The Effects of Public R&D Subsidies on Firms' Innovation Activities: The Case of Eastern Germany

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This study analyzes the effects of public R&D policy schemes on the innovation activities of firms in Eastern Germany. The main question in this context is whether public funds stimulate R&D activities or simply crowd out privately financed R&D. Empirically, we investigate the average causal effects of all public R&D schemes in Eastern Germany using a nonparametric matching approach. Compared to the case in which no public financial means are provided, it turns out that firms increase their innovation activities by about four percentage points.

KEY WORDS: Nonparametric matching; Public innovation subsidies.

1. INTRODUCTION

In 1998, the German federal government spent about 2.2 billion Euro on promoting R&D activities in the business sector. Given this large amount of public R&D subsidies for innovation projects, the question arises as to whether policy schemes stimulate private activities that produce positive externalities, that is, benefits to society.

The economic literature on external effects indicates that innovation projects lead to market failures. Innovations are assumed to have positive external effects, but firms launch only privately profitable innovation projects. Thus there may be projects that would have positive benefits to society but that do not cover the private cost. Therefore, these projects are not carried out, and the quantity of innovations remains below the socially desirable level. This circumstance is the main reason that governments subsidize private R&D projects. Public funding reduces the price for private investors, and thus the innovations are carried out. However, a firm always has an incentive to apply for public R&D support, even if it could perform the R&D projects using its own financial means. If public support is granted, then the firm might simply substitute public for private investment. This possible crowding-out effect between public grants and private investment must be taken into account when public authorities decide on the level of their engagement in R&D support programs.

This study investigates the effects of R&D subsidies in Eastern Germany, which, more than a decade after the break down of the Berlin Wall, remains a transition economy. Public authorities have been trying to accelerate the transition process from a planned economy to a market economy since the German reunification in 1990. The efforts undertaken were and still are enormous, but many industrial firms are still struggling to survive. In most regions of Eastern Germany, the number of producing firms per inhabitant is far below the average of Western Germany. Moreover, firms are mostly too small, that is, they have not reached the minimum efficient size of production. Furthermore, firms suffer from the collapse of "eastern markets," which still induces severe sales difficulties. Various programs and schemes have been set up to overcome these difficulties.

For example, Ragnitz (2000) compared all subsidies granted in Eastern and Western Germany. In relation to the labor force, the amount is twice as high in Eastern Germany. Instead of the labor force, subsidies can be considered in relation to the gross domestic product. In this case subsidies are more than three times higher in Eastern Germany than in Western Germany. According to calculations of Ebling, Hipp, Janz, Licht, and Niggemann (1999), about 60% of innovating firms in Eastern Germany received public R&D funding in 1996—a percentage six times higher than in the Western Germany. Moreover, considerably more is spent on new firms to establish a certain amount of small- and medium-sized firms (SMEs), which are important for a powerful market economy (Almus 2001). These figures make examination of public R&D schemes in Eastern Germany an interesting and necessary task. Therefore, in this article we analyze whether a crowding-out effect among public R&D funds and privately financed R&D activities occurs in the Eastern German economy.

2. THIS STUDY IN THE CONTEXT OF EXISTING LITERATURE

Several empirical studies already exist on the effects of public R&D subsidies. David, Hall, and Toole (2000) reviewed the literature on the relation between R&D subsidies and R&D expenditure on different levels of aggregation. All studies reviewed aim to explore the sign and the magnitude of the "net" effect of public policies. On industry or country level, only 2 out of 14 empirical studies report that public R&D funding crowds out private R&D investment. The evidence is less clear at the firm level; 9 out of 19 studies indicate substitutional effects; that is, public funds crowd out private investment, partially or even completely.

The difficulty of this kind of analysis are potential selection biases coming from the public institutions that—depending on

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the applying firm and the relevant R&D project—decide the recipients of the public funding solely. According to Busom (2000, p. 114), "This makes public funding an endogenous variable, and its inclusion in a linear regression will cause inconsistent estimates if it happens to be correlated with the error term." Furthermore, public institutions might support only those firms and R&D projects that are expected to generate extensive economic spillover effects. To estimate the "real" effects of public subsidies, it is therefore necessary to address the core evaluation question: How much would the subsidy receiving firms have invested had they not participated in a public policy scheme? In fact, only a few studies on the impact of R&D subsidies attempt to model this counterfactual situation. Most of the studies surveyed by David et al. (2000) do not pay attention to this kind of selection bias.

Recently, Wallsten (2000) considered a simultaneous equation model to pay attention to the possible interdependence between public R&D funding and R&D expenditure of firms. He investigated the Small Business Innovation Research (SBIR) program and concludes that it is necessary to account for possible endogeneity of federal R&D grants. According to the results of the study, SBIR awards crowd out firm-financed R&D spending dollar for dollar (full crowding out). The subsidies have no effect on R&D activities or employment. However, Wallsten (2000, p. 98) mentions another possible and important impact of public funding: ". . . while the grants did not allow firms to increase R&D activity, they instead allowed firms to continue their R&D at a constant level rather than cutting back."

Busom (2000) explored the problem of selection bias by applying a two-stage econometric treatment model in which the first stage consists of estimating a probit model on the participation probability in public funding programs and in the second stage the R&D activity is regressed on several covariates, including a selection term that accounts for the different propensities of firms to be publicly funded. This second equation is estimated separately for participants and nonparticipants. The difference in expected R&D expenditure of both groups is according to this approach the result of public funding. Busom concluded that for most firms in her sample, public funding induced more R&D activities, but for 30% of participants complete crowding-out effects cannot be ruled out.

Lach (2000) investigated the effects of R&D subsidies granted by the Israeli Ministry of Industry and Trade on local manufacturing firms. He applied different estimators, including the before-after estimator, the difference-in-difference estimator, and different dynamic panel data models. Although Lach found heterogenous results from the different models applied, he finally concluded that subsidies do not crowd out company-financed R&D expenditure completely. Their long-run elasticity with respect to R&D subsidies is .22.

Other microeconomic approaches do not focus on crowding-out effects but take different output measures into consideration. Examples include the effects of subsidies on patent applications, productivity, fixed-asset investments, returns on capital, returns on sales, and growth of sales or employment (see Klette, Møen, and Griliches 2000 for a comprehensive survey).

This study focuses on the crowding-out issue and introduces another empirical tool to the literature on examining the effects of public R&D funding. We apply a nonparametric matching approach that goes back to the model of potential outcomes developed by Roy (1951) and Rubin (1974). These matching approaches were applied extensively in the literature on the evaluation of labor market policies, including the evaluation of active labor market programs and qualification measures (LaLonde 1986; Dehejia and Wahba 1999; Lechner 1999; Heckman, LaLonde, and Smith 1999). In these cases, people are the subject of the examination, and research questions include whether wages, salaries, or the probability of being hired or reemployed increase if people take part in a specific measure or program. The nonparametric matching approach applied here can clearly identify the effect that goes back to the receipt of public R&D funding, because we are able to approximate a situation with no differences between subsidized and nonsubsidized firms with respect to characteristics that influence the probability of receiving public support and carrying out private R&D. According to Hausman (2001), the matching methodology leads to more robust estimates of the treatment or causal effect compared with alternative approaches.

A major advantage of this study is the ability to identify exactly whether a firm received any subsidies for innovative projects. All programs launched by public authorities are incorporated, so the approach applied can reflect the effects of public R&D policy schemes collectively and is not restricted to a particular measure. Many other studies deal with only one specific public R&D program and cannot control for possible effects of other publicly funded research. In contrast, we can distinguish recipients and nonsupported firms in the sample exactly. Our control group contains only firms that did not receive any public R&D grants. This is not the case for several other studies that analyze one specific R&D program but are not able to control for other sources of public funding. This advantage has its price, however. We are not able to track in which particular program a firm participated. We only observe whether a firm participated in any public R&D scheme under consideration. Therefore, we do not describe the R&D programs in more detail. Of course, the treatments were targeting different types of firms or aims, and thus heterogeneous treatments exist. Hence our study can only be seen as providing broad evidence as to the overall R&D policy in Eastern Germany and is able to discover only average effects over different schemes.

3. DATA

The data used are taken from the Mannheim Innovation Panel (MIP) conducted by the Centre of European Economic Research (ZEW) on behalf of the German Federal Ministry for Education and Research (see Janz, Ebling, Gottschalk, and Niggemann 2001 for a more detailed description of the MIP database). The MIP is a German survey on innovation activities in the business sector. It formed the German part of the Community Innovation Survey (CIS) of the European Commission in 1993, 1997, and 2001. Since 1993, information from about 2,500 German manufacturing firms has been collected in the MIP annually. We use data from the surveys in 1995, 1997, and 1999; the information collected corresponds

Table 1. Mean Comparisons of Subsidized Firms, Firms From the Potential Control Group Without Subsidization, and the Selected Control Groups

	Subsidized firms	Nonsubsidized[a] firms	Selected control[b] firms
Industry 1	.053	.142*	.053
Industry 2	.061	.056	.061
Industry 3	.023	.066*	.023
Industry 4	.084	.066	.084
Industry 5	.068	.089	.068
Industry 6	.058	.086	.058
Industry 7	.143	.214*	.143
Industry 8	.214	.073*	.214
Industry 9	.122	.046*	.122
Industry 10	.093	.059	.093
Industry 11	.042	.056	.042
Industry 12	.040	.046	.040
Cohort dummy 1994	.320	.558*	.342
Cohort dummy 1996	.350	.300	.341
Cohort dummy 1998	.330	.142*	.317
Number of employees	191.8	136.3	178.6
Export ratio	.171	.099*	.174
Market share	.385	.278	.350
Import ratio	.209	.180*	.209
Sellers concentration (CR6)	.185	.169	.186
West German parent company	.196	.191	.220
Foreign parent company	.048	.076	.056
Firm age	5.963	7.376	6.564
Capital intensity	.095	.104	.097
Legal form	.058	.092	.069
R&D department	.603	.248*	.592
Propensity score	.817	.044*	.801
Observations	622	303	622
Different control observations			157

NOTE: * indicates that the means differ with statistical significance in a two-tailed t test at the 5% level between the supported firms (column 2) and either firms from the potential control group (column 3) or from the selected control group (columns 4).
[a] Nonsubsidized firms of the initial sample, that is, before the matching procedure.
[b] Selected nonsubsidized firms, that is, based on the matching procedure.

to the firms' activities in 1994, 1996, and 1998. Firms in the survey are from almost the whole business sector and can be classified according to the European standard classification Nomenclature of Economic Activities in the European Union (NACE). We use the manufacturing sector, and thus firms in the sample belong to 12 industries that are characterized by dummies in the empirical analysis (see the Appendix). Note that only firms with at least five employees are sampled in the MIP. In a critique of previous studies, Lichtenberg (1984) argued that the results of evaluations are often biased because the data used comprise mainly observations on large firms. The MIP data overcome this problem with many observations on small and medium-sized firms. The sample contains 925 observations on innovating firms located in Eastern Germany, of which 622 participated in public R&D schemes. Note that we use our database not as panel data, but rather as three cross-sections. Most firms (more than 70%) are observed only once in the sample; only about 8% are included in all three cross-sections. Table 1 contains descriptive information on participating firms and the potential control group of nonparticipants. According to the Oslo Manual guidelines (Eurostat and OECD 1997), innovators are defined as firms that have introduced at least one product or process innovation in the past 3 years. A product or process innovation is defined as follows:

> Technological product and process (TPP) innovations comprise implemented technologically new products and processes and significant technological improvements in products and processes. A TPP innovation has been implemented if it has been introduced on the market (product innovation) or used within a production process (process innovation). TPP innovations involve a series of scientific, technological, organizational, financial and commercial activities. The TPP innovating firm is one that has implemented technologically improved products or processes during the period under review (Eurostat and OECD 1997, p. 47).

As potential outcome variable in the empirical analysis, the R&D intensity, that is, the ratio of R&D expenditures to sales (multiplied by 100), is considered. We separate our sample with respect to the participation in public R&D schemes into the treatment group (i.e., subsidized firms), and potential control group. The empirical analysis then tries to assess whether firms that received public R&D funds in 1994, 1996, or 1998 have on average a higher R&D intensity compared to firms that did not receive public means in that period. There are three time periods under evaluation, and a firm may belong to the group of subsidized firms (treatment group) in one, two, or all three periods. However, we allow firms to enter the potential control group only if they have previously not participated in any of the R&D support programs. Hence all firms that received public R&D funds in 1994 but not in the subsequent years under examination, or in 1996 but not in 1998, are excluded from the potential control group to avoid biased results. An important part of the empirical analysis is to estimate a firm's probability of that receiving public funds given a number of observable characteristics also have an influence on the success variable, that is, the R&D intensity. Therefore, we briefly review several control variables used in the empirical analysis.

The log of the number of employees and its square take into account possible size effects. A potential concern of using number of employees is the fact that firms that receive subsidies may hire R&D staff, and thus their employment increases. This would cause some endogeneity among the receipt of public funding and firm size. Therefore, it would be preferable to use the lagged number of employees of the year before participation in public policy schemes, but we do not have the required information in our database. However, we think that the possible endogeneity problem is not severe in our study for two reasons: (1) there are only a few programs elaborated toward increasing the R&D staff directly, and (2) R&D staff as a proportion of all employees of the firm amounts to less than 5% on average for the firms in the database, a figure that is quite stable over time. Hence R&D subsidies may influence the number of R&D staff in some cases, but this change is small compared to the number of all employees. These two arguments weaken the concern of a potential endogeneity between the receipt of R&D subsidies and the number of employees.

Eleven industry dummies control for cross-sectional differences, for example, different technological potential in various industries. Two cohort dummies shift intertemporal effects. Another important factor that might have an influence on the probability of funding, as well as on the success measure, is market competition. Thus several variables control for competitive impacts; the market share variable measures the firms' sales in relation to the industries' sales measured on the NACE three-digit level. The import ratio measured on the two-digit sectoral level captures the competitive pressure of foreign firms on the market. Moreover, we consider the firms' export related sales divided by total sales to measure foreign competition. The sellers concentration of the domestic market is also taken into account. This is measured as the concentration ratio CR6, that is, the sum of market shares of the industries' six largest firms. Capital intensity (i.e., the ratio of tangible assets per employee) is included in the analysis to control for different technologies used in the production process. Moreover, we incorporate the firms' age. It is often claimed that older firms are more reluctant to pursue innovation and, thus it may be argued that they are less likely to apply for public research programs. The foundation of a firm usually induces innovation activities and; hence young firms are expected to be more lively regarding R&D.

The legal form indicates the attitude of the firm (owner) toward risk and also the chance to enter public R&D programs. Hence the dummy variable "legal form" separates the sample in firms with liability limiting legal forms [joint stock company (AG), nonpublic limited liability firm (GmbH), or commercial partnership with a nonpublic limited liability firm (GmbH & Co, KG)]. For these firms, the legal form dummy is 0. Using these legal forms, owners can minimize their risk up to a certain amount and thus have higher incentives to pursue more risky projects (Stiglitz and Weiss 1981). The dummy is 1 for firms with remaining legal forms (e.g., joint partnerships). Companies with limited liability have much better options for receiving public subsidies, because firms applying for public grants, must prove that they maintain an operating industrial plant. Firms with a liability limiting legal form must be recorded in the trade register in Germany, which means a publicly available information exists that this firm is already doing business. Companies with other legal forms must prove this within their application for public grants, and the ministry official must inspect this on her or his own. Because ministry officials may behave in a risk-averse manner, companies with limited liability, who have already proved their credibility, are possibly favored.

To control for technological prowess or previous R&D experience, the analysis includes a dummy variable indicating whether firms have R&D departments. The inclusion of this dummy has the potential to create an endogeneity problem; However, this would occur only if firms in the sample were establishing new R&D departments as a result of the receipt of public subsidies. As there are no public R&D schemes in Germany that explicitly support the founding of whole R&D departments, the endogeneity problem is unlikely to occur. Nonetheless, the R&D department dummy reflects the absorptive capacity and R&D experience of firms. The use of other variables is not possible with our data; unfortunately, using the (share of) R&D personnel would cause endogeneity problems, because there are some policy schemes which promote hiring R&D staff. Other indicators of absorptive capacity, like lagged values of R&D expenditure, are not available.

Finally, we incorporate dummy variables that indicate whether the observed firm is a subsidiary of a foreign or West German firm. We do this for two reasons: first, there are many policy schemes specifically for SMEs. However, if a firm is an SME but also belongs to a group with a large parent company, then it would not be accepted to participate in policy schemes designed for SMEs. Moreover, many schemes are exclusively for Eastern German firms, and if the parent company is Western German, its subsidiaries are not allowed to enter in programs for Eastern German firms. Hence the dummy variables Western German Parent Company and Foreign Parent Company should capture these effects.

4. IDENTIFICATION AND MATCHING

4.1 Causal Effects and Potential Results

The situation to be examined is typical for an evaluation. All firms in the database can be separated with respect to the receipt of public R&D subsidies. This leads to a nonexperimental setting because the receipt of subsidies is not random. There are several differences between the groups of firms with and without R&D subsidies, as the empirical analysis herein reveals. The receipt of public R&D subsidies finally leads to potential outcomes Y^1 for the firms that received subsidies and Y^0 for the nonrecipients. The approach that is used to measure the difference between groups (i.e., the causal effect) goes back to the model of potential outcomes by Roy (1951) and Rubin (1974). Rubin defined the term causal effect as "the difference between the likely outcome of a person's participation in the measure and the likely outcome of a person's nonparticipation." The participation of firm i in any R&D scheme is denoted be $S_i = 1$ and $S_i = 0$ otherwise. The evaluation aims to calculate the causal effect of public R&D schemes in the subsidized firms' view, that is, the study concentrates on the causal effect θ^1 that results from receiving R&D subsidies,

$$\theta^1 := E[Y^1 - Y^0|S=1] = E[Y^1|S=1] - E[Y^0|S=1], \quad (1)$$

where $E[\bullet]$ in (1) represents the expectation operator. The causal effect then indicates whether public R&D support has a positive impact on the private R&D intensity. However, the outcome $E[Y^0|S=1]$ is by definition not observable, because nonsubsidized firms cannot be observed in the case of R&D subsidy receipt. The first outcome, $E[Y^1|S=1]$, can be estimated unbiased as the mean value of the outcome variable representing firms that received subsidies. To identify $E[Y^0|S=1]$, we need to incorporate further assumptions.

4.2 Identification

$E[Y^0|S=1]$ cannot simply be calculated as arithmetic mean of the nonrecipients, because

$$E[Y^0|S=1] \neq E[Y^0|S=0]. \quad (2)$$

This condition would be valid only in the case of an experiment in which participants and nonparticipants are randomly assigned to the measure. The descriptive analysis, however, shows that the subsidized and nonsubsidized firms in our sample differ in various important characteristics. Due to selection processes on the part of the authorities that determine how funds are distributed among applicants, the group of firms that received assistance is special and select. Moreover, firms have different information and different access to information regarding possibilities of application for public funds, which may be a further source of potential selection.

Rubin (1977) introduced the conditional independence assumption (CIA) to solve the problem arising in (2). This condition means that participation (receipt of subsidies) and potential outcome (R&D intensity) are independent for individuals with the same set of exogenous characteristics $(X = x_i)$,

$$(Y^0, Y^1) \perp S|X = x(CIA). \quad (3)$$

The condition helps overcome the problem that $E[Y^0|S=1]$ is unobservable. If CIA is valid, then $E[Y^0|S=0, X=x_i]$ can be used as a measure of potential outcome for the R&D recipients (Lechner 1998). However, CIA is plausible only if all variables that influence the outcome Y^0 or Y^1 and participation status S are known and available in the dataset. Although it is not possible to test the validity of CIA formally (see Almus, Egeln, Lechner, Pfeiffer, and Spengler 1999), the MIP contains a rich set of information that we believe makes the CIA a reasonable approximation. If CIA is correct, then the equation

$$E[Y^0|S=1, X=x] = E[Y^0|S=0, X=x] \quad (4)$$

holds, which means that the outcome of nonparticipants can be used to calculate the average outcome for the participants in an unbiased way provided that there are no systematic differences between firms with and without public R&D subsidies. Then the causal effect of public subsidization in (1) changes to

$$\theta^1 := E[Y^1|S=1, X=x] - E[Y^0|S=0, X=x], \quad (5)$$

which can be estimated unbiased using the means of both groups (Lechner 1998). The next step requires a search for pairs of nonsubsidized and subsidized firms that do not differ in characteristics contained in the vector X. Here the study deviates from other examinations. Normally, there are more firms or individuals in the potential control group compared to the group of treated individuals or firms. Our database, however, has about twice as many firms that received public R&D subsidies than nonrecipients because of the special situation in Eastern Germany after reunification. Then the matching approach assigns to every subsidized firm a similar nonsubsidized counterpart. Using this approach, we do not waste information of subsidized firms; however, a nonsubsidized firm may be matched to more than one recipient of R&D subsidies.

4.3 Nonparametric Matching

Rosenbaum and Rubin (1983) pointed to the fact that a large number of exogenous characteristics is required to ensure the validity of the CIA. Thus the vector x_i containing the exogenous variables of firm i has a high dimension. This impedes the estimation of the causal effect, because it is almost impossible to find subsidized and nonsubsidized firms that have exactly the same values in the exogenous variables if there are many to consider. Fortunately, the vector of exogenous variables, x_i, can be condensed into a single scalar measure to solve this problem, the so-called propensity score. This measure represents the probability that a given firm i has received public R&D subsidies at all given a set x_i of individual characteristics $\Pr(S_i = 1|X = x_i)$. Rosenbaum and Rubin (1983) showed that if the CIA is fulfilled, then it is sufficient to condition on the propensity score to ensure statistical independence between potential outcome and receipt of R&D subsidies. Several forms of conditioning can be summarized under the classification "balancing scores" (Rosenbaum and Rubin 1983; Lechner 1998). Balancing scores cover a wide range of measures, ranging from the most complex, $X = x_i$, to the propensity score $\Pr(S_i = 1|X = x_i)$ as the most simple form. This analysis uses the unbounded propensity score $x_i'\hat{\beta}$ as a single matching criterion.

Along with the independence of potential outcome (firm-specific R&D intensity) and participation status (receipt of public R&D funds), identification of the causal effect depends on a further condition. Individual causal effects may not be influenced by the participation status of other firms, that is, the absence of indirect effects [stable unit treatment value assumption (SUTVA) condition] (Angrist, Imbens, and Rubin 1996). SUTVA constitutes a potential caveat of the analysis, but because all R&D programs in Eastern Germany are considered, these possible indirect effects should not cause biased results; the firms compete for the means on many submarkets (various schemes). Regarding a possible demand shift for R&D inputs and thus a change in factor prices, we do not believe that public policy schemes have a remarkable effect.

In our opinion, the market for R&D inputs can be seen as a national market rather than several regional markets. Admittedly, a proportion of the 60% of innovating firms was subsidized in Eastern Germany, but when looking at whole Germany this proportion is rather small, because less than 14% of German innovators are located in Eastern Germany. In Western Germany only about 15% of innovating firms receive any public funding. Thus, most German innovators in the manufacturing sector do not participate in public R&D schemes. Moreover, the amount of subsidies for the recipients is low compared with their private investments. For example, in 1999, firms spent about DM 60 billion on R&D activities in Germany, whereas the public R&D subsidies from the federal government amounted to about DM 2 billion for civilian R&D (BMBF 2000). Unfortunately, there are no figures available for Eastern Germany only. However, because the share of subsidies is only about 3%, it seems unlikely that public R&D schemes have a significant influence on prices for R&D inputs. Hence the SUTVA is assumed to be fulfilled.

Other approaches can be used to estimate the causal effect in nonexperimental settings. Those most often applied are as follows (for a comprehensive overview, see Heckman et al. 1999):

- The "difference-in-differences" method (Ashenfelter 1978; Ashenfelter and Card 1985) became popular with the availability of panel data. Here, potential selection biases stemming from observable time invariant variables vanish in the linear model if differences are calculated over time (Fitzenberger and Prey 1998).
- Complete econometric selection models simultaneously estimate participation and success of the program or measure. These models depend on restrictive assumptions regarding the error terms and their distribution that often cannot be interpreted economically. Therefore, these models have often been criticized (Ashenfelter and Card 1985). However, Heckman and Hotz (1989) pointed out that application of parametric models leads to satisfying results.
- Parametric instrument variable estimators, which have garnered increasing attention in recent years, may be considered a variant of parametric selection models (Angrist et al. 1996).

All of these approaches have advantages and disadvantages, and there are currently no guidelines as to when to use statistical matching or econometric evaluation models. Thus "the choice of an appropriate econometric model critically depends on the data on which it is applied" (Heckman, Ichimura, Smith, and Todd 1996). Moreover, Heckman and Hotz (1989) concluded that "there is no objective way to choose among alternative nonexperimental estimators." We finally apply a matching approach because the dataset has comprehensive information on the firms, thus enabling us to find a "perfect twin," that is, a similar control observation for every subsidized firm in the upcoming matching process. Moreover, Hausman (2001) stated that matching approaches lead to more robust estimates of the treatment effect compared with other methods.

5. EMPIRICAL ANALYSIS

5.1 Initial Situation and Probit Estimation

5.1.1 Prematch Situation. The dataset contains 625 firms (N^1) that received public R&D subsidies. Moreover, there are 303 firms (N^0) that did not receive any public R&D subsidies. Table 1 shows significant differences in the means of several characteristics between both groups. This indicates that the group of firms that received public R&D subsidies is a selective one. The firms' decision to apply for public assistance, as well as the selection mechanisms on the part of the authorities who distribute the means, generates a group of firms with special characteristics. Therefore, a comparison of the firm-specific R&D intensities using the initial dataset would lead to biased results due to the differences between groups.

5.1.2 Specification Tests and Probit Estimation. The best and easiest way to find a counterpart for every firm that received public R&D subsidies is to select the nonsubsidized one with exactly the same values in the selected matching variables (see Table 1), that is, a perfect twin. But the relatively large number of these variables and the availability of only about 300 firms in the potential control group impedes this approach. Matching methods, which have recently become popular in labor market evaluation studies, represent a powerful alternative to avoid these difficulties (Lechner 1998). Rosenbaum and Rubin (1983) pointed out that matching "is a method for selecting units from a large reservoir of potential comparisons to produce a comparison group of modest size in which the distribution of covariates is similar to the distribution in the treated group."

The matching algorithm used corresponds closely to the one applied by Lechner (1998). To reduce the multidimensional problem arising from the relatively large number of covariates to a one-dimensional problem, initially a probit model is estimated. The decision whether the firm has received public assistance ($S_i = 1$) or not ($S_i = 0$) serves as the endogenous variable,

$$E[S_i|X = x_i] = \Pr(S_i = 1|X = x_i) = \Phi(x_i'\beta) \quad \forall\ i = 1, \ldots, N^0 + N^1. \tag{6}$$

The vector x_i contains the set of characteristics that potentially influence the probability of receiving public R&D subsidies. These have been introduced in Section 3. Also, $\Phi(\bullet)$ is the cumulative density function of the standard normal, β is the parameter vector to be estimated, and N^1 and N^0 define the number of assisted and nonassisted firms.

Tests on normality and heteroscedasticity have been carried out to find potential misspecifications that would lead to inconsistent probit estimates. We use Lagrange multiplier (LM) tests to check whether misspecifications of the distributional assumptions (nonnormality, heteroscedasticity) exist (Verbeek 2000). The results of the heteroscedasticity tests are given in Table 2. The statistics are chi-square, distributed with as many degrees of freedom as variables to be tested for heteroscedasticity. The tests do not reject the null hypothesis that error terms are homoscedastic at the 5% level of significance. Moreover, the normality assumption cannot be

Table 2. Heteroscedasticity and Normality Tests

Variable	Degrees of freedom	Statistic	Probability value
Industry dummies	11	13.290	.275
Cohort dummies	2	4.245	.120
Size groups	5	5.050	.410
Export ratio	1	.900	.343
Market share	1	.015	.903
Import ratio	1	.177	.674
Sellers concentration	1	.070	.791
Parent company	2	1.096	.578
1/age	1	.471	.492
Capital intensity	1	.213	.644
Legal form	1	.000	.984
R&D department	1	2.005	.157
Normality	2	4.941	.085
Number of observations		925	

rejected at the 5% level of significance in a chi-squared test with 2 degrees of freedom (see Table 2). This test examines whether skewness and kurtosis are characteristic of a normal distribution.

No indication of potential misspecification of the homoscedastic probit model was found. Thus the results can be used for inferences and the matching process. Table 3 gives the estimated parameters, which we interpret briefly at first. In addition to the estimated parameters, Table 3 contains the marginal effects normally used to interpret the results. Here the effect of marginal changes of an exogenous variable on the probability of receiving subsidies can be examined. The marginal effects for the probit model are calculated according to Greene (2000) in the following way:

$$\frac{\partial E[S\mid X=x]}{\partial x_k}=\frac{\partial \Pr(S=1\mid X=x)}{\partial x_k}=\frac{\partial \Phi(x'\beta)}{\partial x_k}=\phi(x'\beta)\beta_k. \tag{7}$$

Here $\phi(\bullet)$ is the probability density function of the standard normal.

In the probit estimation, several industry dummies, the cohort dummies, the firm size, and the fact that the potential parent company is located abroad have a significant influence on the probability to receive public R&D subsidies. Moreover, the seller's concentration, as well as the existence of an R&D department, significantly determine the probability of being subsidized. No a priori considerations were made regarding the influence of the industry dummies, but it turns out that industries that are rather technology intensive (industries 4, 8–11) have ceteris paribus a higher probability to receive subsidies. The cohort dummies indicate that firms had a higher probability of receiving subsidies in subsidization periods 1996 and 1998 than in the reference period 1994. Other things equal, the effects amount to about 20 to 25 percentage points. The existence of a foreign parent company is connected with a decrease of the probability of receiving public R&D subsidies ceteris paribus by about 26 percentage points. This indicates that German firms without foreign links are the main focus of public support. The insignificant influence of a West German parent company further supports this finding.

Firm size is a further determinant that significantly influences the subsidization probability. The larger the firm, the

Table 3. Results of the Probit Estimation

Variable	Coefficient	t value	Marginal effects[a]	t value
Industry 2	.242	.620	.077	.660
Industry 3	.242	.860	.077	.930
Industry 4	1.015	3.110*	.247	4.940*
Industry 5	.450	1.920	.135	2.240*
Industry 6	.324	1.390	.101	1.540
Industry 7	.355	1.850	.112	2.010*
Industry 8	.880	3.920*	.241	5.080*
Industry 9	1.582	4.010*	.318	8.480*
Industry 10	.723	2.850*	.197	3.760*
Industry 11	1.095	2.330*	.250	4.280*
Industry 12	.038	.130	.013	.130
Cohort 1996	.621	5.420*	.196	5.880*
Cohort 1998	.850	6.190*	.251	7.420*
ln(employees)	.641	2.580*	.218	2.580*
ln(employees)^2	−.051	−1.830	−.017	−1.820
Capital intensity	−.196	−.450	−.067	−.450
1/age	.618	1.170	.210	1.170
West German parent company	−.223	−1.680	−.079	−1.630
Foreign parent company	−.680	−3.280*	−.258	−3.160*
Export ratio	.004	1.550	.001	1.550
Import ratio	.011	1.160	.004	1.160
Sellers concentration	−.022	−2.970*	−.008	−2.970*
Market share	−.006	−.210	−.002	−.210
R&D department	.681	6.300*	.228	6.580*
Legal form	.122	.640	.040	.660
Intercept	−2.459	−4.140*		
Pseudo R^2			.202	
Observations			925	

NOTE: * indicates statistical significance at the 5% level.
[a] $\partial S/\partial x$ is for dummy variables the discrete change from 0 to 1. The marginal effects will be calculated at the means of the variables.

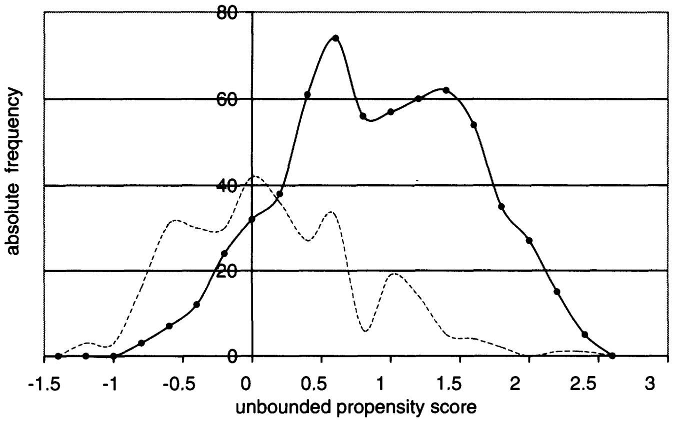

Figure 1. Frequency Distribution of the Unbounded Propensity Scores of the Initial Dataset. Scores ($x_i'\hat{\beta}$) based on the probit model. (—•— firms with subsidies; – – – firms without subsidies.)

better its chances to receive public funds. This is due mainly to information advantages, better capacities to carry out R&D, as well as the existence of more staff and capacity to apply for the funds. According to the marginal effects an increase of the firm size by 10% would raise the probability of receiving subsidies by about 2.2 percentage points. Not surprisingly, an existing R&D department has a significant positive effect on the probability of receiving subsidies, raising the probability by about 23 percentage points. Finally, the legal form does not influence the probability of receiving subsidies.

After (6) is estimated, the unbounded propensity score $x_i'\hat{\beta}$ is calculated for every observation. This measure is used in the procedure to find the counterparts for every subsidized firm. We prefer the unbounded over the bounded propensity scores $\Phi(x_i'\hat{\beta})$ because they have better distribution properties (Hujer, Maurer, and Wellner 1997). We also used $\Phi(x_i'\hat{\beta})$ as matching criterion, but there were only marginal changes in the results of the following matching process. Figure 1 shows frequency distributions of the unbounded propensity scores, $x_i'\hat{\beta}$, of both firm groups for the initial dataset. They fulfill an important assumption for the matching process, because both graphs overlap to a great extent, hence indicating similar distributions of the two groups (Lechner 1998).

5.2 Nonparametric Matching

The general matching process proceeds as follows:

1. Separate the observations with respect to their status of public R&D subsidy receipt.

2. Select a firm i that received public R&D funds.

3. Take the unbounded propensity score $x'\hat{\beta}$. In many empirical studies one wants to balance the participants and control observations with regard to more characteristics than the propensity score. Firm size is an example. Therefore, in addition to the propensity score, one uses a vector ν (where ν is a subset of x) that contains important matching variables. This variant is called *hybrid matching* (Lechner 1998).

4. Calculate a proper measure of metric distance, for example, the Mahalanobis distance. Let

$$d_{ij} = (x_i'\hat{\beta}, \nu_i)' - (x_j'\hat{\beta}, \nu_j)' \quad \forall\, j = 1, \ldots, N^0$$

for every combination of the R&D recipient i and every firm from the potential control group j. Then calculate the Mahalanobis distance,

$$MD_{ij} = d_{ij}\,\mathrm{cov}^{-1} d_{ij} \quad \forall\, j = 1, \ldots, N^0,$$

to find the nearest neighbor. Here cov represents the covariance matrix based on the controls, that is, firms that did not receive public subsidies.

5. After calculating the distance, some restrictions on the neighborhood can be imposed:

- A required criterium to be a neighbor of participant i may be that a potential control firm is recorded in the same industry classification.
- One shortcoming of the nearest-neighbor matching so far is that a neighbor is always picked, even if the metric distance to the ith control observation is very large. To prevent excessively large distances, it is possible to define a confidence interval of the propensity score and other matching variables on basis of the participant group in which a potential control observation should be included. This approach, called *caliper matching*, was indroduced by Cochran and Rubin (1973). Hujer et al. (1997) gave an example of this method.

6. The firm j from the potential control group with the smallest Mahalanobis distance serves as control observation in the following success analysis. The comparison observation is drawn randomly if more than one firm attains the minimum Mahalanobis distance. If no potential control observation remains in the pool after applying the restrictions described in the previous step, then firm i is bypassed, and no match can be made.

7. Remove the ith firm from the pool of firms that received subsidies, but return the selected control observation to the pool of control observations. This is done because of the relatively small number of control firms. Using different data (i.e., a large potential control group), one could also draw without replacement. In this case, it would be important to draw the participants one after the other randomly from the treatment group.

8. Repeat steps 2–7 to find matched pairs for all recipients.

In this article we use only the propensity score and impose the restriction that potential controls must be recorded in the same industry classifications as the participants. If the matching results are not satisfactory, then one would proceed with additional variables in the matching function. However, it turned out that using the unbounded propensity score as the only matching criterion was already sufficient. Table 1 measures the statistical "similarity" of the observations that remain after the matching procedure. Column 2 contains the means of the variables of the firms with R&D subsidies, and columns 4 contains the means of the assigned firms without such subsidies. Matching is regarded as successful if the means of the relevant variables in both groups do not differ significantly. Note that we found for every participant a neighbor within the confidence interval defined by the caliper restriction with regard to the propensity score. As indicated by a t test, the

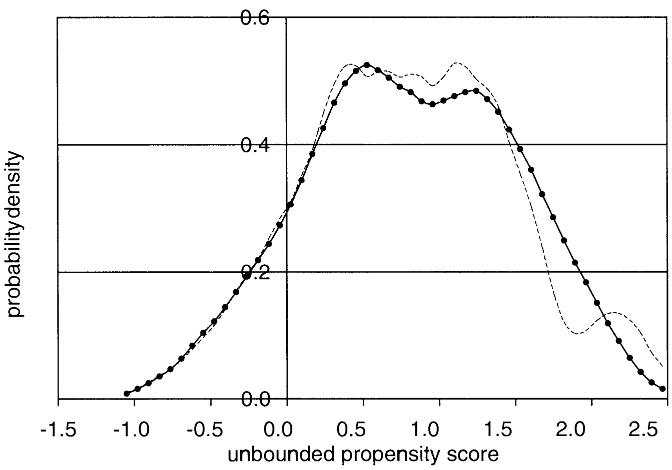

Figure 2. Density Distribution of the Unbounded Propensity Scores after the Matching Process. Scores ($x_i'\hat{\beta}$) based on the probit model. (-•- firms with subsidies; — — — firms without subsidies.)

differences of the means are small and not statistically significant at the 5% level for all variables. Moreover, the unbounded propensity score $x_i'\hat{\beta}$ as a summary measure of various variables does not significantly differ between both groups, indicating a good fit of the matching algorithm applied. We used 157 out of the 303 potential control observations for the selected control group. This means that each selected control group observation is on average assigned to four subsidized firms.

Figure 2 contains kernel density estimates of the unbounded propensity scores $x_i'\hat{\beta}$ for both groups. The Epanechnikov kernel density estimates instead of histograms serve as tools to show the similarity in the relative frequencies (probability densities), because both groups contain the same number of observations after the matching process (Silverman 1986). The are nearly no differences on the left and middle parts of the distribution. Due to the small number of nonsubsidized firms on the right tail (see Fig. 1) it is difficult to find adequate matching pairs. All in all, Figure 2 underlines the quality of the matching procedure.

6. CAUSAL EFFECTS

The success of public R&D subsidies is evaluated by comparing the average firm-specific R&D intensities between the groups of subsidized and nonsubsidized firms; that is, Y_i^1 and Y_i^0. The unbiased estimator for the causal effect $\hat{\theta}^1$ is the difference of the means in these groups,

$$\hat{\theta}^1 = \frac{1}{N^1}\left(\sum_{i=1}^{N^1} Y_i^1 - \sum_{i=1}^{N^1} Y_i^0\right). \tag{8}$$

R&D subsidy programs have on average a positive impact on the firm specific R&D intensity if the causal effect $\hat{\theta}^1$ is significantly greater than 0. The programs do not generate positive effects if $\hat{\theta}^1$ is statistically insignificant. Finally, subsidized firms perform worse than firms without subsidies if the causal effect is significantly smaller than 0. This means that nonsubsidized firms undertake on average more R&D efforts (measured with the R&D intensity) than firms that received funding within the programs under evaluation.

The test on the effect is usually carried out by means of a simple t statistic. In this case, however, the ordinary t value is biased upward, because it does not take into account that the mean of the outcome variable of the control group is not a result of a random sampling, but rather is an estimation based on the estimated propensity scores and the nonparametric matching procedure. Thus the usual t statistic may be misleading for making inferences. To remove the bias of the t statistic, the method of bootstrapping is applied; that is, we simulate the distribution of the mean outcome of the control group by repeated sampling (see Greene 2000 for a sketch of bootstrapping or Efron and Tibshirani 1993 for a comprehensive discussion):

1. A random sample with replacement is drawn from the original sample that has the same size as the original one.
2. The probit model is reestimated and a new matching with this sample is performed; the mean difference $\hat{\theta}^1$ is recorded after the procedure.
3. The whole process is repeated 200 times.
4. This leads to a simulated distribution of mean differences between the participants and their controls. This empirical distribution can subsequently be used to calculate a standard error and thus an unbiased t statistic.

Applying (8) leads to an average R&D intensity of about 6.6 (2.6)% for the subsidized (nonsubsidized) firms. Thus the resulting causal effect amounts to about four percentage points. According to the result of the two-tailed t test, this effect is statistically significantly different from 0, even according to the bootstrapping. As mentioned earlier, this result shows that the ordinary t statistic is biased downward.

Eastern German firms that receive public R&D funds achieve on average higher firm-specific R&D intensities than firms that do not receive public R&D support, given that the firms from both groups do not differ with respect to exogenous variables that influence the probability of receiving public R&D subsidies. The results confirm that public R&D schemes in Eastern Germany are an important factor for stimulating private R&D efforts.

The significantly higher R&D intensities for subsidized firms indicate that complete substitution of public means does not occur, that is, the absence of perfect crowding out. The recipients increase instead their private R&D efforts in the case of public subsidization. This is especially important in a transition economy like Eastern Germany, in which private R&D is indispensable for creating innovative and viable economic structures after more than 40 years of a planned economy.

Of course, it be would interesting to know how large the net effect of public funding is for the Eastern German manufacturing sector. The MIP provides weights for its sampled firms that enable calculation of population-weighted descriptive statistics and, in our case, rough estimation of a macroeconomic effect. According to this information, the total R&D expenditure in the Eastern German manufacturing sector in 1998 was about 3.84 billion DM. Firms that participated in any public innovation scheme spent almost 3.4 billion DM of this amount. According to the result displayed in Table 4, we assume that

Table 4. Causal Effect: Firm-Specific R&D Intensity

Firms	Subsidized firms $\widehat{E}[Y^1 \mid S=1]$ (percent)	Nonsubsidized firms $\widehat{E}[Y^0 \mid S=0]$ (percent)	Causal effect $\hat{\theta}^1$ (percentage points)	Test statistic t value (bootstrap t value)
622	6.57	2.63	3.94	8.24* (5.32*)

NOTE: * indicates statistical significance in a two-tailed t test at the 1% level.

60% of recipients' R&D activities on average are due to public funding. Applying this rule of thumb, we derive a macroeconomic effect of 2.04 billion DM according to subsidies. This effect is large compared with other studies cited in Section 2. However, keeping in mind that the transformation process in Eastern Germany is heavily fostered by the government, this figure seems to be plausible. Of course, it would be desirable to carry out a cost-benefit analysis, but unfortunately the German federal government does not provide any information on how the 2 billion DM of public funding dedicated to the business sector (BMBF 2000) are allocated to Eastern and Western German firms.

7. CONCLUSIONS

This article provides new evidence regarding whether public R&D funds crowd out private investment in innovations. It is analyzed whether the participation in public R&D programs leads on average to a higher R&D intensity at the firm level. Using a nonparametric matching approach, we compare the potential outcome of this group to a matched control group of nonsubsidized firms.

The analysis has some advantages over previous studies. The information collected in the MIP is not restricted to a particular measure but covers all public funding activities by the EU, the federal government, and the federal states in the years after reunification. However, it is not possible to track in which program a firm participated with the available information. The procedure used to identify the causal effect of public R&D schemes is also new to this kind of literature. We use a nonparametric matching approach to define a suitable control group.

Our study has yielded the following results. The causal effect identified is significantly positively different from 0; that is, firms that received public funding achieve on average a higher R&D intensity than firms belonging to the selected control group. This causal effect amounts to about four percentage points on average. For example, a subsidized firm with a turnover of 100,000 monetary units would have invested on average 4,000 monetary units less if it had not participated in public R&D schemes.

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APPENDIX: INDUSTRIES IN THE SAMPLE

Table A.1. Classification of Industry Dummies

Industry dummy	Description
Industry 1	Food and beverages
Industry 2	Textiles, clothes, and leather goods
Industry 3	Wood, paper, publishing, and printing
Industry 4	Fuels and chemicals
Industry 5	Rubber and plastic products
Industry 6	Nonmetallic mineral products
Industry 7	Basic and fabricated metals
Industry 8	Machinery and equipment
Industry 9	Office and communication equipment, electrical machinery and components
Industry 10	Medical and optical instruments
Industry 11	Motor vehicles and other transport equipment
Industry 12	Furniture products and not elsewhere cited

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