the relevant population of interest we define all potential applicants to a European *Joint Programming Initiative* targeted at R&D-performing

[6] We count patent applications at the European Patent Office since 1985 in the *PATSTAT* database. We also checked the effect of an annual discount rate of 15% on the knowledge stock and found very similar results.

[7] The average grant size was equal to EUR 204.375.

SME.

2.3. Instrument Validity

We argue that conditional on the evaluation rank of a firm (or equivalently on both *Score* and *Cutoff*) the availability of national budget resources is independent of potential outcomes. The Virtual Common Pot introduces variation in the funding status for proposals which were evaluated to be of the same overall quality by independent technical experts (see Figure A.2). The region of variation is larger than in, for example, studies that employ a Regression Discontinuity Design (Hahn et al., 2001), where there is only one threshold around which variation in treatment status can be exploited. Consequently, treatment effects can be identified for a larger share of the population. In addition, treatment status perfectly complies to our instrument which means there are no always-takers and never-takers in the region of common support. In the following we will discuss the validity of our instrument.

Although countries were able to adjust their budget contributions even after observing the evaluation ranking, they were committed to the allocation rule under the VCP. Thus, there was no room for discretionary treatment of selected firms by national agencies. However, since national budgets[8] vary in their size (relative to demand) the treatment propensity differs for firms from different countries even after controlling for *Scores*. Therefore, the treatment propensity could potentially be correlated with macroeconomic effects at the country level which affect potential outcomes. To avoid such a source of confounding, we condition on a set of country groups in the estimation of the propensity score (see Table 1).

The VCP also introduces variation in treatment within countries. Some firms did not receive funding because national budget of some partners in their consortium were exhausted. By contrast, for other firms from the same country all budgets were still slack. This mitigates the problem of cross-country comparisons. Nevertheless, the number of applications in some countries were too low such that we are forced to group countries together. Additionally, we control for the average *Growth Rate* of a country's GDP during 2008 until 2013 to allow for heterogeneity within the groups and to capture confounding effects of different growth trajectories.

There is a potential concern about the strategic choice of collaboration partners in order to maximize the funding probability. Firms with high growth potential could have chosen their partners with regard to the relative size of their national budgets; and such a strategic behavior could possibly bias our estimates. In the course of the official evaluation of Eurostars on behalf of EUREKA and the European Commission, the expert

[8]The size of national budgets and therefore the commitment to a multilateral R&D subsidy program such as Eurostars, also in relation to existing national programs, depends on various political reasons. It appears that not all countries were successful in forecasting the actual demand for funding. Especially large countries–which were attractive collaboration partners because of, e.g., market access–were likely to contribute too small budgets compared to the number of applications. There is no clear correlation in the data between relative budget size, on the one hand, and the size of the country or its exposure to credit market risks in the European sovereign debt crisis, on the other hand (see also the Final Evaluation Report, Figure 6-10).

Table 1: Summary statistics

	Mean	Std. Dev.	Min.	Max.
Employment Growth	1.294	6.295	-57	77.667
Employment Start	28.072	44.037	1	375
Funding	0.644		0	1
Score	444.581	29.358	400	510
Patent Stock	10.952	28.844	0	141
Growth Rate	-0.219	1.143	-5.213	2.7
Self-funding	0.09		0	1
Technology Class:				
ICT	0.319		0	1
Engineering	0.327		0	1
Bioscience, Pharma & Chemistry	0.219		0	1
Other	0.134		0	1
Cutoff Dummies:				
Cutoff 1	0.168		0	1
Cutoff 2	0.141		0	1
Cutoff 3	0.155		0	1
Cutoff 4	0.125		0	1
Cutoff 5	0.128		0	1
Cutoff 6	0.158		0	1
Cutoff 7	0.125		0	1
Country Groups:				
DE	0.188		0	1
FR	0.081		0	1
IT	0.073		0	1
UK, IE	0.025		0	1
NL, BE, LU	0.117		0	1
AT, CH	0.066		0	1
FI, SE, NO, DK	0.169		0	1
GR, PT, ES	0.175		0	1

N = 767

group in charge conducted an online survey about firms' experience with the program. One item in the survey[9] was concerned with the question of strategic partner choice. Respondents were asked to state their level of agreement to the following statement on a five-point Likert scale:

> "We chose our project partners strategically from certain countries because we believe that differences in national budgets affect the probability to obtain funding by Eurostars."

15.9% of the responding firms in our sample agreed with the statement that they chose their project partners specifically from certain countries. 4.6% indicated a strong agreement with the statement. Around 79% of firms had a neutral opinion, disagreed or disagreed strongly. Other reasons of partner choice, such as technology transfer, access to new markets, or previously existing business relationships were much more important to respondents (Final Evaluation Report, Figure 6-5). In addition, even if firms were aware of the specific allocation rules within a VCP, information about national budgets was not public. Because there was no clear pattern between relative budget size and indicators such as country size or geography, it is questionable whether firms were able to effectively manipulate their chance to obtain funding by strategic partner choice.

Firms from countries with exhausted budgets had the possibility to self-fund their part of the project. That way, the other consortium member could still obtain funding from their national authorities. In around 9% (see Table 1) of the cases one partner in a consortium exercised this option. The decision to self-fund might be based on private information about the quality of a project and could thus be a sign for a very profitable venture. To hedge against a bias in our estimation we control for whether a consortium member decided to finance the project without public support.

3. EMPIRICAL RESULTS

3.1. Results of the Propensity Score Matching

Table 2 reports results of the propensity score matching. We present three specifications: (1) with the baseline set of covariates, (2) includes the *Country Groups*, and (3) additionally incorporates the average *Growth Rate* of GDP and *Self-Funding* by firms in a consortium.

We apply a trimming method suggested by Crump et al. (2009) to guard against limited overlap. We only consider observations with an estimated propensity score in the interval $[0.02, 0.98]$ for the nearest-neighbor matching. Because the treatment propensity converges to one for high evaluation ranks (see Figure A.2) we thereby lose up to 34 observations from our sample in specification (3). Figure A.3 shows that this conservative

[9] Details about the survey can be found in the final report of the expert group (Final Evaluation Report, 2014). Important for our purposes is that there was a reasonably high response rate by applicants and especially by both, funded and non-funded firms.

Table 2: Propensity score matching results

	(1)	(2)	(3)
$E[Y^1 - Y^0 \| X \in S]$	0.607	0.323	0.900
	(0.490)	(0.744)	(0.797)
Score	✓	✓	✓
Employment Start	✓	✓	✓
Cutoff	✓	✓	✓
Technology	✓	✓	✓
Patent Stock	✓	✓	✓
Country Groups		✓	✓
Growth Rate			✓
Self-funding			✓
Observations	761	738	733

Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, $^{***}p < 0.01$

Note: Estimated average treatment effect of funding in the region of common support for three different sets of covariates. We discard observations with an estimated propensity score outside the interval $[0.02, 0.98]$ as it was suggested by Crump et al. (2009) to guarantee sufficient overlap.

approach results in a good overlap of the distribution of covariates for both treatment levels. Table A.1 reports the Probit propensity score estimation results.

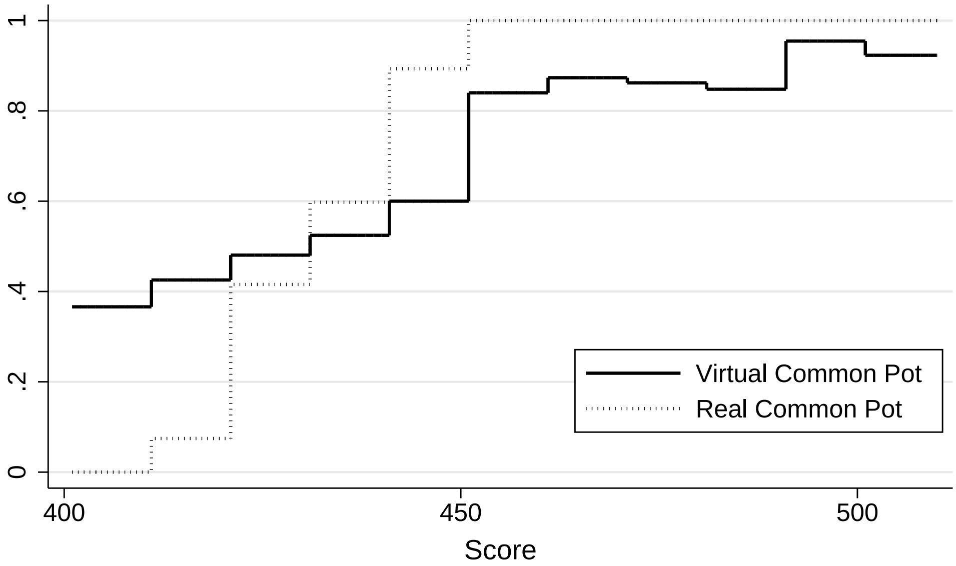
In all three specification we find no significant average treatment effects of R&D subsidies on employment growth. Point estimates vary a bit depending on the set of covariates used in the estimation of the propensity score. Standard errors are quite large which reflects the substantial variation in the outcome variable that already shows up in the descriptive statistics.

3.2. Counterfactual Situation Under a Real Common Pot

Because of the additional national budget constraints under a Virtual Common Pot (VCP) compared to a Real Common Pot (RCP), the average project evaluation score of funded projects under a VCP is lower. We compute the counterfactual situation when the same total budget received by firms in our sample would have been allocated according to an RCP. Figure 1 depicts the treatment propensities depending on project evaluation scores. One can see that starting from a score of around 440, the probability to receive funding is higher under an RCP than under a VCP.

This fact creates a relative inefficiency of a VCP compared to an RCP when treatment effects are heterogeneous and increasing in evaluation scores. We analyze treatment effect heterogeneity for different scores by a two-step regression method. First, we perform a

Figure 1: Treatment propensity



Notes: Sample probabilities to receive funding at different project evaluation scores under a VCP compared to an RCP. Probabilities are calculated within 11 equidistant bins of *Scores*.

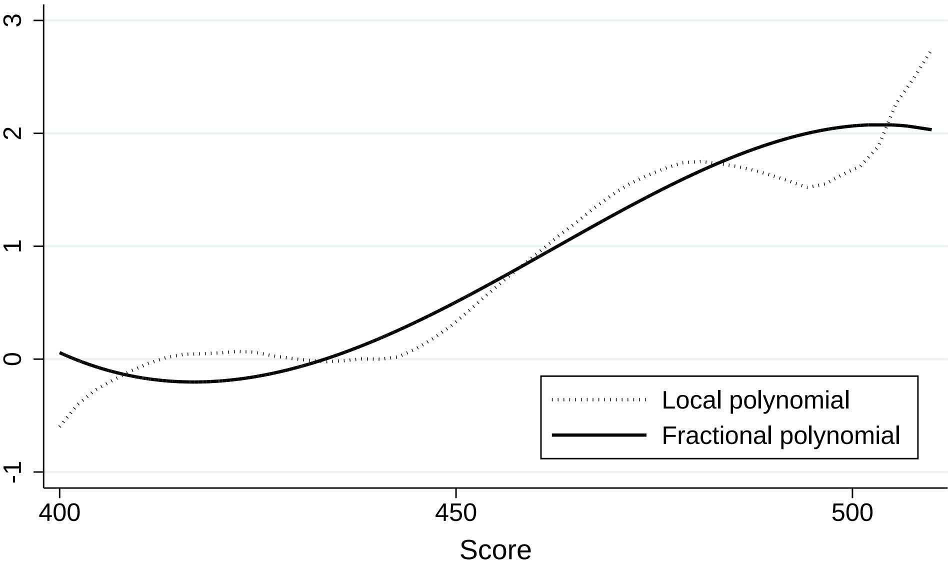
nearest-neighbor matching on the entire covariate vector, given in Table 2 specification (3), instead of the propensity score.[10] Then, we compute the difference in employment growth between matched neighbors and regress these on the *Score* to asses how treatment effects vary with project quality. Appendices A.3 and A.4 provide further details on the method.

Figure 2 shows results for a local linear regression (LLR, Fan and Gijbels, 1996) and a fractional polynomial regression (FPR, Royston and Altman, 1994). The latter is an OLS regression on a higher-order polynomial, which additionally allows for logarithms and non-integer powers of the regressor. One can see that the insignificant average treatment effect conceals a substantial effect heterogeneity depending on project qualities. Up to a score of around 430, employment growth induced by the R&D grants is basically zero. For proposals with higher scores the treatment effects become larger, up to an average of around 2 employees hired per year. In the following we will interpret the results of the FPR because it is smoother than the LLR. Presumably this is an artifact of our limited sample size.

Given the estimated relationship between treatment effects and evaluation scores, a reallocation of the budget according to an RCP increases the number of jobs created by the program because projects with higher average scores receive funding. Table 3

[10] Table A.2 shows these matching results, which at the same time serve as a robustness check for our previous propensity score matching results.

Figure 2: Treatment effect heterogeneity



Notes: Solid line: Fractional polynomial regression (Royston and Altman, 1994) of treatment effects on project scores with powers (2, 3, 3). Dotted line: local polynomial regression of treatment effects on project scores. Degree = 1, kernel = epanechnikov, bandwidth = 12.6.

Table 3: Counterfactual employment growth under a VCP vs. RCP

	VCP	RCP	Mixed Mode
Number of funded firms	460	448	441
Average score of funded firms	451.0	459.4	454.3
Grant size divided by created jobs (EUR)	97,899	71,813	76,574

Notes: Counterfactual situation given treatment propensities (Figure 1) and average treatment effects (Figure 2) when total budget is allocated according to an RCP. Mixed mode allocation assumes 25% of the total budget in a cutoff round are allocated according to an RCP. The remaining national budgets are allocated according to the VCP funding rule. Employment growth is computed per firm from application year until 2013.

shows results of a counterfactual analysis. In the factual situation under the VCP a job was, on average, created by a grant of EUR 97,899. Under an RCP the program would have been considerably more cost-effective, with a ratio of total budget to created jobs of EUR 71,813. This amounts to a reduction in costs of around 27%.

Because a switch to one common budget under an RCP might not be politically feasible, we investigate the effectiveness of a mixed funding allocation. The European Commission committed itself to contribute EUR 100 million to the Eurostars Joint Programming Initiative if the participating countries would provide a budget of at least EUR 300 million (Final Evaluation Report, p. 12) by themselves. This money was used to supplement the national budgets. By contrast, we study the effectiveness of the program if 25% of the allocated budget would have been allocated according to an RCP. The European Commission could have allocated its share of the Eurostars budget strictly according to the evaluation ranking without taking national budget constraints into account. The remaining 75% provided by the participating countries still would have been allocated according to the VCP rule.

Table 3 shows that such a mixed mode works surprisingly well in our sample. A job created under this regime costs 77,710 and is therefore around 22% cheaper than under a VCP. The major part of the impact on employment growth comes from projects with high evaluation scores (Figure 2). A mixed mode makes sure that all these projects receive funding. On the contrary, this result demonstrates that a large part of the spent budget is ineffective in fostering employment growth. Although a mixed mode allocates grants to projects of lower average quality compared to an RCP, the resulting employment growth is at a comparable level as long as only the highest ranks are funded. For the remaining 75% of the budget it does not make much of a difference whether they are allocated according to a VCP or an RCP.

4. DISCUSSION

The independent variation in treatment status, which is induced by a Virtual Common Pot, allows us to identify the local average treatment effect of public R&D grants on firm growth. We find no significant average effect but treatment effects are heterogeneous.

The effectiveness of grants increases with the project evaluation score. A counterfactual analysis reveals that a VCP imposes substantial additional program costs compared to an RCP. A mixed budget allocation rule, by contrast, avoids parts of these extra.

Our study tackles several of the methodological problems researchers encounter when estimating the effectiveness of R&D subsidies. Frequently, public authorities only publish data about funded projects. In these cases, researchers usually rely on matched control samples from the population of all firms by imposing a selection-on-observables assumption. However, firms apply for R&D grants based on potential outcomes. Applicants differ in important, potentially unobserved, characteristics from non-applicants. We avoid this problem by restricting our analysis to firms that applied to Eurostars.

We propose a new instrument, based on the specific VCP budget allocation rule, that deals with the problem of cream-skimming. Public authorities are held accountable to make good use of taxpayers' money. They therefore rarely allocate subsidies randomly but rather try to choose the best projects from a list of applications. In a VCP cream-skimming is partly offset by the additional national budget constraints and grants are not solely allocated based on quality.

Our identification strategy relies on data about project evaluation scores. Knowledge about the authority's quality ranking would also allow to identify local causal effects in a regression discontinuity research design (see Bronzini and Iachini, 2014, for a recent application). However, we are able to estimate effects for a larger share of the population as we can exploit variation not only at one threshold, where treatment status switches, but in a wider region of the evaluation ranking.

A limitation of our data is that we are only able to study the effect of subsidies on one outcome variable. Other studies in the literature often consider the effect on R&D investment (Zúñiga-Vincente et al., 2014). Unfortunately, information about R&D expenditures by firms is very scarce in the Amadeus database used in this paper. We thus cannot contribute to this strain of the literature which is concerned with the input additionality of subsidy programs. Instead, we look at the output additionality of Eurostars in the policy-relevant dimension of firm growth and competitiveness.

The instrumental variable approach we pursue naturally gives rise to questions about the external validity of our results. We are only able to estimate local treatment effects in regions of the covariate support where our instrument has the power to manipulate the funding status. Indeed, we are completely agnostic about the size and sign of treatment effects in other regions of the evaluation ranking (i.e., below the quality threshold or above a score of 510). This is specifically related to a certain country composition in the region of common support.

As we argued earlier, the reasons why countries within Eurostars ran out of budget were not unidimensional and also not constant over time. There was no systematic pattern present, such as, that national budgets of small countries with a below average GDP growth were exhausted the earliest. Instead, large countries often provided too few resources compared to the amount of applications they received. Also, actual demand for grants was difficult to forecast, as it varied over time. Consequently, our instrument is powerful enough to manipulate treatment status over a wide support of the geographical distribution in the sample. In any case, the region of common support, where the VCP introduces variation in treatment, is exactly the region of the evaluation ranking relevant for a comparison of a VCP versus an RCP.

Our results have important implications for innovation policy. In 2014, the successor program Eurostars 2 was launched under the European *Horizon 2020 Framework Programme*.[11] Its earmarked budget increased significantly compared to Eurostars 1 with a contribution by the European Commission of EUR 287 million over six years. In addition, other *Joint Programming Initiatives*[12] are organized in a similar way as Eurostars, also as a Virtual Common Pot.

A VCP is designed to provide incentives for countries to contribute national budgets to a pan-European program and to avoid free-riding. Policy makers face important political constraints when they try to harmonize their innovation policies and promote cross-boarder research projects. In particular, countries are most concerned that their taxpayers' money is used to subsidize research in other EU states and that they get less out of a program than they are paying into it. This issue is usually discussed under the heading "just return" in the European policy sphere. A VCP avoids these problems and is a tool to combine the need for further integration with the EU's highly federal structure.

However, our results illustrate that such a federal structure imposes non-negligible costs. Eurostars would have been much more cost-effective if it had been organized as a Real Common Pot. For a substantial share of the funded projects grants had no additionality effect on firm growth. As a consequence, we also find no significantly positive average effect of the program. We emphasize though that this conclusion should be taken with a grain of salt as we do not estimate treatment effects for the last three cutoff rounds within Eurostars 1 and we are not able to say anything about treatment effects for projects with very high evaluation scores outside of the common support.

A switch from a VCP to an RCP, although desirable from the point of view of efficiency, might not be politically feasible. We propose a mixed mode between a VCP and RCP to allocate joint research budgets. Under article 185 of the *Treaty on the Functioning of the European Union* the European Commission is allowed to contribute its own financial resources to Joint Programming Initiatives. In Eurostars the EC's earmarked contribution was 25%. If this budget had been allocated to the highest-ranked firms instead of supplementing the national budgets, a larger share of projects where grants show a high degree of additionality would have been funded. Because the remaining share of the total budget still could have been allocated according to the VCP, discussions about "just return" could have been avoided.

A mixed mode works extremely well in our sample and comes close to the first-best allocation under an RCP. However, this is a consequence of the shape of the estimated relationship between treatment effects and project evaluation scores. Treatment effects

[11] http://ec.europa.eu/programmes/horizon2020/en/h2020-section/eurostars-programme

[12] Ten of them are currently ongoing. They aim to support research in fields of high relevance for society such as, e.g., climate change, antimicrobial resistance, or Alzheimer research. http://ec.europa.eu/research/era/joint-programming-initiatives_en.html

are only positive for the highest-ranked projects. A mixed mode makes sure that these projects are funded. For the remaining projects with low treatment effects it makes no significant difference whether they are funded under a VCP or an RCP. This does not need to be the case in general, e.g., when treatment effects are increasing linearly in project quality. Still, we think that a mixed mode, especially when the common RCP share comprises a significant part of the total budget, is more suitable than a pure Virtual Common Pot to trade-off between economic efficiency and political feasibility.

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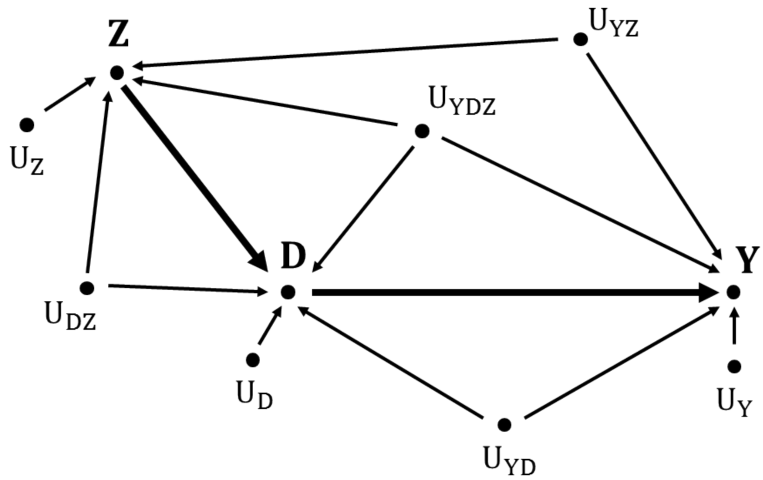
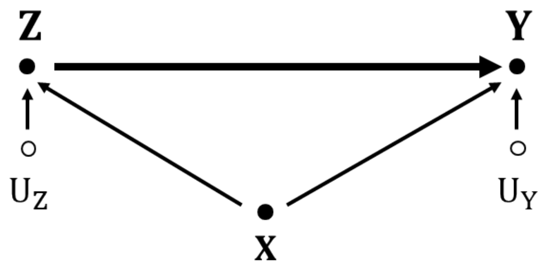
A.1. Additional Figures

Figure A.1: Directed acyclic graph of the empirical model



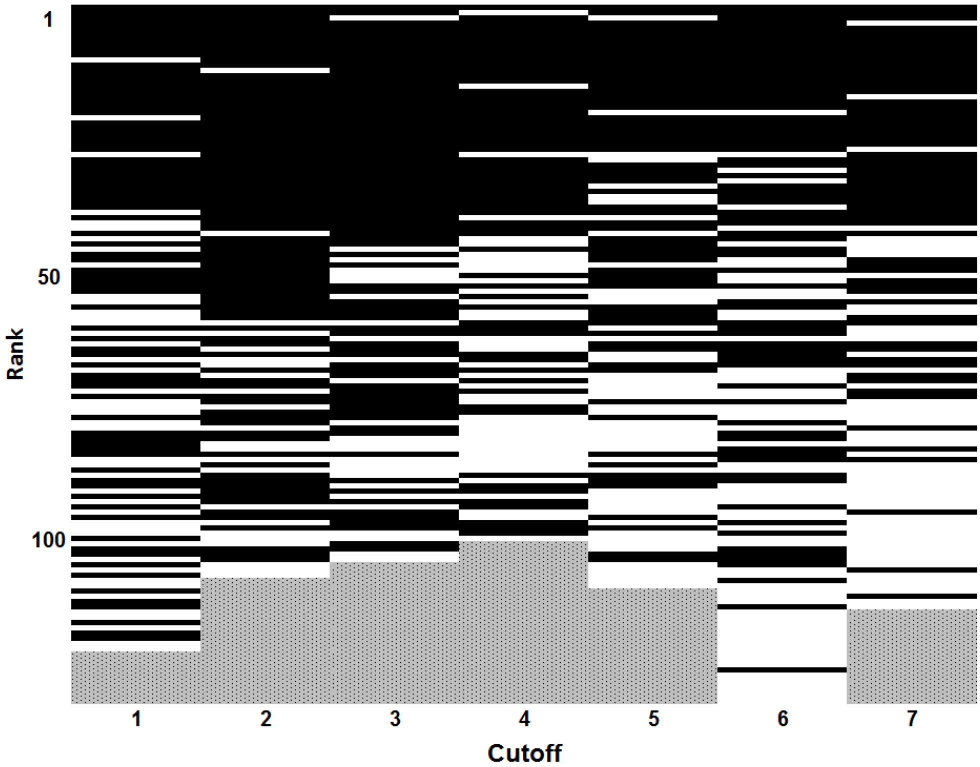
(1)



(2)

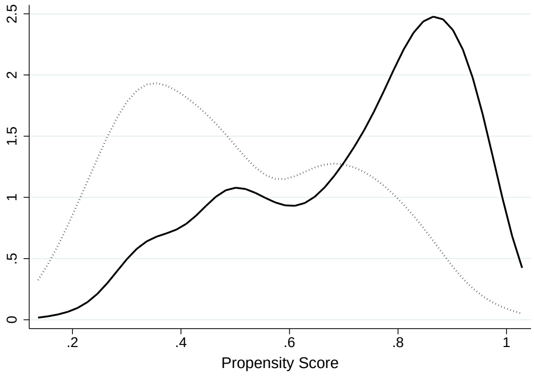
Notes: A graphical representation of our empirical model in equation (1) according to Pearl (2009). Arrows denote the direction of causal relationships between variables. The top panel shows the full model and the bottom panel depicts the reduced graph when we take our identifying assumptions and perfect compliance to the instrument into consideration. Hollow circles are used to indicate unobserved variables in the reduced graph.

Figure A.2: Funding in a Virtual Common Pot

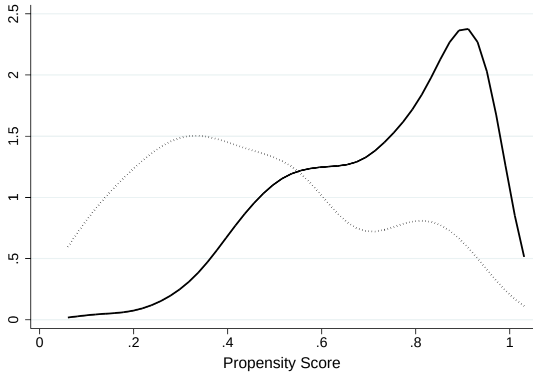


Notes: Funding status of Eurostars projects depending on their rank in the quality evaluation of a given cutoff. Black cells denote funded projects, white cells stand for projects that did not receive funding and gray cells indicate that projects did not pass the eligibility threshold. The figure illustrates that there is sufficient variability in the funding status induced by the VCP.

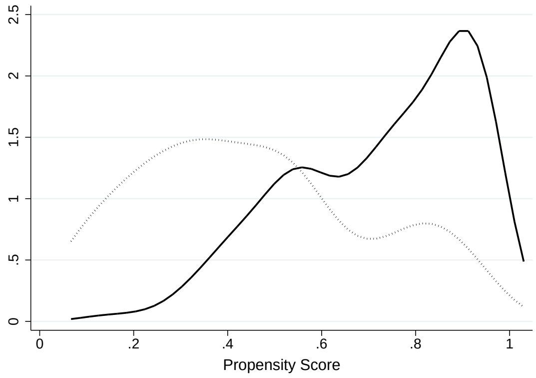
Figure A.3: Overlap plots



(1)



(2)



(3)

Notes: Kernel estimates of the distribution of propensity scores for the matching results in Table 2. Solid line: density of the predicted probability that a funded firm is assigned to funding ($f_{P,D=1}$). Dotted line: density of the predicted probability that a non-funded firm is assigned to funding ($f_{P,D=0}$). Kernel= gaussian, bandwidth = plug-in estimate (Silverman, 1986).

A.2. Additional Tables

Table A.1: Propensity score model

	(1)		(2)		(3)	
Score	0.02***	(0.00)	0.03***	(0.00)	0.03***	(0.00)
Employment Start	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)
Patent Stock	0.00	(0.00)	0.00	(0.00)	0.00	(0.00)
Cutoff 2	-0.15	(0.18)	-0.10	(0.19)	-0.11	(0.19)
Cutoff 3	0.35*	(0.18)	0.48**	(0.19)	0.47**	(0.20)
Cutoff 4	-0.16	(0.19)	-0.09	(0.20)	-0.11	(0.20)
Cutoff 5	-0.05	(0.18)	-0.00	(0.20)	-0.03	(0.20)
Cutoff 6	-0.24	(0.17)	-0.26	(0.18)	-0.28	(0.19)
Cutoff 7	-0.50***	(0.19)	-0.39**	(0.20)	-0.41**	(0.20)
Technology Class 1	0.13	(0.17)	0.11	(0.17)	0.12	(0.17)
Technology Class 2	-0.01	(0.16)	-0.03	(0.17)	-0.02	(0.17)
Technology Class 3	-0.01	(0.18)	-0.07	(0.18)	-0.05	(0.18)
FR			0.91***	(0.23)	0.87***	(0.23)
IT			1.08***	(0.25)	0.87***	(0.30)
UK, IE			-0.02	(0.36)	-0.09	(0.37)
NL, BE, LU			0.52***	(0.20)	0.42**	(0.22)
AT, CH			0.96***	(0.25)	0.96***	(0.25)
FI, SE, NO, DK			0.72***	(0.18)	0.73***	(0.18)
GR, PT, ES			0.98***	(0.18)	0.77***	(0.25)
EU since 2004			1.22***	(0.21)	1.12***	(0.22)
Growth Rate					-9.44	(7.71)
Self-funding					-0.16	(0.18)
Constant	-9.85***	(0.89)	-12.52***	(1.03)	-12.40***	(1.04)
Observations	767		767		767	

Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Probit regression of treatment status on covariates. Fitted values are used in nearest-neighbor propensity score matching. Base categories: Germany (country groups) and Cutoff 1.

Table A.2: Nearest-neighbor Matching on Multidimensional Covariate Vector

	(1)	(2)	(3)
Average Treatment Effect	0.395	0.027	0.497
	(0.500)	(0.476)	(0.446)

Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, $^{***}p < 0.01$
Note: Specifications are analogous to Table 2. The Mahalanobis distance is used as metric and a bias-correction according to Abadie and Imbens (2011) is performed.

A.3. Heterogeneous Treatment Effects

Applied researchers are often interested in how treatment effects of R&D subsidies vary with other observed characteristics such as firm size or age (Czarnitzki and Lopes-Bento, 2013; Hottenrott and Lopes-Bento, 2014b,a; Balsmeier and Pellens, 2015). To explore treatment effect heterogeneity, some researchers perform a propensity score nearest-neighbor matching (PSM) and subsequently regress the difference in outcome variables of matched pairs, $(Y_i^1 - Y_{i,nn}^0)$, on a covariate of interest, W. In the following we show that it is preferable to first match on the entire covariate vector rather than the one-dimensional propensity score when using this method. We additionally perform a Monte Carlo simulation to substantiate our argument.

Let $X = (X_{-W}^{'}, W')^{'}$ be the entire vector of covariates and consider the setting of Section 2.1 in which we restrict attention to the region of common support $X \in S$ where compliance is perfect. Given the conditional independence assumption

$$(Y^1, Y^0) \perp\!\!\!\perp Z|X$$

we are able to estimate the (local) average treatment effect of Z on Y conditional on the one-dimensional propensity score (Rosenbaum and Rubin, 1983)

$$p(X) = Pr(D = 1|X).$$

By the law of iterated expectations it holds that

$$\tau = \mathbb{E}[Y^1 - Y^0] = \mathbb{E}[\mathbb{E}[Y^1 - Y^0|p(X)]].$$

Consider for simplicity that all elements of X are discrete such that exact matching is possible. Under treatment effect heterogeneity, researchers are interested in the average treatment effect conditional on W

$$\tau(W) = \mathbb{E}[Y^1 - Y^0|W = w].$$

because $\tau(W = w) \neq \tau(W = w')$ for $w \neq w'$. In an exact matching on the whole covariate vector, matched pairs (asymptotically) share the same values for all elements of X. The object of interest can then be identified as

$$\mathbb{E}[Y^1 - Y^0|W = w] = \mathbb{E}_{X_{-W}}\left[\mathbb{E}[Y^1|X_{-W}, W = w] - \mathbb{E}[Y^0|X_{-W}, W = w]\right].$$

where the expectation is taken over all elements of X excluding W. For binary W an OLS regression of the matched pairs' differences in Y on W indeed estimates this conditional expectation.

For the propensity score matching, however, observations and their matched neighbors do not necessarily share the same values for all components of X. The PSM only creates a control group of matched observations with the same distribution of X. Because covariates are balanced in this sense, a confounding effect of X is eliminated *on average*. But pairs matched according to the propensity score might possess very different values of W. Averaging differences for observations with $W = w$ over the distribution of $p(X)$ only fixes W for the original observation and does therefore not identify the object of interest

$$\begin{aligned}\mathbb{E}[Y^1 - Y^0|W = \mathrm{w}] \neq \mathbb{E}_{p(X)}\Big[&p(Z = 1)\left\{\mathbb{E}[Y^1|p(X), W = w] - \mathbb{E}[Y^0|p(X)]\right\}\\ &+ p(Z = 0)\left\{\mathbb{E}[Y^1|p(X)] - \mathbb{E}[Y^0|p(X), W = w]\right\}\Big].\end{aligned}$$

For continuous covariates the same basic argument applies although exact matching is not feasible and the matching becomes more fuzzy. In addition, a bias-correction proposed by Abadie and Imbens (2011) should be considered because matching estimators are not $N^{1/2}$-consistent and contain an additional bias term that depends on the number of continuous covariates (Abadie and Imbens, 2006). In the following section, we conduct a Monte Carlo simulation to compare the performance of our proposed method with the standard procedure used in the literature.

A.4. Monte Carlo Results

Let $(\varepsilon_0, \varepsilon_1, X_1, X_2)$ be independently standard normal. We consider two cases: (1) $W = 1(\varepsilon_D > 0.5) - 0.5$ is binary with $\varepsilon_D \sim \mathcal{U}(0, 1)$, and (2) $W \sim N(0, 1)$ is continuous. We specify treatment heterogeneity to be linear in W: $\tau(W) = 0.5 \cdot W + 1$. Thus, the average treatment effect τ is equal to one and a linear regression should estimate the heterogeneity parameter, $\tau(W) = 0.5$, consistently for both discrete and continuous W. We parametrize the binary treatment indicator Z and the outcome variable Y as follows

$$Z = 1(-0.5 + W + X_1 - 0.3 \cdot X_1^2 + \varepsilon_0 > 0) \quad (3)$$

$$Y = 1 + \tau(W) \cdot Z + \exp(X_1 + 0.1 \cdot X_2^2) + X_2^2 + \varepsilon_1 \quad (4)$$

We compare three estimation methods to explore treatment effect heterogeneity. First, we estimate an OLS regression with interaction terms

$$Y = \beta_0 + \beta_1 Z + \beta_2 W + \beta_3 Z \cdot W + \beta_4 X_1 + \beta_5 X_2 + \epsilon$$

which ignores the non-linearity in the data but serves as a benchmark. Second, we estimate equation (3) by Probit and conduct a nearest-neighbor matching on the predicted treatment propensities. Subsequently, we regress the differences in the outcome variable Y between matched pairs on W. Third, we alter the two-step estimation by not matching individuals based on the estimated propensity score but considering the Mahalanobis distance in the complete covariate vector (W, X_1). Additionally, we perform a bias-correction to improve the convergence rate of the nearest-neighbor matching (Abadie and Imbens, 2006, 2011).

Table A.3: Monte Carlo Simulation

	OLS	PSM	NNM	
			bias-correction: without	bias-correction: with
(1) W discrete:				
τ	0.642	1.015	1.015	0.846
	(0.253)	(0.364)	(0.320)	(0.846)
$\tau(W)$	0.293	0.120	0.468	0.606
	(0.408)	(0.430)	(0.587)	(0.617)
(2) W continuous:				
τ	0.620	1.091	1.208	0.747
	(0.310)	(0.485)	(0.295)	(0.364)
$\tau(W)$	0.343	0.182	0.395	0.658
	(0.214)	(0.500)	(0.328)	(0.400)

Standard errors in parentheses. Results of 200 repetitions with $N = 1000$. (OLS): linear regression of Y on $(Z, W, Z \cdot W, X_1, X_2)$. (PSM): propensity score matching and subsequent regression of the difference in Y between matched pairs on W. (NNM): nearest-neighbor matching on the entire covariate vector (W, X_1), with and without bias-correction, and subsequent regression of individual differences on W. Theoretical values: $\tau = 1$ and $\tau(W) = 0.5$.

Table A.3 presents results for 200 simulations with $1,000$ observations. Nearest-neighbor matching performs best among all methods in estimating the treatment effect heterogeneity parameter. Unsurprisingly, estimations are more precise when W is discrete and exact matching is feasible. Abadie and Imbens (2006) show theoretically that the bias increases with the number of continuous covariates which is mirrored in our simulation results for continuous W. Interestingly, for our setup the bias-corrected version is further away from the true values of both the ATE and the heterogeneity parameter compared to the standard nearest-neighbor matching. The correction is based on a linear regression on the covariates (Abadie and Imbens, 2011) which seems to harm estimation precision given the non-linear outcome model and the relatively small sample size. However, we rather regard this as a special case of our setup.

Although OLS cannot capture the non-linear part of the outcome model (equation 4) it nevertheless comes closer to the true heterogeneity parameter than the PSM method.

However, point estimates for the average treatment effect τ show a large bias. In conclusion, the PSM method performs particularly poorly in capturing the treatment effect heterogeneity, which is the main result of this simulation exercise.