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Employment effects of investment subsidies by German Regional Policy - The 45 Billion Euro Intervention

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Abstract

This study investigates the employment impact of investment subsidies in Germany. With 45 billion Euro for about 100,000 firms since the German reunification, these capital grants are the central element of regional policy for strengthening employment in economically weaker regions.

The effects of investment grants are analysed by comparing the employment growth of assisted firms with their counterfactual outcome. The hypothetical growth without grant is estimated with panel data from 1999 to 2009 for each German firm in the social security records which are linked with the official records of investment grants. The control group are selected out of total non-assisted firms by combining covariate and propensity score matching methods. The employment effect is determined by cross-section estimators.

The assisted firms show a significantly stronger employment growth than their control group. This result is quite robust. Even if the control group is restricted to those non-assisted firms which have had a similar employment growth before the year of allowance, the differences in the growth after the allowance persist. Further heterogeneity tests do not reveal significant hidden influences. All subgroups analysed show nearly the same growth difference between assisted and non-assisted firms.

JEL classification: B41, C14, C21, C81, H71, R00, R11, R28, R58

Berlin, May 2020

1. Introduction

It is undisputed in literature on regional economics that economic development on a national level is not spatially uniform, but takes a very differentiated course (Florida 2005). While the economy in some regions is very strong, serious development weaknesses can be observed in others. These patterns are often based on pronounced path dependencies and tend to change slowly, i.e. adjustment processes take place over decades rather than years (Alm/Titze 2018).

As a result of these disparities, regional policies exist in many countries. In the Federal Republic of Germany, such measures are even constitutionally safeguarded. Accordingly, the Basic Law provides the Federal Government with the right to legislate "if and to the extent that the establishment of equivalent living conditions in the territory of the Federal Republic or the safeguarding of legal or economic unity in the interest of the state as a whole necessitates federal regulation".

As a "child of the 1950s" (Eberstein 1999: 8), the regional economic policy of the Federal Government has undergone several major changes over the course of its development in the past decades. The first funding policies were primarily aimed at stabilising the economic situation in particularly underdeveloped regions against the background of the consequences of the destruction wreaked by the Second World War, and could be described as "widespread assistance" in the emergency and peripheral zone areas declared for this purpose. The focus later increasingly shifted to target the funding for central locations and regional action programmes. In 1972, the Joint Task 'Improvement of Regional Economic Structures' (GRW) was introduced as an instrument that has been at the centre of regional economic policy ever since.

The importance of the GRW has changed significantly over the course of the European integration process and German reunification in particular. Its central starting point is the improvement of employment opportunities in the regional assisted areas. Based on this motivation, it is not surprising that particularly investments by firms in structurally weak regions are subsidized under the GRW.

This paper addresses the question as to whether and to what extent state subsidies for investments have enhanced employment growth in the assisted firms. The study covers all firms that received a GRW investment grant between 1998 and 2008. The impact of the investment grants is measured against the result that would have been achieved if the firms had not been assisted. As this counterfactual result cannot be directly observed, it is estimated on the basis of all firms (subject to social insurance contributions) which did not receive any GRW investment grant during the investigation period. The control groups are selected by matching methods.

The paper is structured as follows. Chapter 2 outlines the objective of regional economic assistance and the intended effect of the investment grant. The results of earlier studies are then summarized in Chapter 3 and the research approach used in this paper is addressed in Chapter 4. Following the description of data and investigation method (Chapters 5.1 to 5.5), the results are presented in two steps. Chapter 5.6 shows the effect of the GRW investment grants on employment growth of the assisted firms. The sensitivity analysis follows in Chapter 6. The final part is a short discussion about the significance of the results (Chapter 7).

2. Aim and intended effect of GRW investment grants

The aim of regional investment grants is to "permanently strengthen employment and income in structurally weak regions" (Deutscher Bundestag 2009: 49). In the period from 1991 to 2019, around 45 billion Euro were approved for investment grants for firms as part of the GRW programme.¹ Investment grants are awarded mainly to small and medium-sized firms in the structurally weak regions.² Only sectors with supra-regional markets³ are assisted in order to create additional sources of income in the structurally weak regions. The average value of subsidy is around one quarter of the investment costs; the effective share depends on various individual conditions.⁴

The investment grants are addressed to individual firms whereas the policy objective is the total employment of a region. In order to have an impact on the macroeconomic objective, investment grants must work along a lengthy and multifaceted chain of effects, ranging from the incentive to increase investments via the impact of the assisted investment on the growth of the firm to the effects the firm's growth exert on regional income and employment. We limit this investigation to the causal relationship between the receipt of an investment grant on the one hand and the employment effect within the assisted firm on the other.

In production theory, even this partial relationship does not lead to an unequivocal employment result⁵. The first effect of the investment grant is a reduction of capital costs. Consequently, the relative price of labour increases and favours to substitute labour by capital input; c.p. labour demand declines. Therefore, the desired increase in the input of labour necessarily requires a growth of output which, however, is determined by a number of additional conditions which are essentially independent of the grant. In principle, a firm will only be able to transfer the benefits of the investment grant into additional production and employment if the additional quantity – notwithstanding the company's own sales targets - can also be sold on the market. The competitiveness of the assisted firm must therefore also increase with the investment, e.g. by the reduction of unit costs resulting from the factor substitution or by the realisation of technological or other product innovations with the investment.

Whether a firm succeeds in strengthening its competitiveness by the investment is largely independent of the grant. With the exception of the reduced cost of capital, the performance of the additional investment depends upon the firm itself, as its decision to apply for the financial aid. Nevertheless, the funding authority can exert influence to the extent that there is no legal entitlement to the investment grant.⁶ Within the framework of the eligibility requirements (Deutscher Bundestag 2009: 42 ff.), the authorities decide on a discretionary basis whether to approve a firm's application for investment grant.

¹ Additionally, around 23.5 billion Euro were granted to foster public investments in industrial infrastructure during this period.

² See Deutscher Bundestag (2009:164 ff.) and Alm/Fisch (2014) for the delineation of the assisted area, most of which is located in former GDR.

³ See Deutscher Bundestag (2009: 152). In addition to some services, these mainly include the manufacturing industries. The selection is justified by the demand for a "primary effect", which presupposes that the assisted firm sells most of its products supra-regionally.

⁴ For the individual conditions, see Deutscher Bundestag (2009) or Eberstein/Karl (2008). Theoretically, the maximum rate can reach 50% under certain conditions (see in particular Deutscher Bundestag 2009: 43 ff.). In fact, however, only 4% of all grants between 1998 and 2008 did reach the maximum rate.

⁵ The model-based theoretical approach, in which a substitutional input relationship of capital and labour is mainly assumed, is extensively documented in the literature, see Moore/Rhodes (1973:83 f.), Buck/Atkins (1976:215 f.), Franz/Schalk (1983: 150 f.) or Faini/Schiantarelli (1987:225 f.).

⁶ In contrast to another kind of investment aid ("Investitionszulage") which mainly consisted of a tax reduction available for all firms investing in former GDR. The firms had a legal title to that reduction if the conditions for the tax reduction were met. The Investitionszulage was phased out at the end of the year 2013.

3. Previous micro-economic impact analysis of GRW investment grants

Since the introduction of the GRW at the end of the 1960s, attempts have been made to monitor the success of the investment grants (Tetsch/Benterbusch/Letixerant 1996)⁷. Due to the large number of investigations, the Federal Government considers "the GRW ... with its almost complete statistical coverage [as] one of the best evaluated national funding instruments"⁸. However, the efforts to monitor success have not been without criticism. In particular, the Federal Audit Office repeatedly points out in its reports that the practised "success control ... is insufficient" (e.g. Federal Audit Office 2006: 139).⁹

The GRW's focus is placed on the overall economy of regions; accordingly, macro-economic studies aimed at a comprehensive assessment of all funding effects predominate in the literature. In short, most macro-analyses come to the conclusion that the investment grants have increased investment and employment in the assisted regions, although the strength of the impact varies depending on the specific delimitation of the regions, observation periods and methods of the studies.¹⁰

However, the claim of macro-economic assessment is associated with a high degree of uncertainty of the results. The interdependencies between individual investment support and macro-economic development are so complex that even their theoretical formulation is difficult to implement.¹¹ The difficulties of empirical implementation are even greater. Macro-economic impact analyses require information that is often not available at a regional level, or only available for a subset of companies, and must therefore be replaced to a considerable extent by working hypotheses and proxy variables¹².

Micro-econometric impact analyses avoid some of the estimation uncertainties by concentrating on a special section of the chain of effects, on the effects on the assisted firm as direct beneficiary. The micro-econometric approach is therefore not at odds with macro-economic analysis, but can complement it with findings for a central element of effect.

In the German-speaking countries there are only a few studies on the effects of regional investment grant on individual companies. Gräber et al. (1987) should be seen as a pioneer study in that their analysis is not limited to questionnaires or other surveys of assisted firms - as it was the case with previous studies¹³ - but links the GRW investment grant data with official statistics on the manufacturing industry (see also Holst 1986). In this way, reliable data were obtained both for the assisted and for the other non-assisted firms, free from survey-related bias. As a result, employment and investment in

⁷ One motive for the early monitoring of success is not least the design of the GRW as a joint task of Federal government and the constituent states (Länder): the Federal and Länder governments jointly define the scope and rules of funding. Therefore, they must agree on the eligibility of individual regions, for which regular monitoring of the achievement of regional policy objectives is helpful.

⁸ Response of the Federal Government to a major parliamentary interpellation ('Große Anfrage') October 2008 (Deutscher Bundestag 2008: 4)

⁹ See also the current recommendation of the Scientific Advisory Board of the Federal Ministry of Economics and Technology (Wissenschaftlicher Beirat beim Bundesministerium für Wirtschaft und Technologie (BMWi) 2013). In response to criticism from the Federal Audit Office, the GRW commissioned a study in 2008 (Bade/Alm 2010), which is used as a basis for this study.

¹⁰ For an overview of earlier work, see Lammers/Niebuhr (2002), Alm (2013) or for a description of more recent studies as Dettmann et al. (2016) and Eberle et al. (2019). Other studies, e.g. Eckey/Kosfeld (2005) or Alecke et al. (2012), also find (slight) positive effects, but these are mitigated by spill-over effects (to the detriment of the assisted regions). One exception is Koetter/Wedow (2012), whose estimates based on a neoclassical growth model do not reveal any clear promotional effects.

¹¹ See Lammers/Niebuhr (2002: 30). Very often the relationship is illustrated by macro-economic investment and output functions, which are intended to record the substitution and output effect (e.g. Faini/Schiantarelli 1987; Franz/Schalk 1989). The limitation to a single estimation equation derived from the neoclassical growth model (e.g. Koetter/Wedow 2012) appears to be of little credibility solely because of the large number of unconsidered influences, especially since the effect of investment grants is often still inconsistently specified (see Alecke/Mitze/Untiedt 2012: 5).

¹² E.g. Ragnitz (2003: 37). Bradley/Untiedt (2007) provide a good example of the dichotomy between the demand for knowledge and complexity on the one hand and the significance and validity of the results on the other.

¹³ See the overview e.g. by Fürst (1971), Freund/Zabel (1978) or Krist/Nicol (1982).

the plants of assisted firms changed to a distinctly more positive extent between 1975 and 1982 than in the rest of the economy.

Due to data protection concerns about linking data of individual grants with official statistics, it was only with Koller et al. (2004) that a new attempt could be made to use the employment records of the Federal Employment Agency (BA) as basis. They, too, identify a considerably more favourable development for the assisted firms.¹⁴ At about the same time, several further studies were carried out at the IWH Halle using the so-called IAB Establishment Panel as a basis.¹⁵ The impact of investment grants on the manufacturing industry in former Eastern Germany was examined. In short, despite differences in methodology, the studies reach the unanimous conclusion that in former Eastern Germany "the funding has contributed to a significant increase in investment" (Ragnitz 2003:3).

For other countries, there are numerous micro-economic studies which largely come to a similar conclusion (see Cerqua/Bonzini 2014:115; Criscuolo/Martin/Overman/Van Reenen 2012: 3). However, the strength of the employment effects identified varies, which is not surprising given the large differences in the kind of subsidies and in the national economic conditions. Equally different are the methodology and the data used. Although the impact of regional subsidies is consistently determined by comparing assisted and non-assisted firms, in some cases this is done by aggregating individual data at regional level (e.g. Givord/Rathelot/Sillard 2013 for the Zones Franches Urbaines in France or Busso/Gregory/Kline 2013 for the Urban Empowerment Zone in the USA). In addition to the matching method (e.g. Bergström 2000 for Sweden), various regression methods are mainly used for estimation purposes.¹⁶ In a study for Great Britain, Criscuolo/Martin/Overman/Van Reenen (2012) used an instrument variable (IV) approach, in which the changes in the area of assistance were used to determine the IV. Not least noteworthy are some studies for Italy, which profit from a special and internationally rare way of awarding investment grants for their research approach; e.g. Bondonio/Greenbaum (2014), Cerqua/Pellegrini (2014) or Bernini/Pellegrini (2011). In short, the subsidies of the "L488" Act are awarded in a type of auction in which only a part of the applying firms get a grant. Since the data of these non-assisted firms and their development are available, these firms which are interested in subsidies and were in principle also eligible may be used for comparison with the assisted firms.

¹⁴ Due to some methodological weaknesses, the validity of the results is rather uncertain. However, the study was important in that, at the authors' suggestion, since the year 2000 firms applying for a GRW investment grant must state the code with which their establishment designated for the investment is listed in the BA employment records (Deutscher Bundestag 2000: 213). This created a necessary prerequisite for identifying the assisted plant in the employment statistics and for comparing them with the other non-assisted establishments.

¹⁵ The IAB Panel is a sample from the BA employment records (Bellmann 2002) and covered (at the time of analysis) around 1800 manufacturing establishments for the new Länder in the years 1999 to 2001 (Ragnitz/Lehmann 2004: 225). In addition to a variety of operational characteristics and the level of investment, the IAB panel also contains information on whether a firm was supported by a particular measure. The various IHW studies (Stierwald/Wiemers 2003, Ragnitz 2003, Lehmann/Stierwald 2004 and Ragnitz/Lehmann 2004) primarily differ in the micro-econometric methods used to examine the IAB Establishment Panel.

¹⁶ See the overview in Alm (2013:145 ff.) A detailed description would go beyond the scope of this paper, especially as its findings (consistently positive, but mostly weak effects of financial aid) cannot be simply transferred to the different German conditions in both state aids and economic growth.

Despite the relatively clear results, the studies presented are confronted with some difficulties which affect the validity of its results for the impact of GRW investment grants. The considerable international differences in both funding and corporate development conditions speak against the simple transfer of foreign results. In the GRW investigations, the empirical basis in particular is regarded as a major weak point (e.g. Ragnitz 2003: 26 ff.). Apart from the relatively short period (1999 to 2001), the IWH studies use a small sample whose representativeness is not known.¹⁷ In addition, the analysis focuses on the investment effects, which are only an intermediate objective for regional policy, as explained above.¹⁸

The following impact analysis addresses precisely these weak points. It focusses on the employment growth of the assisted firms after the allowance of investment grant. And it aims at the complete coverage of all assisted firms in order to compare them with the non-assisted rest of firms.

4. Study approach and methods

4.1 Conceptual considerations

In impact evaluation, the effect of a treatment, here the effect of the investment grant on the employment of the beneficiary firm¹⁹ is determined by comparing its employment growth with the outcome that would have happened without the treatment.²⁰

$$(1) \quad \delta = v_i(D=1) - v_i(D=0) = v_i^1 - v_i^0 \quad \text{with } v_i \text{ as employment growth of the firm } i \\ \text{following the investment grant and} \\ D \text{ as binary variable for the grant.}$$

However, both changes cannot be observed simultaneously for a single firm: it has either received a grant or it has not, which since Holland (1986: 947) has been called a "*fundamental problem of causal inference*" (see also Heckman/LaLonde/Smith 1999: 1879). The way out of this dilemma is to estimate the unobservable counterfactual change with the growth of those firms which did not receive an investment grant.²¹

$$(2) \quad E(\delta) = E(v^1 - v^0) = E(v^1|D=1) - E(v^0|D=0)$$

¹⁷ This applies in particular to the selection of firms that have received an investment aid. Other concerns relate to the way in which the level of investment is included in the analysis: it was determined (apparently in cross-section across all firms covered) whether the assisted firms invested relatively more (in terms of employees and/or turnover) than the non-assisted ones (Ragnitz/Lehmann 2004: 226; Stierwald/Wiemers 2003: 12). However, this perspective does not necessarily correspond to the objective of the state aid: the grant is intended to encourage firms to make *additional* investments. Consequently, the focus should be on the *change* in investments after the allowance of grant.

¹⁸ Through further comparisons, in particular with official data on regional employment development, the authors come to the conclusion that the investment grant "in the vast majority of branches of the economy has also had an effect on employment - albeit with a certain delay" (Ragnitz 2003: 31).

¹⁹ This effect is also abbreviated to ATT ("*average treatment effect on the treated*") to distinguish it from other effects such as the "*average treatment effect*" (ATE) on the total population (here: the total employment of all firms); see Heckman/Robb 1985.

²⁰ The theoretical basis is the potential outcome model (Roy 1951; Rubin 1974, 1979; Holland 1986). In short, the model assumes that in principle both results (with and without assistance) are possible for a single firm, regardless of whether it actually participates in the funding.

²¹ With this "statistical solution" mentioned by Holland (1986: 947), the effect of funding can therefore only be determined for a group of firms, but not for a single individual (Fitzenberger/Hujer 2002: 3; Caliendo/Hujer 2006: 3).

The theoretically simplest solution for estimation is the experiment which draws a random sample out of a set of similar firms. The selected ones receive a grant and their following employment change is contrasted with the group of non-assisted firms. However, such an experiment is excluded by the GRW funding system. Quite practically, not all firms are willing to accept a subsidy. In addition, only certain types of investment are eligible for grant; thus, depending on their specific investment the selected firms may be differently affected by the subsidy, contrary to the intended similarity. More generally speaking, it is to be expected that there are systematic differences between the selected firms and the rest which cannot be controlled by the random sample.

These systematic distortions can be subsumed into two groups. The first concerns the probability to receive an investment grant. Some firms are simply not appropriate for a comparison because they are not eligible for GRW investment grants. This includes not least all firms which do not intend to invest at the time in question. Others may invest, but are not eligible because they are located outside the assisted areas, belong to a non-assisted sector, exceed the allowed firm size or would not be able to meet other criteria of the funding authority. Furthermore, the willingness to accept state aid may also play a role. Some firms may not want to be assisted, for example because the stipulated creation of additional jobs or other grant regulations are too strict to them or simply, because they spare the efforts in applying for an investment grant.

The other group of distortions concerns the overall economic conditions of a firm. As a rule, it is to assume that these factors have a far greater influence on the firm growth than an investment grant. The conditions for growth remain effective, while the investment grant (first of all)²² is limited to a one-off stimulus. One of the most important determinants of corporate growth is market demand, which can develop very differently depending on the sector. A comparison of firms belonging to different sectors must therefore inevitably lead to distorted results. Similarly influential can be the regional environment or internal factors that determine the productivity and competitiveness of a firm.

Consequently, in order to avoid systematic distortions only those firms must be selected for the counterfactual change which do not differ significantly from the assisted firms with regard to both growth determinants and the probability of funding. If X denotes the systematically distorting variables, then the unobservable change $E(v^1|D=0)$ is estimated by $E(v^0|X, D=0)$ and the effect is calculated with equation (3):

$$(3) \quad E(\delta) = E(v^1|X, D=1) - E(v^0|X, D=0)$$

Whether the control of the distorting variables is sufficient for the formation of appropriate control groups and for a credible estimate ultimately depends on three central prerequisites. The first concerns the stability of the causal effect of the funding. In order to be able to determine it, the effect must be stable and must not be influenced, for example, by the assistance of other firms and the resulting macro-effects (Garfinkel/Manski/Michalopoulos 1992). Without this "*stable unit treatment value assumption*" (SUTVA) (Holland 1986: 949; Lechner 2002: 6), a micro-econometric analysis is not sufficient to identify the effects on individual firms. Based on the concrete data situation, we consider such macro-effects to be negligible in this study, following the argumentation of Lechner (1997: 6). In comparison to the total number of all firms, the number of assisted firms is in the per-thousand range, as is the share of subsidized investments in the total economic investment volume.²³ In addition, only firms with supra-regional activities are assisted, so that a displacement of other local firms (Harris/Trainor 2005: 52) is not to be expected, at least in the short term.

The second condition refers to possible differences in the basic events without intervention. The estimation procedure is to be designed in such a way that the comparison groups no longer differ system-

²² In the medium term, however, the subsidized investment may improve the growth conditions. In the case of marginal suppliers in particular, the investment might not have been made without the grant or would have been made only with some curtailments.

²³ Even with a differentiation by region and economic sector, the share of assisted firms is on average less than 10%.

atically after controlling for the distorting variables and thus the "*unit homogeneity*" (Holland 1986: 949) is guaranteed. Finally, the third condition points out that even with unit homogeneity, the effect on assisted and non- assisted firms can be fundamentally different. For example, the approval of the investment grant could be subject to certain conditions which can only be met by the assisted firms. In addition, the grant is awarded on a discretionary basis so that further restrictions in favour of a particularly promising target group cannot be ruled out. A consistent estimate must take these influences into account when selecting the control group, which, since Rosenbaum (1984: 426), has been referred to as "*conditional independence assumption*" (CIA)²⁴: After control by corresponding control variables X, the allowance of state aid is conditionally independent of the (later) result.

The three preconditions mentioned are fulfilled if each of the distorting factors is represented by the empirically observed variables (Heckman/Robb 1985: 161; Caliendo 2006: 42). If - after checking all relevant control variables - the control group no longer differs significantly from the assisted firms, the remaining growth differences must be exclusively due to the funding.

4.2 Methods

The study is done in two steps. First, in chapter 5, we start from the assumption - referred to as "*selection on observables*" (Caliendo 2006: 42) - that all relevant determinants of funding probability and growth are observed with the data and that therefore all three above-mentioned preconditions are fulfilled. In the second step, in chapter 6 this assumption is called into question, where it is examined how robust the estimations are against influences that cannot be directly observed with the available data. The robustness test consists on the one hand of the heterogeneity analysis of the estimated investment grant effects. It is tested whether the effect of investment aid varies significantly between different sub-groups of a control variable that may reflect the effect of unobserved determinants.

On the other hand, the employment growth of the assisted firms prior to the date of allowance is used as an indicator for unobserved influences. If hidden factors were active, their effect should be observable in the change of employment before the allowance, too. The simplest estimator for such investigation is the "naive before and after" comparison (Hujer/Caliendo/Radic 2004: 146), in which the problem of the non-observables is "solved" by assuming the *time invariance of all influences* i.e. including the observed as well as other unobserved factors. This strong and rather unrealistic assumption is weakened by the "*difference-in-differences*" estimator (DiD) (Heckman/LaLonde/ Smith 1999: 32; Caliendo 2006: 24). Here, the supposed time invariance is limited to the *previous difference* of growth: the difference before the allowance is taken as estimator for the effects of non-observable influences after the allowance. However, despite the comment use of the DiD estimator in the literature (e.g. Bernini/Pellegrini 2011), we consider this assumption of the temporal invariance of the differences unnecessarily rigid. Firstly, without knowing the causes of (previous) growth differences, it is yet supposed that they will also have *exactly* the same effect in the following years. Secondly, the approach implicitly assumes that the firms selected for the control group are clearly different from the assisted group, at least before the date of allowance.

To avoid these latter implications, the employment growth before the allowance is used as additional control variable for matching (Dehejia/Wahba 1999): only those non-assisted firms are included in the control group which have shown a similar employment change in the period before. Insofar as the difference in previous growth may be considered as estimator for hidden factors, the additional matching implicate that both the assisted firms and their control group should have been exposed to the same (non-observable) influences in the previous period.²⁵

With v_a as employment growth in the period before the allowance [t-1, t], the corresponding estimation equation is:

$$(4) \quad E(\delta) = E\left(v^1 \mid X, v_a, D = 1\right) - E\left(v^0 \mid X, v_a, D = 0\right).$$

²⁴ Rosenbaum/Rubin (1983a: 43) argue similarly with the demand for a "*strongly ignorable treatment assignment*" or Caliendo (2005: 30) with the "*principle of unconfoundedness*".

²⁵ For an illustration of the two different methods, see Figure A1 in the appendix.

The *matching method* is used to estimate the counterfactual result. Alternatively, multivariate regression methods could also be used. However, we apply matching procedures as regression techniques have a number of shortcomings²⁶ and the database used in this study is very rich: Due to the total survey of all firms (with employees subject to social insurance contributions), the assisted firms may be contrasted with a very large number of potential controls. In addition, due to its transparent and largely nonparametric estimation approach, the matching method offers particular advantages for sensitivity analysis, where the effects of non-observable influences are investigated.

The basic idea of matching is based on the idea of the experiment: for each assisted firm, another non-assisted firm is sought which is as similar as possible to it in terms of the control variables. The better the selection, the more the selected non-assisted firm fit the counterfactual estimate. In the ideal case, the assisted firms differ from their selected counterparts ("twins") only in the fact that they have received a GRW investment grant.

The concrete properties of the matching method are laid down in the *assignment model* (Gangl/DiPrete 2004: 16). Since the individual method steps depend on the concrete data situation, the details are explained after the description of the data material (5.1). The core of matching lies in the construction of the control group. It is determined by the control variables (5.2) and the method (5.3) used. The matching algorithm (5.4) defines the rules according to which the observation units are compared and the corresponding twin pairs selected. Finally, the "*balancing tests*" (5.5) examine the similarity of the control group with the group of assisted firms which is decisive for the quality of the selection and thus for the credibility of the estimation results. A reliable estimate of the effect of investment grants requires - irrespective of the question of whether all relevant influences are fully covered - that the structures of both groups of firms are "balanced" with regard to the control variables covered.

5. Empirical analysis

5.1 Data basis

Two different data sources are linked together for the impact analysis. The records of GRW investment grant by the Federal Office of Economic Affairs and Export Control (BAFA) which provide information on the establishments where the firms assisted under the GRW scheme have invested. Available for this study are the investment grants of the years 1998 to 2008.²⁷ For comparison with other, non-assisted establishments, the records of each employee subject to social insurance from the BA are aggregated to establishments records by using the (pseudonymized) identification number of establishment. Available are the years 1999 to 2009 which are linked to one single panel database.²⁸ In total, the employment records consist of about 3.8 million establishments.

²⁶ The most important restrictions are discussed in detail in the literature (see Alm 2013:145 ff. or Smith/Todd 2005:342 ff.) and are therefore only briefly listed here. A central point concerns the dependency on the functional form of the regression function, which is not known and therefore has to be specified with the help of assumptions. Another problem can be caused by the (missing) "*common support*": With the regression function, functional relationships are also estimated for those value ranges for which there are no common observations of assisted and non-assisted firms (see e.g. Caliendo 2006: 71). A similar difficulty caused by the data arises when the two groups differ greatly in terms of numbers and a very small group of assisted firms is contrasted with a much larger number of non-assisted firms, with the consequence that the regression function is largely determined by the control group (Lechner 1997: 12). In the case of GRW funding, an endogeneity problem can also be expected: Some determinants, such as the economic sector briefly discussed above, have an impact not only on the development of the firm but also on whether a firm is will apply for the investment aid.

²⁷ For a detailed description of the data and its processing, see Bade/Alm (2010).

²⁸ For data protection reasons, the BA establishment file could not be used. The aggregation of employee records to establishment records follows the procedure described in Hethey/Schmieder (2010). We use the so-called quarterly material as of 30 June of a year.

Both data bases, the BAFA and BA records are connected by the identification number of establishment. As mentioned above, this number is to be stated by the firm applying for a GRW investment grant.²⁹ Almost all assisted plants (24,117 out of a total of 25,909 for the period 1998 to 2008) could be identified in the 3.8 million plants aggregated from the BA employment records.

The use of the BA employment records allows considerable advantages for impact analyses. Firstly, the panel data provide reliable and consistent information on the actual development of the assisted plants. The BAFA records give only diffuse and less reliable information (Federal Audit Office 2006: 139). Secondly, the employment records allow a homogenous comparison of assisted and non-assisted plants whereas the use of different surveys involves the danger of systematic inconsistencies between the surveys. And thirdly, the BA employment records represent a nearly full survey covering all plants (with at least one employee subject to social insurance contributions) and around 80% of all employed persons (Lüken 2002: 169). Thus, on average per year, two thousand assisted plants are contrasted with around two million potential reference plants. Finally, a further advantage is the large number of characteristics (see Table 1) - compared with other official surveys - which can be used to seize essential determinants of the application for assistance and of plant growth as well.³⁰

5.2 Data variables

The *endogenous* variable of this impact analysis is the *employment growth* of a plant. As the employment effect of an investment grant needs time to show up, the minimum period of analysis is two years beginning from the allowance. Consequently, with 2009 as the last available year of employment records in this study, the impact analysis must be limited to the investment grants of 2007, which reduces the number of plants considered from 24,117 to 22,796.

Employment growth is referred to the number of employees in the final year of 2009. The advantage of this method of calculation is that different lengths of periods could be compared and all available growth information can be evaluated. A uniform period would instead reduce either the number of plants surveyed and/or the significance of the rate of change.³¹ The disadvantage of the calculation is the mixing of short-term and long-term effects. For this reason, a uniform period of two years for all cohorts of allowances is additionally analysed in chapter 6.2.

As the plants have been assisted in different years and therefore the length of the observation period up to 2009 differs, the growth of employment is calculated as an average annual rate (p.a.).

With t , the year of the (first) funding, b_t of the number of employees in t and b_{2009} in 2009, the rate of growth of employment is as follows:

$$(5) \quad v_t = \left(\frac{b_{2009}}{b_t} \right)^{\frac{1}{2009-t}} - 1$$

The information about the *grant allowances* comes from the BAFA records³² and is binary coded: if the plant received a GRW investment grant between 1998 and 2008, it has the value "1" and otherwise

²⁹ See footnote 14: missing data were subsequently collected with the help of the Federal Employment Agency; see Bade/Alm (2010: 9 ff).

³⁰ Linking to other data sources, in particular on investments and finances, was not permitted for data protection reasons. Apart from that, such surveys are hardly available. The few that would be suitable for an extension cover only a small part of all plants, e.g. the IAB Panel and/or differ considerably in the definition of the observation units, e.g. the file of the manufacturing industry.

³¹ In order to be able to analyse all years of allowances, the analysis would have to be limited to a period of two years and thus to short-term effects. If the period were to be extended instead, further younger cohorts would have to be excluded.

³² The classification of "assisted" is restricted exclusively to GRW investment grant; information on other subsidies not available. For example, it is to suppose that the plants located in the former GDR profit from an (additional) investment aid ("Investitionszulage" which consists of a tax reduction). However, there is a framework of maximum funding rates and the Investitionszulage is given in addition to the investment grant. Thus, the re-

the value "0". Conversely, only those plants which have not received any GRW grant within this period are considered as non-assisted.

Table 1 presents all available employment variables that are suspected of affecting the eligibility of grant and/or employment growth. The values refer to the *year of allowance* which is determined by the date of the first allowance.³³ On the one hand, it defines the length of the period in which the endogenous variable, the growth of employment till 2009. On the other hand, it refers to the general economic conditions, such as the economic situation, to which the firm was exposed at the time the funding was granted.

Table 1: Overview of the matching variables

Variable	Explanation
<u>Assisted</u>	"1": Investment grant between 1998 and 2008 "0": No investment grant between 1998 and 2008
<u>Dependent variable</u>	
Employment growth	Annual average growth rate from year of allowance till 2009
<u>Control variable</u>	
Year	Year of allowance
Location	GRW labour market region ¹⁾
Industry	70 industries ²⁾
Plant size	Number of employees
Qualification of employees	- Share of low-skilled workers - Share of academics
Functional structure	- Share of production activities (occupational group 01 to 55, excluding 032, 41 and 52) ³⁾ - Share of technical services (occupational groups 62, 62, 65) - Share of research and development (032, 60, 61 and 883)
Age of plant (see footnote 34)	Period since the first social security notification: - "New formation": = 1 st notification in the year of allowance - "Young": = 1 st notif. max. 2 years before the year of allowance - "Other": = 1 st notif. at least 3 years before the year of allowance - "1999": = 1 st notif. in 1999 and assisted in 1999 till 2001
¹⁾ Deutscher Bundestag (2009: 164 ff); ²⁾ See Table A1 in the appendix. ³⁾ Federal Statistical Office, Classification of Occupations, July 1992, Wiesbaden Source: Own representation	

al amount of subsidy does not vary much between firms with and without receiving the Investitionszulage (Ragnitz 2003: 16).

³³ Approximately one third of all subsidized plants received allowances in several years. Instead of concentrating on the initial year, matching estimates for multiple participation are also conceivable (e.g. Caliendo/Hujer 2006: 13). Another variant is to consider the absolute (or relative) amount of the investment grant instead of binary coding.

The *labour market region* and the *economic sector* are doubly effective. They have a direct impact on the likelihood of receiving funding, since the investment grant is only awarded in certain regions and for certain sectors of the economy (footnote 3). In addition, there is strong theoretical and empirical evidence that the regional and sectoral context affect the employment growth of a firm (see e.g. Blien/Wolf 2002). In order to capture the regional influences we use the delineation of 270 GRW labour market regions. For the sectoral impact the plants are grouped into 70 industries (see appendix, Table A1).

The *plant size* is measured by the number of employees in the year of allowance. It may have an influence both on the probability of receiving a grant and on the employment growth of the plant. However, in both cases the potential effect is not clear because contrary influences cannot be ruled out. On the one hand, the number of investment projects increases with size c. p. and thus the number of aid opportunities. In addition, larger plants are generally part of larger and multi-plant firms. These have greater scope for location decisions which may be affected by the availability of grants. On the other hand, smaller firms are given preference in the allocation of GRW investment grant. They receive consistently higher grant intensities, so that the incentive to invest should be greater for smaller firms. In some regions, investment grants for large firms is even prohibited (Deutscher Bundestag 2009: 41 ff.).

The influence of the *plant age* is similarly ambivalent. Based on their experience, older firms could find it easier to apply for the investment grant. Conversely, younger firms could be given preferential funding. In addition, younger firms (if they survive) seem to grow faster than older ones (e.g. Fritsch/Noseleit 2012). Since the presumed influence of the firm age is exercised within the first years after foundation, the study controls for *young plants*.³⁴

The other variables in Table 1 are intended in particular to reflect the *competitiveness* of a plant. Essentially, these are properties that are assumed to be closely related to the technological and innovative strength. The indicators are calculated individually for each plant on the basis of the training and activity of its employees. These include in particular the proportion of employees working in *research and development* and the broader *technical services* sector. The share of production is used as counterpoint. Whilst production is still an important part of the manufacturing industry nowadays, an above-average share of production may also indicate that the plant in question is likely to be an "extended workbench". The *human capital* of a plant is seized by the formal qualifications of its employees. The lower and upper end of the training spectrum³⁵ is covered: the proportion of *low-skilled* employees primarily describes the risk potential of the plant. In the course of economic structural change, it is above all the low-skilled employees whose jobs are threatened. The proportion of *highly qualified* employees, on the other hand, can be interpreted as an indicator of the competitive potential.

5.3 Matching method

There are various ways of using the characteristics listed in Table 1 for matching. The range extends from the individual comparison of each control variable ("*covariate matching*") to the joint consideration of several variables, the characteristics of which are condensed into a single overall measure.³⁶

The individual comparison offers the advantage of being able to pay more attention to the special features of the assisted plants. On the other hand, there is a danger that cross-characteristic relationships will be overlooked. Furthermore, the individual comparison with the number of characteristics taken

³⁴ The employment records do not include a direct indication of the year of foundation. It can only be determined indirectly by the date of its first social security declaration. The age of plant is then determined by the difference to the year of allowance. A plant is "newly founded" if it has registered for social insurance for the first time in the allowance year. "Young" plants are those which have their first declaration no later than two years before the allowance.

³⁵ The group with the lowest qualification also includes those employees whose educational qualifications are unknown. In fact, they are predominantly persons engaged in simple activities such as household services. It can therefore be assumed that their training qualification is not indicated because it is not relevant in terms of both activity and the level of the qualification.

³⁶ For a detailed description of the different methods see e.g. Caliendo (2006: 46 ff.).

into account inevitably reduces the chance of finding a suitable control plant. This is especially true when *exact covariate matching* is required for each variable. If, instead, the identity requirement is weakened, it must be clarified which differences are still permissible and how the differences in the individual characteristics affect the overall view.

By contrast, a common consideration of all control variables offers the advantage of being able to evaluate the "overall proximity" of two plants, i.e. the similarity with simultaneous evaluation of all considered characteristics. A frequently used method to capture the overall proximity is the *propensity score* (PSC) (Rosenbaum/Rubin 1983a). It is based on the above-mentioned ideal that the assisted plant and its selected twin are similar in terms of the determinants of employment development and only differ in the fact that they have received an investment grant. The propensity score is usually estimated with a probit analysis (Gangl/DiPrete 2004: 16 f., Caliendo 2006: 73 ff.) in which this relationship is reversed. By analysing all the plants, i.e. with and without grant, we estimate the relationship between the control variables and allowance. The PSC as the result of the probit estimator thus indicates the probability of allowance as a function of the control variables considered. The more closely the PSCs of assisted and non-assisted plants coincide, the more similar the two plants must therefore be to each other, taking all influencing factors into account at the same time.

5.4 Matching algorithm

In this study, the matching algorithm is so designed that the information of the data material is exploited as much as possible. To this end, the two matching methods, covariate and PSC are applied in two successive selection steps for each of the 22,796 (up to the year 2007) assisted plants. The first step is covariate matching by checking those variables for which an exact match is required (*exact covariate matching*). Specifically, for each single assisted plant, those non-assisted plants are selected which, in the year of allowance, are located the same region, belong to the same economic sector and have the same age.

A further selection characteristic is the size of plant, for which no exact match is necessary, however. The larger the plant, the less precisely the supposed influence of size corresponds to the exact number of employees. Usually therefore, the plant size is divided into (increasingly broader) classes of employment. However, size classes lead to distorted estimates at their limits.³⁷ To avoid this bias, for each assisted plant intervals (*caliper matching*) ranging from minus 20 to plus 20 per cent of the number of employees are calculated and used for matching.³⁸

The first step results in the selection of about 915,000 non-assisted plants, each of which corresponds at least to one of the assisted plants with regard to the five characteristics of region, sector, age, employment size interval and year of allowance. For 4,499 assisted plants, no corresponding counterpart could be found³⁹, so that a total of 18,297 cases are available for further matching (see table 2).⁴⁰

In the second step, a single plant out of the set of (915,000) possible references is assigned to each assisted plant. As mentioned above, the PSC is used to measure the "overall proximity" of the plant in question - viewed across all control remaining variables which are interval-scaled and do not necessitate exact matching. By "*nearest neighbour matching*" that non-assisted plant is selected which, in the year of allowance, most closely corresponds to the assisted plant in terms of the PSC value.⁴¹ As result

³⁷ For plants at the borders of a size class, the plants of the neighbouring class would not be eligible for selection, although some of those could actually be closer to it than other members of their own class.

³⁸ The concrete interval limits were determined iteratively in order to achieve the closest possible agreement between the control and assisted plants.

³⁹ The main reason for the exclusion lies in the regional and economic sector criteria. With regard to the endogenous variable, the growth rate after allowance, there is no significant difference between the excluded plants and those remaining in the analysis.

⁴⁰ The number of possible references varies between the assisted plants. On average, more than 50 non-assisted plants are available for selection to an assisted plant (of the 18,297). 90% of all cases had at least five alternatives; in the worst case there were still three possible plants.

⁴¹ Alternatively, other matching methods such as caliper or kernel matching are also conceivable in the second step. However, the step-wise selection involves that the number of reference plants in the second step is often

of the matching process, to each of the 18,297 assisted plant is assigned a non-assisted reference plant, which, in the year of allowance, is as similar as possible, except for being assisted.

5.5 Balancing tests

In short, the selection procedure is only a means to an end (Morgan/Harding 2006:41): the credibility of the impact analysis is primarily determined by the similarity of the matched plant, however it has been achieved. This condition is fulfilled if both groups of assisted and non-assisted groups are characterised by similar frequency distributions for the control variables. In the following, the mean value is used as a distribution measure (Rosenbaum 2002) and the correspondence between both groups is estimated by the Cochran-Cox t test. On the one hand, the usual zero hypothesis is tested; on the other hand, we estimate the (absolute) difference of the mean values with a 1% error probability.⁴²

Table 2: Differences between assisted and non-assisted plants before and after matching

Variable	Mean value					Tolerance threshold ²⁾
	Before matching		After matching			
	Assisted n=22,796	Non-assisted n=3.8 million	Assist. n=18,297 ^{a)}	Non-ass.		
Number of employees	41.9 (2.6848)	8.9 (0.0401)	***	25.9 (0.5546)	25.5 (0.5760)	1,1
Low-skilled workers ¹⁾	30.3% (0.0023)	40.0% (0.0002)	***	30.8% (0.00257)	29.8% (0.00259)	1,8 ^{b)}
Academics ¹⁾	7.3% (0.0011)	4.3% (0.0001)	***	7.1% (0.00118)	6.4% (0.00127)	1.1 ^{b)}
Production activities ¹⁾	47.3% (0.0025)	23.4% (0.0002)	***	45.8% (0.00290)	46.7% (0.00285)	1.9 ^{b)}
Technical services ¹⁾	5.0% (0.0010)	2.1% (0.0001)	***	4.8% (0.00107)	3.8% (0.00097)	1.5 ^{b)}
Research and development ¹⁾	3.7% (0.0008)	1.4% (0.0000)	***	3.5% (0.00077)	2.8% (0.00088)	1.1 ^{b)}
Propensity score	0.9806 (0.00004)	0.8783 (0.00087)	***	0.9505 (0.00049)	0.9502 (0.00049)	0.02

^{a)} Only the assisted plants for which a control plant has been found. ^{b)} Percentage points.
¹⁾ Percentage of all employees. In brackets: standard error.
***1% significance level (Cochran/Cox t-test).
²⁾ Maximum absolute difference of the two means at 1% error probability.
Source: BAFA records; BA employment records; own estimations.

too small for significant kernel or other statistical estimates. One argument against the caliper or similar method is the difficulty to determine the exact cut-off limit objectively. In addition, the closest approximation is here realized by selecting the closest neighbour.

⁴² Other distribution parameters and tests are also conceivable; see the overview in Rosenbaum (2002) and Caliendo (2005:78 ff.). Additional information on the location and shape of the distributions is given below in the heterogeneity analysis and in the appendix.

Table 2 shows the mean values of the two groups before and after matching for each characteristic.⁴³ Before matching, the composition of the assisted plants group clearly differs from the control group in all characteristics (including the nominal scaled variables not shown). The assisted plants with around 42 employees are on average more than four times as large as all other non-assisted plants (8.9 employees). Equally striking are the differences in the other variables. Accordingly, the plants receiving an investment grant are much more research-intensive in terms of the share of research and development (3.7 to 1.4%) or the share of technical services (5.0 to 2.1%). The qualification of the employees is also clearly better, especially the proportion of academics (7.3 to 4.3%). The differences in the individual characteristics are also reflected in the overall estimator across all control variables: the PSC shows a significantly lower average probability of allowance with a significantly higher standard error for the non-assisted plants.

After matching, the distance between the two groups of plants has shrunk considerably for all variables. On the one hand, the control group (due to exact covariate matching) has exactly the same economic, regional and age structure as the assisted plants at the year of allowance. On the other hand, the subsequent selection by the PSC ensures that the mean values and standard errors of the interval-scaled control variables of both groups were considerably closer. With around 26 employees on average, assisted and reference plants have nearly the same employment size. Similarly, regarding the qualification structure and the share of research and development, both groups of plants correspond almost exactly on average.

The Cochran/Cox t-test clearly confirms the impression made by the simple numerical comparison. In addition to the significance of the zero hypothesis, the last column contains the value that the (absolute) difference of the mean values does not exceed with an error probability of 1%. According to this, even in the case of technical services, the mean values differ significantly by only 1.5 percentage points. The maximum difference of plant size is about one employee on average. For the PSC, the figure A5 in the appendix shows that not only is the mean value almost identical, but that the frequency distributions of the two groups of plants also overlap almost completely. Thus, by the matching algorithm it was possible to select a control observation for each assisted plant with approximately the same PSC value.

5.6 Determining the employment effect of the GRW investment grant

The employment effect of the GRW investment grant is the difference between the actual and estimated counterfactual growth of employment. As explained above, this interpretation requires firstly that the two groups of plants do not differ significantly with regard to the control variables; this has been clearly achieved by the matching. A second requirement for this interpretation is the completeness of the observed variables what will be discussed below in more detail. At this point, we start from the hypothesis that all influences to growth and allowance probability have been captured by the observed control variables. At least in comparison to earlier studies, this study makes a clear advance as the data of the study permit not only a comprehensive analysis of all assisted plants, but also contains essential features on the probability of allowance and growth determinants.

⁴³ For the sake of clarity, the variables of year, region, economic activity and age are not shown. Due to exact covariate matching, assisted and non-assisted plants are identical for these characteristics. In addition, these variables are nominally scaled and their average values of no meaning.

Table 3: Average effect of the GRW investment grant 1998 to 2007

	Mean value	
	Assisted n=18,297	Non-assisted n=18,297
Growth in employment from the year of allowance to 2009 (average of all cohorts 1998 to 2007)	-0.33% (0.00198)	-12.72 *** (0.00142)
Average grant effect p.a.	+12.4 percentage points	
In brackets: Standard error. ***: 1% significance level (Cochran/Cox t test). Source: BAFA records; BA employment records; own estimations.		

According to Table 3 the grant effect, measured by the average growth difference between the assisted plants and their control group, is 12.4 percentage points p.a. on average over all cohorts. The average number of employees in the assisted plants decreased only slightly by 2009 to -0.3% p.a. In contrast, the control group had a loss of 12.7% p.a.⁴⁴, even though these plants do not differ significantly from the assisted ones in the determinants of growth and allowance probability. The low standard errors also show that the distributions of the growth rates of both groups of plants overlap only marginally (see in detail the histogram in the top of Fig. A3 of the appendix). Thus, with an error probability of 1%, it cannot be rejected that the distance between the two mean differs by 10.6% at minimum.

6. Sensitivity analysis

6.1 Employment growth before the year of allowance

The large growth advance of the assisted plants is statistically significant. However, its size is somewhat surprising compared to the theoretical effect of the investment grant. On its own, it merely means a reduction in the cost of capital, which does not necessarily have to be accompanied by an increase in competitiveness. The following section will therefore examine whether, in addition to the observed control variables, there are further systematic differences between the two groups of plants infringing the above mentioned requirements for a counterfactual estimate.

In particular, two areas of influence can be considered for distortion, which cannot be adequately tested with the data from this study. The first concerns the selection of reference plants and results from the link between grant and investment. By deploying additional capital firms intend to improve their production possibilities and thus ultimately their competitiveness and growth. Actually, all assisted plants invest, but there is no information for the non-assisted plants in the BA records. Therefore, firms without investments cannot be excluded from the selection as reference and may contribute to the weak growth of the control group.

The second area of influence relates to the assisted plants and the tendency that - from the set of all eligible plants - the dynamic ones are preferred in awarding the investment grant. One reason for this may be the willingness to apply for the grant: since the firm submitting the application has to state

⁴⁴ In comparison with the (usually calculated) rate of change in total employment, the loss of -12.7% p.a. may seem very high, as total employment in 2009 is only 0.4% (or 0.03% p.a.) below that of 1999. The reason for the discrepancy is, on the one hand, that the rates of change calculated here only take into account those plants that already existed in the starting year. By contrast, total employment in 2009 also includes start-ups after 1999. On the other hand, the -12.7% is an unweighted mean value, where each plant is included equally in the average calculation. In a calculation weighted by size (e.g. Bade/Alm 2010), the annual growth rate would be -5.7% p.a. (and +2.6% for the assisted plants).

concrete job targets and assure their achievement, companies that are uncertain about the (job) success of their investment tend to be deterred. The funding authority could also try to exploit its discretionary scope by giving preference to promising investments⁴⁵. Both influences can neither be excluded nor tested with the available data. Thus, the (above-mentioned CIA) condition that the allowance is conditionally independent of the subsequent employment development is obviously not satisfied.

In addition, distortions are still conceivable with regard to growth conditions. Those determinants of growth commonly used in empirical regional research are taken into account. However, other important characteristics of firm competitiveness, such as productivity and profitability, are not available and therefore cannot be tested.

The available panel data, however, offer an alternative solution in that plant growth before the allowance can be taken into account as additional control variable, As far as some non-observable influences were already effective before the investment grant, they should be reflected in the development before the allowance. This idea is supported not least by the experience that the need for investments mostly arises as a result of expansions. The grant for an investment could therefore be more attractive for plants which a particular growth in the recent years. Furthermore, they are likely to be more confident with regard to the required job targets and other restrictions of the application. The data (cf. Table 4) are in line with this hypothesis. In the last two years before the allowance, the assisted plants perform clearly better (+11.5% p.a.) than the rest (+3.5% p.a.).

Table 4: Differences between assisted and non-assisted plants before and after matching - only the cohorts 2001 to 2007, with the additional control variable 2-years-growth before the year of allowance -

Variable	Mean value					Tolerance threshold ²⁾
	Before matching		After matching			
	Assisted n=6,302	Non-assisted n=2.3million		Assist. n=4,525 ^{a)}	Non-ass.	
Number of employees	65.1 (9.1943)	12.6 (0.0066)	***	34.0 (1.7230)	33.2 (1.8532)	1
Low-skilled workers ¹⁾	30.4% (0.00391)	35.5% (0.00026)	**	30.7% (0.0047)	30.5% (0.0047)	1.1 ^{b)}
Academics ¹⁾	6.5% (0,00172)	4.6% (0.00011)	***	6.3% (0.00207)	5.9% (0.00213)	0.7 ^{b)}
Production activities ¹⁾	48.0% (0.00450)	24.1% (0.00023)	***	45.6% (0.00548)	45.4% (0.00565)	0.6 ^{b)}
Technical services ¹⁾	4.9% (0.00167)	2.2% (0.00008)	***	4.7% (0.00199)	4.1% (0.00178)	0.9 ^{b)}
Research and development ¹⁾	3.4% (0.00128)	1.4% (0.00006)	***	3.2% (0.00147)	2.9% (0.00160)	0.5 ^{b)}
Change in employment from year t-2 till the year of allowance t	+11.5% (0.00404)	+3.5% (0.00020)	***	+7.3% (0.00335)	+8.5% (0.00351)	1.3 ^{b)}
Propensity score	0.9765 (0.00167)	0.8486 (0.00009)	***	0.987 (0.000234)	0.987 (0.000234)	0

^{a)} Only the assisted plants for which a control plant has been found. ^{b)} Percentage points.
¹⁾ Percentage of all employees. In brackets: standard error.
***1% significance level (Cochran/Cox t-test).
²⁾ Value not exceeded by the difference of the two means; with 1% error probability.
Source: BAFA records; BA employment records; own estimations.

⁴⁵ Precise information on the extent to which and according to which criteria applications are rejected is not available in the BAFA records or in other officially published documents.

However, the inclusion of the previous growth involves a major disadvantage: Only those plants can be analysed which already show up in the employment records two years before the respective year of allowance. That excludes both the early years 1998 to 2000 (10,643 cases) and the start-ups and one-year-old plants (5,673), so that only one quarter of all assisted plants remain for the study (6,302 of the original 22,796). The reduction in the number of cases also applies to the non-assisted plants, of which only 2.3 (instead of 3.8) million can be considered as possible reference.

Nevertheless, the systematic differences between assisted and non-assisted plants remain largely unchanged despite the much smaller sub-set (Table 4). As before, both groups of plants differ significantly in the observable characteristics. In order to avoid these systematic distortions, we use the same matching methods as above in 5.4, with one exception: the growth effect is now estimated with equation (4) above, i.e. growth before the year of allowance is included as control variable. In addition to the exact match (at the time of the grant) of sector, region, size and age and an approximately equal number of employees, a similar employment growth during the two years before the allowance is required, too.⁴⁶ From the remaining set of non-assisted plants fulfilling these conditions, that one is selected for the control group whose PSC is closest to the assisted case in question.

No reference could be found for 1,777 assisted plants.⁴⁷ This leaves 4,525 plants to determine the grant effect. After matching, the structures of the groups with and without grants largely agree (Table 4 above). For all control variables, the differences between the mean values are not significant except for a small negligible difference. The goal of finding similarly dynamic plants was even "exceeded", as the selected reference plants were more expansive than the assisted ones (8.6% to 7.3%) in the two years before the allowance.

**Table 5: Average effect of the GRW investment grant 1998 to 2007
- only the cohorts 2001 to 2007, with the additional control variable 2-years-growth before the year of allowance -**

	Mean value	
	Assisted n=4,525	Non-assisted n=4,525
Growth in employment from the year of allowance till 2009 (average of all cohorts 2001 to 2007)	-0.28% (0.00108)	-10.49 *** (0.00112)
Average grant effect p.a.	+10.2 percentage points	
In brackets: Standard error.		
***: 1% significance level (Cochran/Cox t test).		
Source: BAFA records; BA employment records; own estimations.		

The size of the growth difference after the year of allowance has not changed much including the additional control variable. In spite of restricting the contrast group to plants with similar development before the year of allowance, the results on the growth rates after the allowance year (Table 5) present the familiar picture: the number of employees in the assisted plants has changed only slightly at -0.3%

⁴⁶ Caliper matching with an interval of 0.9 to 1.2 times the growth rate of the respective assisted plants was used - analogously to the size of the plant.

⁴⁷ The excluded cases do not differ significantly from the remaining plants in their growth after allowance. Nor are there any significant growth differences between the remaining subset and the total of all assisted cases (see Fig. A2 in the appendix).

per annum. In the control group, on the other hand, it fell by -10.5% p.a. and is therefore only marginally better than for total plants above (-12.7%; Table 3)⁴⁸. At 10.2 percentage points, the average growth difference therefore remains in a similar magnitude to the 12.4 percentage points above.

6.2 Heterogeneity analysis

Obviously, the results on the growth lead of assisted plants are very robust. Despite the restriction to (previously) similarly dynamic plants for checking indirectly of unobserved influences such as the willingness to invest, the assisted plants prove to be significantly more dynamic after the allowance than their control group.

Table 6: Average growth rates of assisted and non-assisted plants

	Mean growth rate from the year of allowance till 2009			
	Total plants	Non-assisted plants		Assisted plants
		Identical by region, industry, size and age	Final control group	
Cohorts 1998-2007: All cases	-7.09% (0.1810)	-12.74% (0.1941)	-11.64% (0.1561)	-0.33% (0.2683)
Cohorts 2001-2007: Only plants which have existed at least from year t-2 till the year of allowance t	-6.90% (0.1818)	-9.69% (0.1851)	-10.50% (0.2002)	-0.28% (0.2147)
Standard deviation in brackets.				
Source: BAFA records; BA employment records; own estimations.				

An indication of the causes for this stability of the growth difference is provided by Table 6. The step-by-step selection process of the control group does not reduce, but increases the lead of the assisted plants. Even at the beginning, in comparison to total plants, the assisted plants developed significantly more favourably after the allowance (with -0.3 compared to -7.1%) (see also Fig. A2 and A3 in the appendix for the whole distribution).⁴⁹ More than 56% of the assisted plants increased their number of employees after the allowance up to 2009, compared to 25% of total plants.

In the first selection step this growth difference increases even further. If region, sector, size and age are considered, the growth rate of non-assisted plants drops to -12.7% and the share of positive rates to 14%. This decline is not surprising, as the assisted areas consist of structurally weak regions. In addition, the investment grant is predominantly given to the manufacturing industry whose employment growth is below-average.

In the final selection step the remaining control variables are taken into account. Thus, only plants with stronger growth (before the year of allowance) are included. But this has little effect on the growth rate after allowance: it improves only slightly to -11.6%, and only 20% of the final control

⁴⁸ The distribution of the assisted cases after 2001 does not deviate significantly from total assisted plants. The differences between the control groups are greater: If the development before funding is taken into account, the proportion of plants with greater job losses is reduced; see Fig. A3 in the appendix.

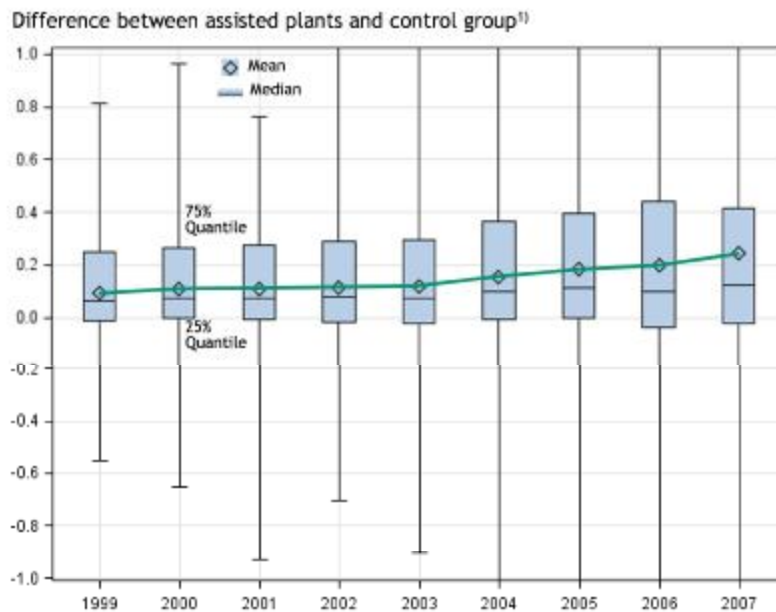
⁴⁹ In order to present the distribution of the rates of change comprehensively, Figure A3 in the appendix is restricted to the comparison of the assisted plants with their final control group.

group increased in employment. Reciprocally seen, the only solution of reducing the growth lead between assisted and non-assisted plants may be in selecting disproportionately more expansive cases for the control group (out of the set of total non-assisted plants).

As further control variables are not available, the search for further influences is restricted to the heterogeneity analysis of the assisted plants (Rosenbaum/Rubin 1983b; Gangl/DiPrete 2004). If the distribution of a control variable shows significant deviations in the size of grant effect, it can be assumed that they are caused by some third, unobserved factor. Above all, two control variables are suspected as being influenced by further determinants.

The first variable concerns the date of the allowance. If the grant effect shows considerable fluctuations between the years, then influences are effective which may be identified by their temporal localisation. Firstly, the year of allowance represents the economic situation at and after the time of investment which could be significant for further growth. It is conceivable that firms investing at the beginning of an economic upswing will benefit particularly from the favourable overall development. Conversely, an investing firm could be particularly at risk in an economic downturn because it is exposed to additional financial burdens by the investment. Secondly, the duration of the grant effect may have an influence. The calculation method used so far, the rate of change till the end year 2009, mixes short- and long-term effects, which probably differ in strength. In principle, it is to be expected that the investment grant as additional liquidity will primarily have a short-term effect and that the grant effect will therefore decrease with the time.

Figure 1: Average effect of the GRW investment grant by year of allowance



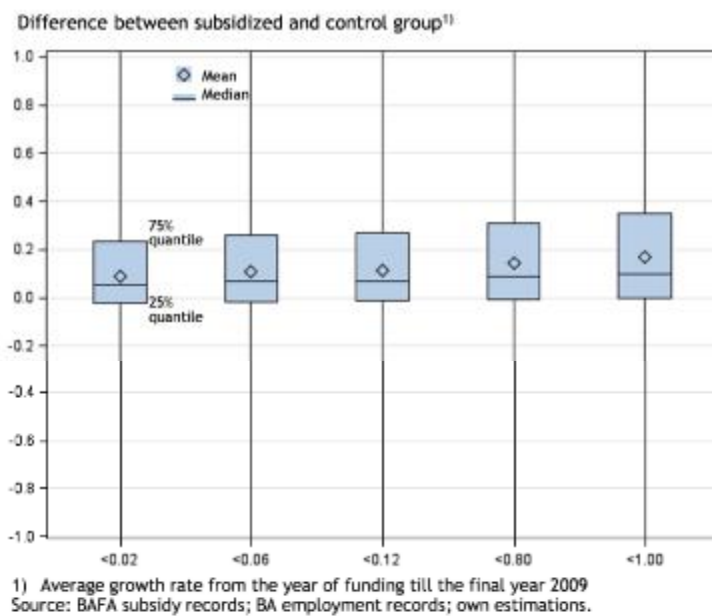
1) Average growth rate from the year of allowance till 2009.
Source: BAFÄ subsidy records; BA employment records; own estimations.

To examine the temporal variance, in Figure 1 the average grant effect is shown by year of allowance. In essence, the temporal break-down gives no indication of further influences. The difference between the growth rates of assisted and non-assisted plants is somewhat greater for the younger years. Nonetheless, the older cohorts show a growth lead of more than 10 percentage points on average. Thus, the effect of the investment grant is relatively stable in the long term, too. In addition, the Boxplot presentation shows that the frequency distributions over the years are very similar. In the younger years, the range of the grant effect is somewhat larger, but after three to four years it levels to a narrower distribution. For most years, the difference is positive in about three quarters of all assisted plants, i.e. three out of four assisted plants had a higher employment growth than their respective control plant.

The phenomenon that the grant effect is somewhat stronger in the short term can be observed for all cohorts. Figure A4 in the appendix presents the two-year growth difference for each year of allowance, which shows no significant temporal fluctuations. If only the growth rate of the assisted plants is considered (Fig. A4 in the appendix, bottom half), it is (somewhat) smaller after to the economic downturn in 2000 than later in the mid-2000s. However, since the control groups have developed similarly, the growth difference remains largely stable.

The last variable tested is the heterogeneity of the PSC. Its critical importance is firstly attributable to the fact that it decides on the final selection in the last matching step. Secondly, the PSC represents an integrated evaluation of all control variables. Since both groups of plants do not show any significant differences after the balancing tests (s. above), their PSC must therefore also coincide on average (see Figure A5 in the appendix).

Figure 2 : Average effect of the GRW investment grant by propensity score



For the question of whether the grant effect varies with the size of the PSC, the PSC is divided in quintiles, which are labelled by the value of their upper limit in Figure 2. The differences between the five PSC groups are so small that the distribution of the PSC does not point to any (indirect) influence. With the largest PSC values (0.80-<1.00), the funding effect is somewhat greater and has a greater upward dispersion. Overall, across all quintiles, the difference between the assisted plants and their control group is over 11 percentage points for each subset on average. In addition, all distributions are characterised by a common location and a similar variance. For all subsets, the grant effect - as above for differentiation by year - is positive in three quarters of all cases.

7. Conclusions

The result of the analyses is unequivocal: plants assisted by an investment grant mainly develop more favourably than those without grant. The growth lead does not weaken if the comparison is limited to those plants which, apart from the grant, are similar to the assisted plants in all observable characteristics. Even if only the subset of particularly dynamic plants is considered or the results are differentiated by years or by PSC, the growth leads remains remarkably stable.

Despite or precisely because of this stability, the question yet arises as to whether the identified control groups are sufficient for a counterfactual comparison. Compared to earlier GRW impact analyses, the selection process could be improved as additional influences on the probability of investment grant

and on the employment could be taken into account. Likewise sampling errors are avoided by the full survey and its panel data. Nevertheless, the clear and stable growth advantage is theoretically surprising insofar as the direct effect of the grant should primarily be a liquidity effect that would have to weaken over the years.

In particular, two areas of influence may distort the impact estimation, which cannot be adequately tested with the data from this study. The first concerns the selection of non-assisted firms for the control group and results from the link between grant and investment. Firms invest in capital because they seek to strengthen their market position. Thus, in general, firms applying for investment grants should be more dynamic. The data material does not provide any information on investment activity, except for the assisted plants. Therefore, we cannot exclude that investing firms are under-represented in the control group what may have caused its relative weakness in growth.

However, the neglect of investment and its non-observable effect on employment growth mainly affect the first control group (Chapter 5.6). In the subsequent extension (Chapter 6.1), we try to capture this bias by selecting those plants for the control group which were similarly dynamic before the year of allowance. If the presumed relation between growth and investment is effective, investing firms should have been preferred by the selection procedure. Even so, the growth deficit of the control group has not been essentially changed.

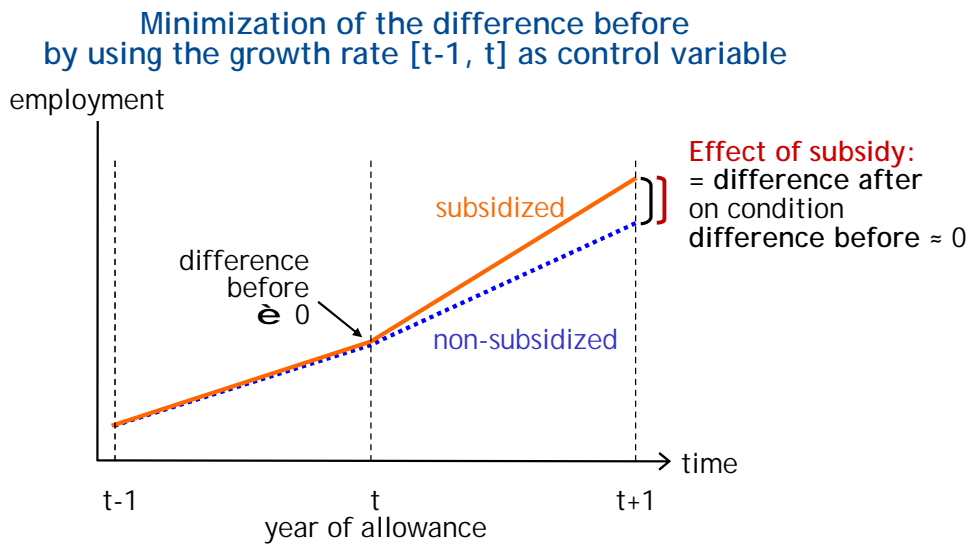
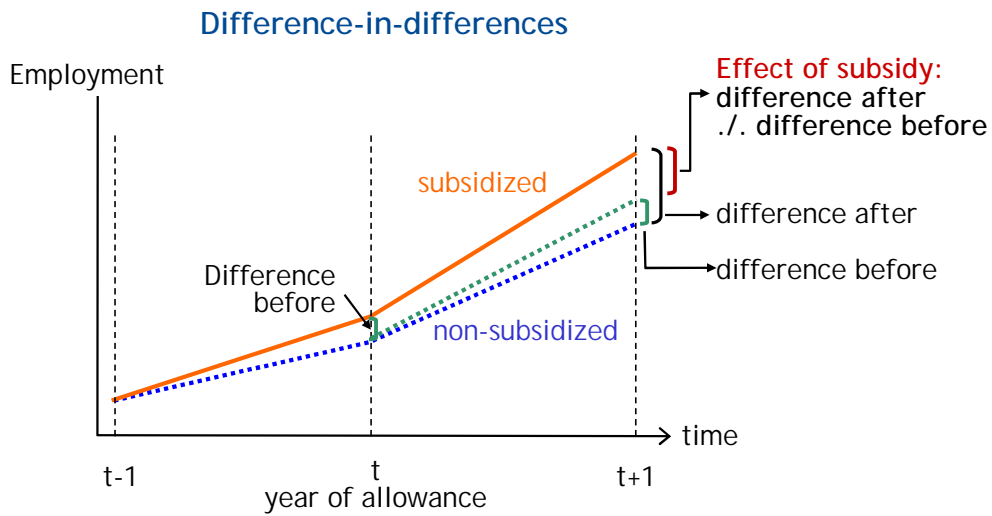
The second area of influence concerns the assisted firms. It is also conceivable that – out of the set of all investing firms - particularly dynamic firms will be preferred by the investment grant. One explanation could be that the funding agencies practise the strategy of “picking the winner”. Information on this is not available and, even if it existed, it could not be used for the selection of the control group. Furthermore, it is questionable whether the funding authority is competent enough for such evaluation of (future) business performance. Another explanation could be a kind of self-selection process: The applicant firm must guarantee its job targets. Consequently, firms with an inferior growth perspective should be less willing to give that guarantee and more likely to refuse the application for an investment grant. Thus, firms that are rather optimistic about their future success will prevail among the assisted firms.

In spite of these possible distortions, from the point of view of regional policy, the findings of this study unequivocally show that mostly expansive firms make use of the GRW investment incentives. At the end, the public funds reach the right firms. In addition, the majority of the assisted firms are obviously able to continue their expansion for a longer period of time.

The influence of the funding intensity and the connected question of the efficiency of the state aid remain open. In this study, the investment grant is only treated in binary terms. Whether the effect varies with the intensity of funding has to be clarified in further investigations. Nevertheless, a preliminary conclusion can be drawn already: As the funding intensity affects only the assisted firms, it cannot essentially touch the low performance of the non-assisted firms which explain one major part of the growth difference between the assisted firms and their control group.

Appendix:

Figure A1: Alternative consideration of the development before the year of allowance

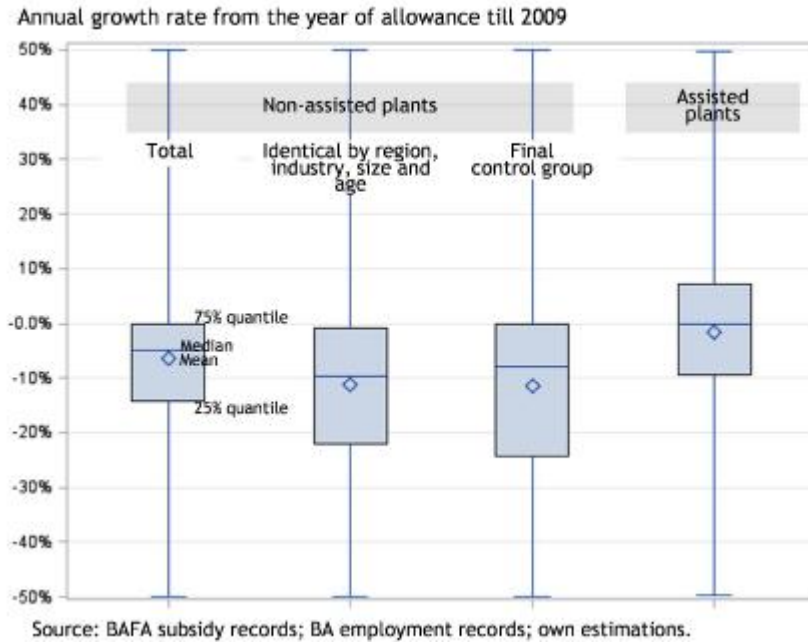


Annotation:

The upper figure exemplarily supposes that already before the year of allowance, the assisted firms have a stronger growth than their control group. The DiD-estimator is based on the assumption that this growth difference (and its underlying non-observable influences) have not changed in the period after the allowance. Consequently, this difference based on factors independent from the investment grant has to be subtracted from the overall difference in order to determine the grant effect.

In the bottom figure, only those non-assisted firms are selected for the control group which, before the year of allowance, have had a similar growth like the assisted firms. Thus, only those firms are included in the control group which should have been influenced by the non-observable factors in a similar way.

Figure A2: Boxplot of growth rates of all assisted and non-assisted plants
Period 1998-2007: All cases



Period 2001-2007: Only cases which have existed from year t-2 to the year of allowance t

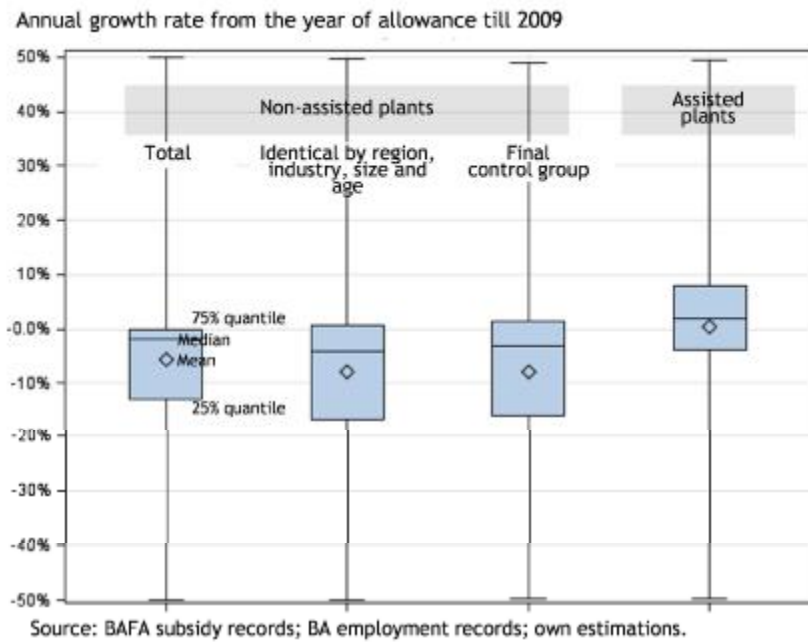
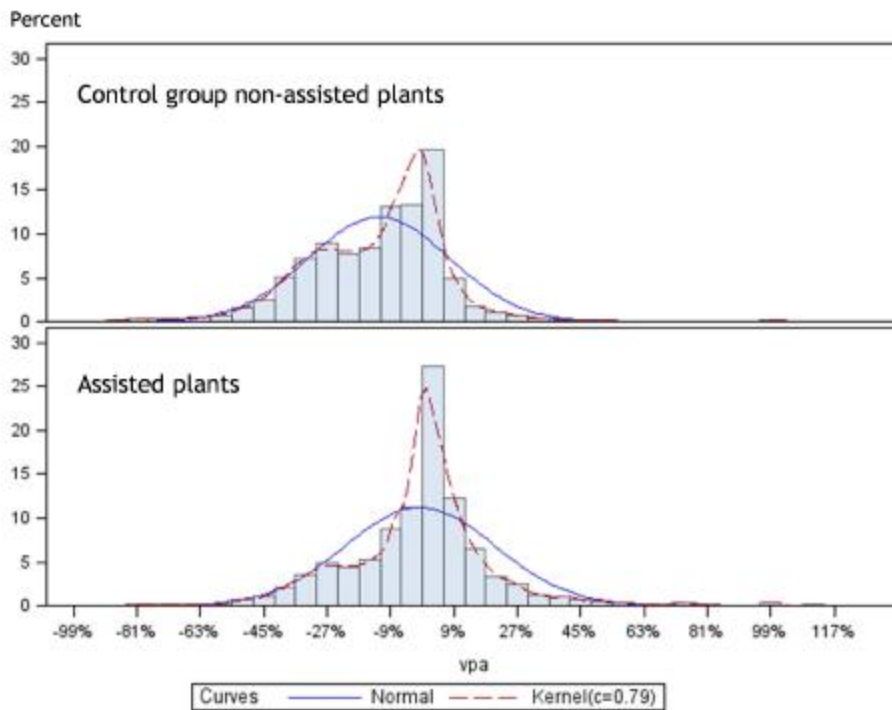


Figure A3: Frequency distribution of growth rates of assisted plants and control group

Period 1998-2007: All cases



Source: BAFA subsidy records; BA employment records; own estimations.

Period 2001-2007: Only cases which have existed from year t-2 to the year of allowance t

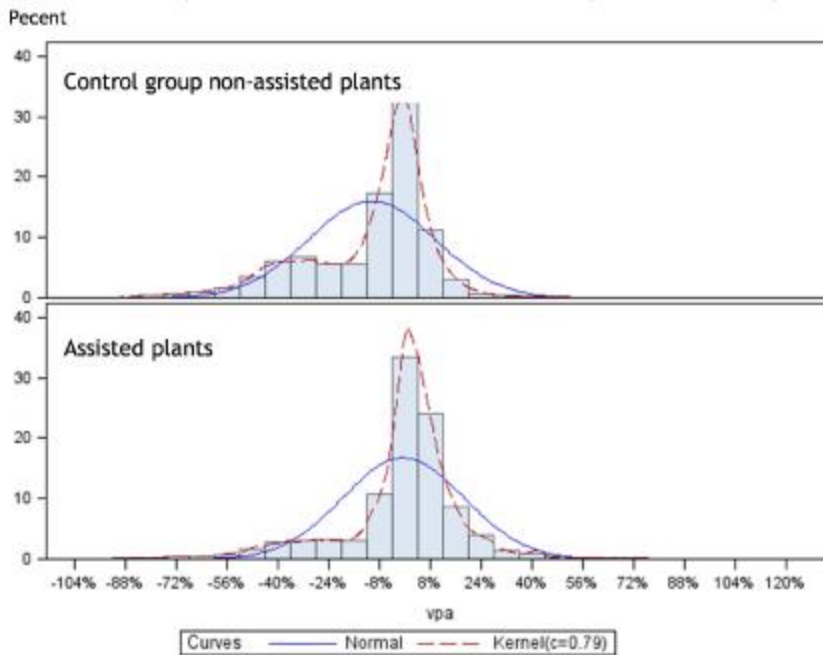
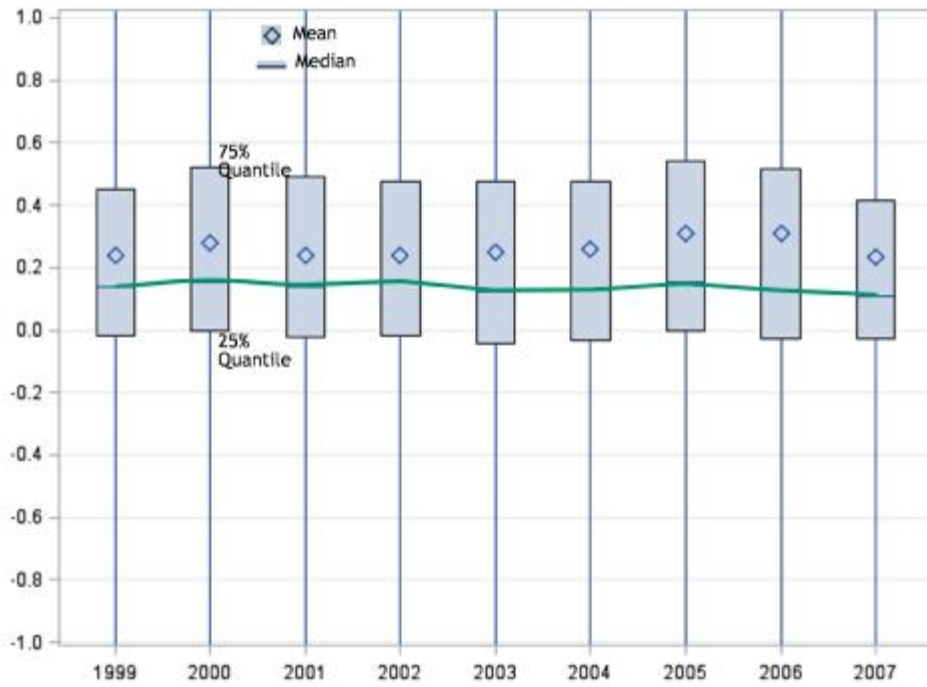


Figure A4: Growth rate two years after the year of allowance

Difference between assisted plants and control group



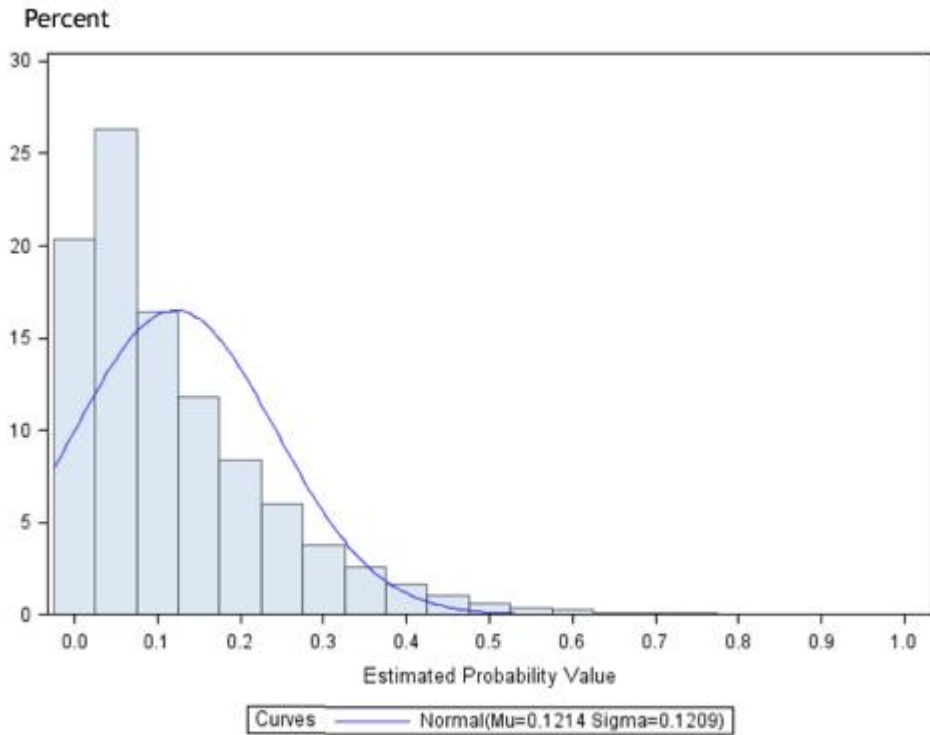
Assisted plants



Source: BAFA subsidy records; BA employment records; own estimations.

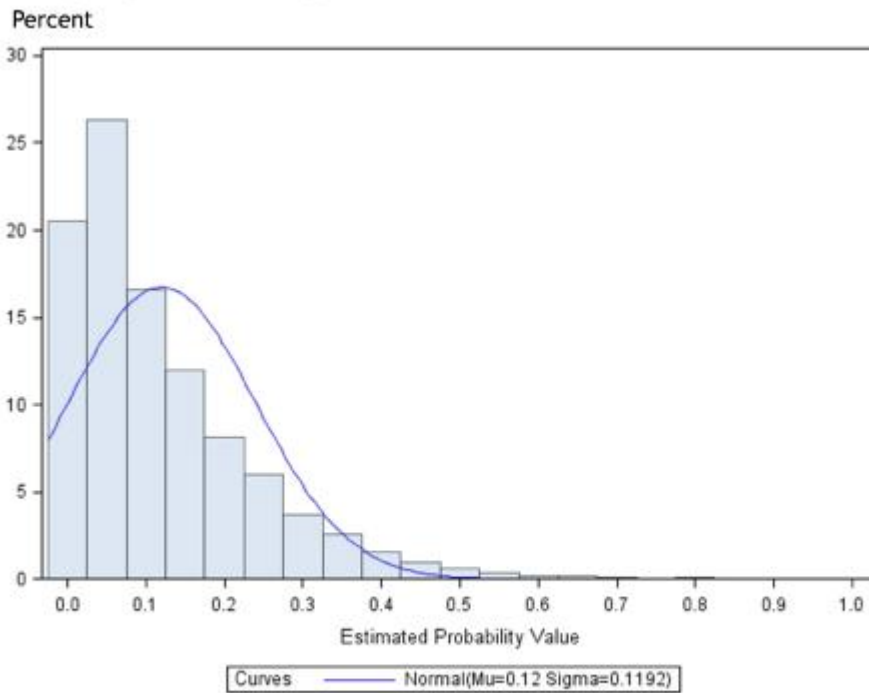
Figure A5: Frequency distribution of the propensity score (PSC)

Assisted plants



Source: BAFA subsidy records; BA employment records; own estimations.

Control group non-assisted plants



Source: BAFA subsidy records; BA employment records; own estimations.

Tabelle A1: Classification of industries

Agriculture, Manufacturing			Service Sector		
Nr.	Title	NACE Rev2	Nr.	Title	NACE Rev2
1	Agriculture, forestry	01; 02; 03	34	Wholesale, retail trade and repair of motor vehicles	45; 473
2	Mining of coal and lignite	05	35	Wholesale trade, ex. motor vehic.	46
3	Other mining, quarrying	06; 07; 08; 09	36	Retail trade, ex. motor vehicles	471-2; 474-9; 79; 952
4	Food products, beverage	10; 11; 12	37	Accommodation	55
5	Textiles	13; 143	38	Food beverage service activities	56
6	Wearing apparel, leather	141-2; 15	39	Rail transport	491; 492
7	Wood and wood products	16	40	Land, freight transport	493; 494; 495
8	Paper and paper products	17	41	Water transport	50
9	Printing	18	42	Air transport	51
10	Coke and refined petroleum products, basic chemicals	19; 201	43	Warehousing and transport support activities	52
11	Chemicals, chemical products, basic pharmaceutical products	202-6; 21	44	Postal activities; telecommunications	53; 61
12	Rubber	221	45	Financial service activities	64; 661; 663
13	Plastic products	222	46	Insurance activities	65; 662
14	Glass, ceramic products	231-4	47	Real estate activities	411; 68
15	Cement, stone	235-7; 239	48	Legal and accounting activities	691; 692
16	Basic metals	24	49	Management consultancy	701; 702
17	Metal products, except machinery and equipment	251-3; 255; 256	50	Computer consultancy, Information service activities	62; 63; 951
18	Other fabric. metal products	257; 259	51	Scientific research&development	72
19	Machinery and equipment	254; 275; 281-3; 331	52	Architectural activities	7111
20	Metal forming machinery and machine tools	284; 289	53	Engineering activities, technical consultancy	7112; 712
21	Computer, magn.+opt. media	262; 268	54	Advertising and market research	731, 732
22	Electrical products	271-4	55	Rental and leasing activities	77
23	Electrical equipment	279	56	Employment placement agencies	78
24	Electronic components	261; 263; 264	57	Facilities support activities	811; 812
25	Optic, medical and other instruments	265-7; 325; 332	58	Security and investigation activities	80
26	Motor vehicles	29; 309	59	Landscape service activities	813
27	Other transport equipment	301-2; 304	60	Other business support activities	74; 82
28	Air and spacecraft	303	61	Publishing activities	58
29	Furniture	310	62	Television, broadcasting; libraries, entertainment; cultural activities	59; 60; 90;91
30	Jewellery, music instruments, sport and toys products	321-4; 329	63	Education	851-3; 855; 856
31	Electricity, gas; water supply	35; 360	64	Higher education	854
32	Sewerage, waste	37; 38; 39	65	Human health activities	750; 86; 872
33	Construction, civil engineering	412; 42; 43	66	Residential care activities	871; 873; 879; 88
Source:			67	Other person. services	92; 93; 96-98
Eurostat (2008): NACE Rev. 2, Statistical classification of economic activities in the European Community, Luxembourg; own representation			68	Public administration	841-2; 99
			69	Social security activities	843
			70	Membership organisations	94

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