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# Evaluating the Impacts of Subsidies on Innovation Activities in Germany

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## Non-technical Summary

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**by Reinhard Hujer and Dubravko Radić**

Innovations are crucial, not only from an individual firm perspective but also from an economy wide viewpoint. However, more than any other economic activity, decisions about innovations and R&D expenditures are plagued by failures of the market mechanism. Innovations represent new knowledge which could be imitated or even stolen by competitors. Furthermore, research as well as the development of new products is a risky and uncertain undertaking and thus must be financed out of own financial resources or venture capital which are both scarce, especially in Germany. As a result of these spillover effects, financial constraints, uncertainties and risk aversion, the level of private innovation activities will be below the social optimum.

All OECD countries are aware of these problems as well as of the importance of technological change and innovations for the future growth. As a response, public instruments have been implemented to overcome this dilemma and to stimulate private innovation activities. One of the oldest are patents which were already implemented in Germany in 1877. Besides, there are various other instruments. Some of them, like competition policy or technology transfer, act more indirectly while others, e.g. tax incentive schemes and subsidies, operate in a more direct way to induce innovation activities.

This study estimates the microeconomic effects of policy measures on innovation activities of German establishments. We will focus on financial measures, like e.g. subsidies, tax incentives and public credits. Despite the considerable amount of money spent and tight public budgets empirical evidence, especially for Germany, is rather limited. This paper thus contributes to the ongoing political debate about the effectiveness of public R&D measures. An representative dataset for Germany, the IAB Establishment Panel, is used and various microeconometric methods to overcome the inherent sample selection problem applied.

Estimating nonparametrical matching models which accounts for sample selection due to observable characteristics points to the view that public subsidies have a positive impact on innovations with differences for West and East Germany and different size classes. However, we also show that especially with establishment data one has to take sample selection due to unobservables into account as well. Estimating a simultaneous probit and a conditional difference-in-differences model changed the results dramatically: We only find positive effects for East German establishments whereas in all other cases the results are at most insignificant. Obviously, public R&D programs subsidize to a large part innovation projects which would have been undertaken successfully also in the absence of such subsidies.

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# Evaluating the Impacts of Subsidies on Innovation Activities in Germany*

Reinhard Hujer[†]and Dubravko Radić[‡]

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**Abstract**

Innovations are a key factor to ensure the competitiveness of establishments as well as to enhance the growth and wealth of nations. But more than any other economic activity, decisions about innovations are plagued by failures of the market mechanism. As a response, public instruments have been implemented to stimulate private innovation activities. The effectiveness of these measures, however, is ambiguous and calls for an empirical evaluation. In this paper we make use of the IAB Establishment Panel and apply various microeconometric methods to estimate the effect of public measures on innovation activities of German establishments. We find that neglecting sample selection due to observable as well as to unobservable characteristics leads to an overestimation of the treatment effect and that there are considerable differences with regard to size class and between West and East German establishments.

Keywords: R&D policy, innovation, microeconometric evaluation
JEL Classification: C14, C25, C35, O31, O38

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# 1 Introduction

Innovations are crucial, not only from an individual firm perspective but also from an economy wide viewpoint. However, more than any other economic activity, decisions about innovations and R&D expenditures are plagued by failures of the market mechanism. Innovations represent new knowledge which could be imitated or even stolen by competitors. Furthermore, research as well as the development of new products is a risky and uncertain undertaking and thus must be financed out of own financial resources or venture capital which are both scarce, especially in Germany. As a result of these spillover effects, financial constraints, uncertainties and risk aversion, the level of private innovation activities will be below the social optimum.

All OECD countries are aware of these problems as well as of the importance of technological change and innovations for the future growth. As a response, public instruments have been implemented to overcome this dilemma and to stimulate private innovation activities. One of the oldest are patents which were already implemented in Germany in 1877. Besides, there are various other instruments. Some of them, like competition policy or technology transfer, act more indirectly while others, e.g. tax incentive schemes and subsidies, operate in a more direct way to induce innovation activities.

In 2000, total R&D spending in Germany amounted to € 49.8 billions. A considerable fraction was financed by the government. The total public R&D expenditures amounted to € 15.9 billions with € 2.6 billions paid directly to establishments in form of R&D subsidies.[1] The rationale for such measures is to increase the innovation incentives of establishments by lowering marginal costs of R&D and to decrease uncertainties regarding planning reliability. In addition to these direct effects at the establishment level, positive indirect impacts are expected to arise due to spillover effects, e.g. when new technologies and products diffuse and are adopted by other establishments.

However, counteracting effects have to be taken into account as well: At the individual level it could be the case e.g. that establishments would have undertaken innovation activities also in the absence of subsidies or that public R&D expenditures only crowd out private ones. On a more aggregated level subsidized establishments could rule out

[1] These figures were taken from the Federal Ministry of Education and Research, BMBF (2001). For an overview of public R&D instruments in Germany we refer to Czarnitzki et. al. (2003) or Fier and Harhoff (2001).

non-subsidized ones. The net effect of public R&D policy on innovation activities is thus not clear cut and calls for an empirical evaluation.

This study estimates the microeconomic effect of policy measures on the innovation activity of German establishments. We will focus on financial measures, like e.g. subsidies, tax incentives and public credits. Despite the considerable amount of money spent and tight public budgets empirical evidence, especially for Germany, is rather limited. Table 1 contains a synopsis of studies known to us which all point to the view that public R&D subsidies have a positive impact on private R&D and innovation activities.[2] These studies differ with regard to the empirical strategy and outcome variable but all make use of the same dataset, namely the Mannheim Innovation Panel.

**Table 1: Microeconometric evaluation studies of public R&D subsidies**

| Study | Sample | Outcome-variable | Method | Result |
|---|---|---|---|---|
| Czarnitzki (2001) | East German establishments, manufacturing industry | Innovation intensity | Selection models | Positive |
| Almus & Czarnitzki (2002) | East German establishments, manufacturing industry | R&D intensity | Matching models | Positive |
| Czarnitzki & Fier (2002) | German establishments, service sector | Innovation intensity | Matching models | Positive |
| Licht & Stadler (2003) | German establishments, manufacturing industry | R&D expenditures | Selection models Matching models | Positive |

This paper adds a new piece of evidence to the ongoing political debate about the effectiveness of public R&D measures. An alternative representative dataset, namely the IAB Establishment Panel is used and special attention paid to the problem of sample selection. In the next section we will present the dataset as well as some first descriptive results. Section three addresses the problem of sample selection due to observable characteristics by estimating matching and probit models. Section four additionally takes unobservable characteristics into account by employing a simultaneous probit model and conducting a conditional difference in differences estimator. Section five concludes.

[2]For an overview of international studies see Klette, Møen, and Griliches (2000) or David, Hall, and Toole (2000) who surveyed 19 (14) studies on an establishment (sectoral) level from which 10 (12) revealed a complementary relation between public and private R&D expenditures. Irwin and Klenow (1996) analyzed the SEMATECH consortium in the American semiconductor industry while Branstetter and Sakakibara (1998) focused on Japanese research consortia. Lerner (1998) examined the Small Business Innovation Research Program in the United States while the studies of Griliches and Regev (1999) and Klette and Møen (1999) analyze the situation in Israel and Norway, respectively.

## 2 Data, variables and first results

The IAB Establishment Panel conducted by the German Federal Employment Office started as a reaction to a situation of lacking information about the demand side of the labor market, at least on the microeconomic level.[3] Its population are all firms employing at least one employee subject to the compulsory social security scheme. The unit of interest is the establishment, i.e. the unit where economic activities take place. All establishments reporting to the German Federal Employment Office are collected in the establishment file from which a stratified representative sample is drawn. The IAB Panel started in 1993 with 4,365 establishments and an average response rate of 71%. After the first wave most of these establishments were re-examined. Additionally, the dataset was complemented by first time or repeated registered firms. In 1996, East German establishments were also included in the panel which in 2001 contained about 17,650 establishments.

The panel is organized in a modular form. There are topics covered annually like changes in the level and structure of employment, questions about employment policy, business volume and investment. Other topics are only covered irregularly, e.g. information about innovations which are latest available for 1999/2000. Additionally, there is also a special questionnaire about current topics included into the panel every year.

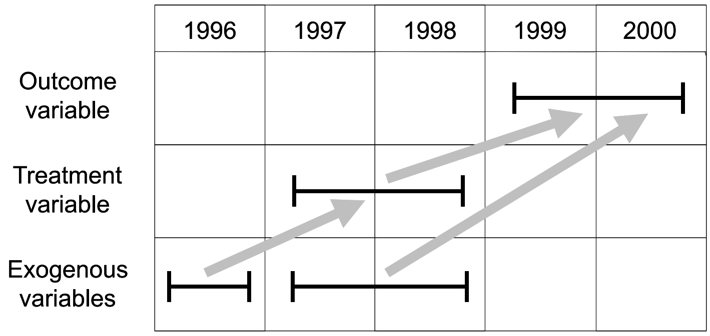
For our analysis we assume a dependency structure according to figure 1: We focus on innovation activities of establishments in 1999 and 2000 and analyze whether they were influenced by public subsidies granted during the years 1997 and 1998.[4] In terms of the evaluation literature, the innovation decision is the outcome and granted subsidies the treatment variable. In addition to the treatment variable we also consider a set of covariates from 1997 and 1998 which might have an impact on the outcome variable. The treatment variable itself, i.e. the decision whether or not an establishment has received a subsidy, is assumed to be determined by exogenous variables in 1996.

Keeping this time structure in mind, the sample used for the estimation was constructed as follows: In a first step we maintained those establishments which participated

[3] For more details see e.g. Bellmann (1997).

[4] The time lag allowed between treatment and outcome seems quite short and rules out long term effects. Unfortunately, information about innovations are latest available for the years 1999/2000 and questions regarding subsidies granted before 1997 are not comparable with subsidies granted 1997 and later. As an additional justification we refer to Hall, Griliches, and Hausman (1986) who estimated a dynamic 'knowledge function' and found that the relationship between patents and R&D is mainly a contemporaneous one.

**Figure 1: Time and dependency structure**




continuously in the panel from 1997 until 2001 yielding a sample size of 5,569 observations.[5] In order to isolate the impact of subsidies granted in 1997/1998, we excluded those establishments which received a subsidy in 1999/2000 thereby reducing our sample size to 3,164 establishments.[6] Finally, we excluded establishments from the agricultural and public sector which leaves us with 2,714 observations.

Let us now turn to the precise definition of the treatment variable, i.e. answer the question which public measures are considered in this study. Our dataset contains information whether establishments received one of the following measures during the years 1997 or 1998:[7]

– Programs financed by the federal government and federal states to enhance the regional economic structure ("Gemeinschaftsaufgabe Verbesserung der regionalen Wirtschaftsstruktur") → 35 subsidized establishments

– Programs financed solely by the federal government, e.g. wage subsidies for R&D personnel → 77 subsidized establishments

– Programs financed by federal states to increase the competitiveness of small and medium sized enterprises → 102 subsidized establishments

– Programs financed by the European Union → 55 subsidized establishments

– Tax incentives, e.g. investment subsidies or special depreciation → 291 subsidized establishments

[5]Notice that a questionnaire in year $t$ contains information for year $t-1$.

[6]We were not able to consider public subsidies granted in 1996 because the questionnaire changed between 1997 and 1998.

[7]Unfortunately, our dataset contains no information about the amount of R&D subsidies received, i.e. the treatment intensity.

– Other programs, e.g. favorable credits from the German Bank for Reconstruction ("KfW") or the European Investment Bank ("EIB") → 90 subsidized establishments.

Establishments which received at least one of these measures are regarded as treated establishments. Although an obvious shortcoming of such an aggregation is that we are not able to disentangle the effects of different measures, several reasons argue for such a proceeding. An obvious one is the necessity to increase the number of observations, e.g. the number of establishments which received federal funds amounts to 77 thus making a reliable estimation of the treatment effect difficult.[8] Another reason concerns the fact that a considerable number of establishments received more than one type of measure so that for these cases an identification of the treatment effect of one single measure is not possible.[9] And finally, since the various measures granted by different institutions follow the same objectives, namely to increase the competitiveness and innovation capacity of establishments, it makes sense to focus on the total effect of all public subsidies instead on separate measures.[10] Thus, instead of evaluating different policy schemes, which might be of special interest for policy makers, what we actually evaluate is the microeconomic effect of the German system of R&D subsidies as a whole.

Table 2 contains some basic information about treated and non-treated establishments in the sample. 2,222 (81.87%) of the 2,714 establishments constitute the control group, i.e. establishments which have not received a public subsidy in the years 1997-2000, whereas 492 (18.13%) make up the group of treated establishments. 243 of them (8.95%) received a subsidy only in 1997, 119 (4.38%) only in 1998 and 130 (4.79%) both in 1997 and 1998. In order to increase the total number of observations we pooled treated establishments in separate years into one group. Looking at table 2 one can see that the participation rate is higher among East German (47.52%) than West German establishments (8.22%). Another feature is the obvious correlation between establishment size and participation rate, e.g. the participation rate for small and medium sized establishments (SME) with 10-250 employees amounts to 24.36% and rises to 32.79% for the group of large establishments

[8]This argument becomes more convincing if one considers that we also included industrial and regional dummies in the estimation which additionally reduces degrees of freedom.

[9]In our dataset these were 121 (25%) of 492 establishments: 91 received two different measures, 24 received three, 5 received four and 1 establishment even five different types of R&D subsidies.

[10]For an overview of the programs see BMBF & BMWi (2001). A minor point concerns comparability with the other studies mentioned in table 1 which also aggregated separate measures into one single treatment variable.

with more than 250 employees.[11] Since most of the programs place a special emphasis on SME and East German establishments, all of the following estimates will be conducted separately for these two groups.

**Table 2: Basic information about the sample**

| | Treated | Controls | Participation rate |
|---|---|---|---|
| West German establishments | 118 | 1,435 | 8.22% |
| East German establishments | 374 | 787 | 47.52% |
| Micro establishments (employees $<$ 10) | 143 | 916 | 15.61% |
| SME (10 $\leq$ employees $<$ 250) | 229 | 940 | 24.36% |
| Large establishments (250 $\leq$ employees) | 120 | 366 | 32.79% |
| All establishments | 492 | 2,222 | 22.14% |

Having defined the treatment variable we now turn to the outcome variable. An often used indicator for innovation activities and therefore the most obvious candidate are expenditures for R&D or R&D personnel. Due to data limitations and several conceptual considerations, however, we decided to use an alternative concept. As a monetary input variable, R&D expenditures exhibit a close relation to the innovation process especially in knowledge intensive sectors. A disadvantage is the fact that especially smaller establishments do not feature a separate R&D department and thus no explicit R&D expenditures incur. In the case of smaller establishments, innovations are rather generated through practical experience and as a result R&D expenditures would underestimate innovation activities. Another issue concerns the input character of R&D expenditures which do not necessarily reflect the success of R&D efforts.[12]

Since especially new products and services determine the future success and competitiveness of a single firm as well as the economy as a whole, it is more reasonable to focus on an output based measure of the innovation process. In addition, most of the R&D subsidies considered in this paper aim explicitly at encouraging the introduction of new products and processes.[13] Output oriented innovation indicators thus closely reflect

[11] Regarding the definition of small and medium sized establishment we follow the recommendations of the Commission of the European Union from 1996.

[12] For more on this discussion see e.g. Patel and Pavitt (1995).

[13] For an overview of German programmes see e.g. BMBF & BMWi (2001). In the U.S. the Small Business Innovation Research Program e.g. aims to increase private sector commercialization of innovations. Cf. Wallsten (2000). Unfortunately, again due to data limitations we are not able to follow up process innovations.

the intended goals of R&D subsidies in Germany and therefore lend themselves as an appropriate evaluation criterion.

In particular, we consider an outcome variable which indicates whether establishments have introduced a new product/service during 1999/2000 (→ narrow innovation concept) and a broader concept which also contains improvements of existing products/services (→ broad innovation concept). The rationale for this second concept is that not only new products/services but also improved old ones are valued by customers and therefore might increase competitiveness. Further on, a series of incremental innovations may enable radical innovations due to learning effects. And finally, in slow growing economies like Germany, continuous improvement may be a more promising strategy than radical changes.

A first and tempting proceeding to assess the impact of public subsidies is to compare the share of innovative establishments in the group of treated and non treated establishments, respectively. Table 3 contains in the first two rows the appropriate figures. The share of establishments which improved an already existing product/service or even introduced a new one is 42.30% among subsidized but only 36.19% among non-subsidized establishments. The corresponding figures for the narrow innovation concept which excludes improvements are 27.90% for subsidized and 18.33% for non-subsidized establishments. In both cases the differences are statistically significant which points to the view that subsidies seem to have an innovation enhancing impact.

A simple mean comparison by treatment status, however, will surely not yield an unbiased estimate of the "true" treatment effect. To see why look at the remaining figures in table 3 which reveal that there are a couple of factors which simultaneously drive the innovation and the subsidy decision. Examples include the qualification structure of employees, R&D department, R&D cooperations and newer equipment. Not taking this positive sample selection into account will lead to an upward biased estimate of the treatment effect. In the following section we will present appropriate estimation strategies which explicitly account for this sample selection mechanism due to observable covariates.

**Table 3: Mean comparison by treatment and innovation status**[a]

| Variable | Treated | Controls | p-value[b] |
|---|---|---|---|
| Broad innovation concept (improvement and new products) | 0.42 | 0.36 | 0.01 |
| Narrow innovation concept (only new products) | 0.28 | 0.18 | 0.00 |
| Number of employees | 305.68 | 253.81 | 0.40 |
| Share of high qualified employees[c] | 0.67 | 0.59 | 0.00 |
| State of technology (1: Up-to-date, ..., 5: Out-of-date) | 2.03 | 2.12 | 0.02 |
| R&D department existing | 0.22 | 0.12 | 0.00 |
| Number of R&D cooperations | 0.56 | 0.29 | 0.00 |
| Capital company | 0.04 | 0.07 | 0.00 |
| Competition intensity (1: None, ..., 4: High) | 3.48 | 3.42 | 0.20 |
| Market concentration[d] | 0.80 | 0.82 | 0.00 |
| East German establishments | 0.76 | 0.35 | 0.00 |
| **Variable** | **Innovators[e]** | **Non innovators[e]** | **p-value[b]** |
| Number of employees | 506.39 | 112.20 | 0.00 |
| Share of high qualified employees[c] | 0.64 | 0.58 | 0.00 |
| State of technology (1: Up-to-date, ..., 5: Out-of-date) | 2.02 | 2.16 | 0.00 |
| R&D department existing | 0.28 | 0.05 | 0.00 |
| Number of R&D cooperations | 0.72 | 0.11 | 0.00 |
| Capital company | 0.11 | 0.04 | 0.00 |
| Competition intensity (1: None, ..., 4: High) | 3.56 | 3.36 | 0.00 |
| Market concentration[d] | 0.81 | 0.82 | 0.02 |
| East German establishments | 0.35 | 0.48 | 0.00 |

[a] All control variables referring to 1996.
[b] t-test for continuous and test of equality of proportion for dummy variables.
[c] Blue and white collar employees for qualified tasks.
[d] Gini concentration of business volume for 41 different industry sectors.
[e] Referring to the broad innovation concept.

# 3 Sample selection on observable covariates

A simple mean comparison by treatment status, which we conducted previously will only yield an unbiased estimate of the treatment effect if assignment into treatment, $D$, and potential outcome, $Y$, are independent, i.e. $Y \perp D$.[14] This independence assumption is unlikely to hold outside a non-experimental setting but is more likely to be fulfilled in our application if we additionally take a set of covariates, $X$, into account:

$$Y \perp D | X. \tag{1}$$

Under this conditional independence assumption, i.e. if sample selection is solely due to observable covariates, the average treatment effect can be estimated using either para-

[14] See Rubin (1979).

metrical or non-parametrical approaches.

The most simple estimation strategy in our application consists in estimating the following probit model:

$$Y_i = \begin{cases} 1 \text{ if } Y_i^* = \beta' X_i + \Delta D_i + \epsilon_i > 0 \\ 0 \text{ otherwise,} \end{cases} \tag{2}$$

with $Y_i$ indicating the innovation decision of the $i$-th establishment and $D_i$ whether it received a public subsidy or not.

The interested reader can find in the appendix a list of covariates contained in $X$ and some reasons for their inclusion. We will not present all estimation results but only focus on the impact of subsidies, i.e. on the parameter estimates for $\Delta$, and some diagnostics for the models (see table 7 in the appendix).[15] To quantify the impact of public subsidies on innovations, we first of all report the marginal effect of $D$ for a reference establishment according to:

$$\frac{\partial Y_i}{\partial D_i} = \Phi(\widehat{\beta}' X_i + \widehat{\Delta}) - \Phi(\widehat{\beta}' X_i). \tag{3}$$

Additionally, we also calculate the average treatment effect on the treated given by:

$$ATE^{Probit} = \frac{1}{N} \left\{ \sum_{i=1}^{N} \Phi(\widehat{\beta}' X_i + \widehat{\Delta}) - \Phi(\widehat{\beta}' X_i) \right\}, \tag{4}$$

i.e. the average difference between the probability that establishments would have introduced innovations if they had received a subsidy and the probability that establishments would have been innovative under no subsidy.[16] The standard error of the marginal effect as well as the average treatment effect were calculated using the delta method.

For the narrow innovation concept, we find positive and significant effects for the pooled estimation as well as for SME and East German establishments. The average treatment effects range from 6% to 9%. After including also improvements of already existing products/services into the outcome variable, only the parameter for East German

[15] Detailed results are available from the authors.

[16] We adopted the following reference establishment: All continuous variables are assumed to take on their mean values. Additionally we assume that the representative establishment does not possess a R&D department, is a private limited company, operates in the transportion/telecommunication sector and is located in Baden-Wuerttemberg and accordingly in Thuringia for the sub-sample of East German establishments.

establishments still remains significant (6%). Hence, accounting for observable covariates, public subsidies seem to increase the probability to introduce new products for SME and East German establishments.

An alternative estimation strategy to this parametrical approach is the matching model. Intuitively, a matching estimator tries to find in a large group of non-participants those "twin" establishments which are similar to the participating ones in all aspects except for the fact that they have not received a public subsidy. That being done and if we can assume that the selection process is only due to observable covariates, the difference in the outcome variables between participating and matched not-participating establishments is solely attributable to the program. In that sense matching estimators simulate an experimental setting.

A practical obstacle of such a proceeding lies in the so called dimensionality problem. If our vector of covariates $X$ e.g. contains $K$ binary variables, the number of combinations amounts to $2^K$. If there are also continuous variables like it is the case in our dataset, it becomes even more likely that for some combinations of the covariates a mean comparison cannot be conducted.

As an alternative, Rosenbaum and Rubin (1983) proposed the concept of propensity score matching. Defining the propensity score as the conditional probability to participate, i.e. $p(X) = p(D = 1|X)$, Rosenbaum and Rubin showed that $Y \perp D|X \Rightarrow Y \perp D|p(X)$ and hence one needs not condition on all covariates contained in $X$ but only on the propensity score $p(X)$. Given this implication, we conducted the following steps in order to estimate the average treatment effect on the treated:[17]

1. We estimated the propensity score using a logit model. Additionally to the variables contained in table 3, we included the squared number of employees, industrial and regional dummies.[18] In order to improve the matching quality we imposed the so called common support restriction, i.e. we dropped those controls which have a propensity score lower than the minimum or higher than the maximum propensity score of the treated establishments and vice versa.

[17] For more details see e.g. Heckman, Ichimura, and Todd (1998).

[18] We use regional dummies for every federal state. Due to an insufficient number of observations we only considered 7 industry sectors. Since this model has no behavioral interpretation but only serves to balance the distribution of the observable covariates, we will not present the estimation results in this paper. They are, however, available from the authors by request.

2. Using the propensity score we matched treated with non-treated establishments according to two different protocols: In a first step, we conducted nearest neighbor matching where those non-treated establishments are matched to treated ones with most similar score. Additionally, we also conducted kernel matching where more than one non-treated establishment is used as matching partners. Thereby establishments which are less similar regarding their propensity score are downweighted using the Gaussian kernel as a weighting function. In order to improve the matching quality further, in a second step we conditioned on the propensity score and on the industrial sector.[19]

3. Finally, we calculated the means of the outcome variables for treated and matched control establishments. The difference in the means may serve as an estimator of the average treatment effect on the treated. The reliability of the matching was checked by reporting the absolute standardized percentage bias of the covariates before and after the matching.[20]

The matching estimator has become the working horse in the evaluation literature, although Angrist (1998) notes that "the differences between regression and matching strategies for the estimation of treatment effects are partly cosmetic," since the approximation of any functional form imposed by a parametrical model like the probit model and the conditioning on the propensity score in the matching model become more and more similar the more interaction terms are included in the estimation.

The appendix contains the detailed estimation results for the matching models as well as some diagnostics (see tables 8-11). At this point, it is sufficient to note that the previous results are largely confirmed. Indeed, matching yields even larger effects than the probit model. The estimates are sensitive to the chosen matching algorithm with larger treatment effects for kernel matching. Note however, that even after the matching the groups of treated and non-treated West German establishments are still considerably heterogenous.

[19] Due to an insufficient number of observations conditioning on the industrial sector was not possible for different size groups.

[20] The absolute standardized bias was proposed by Rosenbaum and Rubin (1985) and is defined for a single covariate $x$ as follows: $|(\overline{x}(1) - \overline{x}(0))/(\sqrt{[var(x(1)) + var(x(0))]/2}|$ where $\overline{x}(1)$ $(var(x(1)))$ is the mean (variance) of the covariate in the group of treated and $\overline{x}(0)$ $(var(x(0)))$ the mean (variance) in the group of non-treated establishments. The figures in table 8 were obtained by taking the average over the covariates. Thus the smaller the bias, the better the match quality.

# 4 Sample selection on unobservable covariates

In the previous section we have introduced various econometric methods which can be used to estimate the effect of a treatment on an outcome variable in the presence of sample selection due to observable covariates. But what if, even after we have balanced the observable covariates, there are still differences between these two groups? Or stated differently, what if the conditional independence assumption, which is the cornerstone of the matching as well as the probit estimation, does not hold and hence the sample selection mechanism is not only due to observable but also to unobservable covariates? In our application it could be the case e.g. that management ability or corporate culture play an important role in both determining the innovation behavior and the decision whether the establishment will successfully apply for a public subsidy or not. In this case the estimates would still be plagued by a "hidden" bias, upward we suspect, which could not be remedied by observable covariates, since both management ability and corporate culture are hardly quantifiable.

In the following we will apply estimation strategies which can be used under these circumstances. In order to control for a possible endogeneity of $D$, we will explicitly model the treatment decision in a simultaneous equation framework:

$$Y^* = \beta_1' X_1' + \Delta D + \epsilon_1 \tag{5}$$

$$D^* = \beta_2' X_2' + \epsilon_2 \tag{6}$$

where the latent variables $Y^*$ and $D^*$ are connected with their observable counterparts via a threshold model, i.e. $Y = I(Y^* > 0)$ and $D = I(D^* > 0)$, respectively.

Note, that equation (6) was estimated to obtain the propensity score for the matching while equation (5) was estimated as its nonparametrical counterpart. But now we allow these two equations to be connected with each other via the disturbances, $\epsilon_1$ and $\epsilon_2$, for which we assume that

$$\begin{pmatrix} \epsilon_1 \\ \epsilon_2 \end{pmatrix} \sim i.i.d.N(0, \Sigma) \text{ and } \Sigma = \begin{pmatrix} 1 & \rho \\ \rho & 1 \end{pmatrix}.^{21} \tag{7}$$

Equations (5) and (6) constitute a simultaneous equation model. Since both of the dependent variables are binomial, such models are also called mixed simultaneous equation

[21] Note the necessary normalization of the two probit equations. We have skipped the individual index for convenience.

models (MSEM) and since the observable counterpart of the latent endogenous dummy variable $D^*$ enters equation (5), the model is of type II.[22] Type II MSEM exhibit not only the usual identification problem, which is ensured in our example if one looks at the list of variables included in $X_1$ (table 6 in the appendix) and $X_2$ (table 3), but additionally also the so called coherency problem which makes some additional parameter restrictions necessary. The coherency problem arises since due to the inclusion of dummy endogenous variables as additional regressors, no explicit reduced form exists. Without going into detail how to derive these consistency restrictions, it is sufficient to notice that a two equation type II probit model in order to be coherent must feature a recursive structure between the endogenous variables.[23] In our model this restriction is ensured by assuming that subsidies in 1997/1998 might have an impact on innovations in 1999/2000 but not vice versa.

The model in (5) and (6) has been estimated simultaneously by maximum likelihood as described in Maddala (1983).[24] Due to space limitations we will not present all estimation results in the paper. Table 12 in the appendix contains the same figures as for the previous separate probit estimation. Recall once again that the probit as well as the matching estimator yielded mostly positive results. However, if we additionally account for sample selection due to unobservables, the results change dramatically. For the pooled estimation e.g. using the probit model we found a significant average treatment effect on the treated of 6% for the narrow innovation concept. Now, this effect becomes negative and amounts to -17%. And the reason for this reversing sign is a positive and significant correlation between the treatment and innovation equation ($\rho = 0.64$). It thus seems that there are unobservable covariates which have a positive impact on both the subsidy and the innovation decision and which, if we do not account for them, will lead to an overestimation of the true treatment effect.

This result can be generalized to the other sub-samples as well. We find evidence that in all cases there is a substantial correlation between the two equations and hence the previous positive average treatment effects turn out to be overestimated. For small and medium sized establishments e.g. we now get negative results. The only remarkable

[22]For a more thorough discussion of mixed simultaneous equation models see Blundell and Smith (1993, 1994). For an application see Hujer, Caliendo, and Radić (2002).

[23]For details see e.g. Schmidt (1990).

[24]The estimation was done using STATA. Codes and detailed estimation results are available from the authors.

exception are East German establishments. Conditioning only on observable covariates as we did in the previous section yielded an average treatment effect on the treated of 6% for the broad and 7% for the narrow innovation concept. However, assuming that public subsidies are still endogenous, we now find a negative self selection process due to unobservables ($\rho$ = -0.61/-0.45) and hence the estimated effects even increase to 39% for the broad and 31% for the narrow innovation concept.

An obvious question at this point concerns the reliability of these results. Having found convincing evidence for the endogeneity of the treatment decision, the more crucial question is whether our simultaneous probit model is able to mitigate this endogeneity. To this aim note that the model contained in equations (5) and (6) may be interpreted as an instrumental variable approach where the instruments used to model the endogenous dummy variable $D$ are contained in $X_2$. If one looks at the variables contained in $X_1$ and $X_2$ which can be found in the tables 6 and 3, one can see that these are by and large the same exogenous variables. However, the variables contained in (5) refer to the year 1998 while the instruments refer to the year 1996. Our model is thus identified, but to be optimal and valid instruments, the exogenous variables in $X_2$ must satisfy the following two conditions: First, they must be significant in the participation equation and second, they must be insignificant in the outcome equation.

In the following we conducted an informative test to check the validity of the instruments. In order to check whether $X_2$ is informative for $D$, we estimated a simple probit with $D$ as the dependent and $X_2$ as the independent variables and conducted a $LR$-test for the overall significance of $X_2$. In a second step we tested whether $X_2$ has any explanatory power for the outcome equation by estimating a probit with $Y$ as the dependent and $X_1$ and $X_2$ as the independent variables. Again we conducted a $LR$-test for the joint significance of $X_2$.[25]

Table 4 contains the results of these $LR$-tests and although this is just an intuitive test, we find convincing support for the validity of our instruments. Only for micro establishments using the broad innovation concept as the outcome variable, the instruments have also explanatory power in the outcome equation and thus may still be correlated with the error term. In all other cases, however, they are highly significant in the participation

[25]For a similar approach see Evans and Schwab (1995). This test is only an informal test because in the presence of endogenous regressors we would have to apply again a simultaneous estimator.

but insignificant in the outcome equation.

**Table 4: Validity test for the instruments**

| | | Broad innovation concept | | Narrow innovation concept | |
|---|---|---|---|---|---|
| | Equation | $\chi^2$-value | p-value | $\chi^2$-value | p-value |
| All establishments | Participation | 31.64 | 0.00 | 31.64 | 0.00 |
| | Outcome | 4.34 | 0.63 | 7.07 | 0.31 |
| West Germany | Participation | 10.42 | 0.11 | 10.42 | 0.11 |
| | Outcome | 2.47 | 0.87 | 8.47 | 0.21 |
| East Germany | Participation | 29.42 | 0.00 | 29.42 | 0.00 |
| | Outcome | 5.44 | 0.49 | 8.59 | 0.20 |
| Micro establishments | Participation | 14.64 | 0.02 | 14.64 | 0.02 |
| | Outcome | 11.63 | 0.07 | 6.95 | 0.33 |
| SME | Participation | 18.93 | 0.00 | 18.93 | 0.00 |
| | Outcome | 2.67 | 0.85 | 6.83 | 0.34 |

## Nonparametrical crosscheck

Our access to longitudinal data is limited. In particular we do not have regular information on the outcome decision. The foregoing time period for which such information is available refers to 1996/1997 and hence overlaps with the treatment period. The following considerations must therefore be treated with caution. An often used empirical strategy in the presence of individual specific but unobservable effects is the difference-in-differences estimator.[26] This estimator takes the difference of the change in the outcome variable after, i.e. 1999/2000, and before the treatment period, i.e. 1996/1997, between treated and non-treated establishments and thus cancels out any individual specific unobservable effects:

$$\Delta^{DID} = E\left[\left(Y^T_{\text{after}} - Y^T_{\text{before}}\right) - \left(Y^C_{\text{after}} - Y^C_{\text{before}}\right)\right] \tag{8}$$

where $Y^T$ $(Y^C)$ stands for the outcome variable of treated (non-treated) establishments and the subscript denotes the time period.

An extension which also takes sample selection due to observables into account is the conditional difference-in-difference estimator which replaces the expression for the non-

[26]For more details see Meyer (1995).

treated establishments $\left(Y^C_{\text{after}} - Y^C_{\text{before}}\right)$ by its matched counterpart.[27] The following table 5 contains the corresponding estimation results. The estimation was conducted conditional on the same propensity score as previously and by using an one-to-one and Gaussian kernel matching approach, respectively.

**Table 5: Conditional difference-in-differences estimator (in %)**

| | Broad innovation concept | | Narrow innovation concept | |
|---|---|---|---|---|
| | One-to-one | Kernel | One-to-one | Kernel |
| All establishments | -2.27 (4.65) | 0.87 (3.59) | 4.44 (4.81) | 4.56 (3.91) |
| West Germany | 2.06 (8.80) | 2.58 (5.83) | 1.01 (11.54) | 8.75 (6.74) |
| East Germany | -2.84 (5.99) | 1.55 (3.90) | -3.78 (5.71) | 2.94 (3.53) |
| Micro establishments | -2.98 (7.65) | -1.77 (5.91) | -5.25 (7.30) | 1.42 (4.74) |
| SME | 9.84 (7.05) | 4.98 (5.28) | 9.33 (6.27) | 8.58 (5.74) |

Note: Standard errors in parentheses.

The results reveal that in most of the cases, except for small and medium sized establishments where we found a weak significant and positive impact, the average treatment effect on the treated is insignificant. We once again repeat that these results have to be treated with caution since due to data limitations the treatment period overlaps with the pre-treatment outcome period, but we also find support for our previous empirical findings, where the estimated treatment effects turned out to be insignificant or even negative if we additionally condition on unobservable factors.

# 5 Conclusions

The objective of this paper was to add another piece of evidence to the ongoing political 'evergreen' debate about the necessity to reduce public deficits by decreasing public subsidies. Although there is broad consensus about this point, the dispute which programs should be cut down is much more controversial. An often raised objection in this context is that public R&D subsidies should be excluded from this reduction since they are helpful to stimulate technological change and innovations. Despite the considerable amount spent in this area, the number of evaluation studies which try to assess the impact of such programs is rather limited.

[27] See Heckman, Ichimura, Smith and Todd (1998) and for an application Hujer, Caliendo and Radić (2001).

In this paper we applied various microeconometric methods to overcome the inherent sample selection problem in estimating the effect of public subsidies on the innovation capacity of German establishments. In contrast to other studies which analyze the relation between private and public R&D activities, we used an output oriented innovation concept, namely new products and services. Such a concept closely resembles the intended goals of most of the R&D programs in Germany. We started with a simple mean comparison which yielded significant positive impacts. We then accounted for sample selection due to observable characteristics by employing a parametrical multivariate probit and a nonparametrical matching estimator. The result was a reduction in the effects which, however, still pointed to the view that public subsidies have a positive impact on innovations with differences for West and East Germany and different size classes.

Finally, we were able to show that especially with establishment data one has to take sample selection due to unobservables into account as well. Estimating a simultaneous probit and a conditional difference-in-differences model changed the results dramatically: We only find positive effects for East German establishments whereas in all other cases the results are at most insignificant. Obviously, public R&D programs subsidize to a large part innovation projects which would have been undertaken successfully also in the absence of such subsidies. Another upshot of all the estimation is the following: The more observable and unobservable factors one takes into account in order to make treated and control establishments more comparable, i.e. the better our understanding of the sample selection process, the smaller the estimated treatment effect of subsidies on innovations becomes.

Of course we are aware that there are several limitations and drawbacks of our study which have to be taken into account. In this paper we have only considered one kind of heterogeneity, namely with regard to West and East Germany and regarding different size classes. However, it could be the case that different measures have a different treatment effect and hence a multiple treatment framework would be more appropriate.[28] Another point concerns the assumed time structure. In this study we were forced to abstract from the possibility that there might be more variable lag structures. Additionally, it might also be the case that different "dosing schemes", i.e. the fact whether establishments received a

[28] See e.g. Lechner (2002). Note, however, that limitations in the number of observations hindered us to conduct such an analysis.

subsidy only once or more times, might have a different impact on the outcome variable.

One also has to keep in mind that a microeconometric evaluation like this one is only a first step to evaluate the total net effect of public subsidies on innovation activities. Only in the absence of spillover effects, these microeconomic results can be generalized to the whole economy. Despite all these caveats, we think that it is justified to close with the following quote of Lichtenberg (1984) who also found, after accounting for unobservable heterogeneity, unsatisfactory impacts of public R&D subsidies for the United States and claims that: "These findings thus make heavier the burden of proof on those who would claim that federal contract R&D makes a positive contribution to aggregate technical progress."

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# A Variables used for the outcome equation

There is a vast literature about the determinants of innovations.[29] A useful classification is a differentiation between market and firm specific factors. Market factors include e.g. the competition intensity, market concentration, exposure to international trade but also demand factors, like profitability and expected development of the business volume which are all expected to have an innovation enhancing impact (see also table 6).

Firm specific factors on their part can be further split into internal and external technological capabilities. The idea behind this classification is the following: Industry sectors are characterized by differing technological opportunities which have also an impact on the innovation behavior of individual establishments. Additionally, innovations can be boosted if establishments co-operate with other institutions. However, in order to benefit from such external co-operations, there must be some technological expertise within the establishment. External technological capabilities may be captured by variables indicating R&D cooperations with other institutions like universities. Internal technological capabilities include e.g. the state of technology, the existence of a R&D department, the share of high qualified employees and employees devoted to R&D. Other firm specific factors which were also included in the estimation are the size of establishments measured by the number and squared number of employees, industrial and regional dummies.

**Table 6: Variables used for the estimation of the outcome equation**

| Variable | Mean | SE |
|---|---|---|
| Competition intensity in 1998 (1 = No pressure, ..., 4 = High pressure) | 3.43 | 0.83 |
| Gini concentration of business volume in 1998 | 0.82 | 0.06 |
| Export share in 1998 | 6.05 | 16.85 |
| State of technology used in 1998 (1 = Up-to-date, ..., 5 = Out-of-date) | 2.12 | 0.79 |
| R&D department existing | 0.14 | 0.35 |
| Share of high qualified employees in 1998 | 0.62 | 0.42 |
| Number of R&D co-operations | 0.34 | 0.99 |
| Number of employees in 1998 | 252 | 1,174 |
| Share of one man businesses | 0.33 | 0.47 |
| Share of establishments organized as a business partnership | 0.10 | 0.29 |
| Share of private limited companies | 0.39 | 0.49 |
| Share of capital companies | 0.07 | 0.25 |
| Business development in 1998 (1 = Very good, ..., 5 = Insufficient) | 1.98 | 0.72 |

[29] For an overview see e.g. Cohen (1998).

**Table 7: Probit estimation of the innovation decision**

| | Broad innovation concept | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | All establishments | | West Germany | | East Germany | | Micro establishments | | SME | | Large establishments | |
| | Estimate | SE | Estimate | SE | Estimate | SE | Estimate | SE | Estimate | SE | Estimate | SE |
| $\Delta$ | 0.07 | 0.09 | -0.10 | 0.18 | 0.20 | 0.11 | 0.11 | 0.15 | 0.13 | 0.14 | -0.52 | 0.30 |
| Marg. effect | 0.07 | 0.09 | -0.03 | 0.06 | 0.08 | 0.04 | 0.04 | 0.05 | 0.04 | 0.05 | -0.15 | 0.09 |
| ATE | 0.02 | 0.03 | -0.03 | 0.05 | 0.06 | 0.03 | 0.02 | 0.04 | 0.04 | 0.04 | -0.11 | 0.08 |
| | $\chi^2$-value | p-value | $\chi^2$-value | p-value | $\chi^2$-value | p-value | $\chi^2$-value | p-value | $\chi^2$-value | p-value | $\chi^2$-value | p-value |
| $LR$-overall | 554.66 | 0.00 | 429.01 | 0.00 | 140.81 | 0.00 | 156.35 | 0.00 | 182.56 | 0.00 | 145.15 | 0.00 |
| $LR$-industry | 69.55 | 0.00 | 36.35 | 0.00 | 39.98 | 0.00 | 55.99 | 0.00 | 23.28 | 0.11 | 20.61 | 0.04 |
| $LR$-region | 37.27 | 0.00 | 16.06 | 0.10 | 21.92 | 0.00 | 38.01 | 0.00 | 20.68 | 0.19 | 6.73 | 0.92 |
| $LR$-controls | 207.81 | 0.00 | 161.26 | 0.00 | 53.08 | 0.00 | 58.20 | 0.00 | 78.30 | 0.00 | 29.24 | 0.02 |
| Observations | 2,013 | | 1,172 | | 837 | | 860 | | 852 | | 281 | |
| | Narrow innovation concept | | | | | | | | | | | |
| | All establishments | | West Germany | | East Germany | | Micro establishments | | SME | | Large establishments | |
| | Estimate | SE | Estimate | SE | Estimate | SE | Estimate | SE | Estimate | SE | Estimate | SE |
| $\Delta$ | 0.28 | 0.09 | 0.28 | 0.16 | 0.34 | 0.12 | 0.17 | 0.17 | 0.41 | 0.15 | 0.22 | 0.22 |
| Marg. effect | 0.09 | 0.03 | 0.09 | 0.05 | 0.11 | 0.04 | 0.04 | 0.04 | 0.13 | 0.06 | 0.08 | 0.08 |
| ATE | 0.06 | 0.02 | 0.07 | 0.04 | 0.07 | 0.03 | 0.03 | 0.03 | 0.09 | 0.04 | 0.07 | 0.06 |
| | $\chi^2$-value | p-value | $\chi^2$-value | p-value | $\chi^2$-value | p-value | $\chi^2$-value | p-value | $\chi^2$-value | p-value | $\chi^2$-value | p-value |
| $LR$-overall | 219.29 | 0.00 | 128.68 | 0.00 | 124.04 | 0.00 | 107.66 | 0.00 | 103.37 | 0.00 | 46.79 | 0.32 |
| $LR$-industry | 44.41 | 0.00 | 19.15 | 0.26 | 177.77 | 0.00 | 51.40 | 0.00 | 40.70 | 0.00 | 11.60 | 0.39 |
| $LR$-region | 19.11 | 0.26 | 7.92 | 0.64 | 6.39 | 0.17 | 19.74 | 0.14 | 11.53 | 0.78 | 7.79 | 0.93 |
| $LR$-controls | 83.71 | 0.00 | 54.42 | 0.00 | 41.00 | 0.00 | 44.68 | 0.00 | 36.42 | 0.00 | 19.89 | 0.18 |
| Observations | 2,012 | | 1,172 | | 837 | | 844 | | 846 | | 283 | |

**Table 8: Some matching diagnostics**

| | Dropped establishments due to common support (%) | | Absolute standardized bias of observable covariates (%) | | |
|---|---|---|---|---|---|
| | Treated | Controls | Before | After matching[a] | |
| All establishments | 0.19 | 0.17 | 0.19 | 0.05 | 0.15 |
| West Germany | 0.24 | 0.15 | 0.28 | 0.12 | 0.11 |
| East Germany | 0.20 | 0.17 | 0.21 | 0.07 | 0.16 |
| Micro establishments | 0.08 | 0.21 | 0.17 | 0.08 | n.a.[b] |
| SME | 0.18 | 0.31 | 0.18 | 0.07 | n.a.[b] |
| Large establishments | 0.44 | 0.43 | 0.21 | 0.20 | n.a.[b] |

[a] One-to-one matching with replacement conditional on the propensity score and additionally on the industry sector (second column).
[b] Conditioning on the industrial sector not possible due to an insufficient number of observations.

**Table 9: Matching estimator of the average treatment effect of the treated**

| | All establishments | | | | | |
|---|---|---|---|---|---|---|
| | Broad innovation concept | | | Narrow innovation concept | | |
| | ATE | SE | t-value | ATE | SE | t-value |
| One-to-one matching | 0.05 | 0.04 | 1.16 | 0.11 | 0.04 | 2.97 |
| Conditional one-to-one | 0.07 | 0.04 | 1.66 | 0.12 | 0.04 | 3.12 |
| Kernel matching | 0.06 | 0.03 | 1.92 | 0.11 | 0.02 | 5.38 |
| Conditional kernel | 0.08 | 0.03 | 2.70 | 0.12 | 0.03 | 4.29 |

**Table 10: Matching estimator of the average treatment effect of the treated**

| | West German establishments | | | | | |
|---|---|---|---|---|---|---|
| | Broad innovation concept | | | Narrow innovation concept | | |
| | ATE | SE | t-value | ATE | SE | t-value |
| One-to-one matching | 0.00 | 0.08 | -0.01 | 0.10 | 0.08 | 1.25 |
| Conditional one-to-one | 0.18 | 0.09 | 2.04 | 0.21 | 0.08 | 2.67 |
| Kernel matching | 0.09 | 0.05 | 1.72 | 0.17 | 0.06 | 2.71 |
| Conditional kernel | 0.15 | 0.07 | 2.29 | 0.17 | 0.07 | 2.26 |
| | East German establishments | | | | | |
| | Broad innovation concept | | | Narrow innovation concept | | |
| | ATE | SE | t-value | ATE | SE | t-value |
| One-to-one matching | 0.02 | 0.05 | 0.35 | 0.06 | 0.04 | 1.29 |
| Conditional one-to-one | 0.11 | 0.05 | 2.29 | 0.12 | 0.04 | 2.82 |
| Kernel matching | 0.09 | 0.03 | 3.33 | 0.11 | 0.03 | 3.67 |
| Conditional kernel | 0.11 | 0.04 | 2.89 | 0.12 | 0.03 | 3.99 |

**Table 11: Matching estimator of the average treatment effect of the treated**

| | Micro establishments | | | | | |
|---|---|---|---|---|---|---|
| | Broad innovation concept | | | Narrow innovation concept | | |
| | ATE | SE | t-value | ATE | SE | t-value |
| One-to-one matching | 0.05 | 0.07 | 0.83 | 0.04 | 0.06 | 0.74 |
| Kernel matching | 0.09 | 0.05 | 1.97 | 0.10 | 0.05 | 2.13 |
| | Small and medium establishments | | | | | |
| | Broad innovation concept | | | Narrow innovation concept | | |
| | ATE | SE | t-value | ATE | SE | t-value |
| One-to-one matching | 0.16 | 0.06 | 2.70 | 0.17 | 0.05 | 3.40 |
| Kernel matching | 0.07 | 0.04 | 1.94 | 0.14 | 0.04 | 3.19 |
| | Large establishments | | | | | |
| | Broad innovation concept | | | Narrow innovation concept | | |
| | ATE | SE | t-value | ATE | SE | t-value |
| One-to-one matching | -0.18 | 0.12 | -1.58 | 0.01 | 0.11 | 0.12 |
| Kernel matching | -0.08 | 0.08 | -1.10 | 0.07 | 0.08 | 0.88 |

**Table 12: Simultaneous Probit estimation of the innovation decision**

| | Broad innovation concept | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | All establishments | | West Germany | | East Germany | | Micro establishments | | SME | |
| | Estimate | SE | Estimate | SE | Estimate | SE | Estimate | SE | Estimate | SE |
| $\Delta$ | -0.76 | 0.31 | -1.59 | 0.33 | 1.23 | 0.46 | -0.68 | 0.83 | -1.18 | 0.27 |
| Marg. effect | -0.20 | 0.06 | -0.32 | 0.05 | 0.46 | 0.15 | -0.16 | 0.16 | -0.22 | 0.06 |
| ATE | -0.20 | 0.07 | -0.32 | 0.04 | 0.39 | 0.15 | -0.13 | 0.16 | -0.29 | 0.10 |
| $\rho$ | 0.51 | 0.18 | 0.81 | 0.17 | -0.61 | 0.29 | 0.49 | 0.50 | 0.80 | 0.15 |
| | $\chi^2$-value | p-value | $\chi^2$-value | p-value | $\chi^2$-value | p-value | $\chi^2$-value | p-value | $\chi^2$-value | p-value |
| $LR$-overall | 372.36 | 0.00 | 245.60 | 0.00 | 95.02 | 0.00 | 122.33 | 0.00 | 156.15 | 0.00 |
| $LR$-industry | 61.16 | 0.00 | 33.78 | 0.01 | 42.60 | 0.00 | 24.57 | 0.10 | 21.97 | 0.11 |
| $LR$-region | 41.46 | 0.00 | 15.56 | 0.11 | 14.67 | 0.01 | 42.54 | 0.00 | 32.28 | 0.01 |
| $LR$-controls | 209.60 | 0.00 | 151.07 | 0.00 | 43.31 | 0.00 | 50.55 | 0.00 | 91.84 | 0.00 |
| Observations | 1,970 | | 1,150 | | 820 | | 851 | | 843 | |
| | Narrow innovation concept | | | | | | | | | |
| | All establishments | | West Germany | | East Germany | | Micro establishments | | SME | |
| | Estimate | SE | Estimate | SE | Estimate | SE | Estimate | SE | Estimate | SE |
| $\Delta$ | -0.78 | 0.49 | -0.84 | 0.90 | 1.11 | 0.83 | -1.03 | 0.47 | -0.89 | 0.07 |
| Marg. effect | -0.15 | 0.06 | -0.16 | 0.11 | 0.39 | 0.31 | -0.11 | 0.06 | -0.15 | 0.04 |
| ATE | -0.17 | 0.09 | -0.17 | 0.12 | 0.31 | 0.26 | -0.15 | 1.00 | -0.19 | 0.01 |
| $\rho$ | 0.64 | 0.28 | 0.62 | 0.50 | -0.45 | 0.51 | 0.74 | 0.28 | 0.83 | 0.06 |
| | $\chi^2$-value | p-value | $\chi^2$-value | p-value | $\chi^2$-value | p-value | $\chi^2$-value | p-value | $\chi^2$-value | p-value |
| $LR$-overall | 216.44 | 0.00 | 68.91 | 0.00 | 80.73 | 0.00 | 97.83 | 0.00 | 105.88 | 0.00 |
| $LR$-industry | 41.25 | 0.00 | 127.56 | 0.00 | 30.50 | 0.01 | 29.24 | 0.02 | 20.14 | 0.21 |
| $LR$-region | 24.26 | 0.08 | 23.26 | 0.11 | 3.86 | 0.43 | 27.13 | 0.04 | 30.96 | 0.01 |
| $LR$-controls | 104.16 | 0.00 | 7.05 | 0.72 | 39.99 | 0.00 | 36.72 | 0.00 | 49.18 | 0.00 |
| Observations | 1,969 | | 1,150 | | 819 | | 850 | | 843 | |