IWH
Halle Institute for Economic Research
Member of the Leibniz Association

Discussion Papers

No. 6
February 2023

Employment Effects of Investment Grants and Firm Heterogeneity – Evidence from a Staggered Adoption Approach

Eva Dettmann, Mirko Titze, Antje Weyh

Eva Dettmann
Corresponding author
Halle Institute for Economic Research (IWH) – Member of the Leibniz Association,
Centre for Evidence-based Policy Advice (IWH-CEP)
E-mail: eva.dettmann@iwh-halle.de
Tel +49 345 7753 855

Mirko Titze
Halle Institute for Economic Research (IWH) – Member of the Leibniz Association,
Centre for Evidence-based Policy Advice (IWH-CEP)
E-mail: mirko.titze@iwh-halle.de
Tel +49 345 7753 861

Antje Weyh
Institute for Employment Research of the Federal Employment Agency (IAB)
E-mail: antje.weyh@iab.de

Editor

Halle Institute for Economic Research (IWH) – Member of the Leibniz Association

Address: Kleine Maerkerstrasse 8
D-06108 Halle (Saale), Germany
Postal Address: P.O. Box 11 03 61
D-06017 Halle (Saale), Germany

Tel +49 345 7753 60
Fax +49 345 7753 820

www.iwh-halle.de

ISSN 2194-2188

Employment Effects of Investment Grants and Firm Heterogeneity – Evidence from a Staggered Adoption Approach*

First version: 02.02.2023
This version: 06.12.2023

Abstract

This study estimates the establishment-level employment effects of investment grants in Germany. In addition to the average treatment effect for the treated, we focus on discrimination in the funding rules as potential source of effect heterogeneity. We combine the difference-in-differences approach of Callaway and Sant'Anna (2021) that explicitly models variation in treatment timing with a ties matching at the cohort level. We observe a positive effect of investment grants on employment development in the full sample. The subsample analysis yields strong evidence for effect heterogeneity due to firm characteristics and the economic environment.

Keywords: causal inference, heterogeneous effects, place-based policy, staggered adoption design, variation in treatment timing

JEL classification: A11, D61, H20, Z0

* Acknowledgements: We thank B. Alm, M. Brachert, A. Diegman, S. Fritz, M. Fuchs, S. Mueller and the participants of the ERSA conferences 2023, 2021, 2017, the Geography of Innovation Conference 2018 and the participants of research seminars of the universities Osnabrueck and Kassel for interesting discussions and many valuable comments. The excellent work of A. Giebler in the preparation and combination of data sets using record linkage is gratefully acknowledged. The paper takes up research that started with a pilot study in 2017. The analysis uses administrative funding data, provided as part of an official evaluation project (tender number 8/18) for the German Federal Ministry of Economic Affairs and Climate Action. A. Kueffe (BAFA) provided us with additional sector-specific information on the eligibility for investment grants.

1 Introduction

In this study, we analyse the effects of investment grants issued under the most important place-based policy regime in Germany. We estimate the employment effects for the funding period from 2007 to 2013 at the establishment-level. Our contribution to the empirical literature is the explicit focus on potential sources of effect heterogeneity linked to the programme rules: the establishments' characteristics and the economic environment.[1]

Place-based policy schemes are common globally. In the European Union (EU), for example, a considerable share of the overall budget is allocated to such policy schemes: €278 billion in the 2007 to 2013 funding period (Ciani and de Blasio 2015). Beyond that, almost all member states offer national and regional policy programmes including investment grants (Criscuolo et al. 2019). The total expenditures for the German programme amounted to approximately €9 billion within the analysed period (BAFA 2016), while approximately one-third of all investments in eligible regions are funded by the analysed grogramme (BAFA 2016).

This type of policy is mainly designed to foster the economic development in structurally weak regions by enhancing employment and income (Neumark and Simpson 2015). The intention of the interventions is discussed ambiguously. Imperfect markets may justify the introduction of such programmes, in principle. The literature mainly highlights externalities, indivisible production factors, imperfect labour mobility, financial constraints due to asymmetric information, as well as regional equalisation issues as rationales for such policy schemes (Calmfors et al. 2002, Neumark and Simpson 2015). However, the literature also discusses potential side-effects and difficulties for the implementation of place-based policy programmes, e. g. a lack of information about the type and magnitude of market failure or allocative inefficiencies due to rent seeking and rent shifting (Calmfors et al. 2002, Guerzoni and Raiteri 2015, Neumark and Simpson 2015).[2]

The pros and cons discussed in the literature highlight the demand for credible empirical evaluation studies that address the above mentioned arguments taking into account specific regional features. Recent studies mainly show that investment grants in disadvantaged regions positively influence key figures of regional economic development, such as private-sector investments, employment, and productivity (Brachert et al. 2019, de Castris and Pellegrini 2012, Criscuolo et al. 2019, Eberle et al. 2019, Siegloch et al. 2021, Wardenburg and Brenner 2019). However, for selected place-based investment policies in (the South of) Italy, the literature also provides evidence on negative effects on the regional economic performance (e. g. Accetturo et al. 2020, Accetturo and de Blasio 2012, Andini and de Blasio 2016).

[1] Throughout this study, we use the terms 'establishment' and 'firm' synonymously. The same is true for 'treated' and 'subsidised'.

[2] Barca et al. (2012) provide an systematic overview on the main arguments related to this type of policy intervention and distinguish between place-based and place-neutral policy interventions.

Besides, the last 20 years saw a rising number of empirical studies on the effect of place-based policy schemes, especially investment grants, at the establishment level. The best analysed examples are the Italian Law 488/1992 and the British Regional Selective Assistance, however, we also find evidence for other European countries. Summing up, the empirical results suggest that investment grants positively influence overall firm-level employment, investments, turnover, output, and firm survival (see e. g. Bernini and Pellegrini 2011, Cerqua and Pellegrini 2014, Criscuolo et al. 2019, Decramer and Vanormelingen 2016, Harris and Trainor 2007, Pellegrini and Muccigrosso 2017). The effects on productivity and location choice are rather negative or negligible (see Bernini et al. 2017, Bergström 2000, Brachert et al. 2018, Devereux et al. 2007, Moffat 2014).

Surprisingly, we find only few studies that take into account the heterogeneity of treated establishments and its influence on the treatment effect. Except for establishment size (Bade 2012) and ownership (Girma et al. 2007), little attention has been paid to these aspects in the existing empirical literature thus far. Based on their meta analysis of the empirical literature on public grants in the European Union, also Dvouletý et al. (2020) state a need to dive further into the heterogeneity of effects due to observable firm characteristics and propose to address research questions like: "[...] Are the effects of public grants heterogeneous across industries? Do firms supported in more prosperous regions perform better compared to firms subsidized in lagging regions?" (Dvouletý et al. 2020, p. 257).

Our study aims to fill this gap by providing evidence on the impact of heterogeneity among treated establishments on the employment effect of investment grants, since employment is the focus of the programme. Our contribution to the literature consists of a systematic analysis of heterogeneity resulting from firm characteristics and economic environment. Since the rules in the analysed German programme differentiate the funding based on specific establishment characteristics and the economic environment, we can use this treatment discrimination as a guideline for our analysis of heterogeneity. Using an exceptionally rich dataset including detailed information on establishments and their economic environments, we can compare the magnitude of the employment effect in different subsamples. Our results may be a step on the way to optimize the allocation of investment grants among eligible establishments in the future.

In addition, the monetary information in the project data enables us to calculate the actual costs per additional job. Compared to previously provided evidence for Germany, these calculations provide a more realistic impression on the real costs connected to created or safeguarded jobs via investment grants, not only for the sample as a whole, but also for the analysed subsamples.

Finally, our study contributes to the current empirical discussion on the evaluation of time dependent treatment effects. We take up the idea of simultaneous control for selection bias

resulting from observable and unobservable heterogeneity and transfer it to the estimation of time-varying treatments. We combine the difference-in-differences (DID) approach of Callaway and Sant'Anna (2021) that explicitly models variation in treatment timing with a matching procedure which aligns the most diverging relevant characteristics at the cohort level.

The remainder of the paper is organised as follows: The next section describes the legal framework for investment grants in Germany, section 3 gives an overview on the our data sources and the analysed sample. Section 4 explains the construction of subsamples based on the treatment discrimination in the programme. Section 5 introduces the estimation approach, in section 6 we present our results for the full sample and defined subsamples. Section 7 presents some quality and robustness checks, section 8 concludes.

2 Institutional framework

The Joint Federal Task for the Improvement of Regional Economic Structures (GRW[3]) is the most important place-based policy scheme in Germany. The main goal is to reduce spatial disparities across Germany, particularly in terms of (un)employment and income. The programme provides investment grants in poor regions.[4] Since investment grants distort competition in the Common Market, the programme rules must be approved by the EU for a programming period of typically seven years.

A key feature of place-based policies is spatially limited programme access. Thus, only establishments in structurally weak regions have access to the funding. The regional eligibility relies on a structural weakness score comprising several single indicators (for 2007-2013: regional unemployment, gross wages and salaries, quality of infrastructure, and employment projection).[5] Consequently, mainly regions in East Germany, regions bordering the Czech Republic, and some regions in the north are eligible in the analysed funding period (see figure A.2).

The GRW programme has an implicit sectoral scope in the analysed funding period: Applicants must satisfactorily show supra-regional sales, namely, sales entailing more than 50 km from the place of production. For simplicity, the funding rules include a whitelist that announces all industries that are expected to automatically fulfil this criterion. Here, predominantly

[3]The abbreviation GRW refers to the German title for the programme, 'Gemeinschaftsaufgabe Verbesserung der Regionalen Wirtschaftsstruktur'.

[4]The programme provides investment grants for establishments and municipalities in disadvantaged regions. In our analysis, we focus on investment grants for establishments.
A detailed description of the programme's legal framework and the funding rules are provided in tables A.1 and A.2 in the appendix.

[5]Figure A.1 in the appendix provides a detailed description of the score components and calculation.

manufacturing-sector industries are listed.[6] Establishments operating in the service sector are generally eligible if they meet the aforementioned criteria.

The application process follows a normalised procedure, which is managed by the responsible federal state. An applying firm must describe the planned investment project and provide business plans, including information on the technical and financial feasibility of the project (confirmed by the firm's house bank). The application form also requires information on the number of additional or safeguarded jobs connected to the investment project.

3 Data and descriptive statistics

Our database combines information from multiple sources. The treatment information is obtained from the Federal Office for Economic Affairs and Export Control (BAFA), employment information at establishment level is provided by the Institute for Employment Research (IAB) of the Federal Employment Agency, while regional information is obtained from the INKAR database of the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR).

3.1 Data

The BAFA treatment database comprises the reports of the federal governments responsible for the implementation of the GRW investment grants. It contains project-level information, e. g. the start and end of the subsidised project, as well as location of the applicant and the investment. In addition, monetary information are available.

For the analysis, we consider all projects that were approved under the master plans applied for the funding period from 2007 to 2013[7] and started not earlier than January 2007. We only include projects that were actually realised and received financial support. Overall, we observe 13,384 projects in the treatment data (1). This corresponds to 11,031 treated establishments. As we observe in table 1, the total investments of 33.5 billion € are very differently distributed among the treated. The funded projects last on average two years, the average subsidy rate is about one third. The funding costs amount to 5.5 billion €.

Unfortunately, the database contains no information on rejected applicants and projects. To obtain information also on non-treated establishments, we use the employment history data provided by the IAB for the years 2002 to 2016, aggregated at the establishment level. The IAB

[6] Meanwhile, the EU's legal framework contains a blacklist with industries that are excluded from this type of state aid. This mainly applies to the agricultural sector, fishery, coal, and steel industries, the production of synthetic fibres, and transportation. Additional industries can be excluded by the federal governments.

[7] This GRW funding period is the first one with uniform eligibility rules for East and West Germany, both regarding the score calculation and the allocation of funds. The period coincides to one EU programming period.

Table 1: **Key facts of GRW funding in the period 2007-2013**

Number of projects		13,384
Number of establishments		11,031
Total amount of investments	*million €*	33,488.09
Mean investment costs	*thousand €*	2,502.10
Variation of investment costs		
min.	*thousand €*	1.36
max.	*thousand €*	711,053.75
Total amount of funds	*million €*	5,483.37
Mean treatment intensity	*percent*	34.01
Mean treatment duration	*quarters*	7.9
Mean time from application to treatment	*quarters*	0.64

Source: Federal Office for Economic Affairs and Export Control (BAFA).

data comprise information on an establishment's number of employees and employee structure in terms of age, gender and professional qualifications. We summarise the size information based on the EU definition: micro establishments with up to 9 employees, small establishments with 10 to 49 employees, medium-sized establishments with 50 to 249 employees, and large establishments with 250 or more employees. Additionally, we use the information on vocational qualifications to characterise the firm's human capital endowment. The share of at least medium-skilled employees is defined as the proportion of employees with vocational qualifications or higher formal degrees, the share of low-skilled employees as the proportion of employees without vocational education. The age structure is described by the share of young employees (under the age of 30 years), and the share of older employees, defined as the proportion of persons aged 50 years or older.

In addition, our data include information on the establishments' date of foundation, location, and economic sector. Information on the economic sector is based on the 'German Classification of Economic Sectors', which is consistent with the Nomenclature of Economic Activities (NACE) classification system. Based on this information, we can restrict the sample to establishments operating in economic sectors formally eligible to GRW.[8]

The establishment's location enables us to enrich the data with regional information from the INKAR database of the BBSR. We include information on a district's unemployment rate, the GDP per inhabitant, tax revenues per 1,000 inhabitants, and gross wages and salaries per employee. Additionally, we consider the BBSR definition of the districts' settlement-structure.[9] We summarise the information in two categories: urban regions (which comprise cities and urban districts) and rural districts.

[8]The BAFA provided us with detailed sector-specific information on the eligibility for investment grants. The information is provided at the 4-digit level for the WZ2003 and the WZ2008 classifications (which correspond to NACE Rev.1.1 and NACE Rev.2, respectively).

[9]The BBSR provides a classification of four settlement-structure district types (in German: 'siedlungsstrukturelle Kreistypen'). This characterisation is based on three components: population share in large and medium-

The result is a rich, unbalanced, panel dataset with quarterly information for the years 2002 to 2016. It consists of detailed information on treatment, establishment and regional characteristics.

3.2 The sample

Our sample comprises 1,163,668 establishments operating in sectors eligible for investment grants in Germany, of which 10,281 are treated establishments located in eligible regions.[10]

When selecting non-treated establishments as potential controls, we face a trade-off between two sources of distortion of the estimation results. The *first source* is the selection bias due to the unobserved characteristics of the non-treated establishments in eligible regions: The GRW is a demand-driven programme in which all establishments in eligible regions (in the eligible sectors) have access to the GRW programme, and we cannot observe why some establishments apply for grants and others do not. The *second source* applies to the non-treated establishments located in non-eligible regions: They benefit from a more favourable environment in economically stronger regions. Since the economic environment has an influence on the estimated effect (Heckman et al. 1997, 1999), a comparison between the treated establishments located in disadvantaged regions and non-treated establishments in wealthy regions may result in an underestimation of the employment effect – if the regional developments exhibit different trends.

Since neither the direction nor the amount of the bias potentially resulting from unobservable selection can be assessed, we regard unobservable selection as the more serious problem. To control for unobservable selection, we exclude non-treated establishments in eligible regions from the sample. We only consider establishments that do not have access to GRW funding as potential controls. In the estimation, we consider the pre-treatment development of regional characteristics to compare establishments in similarly developing regions. In addition, we conduct comprehensive robustness checks regarding the choice of potential controls. We specify four alternative samples focusing on non-treated firms located in the treated's neighborhood to minimize the probability of different regional developments, see section 7.

In table 2, we summarise potentially relevant firm and environment-related characteristics. They may influence the establishments' employment development and (successful) application for investment grants.[11] Since we have unbalanced panel data, table 2 provides some descriptive

sized cities, population density, and population density excluding large and medium-sized cities. See BBSR (2018) [in German].

[10] Out of 11,031 establishments receiving GRW funding, we could assign information for 10,281 firms using record linkage techniques.

[11] The exclusion of non-treated establishments in eligible regions causes missing values for non-subsidised establishments in Bremen, Berlin, Brandenburg, Mecklenburg Pomerania, Saxony, Saxony Anhalt, and Thuringia in table 2.

statistics of the establishments in the sample at the beginning of the funding period, the first quarter of 2007.

The descriptive statistics in table 2 suggests that the GRW programme is very selective. We observe substantial differences regarding some firm characteristics. The vast majority of both, treated and non-treated firms are small or very small. But the subsidised establishments are larger, on average – compared to the non-subsidised establishments, the share of medium-sized establishments is about three times as high, the share of micro establishments only about one half. The distribution of the establishments by sector is also very different: many of the treated establishments operate in the manufacturing of fabricated metal products (16 percent) and machinery and equipment (9 percent), whereas non-subsidised establishments operate mainly in business-related activities (14 percent)[12] or construction (11 percent). There is also a remarkable divergence in the location of the firms. Only one-third of the subsidised establishments are located in urban areas, compared to approximately four-fifths of the non-treated establishments.

In contrast, we observe a very similar distribution of firms in the age groups and a similar employee structure. Both age structure and qualification structure show only minor differences. Despite the similar employee structure, a difference in terms of the median monthly salary is observable: treated establishments pay, on average, €1,900 per month, whereas non-subsidised establishments pay approximately €300 more. This may be partially explained by the different economic environment – as expected, the (non-treated) establishments in regions not eligible for investment grants benefit from better economic conditions. Here, we observe significantly lower unemployment rates, a substantially higher GDP per capita, and higher tax revenues. And the difference in gross wages and salaries per employee is remarkable, at approximately €450. Last, we observe different distributions of treated and non-treated firms across the federal states. The difference for the East German federal states (including Berlin) is most striking, where we find approximately 70 percent of the treated establishments, but no non-treated establishments. This is surprising at first sight; however, the map in figure A.2 shows that all districts in East Germany are eligible for investment grants.[13]

[12] The NACE category 'other business activities' includes activities such as accounting, tax consultancy, market research, advertising, labour recruitment, or industrial cleaning.

[13] We exclude non-subsidised establishments in eligible regions from our sample for selectivity reasons, as is mentioned above.

Table 2: **Descriptive statistics for subsidized and non-subsidized establishments in the sample**

	subsidized			non-subsidized		
	N	mean/share	std.dev.	N	mean/share	std.dev.
total number of establishments	7,402			683,966		
establishment characteristics						
establishment size						
micro est.	2,317	31.30		408,771	59.76	
small est.	3,437	46.43		222,662	32.55	
med.-sized est.	1,442	19.48		44,452	6.50	
large est.	206	2.78		8,081	1.18	
establishment age						
young establishment	1,665	22.49		162,833	23.81	
settled establishment	5,737	77.51		521,133	76.19	
sector of the establishment (5 largest sectors in terms of 2-digit level of NACE Rev.1.1)						
manufacture of fabricated metal products[(2)]	1,222	16.51				
manufacture of machinery and equipment	674	9.11				
other business activities	493	6.66		97,048	14.19	
wholesale trade, commission trade[(3)]	455	6.15				
hotels and restaurants	432	5.84		60,926	8.91	
construction				73,375	10.73	
retail trade, repair of goods[(4)]				64,657	9.45	
health and social work				49,363	7.22	
share of high-skilled employees	7,402	0.12	0.17	683,966	0.09	0.18
share of at least medium-skilled employees	7,402	0.87	0.15	683,966	0.81	0.23
share of low-skilled employees	7,402	0.11	0.14	683,966	0.16	0.21
share of young employees	7,402	0.27	0.21	683,966	0.25	0.25
share of experienced employees	7,402	0.23	0.18	683,966	0.25	0.27
median salary per month	7,266	1,917.42	659.15	565,590	2,210.06	973.58
regional characteristics (district level)						
urban district	2,527	34.14		555,612	81.23	
rural district	4,875	65.86		128,354	18.77	
unemployment rate (percent)	7,402	13.22	3.68	683,966	6.84	2.47
GDP p.c. (thousand €)	7,402	22.48	6.31	683,966	35.83	16.29
tax revenues per 1,000 inhabitants	7,402	421.13	121.82	683,966	764.76	243.59
gross wages and salaries per employee (€)	7,402	1,885.54	242.00	683,966	2,350.43	304.93
location in ... (Federal state)						
... Schleswig Holstein	165	2.23		10,363	1.52	
... Hamburg	0	0.00		25,978	3.80	
... Lower Saxony	717	9.69		48,084	7.03	
... Bremen	30	0.41		–		
... Northrhine Westphalia	477	6.44		182,923	26.74	
... Hesse	123	1.66		64,469	9.43	
... Rhineland Palatinate	103	1.39		45,228	6.61	
... Baden Wuerttemberg	0	0.00		146,713	21.45	
... Bavaria	497	6.71		150,051	21.94	
... Saarland	41	0.55		10,157	1.49	
... Berlin	583	7.88		–		
... Brandenburg	937	12.66		–		
... Mecklenburg Pomerania	637	8.61		–		
... Saxony	1,851	25.01		–		
... Saxony Anhalt	584	7.89		–		
... Thuringia	657	8.88		–		

Notes: The information refer to the first quarter 2007.
[(1)] Standard deviation. [(2)] Manufacture of fabricated metal products, except manufacture of machinery and equipment; [(3)] Wholesale and commission trade, except of motor vehicles and motorcycles; [(4)] Retail trade, except of motor vehicles and motorcycles, repair of personal and household goods.

Sources: Employment History of IAB, GRW treatment data of BAFA, INKAR data of BBSR; own calculations.

4 Discrimination in the funding as guideline for the analysis of heterogeneity

Besides the general funding rules described above, the programme allows some degree of variation in treatment intensity. We discuss some theoretical impact channels and their expected effects linked to these discrimination rules. Since the impact on the success of the programme cannot be clearly predicted from theory, it is the task of empirical research to shed some light on this. We use the variation in the treatment intensity as a guideline for our heterogeneity analysis and create subsamples according to the differentiation.

Regarding the *regional economic environment* of a firm, the programme allows for higher maximum aid intensities in structurally weaker regions. Neo-classical growth theory (Solow 1956, Swan 1956) argues that a decreasing marginal factor productivity would yield higher returns to GRW funding in more disadvantaged regions. However, the endogenous-growth theory (Romer 1986, Lucas 1988) suggest the opposite. In order to analyse the influence of disadvantages, we split the sample according to the degree of regional structural weakness in East and West German regions.

In addition, agglomeration economies highlighted in the New Economic Geography (Krugman 1991, Fujita et al. 1999) may affect the success of the programme. The basic idea relies on the assumption that productive regions grow more rapidly in terms of employment (Rosenthal 2004). Empirical evidence confirms the positive correlation between agglomeration and employment growth (e. g. Henderson et al. 1995, Holl 2018, Saito and Wu 2015). Based on this, a preferential allocation of funds in regions endowed with specific location conditions is subject of an intense and long-lasting political debate (in the sense of abandoning the principle of 'one size fits all' in favour of 'tailor-made solutions'). To investigate if the employment effect of investment grants is also influenced by agglomeration, we consider two different types of regions, urban and rural regions, in our analysis.

Also *firm characteristics* may affect the treatment effect. The programme provides higher maximum aid intensities for small firms than for medium-sized and large firms. Unfortunately, we are not able to consider this aspect in our heterogeneity analysis. The constructed subsamples representing the respective size categories according to the EU definition do not fulfil the identification assumption of the estimation approach for causal analysis.

GRW funding rules do not explicitly favour some economic sectors over others; however, the programme reveals an implicit sectoral scope due to the criterion of inter-regional sales. This is automatically fulfilled if an applying firm is classified in a sector belonging to a 'whitelist'. Moreover, when looking at the distribution of investment grants, we observe a clear concentration of funding in some sectors. Since the economic sector implies distinct production processes based on specific technology and equipment, requiring different types of employees, we presume

heterogeneous employment effects in different sectors. Based on the 2-digit level of the German Classification of Economic Activities system, we broadly aggregate the eligible sectors in five sector groups.[14] See table A.3 in the appendix for more details.

Maximum subsidy rates are provided for investment projects that reveal a *special structural impact*, e. g. investments that strengthen regional innovative capacities or business start-ups. A firm's age represents some part of the special structural impacts: Young firms are presumed to have more entrepreneurial spirit (another risk behaviour), new ideas and products, and to act more flexibly in the market (Dhawan 2001, Pagano and Schivardi 2003). Simultaneously, young firms show a higher probability for market exit as newcomers have to adapt to rules, routines, and skills in a new econommic environment (Fackler et al. 2013). In order to capture this potential source of heterogeneity, we divide the sample into two subsamples based on establishment age: young establishments (less than five years old) and mature establishments that are at least five years old.

An important precondition for innovative capacity is the composition of an establishment's workforce. Since the seminal work of Mincer (1962), the workforce's qualification level and work experience have been developed as standard proxies for a firm's human-capital endowment, or labour quality. Recent empirical studies confirm a positive relationship between labour quality and firm performance; see e. g. Conlon et al. (2023), Galindo-Rueda and Haskel (2005), Morris et al. (2020). We verify the influence of an establishment's labour quality on the employment effect of investment grants. Variations in workforce composition are represented by different shares of certain employees in an establishment. We differentiate between a high and low share of at least medium-skilled employees, low-skilled employees, as well as young and old employees as proxies for experience levels.[15]

[14]The aggregation scheme follows the IAB Establishment panel's aggregation scheme and represents the best possible solution to the trade-off between the similarity of establishments in a group and a sufficiently large number of observations in the group to be able to interpret the results.
The sector group 'manufacturing of products for private consumption' contains, for example, manufacturing of food and beverages, textiles, and furniture. A second, comparatively narrow classification, 'chemicals and pharmaceutics', contains sectors such as petroleum processing and manufacturing of chemicals, pharmaceutics, and ceramics; production processes require large machinery and are comparatively less labour-intensive. In the classification 'machinery and equipment for industrial production', we summarise manufacturing sectors producing electrical equipment, machinery, vehicles, metal products, and construction. This group represents the core of the GRW treatment in terms of both the number of treated establishments and the amount of the subsidy; see table A.4 for more details. It contains strongly supported sectors such as those that manufacture fabricated metal products and machinery and equipment. The sector group 'services and health care' summarises all the treated establishments in the service sectors and health services. It incorporates relatively labour-intensive sectors – thus, we would expect comparatively large effects on the number of employees. In this group, we also find strongly subsidised sectors such as wholesale trade and accommodation. The group of 'exploitation of natural resources' contains sectors such as agriculture, forestry, mining, and basic supply, and represents sectors that are not central to the GRW programme. In the group, we combine less supported economic sectors in terms of both the number of treated establishments and the total amount of the subsidy; see table A.4 for more details.

[15]For the definition of a low share, we use the 30-percent percentile of the distribution of the respective variable among the treated establishments, while the 70-percent percentile among the treated establishments marks the threshold for a high share. Since we use panel data and shares of certain employees may vary over time, we consider an establishment's mean share over time.

5 Identification strategy

For reliable estimation results, we must consider different sources of potential biases. The above mentioned selection bias due to differences in the relevant observable characteristics are a serious issue for our analysis, as the description of the sample shows (see table 2). Also unobservable heterogeneity, e. g. a general company strategy or management quality, may influence the results. Another issue is the influence of time on the treatment effect: the strength of the treatment effect may depend on the duration of exposure (Callaway and Sant'Anna 2021), the elapsed time after a treatment affects the current level of the effect (Jacobson et al. 1993), and also the economic situation may change over time – which can affect the effectiveness of a programme (Bergemann et al. 2009). This is of particular importance for our study: Our observation period covers the period of the 2008 financial crisis and remarkable economic changes in subsequent years. It is apparent that we should not compare establishments at different points in time, e. g. a treated establishment during the crisis with a non-treated one in the recovery phase.

In order to consider the mentioned sources of bias in our analysis, we take up the idea of simultaneous control for selection bias resulting from observable and unobservable heterogeneity (see e. g. Bernini and Pellegrini 2011, Caliendo and Künn 2011, Gustafsson et al. 2016) and transfer it to the estimation of time-varying treatments. We combine the difference-in-differences (DID) approach of Callaway and Sant'Anna (2021) that explicitly models variation in treatment timing with a matching procedure which forces the alignment of the most diverging relevant characteristics.

5.1 Assumptions

When matching and DID are combined in a panel context, the *assumption of sequential ignorability* (Robins et al. 2000) for matching and the *common trend assumption* required for DID can be replaced by a less strong assumption. The *conditional parallel-trend assumption* allows for covariate-specific trends of an outcome in different groups (Heckman et al. 1997, 1998). This implies that unobservable individual characteristics must be invariant over time for units with equal observed characteristics. The conditional parallel-trend assumption is not testable. We regard the pre-treatment employment development in the establishments as a proxy for the development in the absence of treatment and use placebo tests for different periods prior to the treatment to verify if this assumption is fulfilled (see section 7).

As emphasised by Heckman et al. (1997, 1998) for matching in cross-sectional data, *common support* is an important (necessary) condition for unbiased estimation results. This is also true in a panel-data context (Callaway and Sant'Anna 2021); overlap is required for each

treated unit and each time period included in the analysis. In the estimation approach, we only consider establishments that fulfil the common-support condition.

The *no-anticipation assumption* states that the treatment must not have a causal influence on the outcome prior to its implementation. Otherwise the changes in the outcome for the treated group between pre- and post-treatment could reflect not just the causal effect but also the effect of behavioral changes in expectation of the treatment (Malani and Reif 2015). Since anticipation effects usually occur rather in the short term (Abbring and van den Berg 2003), we conduct period-to-period placebo tests to verify the assumption (see section 7).

The strict interpretation of the *irreversibility of treatment* or *staggered treatment adoption* assumption is that units adopt the policy or treatment of interest at a particular point in time, and then remain exposed to this treatment at all times thereafter (Athey and Imbens 2022). For the applied approach, this 'once treated – always treated' assumption is relaxed and interpreted as '... if units do not 'forget' about the treatment experience' (see Callaway and Sant'Anna 2021, p. 6) to consider the fact that a treatment may have an impact on an outcome (current or future) even when it is completed. In our estimations, an establishment is regarded as treated from the start of first treatment on for all the following time periods.

In addition, our identification strategy requires *no uncontrolled carryover* and *spillover effects*.[16] The potential outcome of an observed establishment depends neither on its own previous treatments – or they can be controlled for – nor on the current treatment status of other establishments. In Germany, there exist a wide range of potential funding alternatives for establishments; however, these alternative funding options do not have an explicit spatial scope. In other words, these programmes can be used by establishments all over Germany. Additional place-based policies (e. g. those implemented by the states or municipalities) are explicitly forbidden as they would violate the EU rules. As a robustness test, we exclude all (treated and non-treated) establishments that received GRW investment subsidies in the years prior to the analysed funding period to address potential carryover effects.
As described in section 2, the GRW investment subsidies are provided on application for a particular investment project, and all applications are subject to an assessment by the funding authorities. Since the number of applicants is much less than that of eligible establishments in the eligible sectors, we regard the assumption of no spillover effects as fulfilled.

5.2 The estimation approach

The core of our estimation strategy consists of a semiparametric difference-in-differences approach (DID) that considers time varying treatments and potentially time dependence of the

[16]This assumption replaces the *random sampling assumption* of the 'pure DID approach for the combination of matching and DID.

treatment effect. Instead of the previously common average treatment effect for the treated, this approach estimates group-time-average treatment effects (Callaway and Sant'Anna 2021): partial average treatment effects in each cohort at each time. A group, or cohort, is defined according to the first treatment start date, the time refers to the number of periods after the treatment start. The average effect of units in a particular group at a particular time period is estimated as the comparison of two outcome developments:

$$ATT\left(g,t\right)=E\left[Y_t(g)-Y_t(0)|G_g=1\right].$$

where $G_g=1$ means that group g becomes treated at time G for the first time. The average treatment effect in this group g at time t, $ATT\left(g,t\right)$ is estimated by comparing the outcome development in case of treatment $Y_t(g)$ and in case of non-treatment $Y_t(0)$. This requires at least one pre-treatment period for each group (see Callaway and Sant'Anna (2021) for more detailed explanations).

The partial effects can be aggregated in different ways. The general aggregation scheme allows for different estimators depending on the choice of the weighting function for the group-time-average treatment effects:

$$\theta=\sum_{g\in G}\sum_{t=2}^{T}w\left(g,t\right)\cdot ATT\left(g,t\right).$$

θ denotes the aggregated effect of all group-time-average treatment effects $ATT\left(g,t\right)$ over all groups $g\in G$ and all time periods $t=2,\ldots,T$. $w\left(g,t\right)$ is the weighting function. We estimate the overall average treatment effect using the following weighting function:

$$w\left(g,t\right)=\frac{1\left\{t\geq g\right\}P(G=g|G\leq T)}{\sum_{g\in G}\sum_{t=2}^{T}1\left\{t\geq g\right\}P\left(G=g|G\leq T\right)},$$

which is a simple weighted average of all partial treatment effects, where the weights correspond to the respective group size.[17]

In the estimation, the controls are weighted by a combination of regression adjustment and inverse probability weighting based on the propensity score (this is regarded as doubly robust approach, see Callaway and Sant'Anna (2021)). For the weighting procedure the pre-treatment employment turned out to be most important. So we include covariates representing the employment development prior to the treatment.

As we know from the descriptive analysis (see table 2), we are faced with a rather selective group of treated firms. In addition, the described estimation process is very computing-time

[17]The estimator corresponds to the estimation equation 3.10 of Callaway and Sant'Anna (2021).

and memory-intensive in large data sets. Thus, we implement a matching procedure to focus on those non-treated establishments that are actually comparable to the treated firms and to reduce the size of the data set. This data preprocessing also adjusts the data in that it reduces potential inefficiency and model dependency of the subsequent estimations (Ho et al. 2007).

5.3 Data preprocessing

To find the best comparable potential controls for the treated establishments, we use a two-step ties matching at the cohort level. In the first step, an exact matching of the most diverging firm characteristics, i. e. *sector classification, EU firm-size category, regional settlement-structure definition*, preselects potential controls. For sector classification, we use the 4-digit level of the NACE classification, the so-called 'economic class'.[18] Exact matching regarding the EU firm-size categories and the two firm-age categories ensures that we compare establishments that share similar economic and financial constraints (Müller and Stegmaier 2015). Additionally, we require equality in the settlement structure of the firm location, i. e. urban vs. rural regions.

In the second matching step, we include variables that characterise the establishments' employee structures and their economic environments. We include the share of high-skilled employees, that of low-skilled employees and of young employees to characterise the establishments' employee structures. Since the regional economic environment of treated on non-treaed firms is different 'by definition' (treated firms in eligible, structurally weak regions vs. non-treated firms in stronger, non-eligible regions, see section 3.2), it is not useful to match the level values of regional characteristics. Instead, we consider the regional development prior to the treatment start and match establishments in regions that exhibit similar developments in the pre-treatment years.[19] We use three regional characteristics to capture the regional development of the compared establishments: the GDP per capita and the tax revenues per 1,000 inhabitants as broad proxies for the regional prosperity, and gross wages and salaries per employee as a proxy for the competitiveness and productivity of the regional economy.

[18] Classes are the most detailed categories of the NACE classification system and corresponds to a very detailed description the production process and technology used. They are intended to ensure that "[...] the units falling into each class will be as similar [...] as is feasible. [...] activities are grouped [in classes] when they share a common process for producing goods or services, using similar technologies". See Eurostat (2008) p.21.

[19] Here, we face a trade-off between the reliability of the regional pre-treatment development (which is better the longer we can observe the regional development) and the loss of observations (which is higher the longer the observed pre-treatment development). We regard the mean development over the last two years as 'optimal' to resolve this conflict.
This means that the treated establishments must be observable at least two years before they are treated by GRW investment subsidies; thus, start-ups and very young establishments are ruled out.

6 Results

This section presents the results of our estimation. The units of observation are establishments in Germany located in eligible regions that were treated during the funding period of 2007–2013. The control group comprises non-treated establishments located in non-eligible regions to exclude unobservable selection.

As stated in the introduction, we differentiate between an average treatment effect and heterogeneous effects in selected subsamples. In the following tables, we report the effects in both absolute and normalised figures. For normalisation, we relate the estimated effect to the amount of the subsidies paid for the establishments in the (sub)sample(s). The resulting employment per €100,000 subsidy allows us to compare the results across different subsamples.[20] Since the location, employee structure, and other characteristics of every establishment represent the result of rational management decisions, the variable distributions in the subsamples are not random (and cannot be randomised). Therefore, it is not possible to conduct ceteris paribus comparisons between the subsamples in the sense, for example, of interpreting the coefficients of parametric models. This means that we cannot identify the influence of a single covariate on the employment effect. Nevertheless, we can draw reliable conclusions about the extent to which the normalised effects differ in the subsamples.

In some of the subsamples we observe diverging trends in the employment development prior to the treatment (see table 7 in section 7). Since they do not fulfil the identifying assumption of the estimation approach, we cannot interpret the results. The corresponding entries in the tables 3 and 4 are marked in gray.

For the full sample, we observe a positive effect of investment grants on the employment development; see table 3. On average, treated establishments grew by 7.3 employees more than their controls. Considering the total number of subsidised firms, 8,079, we observe a total employment effect of 58,967 additional jobs. Relating this to the amount of the subsidies paid, the normalised effect is 1.1 employees per €100,000 subsidy. In other words, an additional job is subsidised by approximately €88,300 (see table A.4). The result corresponds to the empirical evidence of positive employment effects of investment grants e. g. in Italy (Bernini and Pellegrini 2011, Cerqua and Pellegrini 2014), the UK (Criscuolo et al. 2019), Danmark (Decramer and Vanormelingen 2016) and Northern Ireland (Harris and Trainor 2007).[21]

[20] As additional information, we present the total amount of the subsidies in the subsamples as well as the costs per additionally created job in table A.4 of the appendix.

[21] The costs per job in our analysis are higher than those presented in the mentioned empirical studies. Criscuolo et al. (2019) report costs of approximately €23,000 (£ 26572) per job for the UK, Cerqua and Pellegrini (2014) report costs between €46,343 and €77,520 for Italy.

6.1 Effect heterogeneity linked to economic environment and firm characteristics

To exploit the influence of the *economic environment* of a treated firm, we focus on the location of the establishments and split the sample into establishments located in more vs. less disadvantaged regions and those in urban vs. rural regions.

Treated establishments in strongly disadvantaged regions grow significantly stronger than their controls with an ATT of 5.9 workers. The normalised effect amounts to 0.7 employees per €100,000 subsidy, lower than the average effect of 1.1.[22] The sample of treated establishments in most disadvantaged regions exclusively comprises establishments in East Germany reflecting the highest aid intensities possible in the GRW programme. Given these beneficial programme incentives, the highest share of GRW funds in absolute terms is allocated to this German macro-region. Our findings suggest that the programme has achieved its goals in terms of safeguarding existing and creating new jobs particularly in East Germany, which is mostly targeted by this policy. Our result is consistent to the findings of Siegloch et al. (2021) who also present positive effects at the regional level.[23] The result for the subsample of treated establishments in less disadvantaged regions is not interpretable.

A closer look at the effect heterogeneity resulting from agglomeration reveals that employment among the treated establishments in rural areas rises more strongly than among the control establishments within the observation period – despite economically less favourable environmental conditions. The effect on establishments in rural regions is significant and positive. The strength of the effect in rural areas is, at 0.6 employees per €100,000 subsidy, below the average effect of 1.1. Initiating employment growth in rural areas seems to be more expensive than the average cost – as is also confirmed by the substantially higher amount of subsidy per additional job (€159,200), see table A.4. Although not explicitly announced in the targets of the GRW, balancing out spatial disparities between urban and rural areas is also on the policy agenda in Germany. Our findings suggest that the GRW programme contributes to this policy goal in a positive manner. The results for the subsample of urban regions is not interpretable.

As stated above, effect heterogeneity may also be linked to *internal firm characteristics*. We differentiate establishments based on a broad classification of the economic sectors in which they operate. We find significant positive employment effects for the sectors 'chemicals and

[22] The subsidy per additionally created job in treated establishments in those regions is, at €134,200, substantially higher than the average (€88,350) and even more than that in establishments located in less disadvantaged regions (€51,600).

[23] Siegloch et al. (2021) calculate costs per job of €19,935. The remarkable difference to our study might be explained as follows. *First*, they estimate an intention-to-treat effect instead an ATT. *Second* and more importantly, we calculate the costs on the basis of the gross grant equivalent which include not only GRW expenditures reported in the BAFA statistics, but reflect the total funding costs – including project-specific subsidies from other programs like the investment tax credit programme (Investitionszulagengesetz) or loans.

Table 3: **Heterogeneity due to economic environment and firm characteristics**

	number	estimated effect		
	unique obs.	ATT	(std.err.)	normalized[a]
full sample	16,379	7.30***	(1.82)	1.13
subsamples: level of regional disadvantage				
strong	10,240	5.89***	(2.20)	0.75
less strong	6,284	7.63***	(1.30)	1.94
subsamples: settlement structure				
urban	6,091	14.29***	(3.39)	2.55
rural	10,263	4.29***	(1.02)	0.63
subsamples: aggregated sector class[(1)]				
products for private consumption[(2)]	1,913	0.78	(5.03)	0.11
chemicals and pharmaceutics[(3)]	1,132	2.92**	(1.40)	0.39
equipment for industrial production[(4)]	6,359	5.26***	(1.59)	0.83
services and health care	6,306	3.36***	(1.06)	0.89
exploitation of natural resources[(5)]	315	4.62	(4.79)	1.17

Notes: [a] effect per €100,000 subsidy. Gray records indicate non-interpretable results due to pre-treatment trends of the outcome (see table 7). Results significant on the level: *** p<0.01, ** p<0.05, * p<0.1.
[(1)]sector aggregation follows the aggregation scheme of the IAB Establishment panel, see table A.3 in the appendix; [(2)]production of food, fabrics, other goods; [(3)]petroleum processing, manufacture of chemicals, pharmaceutics and ceramics; [(4)]production and maintenance of electrical equipment, machinery and vehicles, metal production, construction; [(5)]agriculture, forestry, mining, basic supply.

Sources: Employment History (IAB), GRW treatment data (BAFA), INKAR data (BBSR); own calculations.

pharmaceutics', 'equipment for industrial production' and 'services and health care' (see table 3).

In absolute numbers, the effect is highest in the sector group 'equipment for industrial production' with an ATT of about five jobs.[24] Also the total amount of funding is with €2 billion the largest in this group (see table A.4). As a result, the a normalised effect is with 0.8 employees per €100,000 below the average.

One would expect comparatively large employment effects in labour-intensive sectors, which would be the case for the sector group 'services and health care', in particular.[25] Surprisingly, the effect in absolute terms is with an ATT of three jobs rather small. However, the normalised effect is with 0.9 employees per €100,000 slightly below the average, but the largest observed among the sector groups. In the rather capital-intensive sectors summarised in the 'chemicals and pharmaceutics' group, both the ATT and the normalised effect are with three jobs and 0.4 employees per €100,000, respectively, the smallest observed among the sector groups. The effect for the subsample 'exploitation of natural resources' is insignificant, and for the sector group 'manufacturing of products for private consumption' not interpretable.

[24]This group contains the most strongly treated sectors, manufacture of fabricated metal products and manufacture of machinery and equipment.
[25]In this group, we also find strongly subsidised sectors such as wholesale trade and accommodation.

6.2 Effect heterogeneity linked to the GRW rule of special structural impacts

Regarding the criterion of *special structural impacts* in the funding rules, we consider firm age and the human capital endowment. The corresponding results are presented in table 4. For the large group of mature firms, we observe a significant and positive effect of 1.2 employees per €100,000 (part I in table 4), which is above average at 1.1 job per €100,000. Obviously, the programme is highly effective for the group of mature establishments. We cannot interpret the result for the subsample of young establishments.

Table 4: **Heterogeneity due to GRW rule of special structural impacts**

	number	estimated effect		
	unique obs.	ATT	(std.err.)	normalized[a]
full sample	16,379	7.30***	(1.82)	1.13
subsamples: establishment's age				
young firms	3,539	0.90	(1.92)	0.25
mature firms	12,697	7.85***	(2.06)	1.18
subsamples: employees' formal skill level				
low share at least medium-skilled[(1)]	4,883	5.56***	(1.08)	1.28
high share at least medium-skilled[(2)]	5,697	9.48***	(3.24)	1.47
low share low-skilled[(3)]	5,119	7.78***	(1.36)	1.44
high share low-skilled[(4)]	6,199	6.37***	(1.23)	1.31
subsamples: employees' experience				
low share experienced[(5)]	4,730	6.24**	(3.23)	1.09
high share experienced[(6)]	5,588	3.64***	(0.73)	0.71
low share young[(7)]	5,199	4.73***	(1.12)	0.99
high share young[(8)]	5,683	4.51	(2.88)	0.79

Notes: [a] effect per €100,000 subsidy. Gray records indicate non-interpretable results due to pre-treatment trends of the outcome (see table 7). Results significant on the level: *** p<0.01, ** p<0.05, * p<0.1.
[(1)] share below or equal to 85 percent; [(2)] share above 93 percent; [(3)] share below or equal to 5 percent; [(4)] share above 10 percent; [(5)] share below or equal to 20 percent; [(6)] share above 33 percent; [(7)] share below or equal to 17 percent; [(8)] share above 27 percent.

Sources: Employment History (IAB), GRW treatment data (BAFA), INKAR data (BBSR); own calculations.

A second source of special structural impacts of firms may be connected with their human capital endowment. We obtain subsamples based on employees' qualification and experience structure (see section 4). From theoretical considerations, we expect higher effects of the GRW programme for establishments employing better qualified and more experienced employees. The estimation results only partially confirm our presumptions.

As is described in section 4, our subsamples represent the the top and the bottom 30 percent of the distribution of the respective qualification and experience proxies. Regarding the quali-

fication, we observe at the top of the distribution (that means, in firms with a high share of at least medium-skilled and firms with a low share of low-skilled employees) positive employment effects which, at 1.5 and 1.4 employees per €100,000, respectively, are clearly above average. This result suggests that GRW investment grants are issued to establishments that have the potential to initiate or foster regional economic development.

Surprisingly, also at the bottom of the distribution (i.e. firms with a low share of at least medium-skilled and firms with a high share of low-skilled employees), the employment effect is positive and with 1.3 in both subsamples slightly above average. In addition, one additional job is approximately €9.000 more expensive here than in firms at the top of the distribution (see table A.4).

As expected, employees' experience also influence the strength of the treatment effect. Among establishments with a low share of young workers, we find positive effect on employment development. The normalised ATT amounts to 1.0 jobs per €100,000, slightly below the average. The effect for the subsample representing a high share of young workers is not significant, meaning that the control firms create just as many jobs as the subsidised firms. Unfortunately, we cannot interpret the results for the subsamples with respect to the share of experiences workers.

7 Quality and robustness checks

7.1 Reliability of the estimations in the full sample

In the following, we present the results of different quality and robustness tests for our estimations. As we argue in section 5, the impact of observable and unobservable heterogeneity is problematic if it causes diverging developments between the treated and control establishments over time. In other words, without a treatment, we should observe equal employment trends in the treated and the control group. We regard the pre-treatment employment development in the establishments as a proxy for the development in the absence of treatment and verify whether we observe differences in the employment trends in the treated and control groups before treatment. Figure 1 shows the result of a placebo test for the five-years-period (which coincides to 20 quarters) prior to treatment. The figure reveals no significant differences between the employment development in the treated and the control establishments over the period of five years before the treatment starts. The parallel trend assumption can be considered as fulfilled.

Also the results presented in table 5 confirm this conclusion. The first panel of the table contain the results of pre-treatment placebo tests to validate the conditional parallel-trend assumption for different time periods related to the treatment start. In any of the analysed periods, we

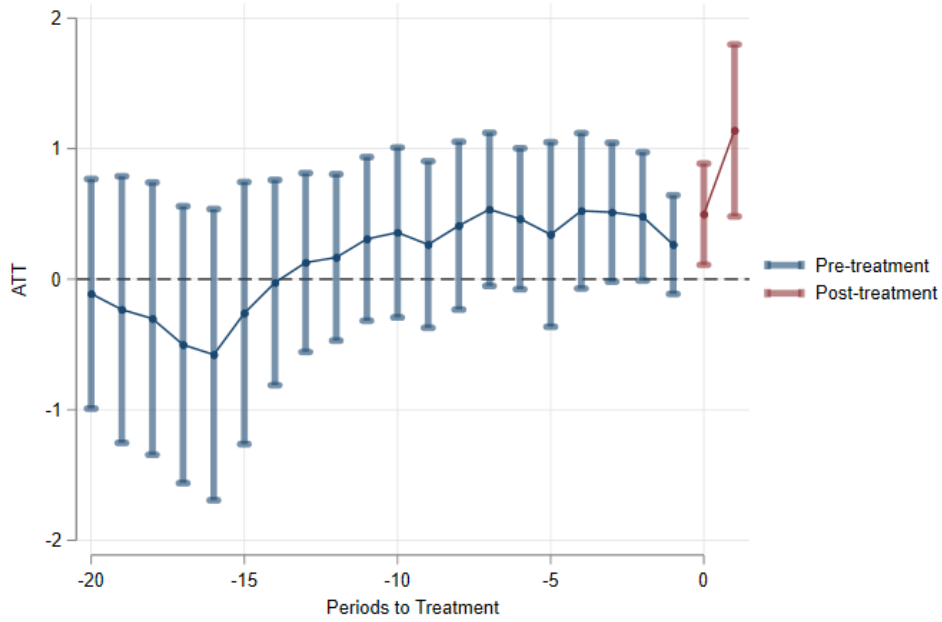

Figure 1: *Placebo test of conditional parallel trend assumption*

Note: The graph gives the estimated pre-treatment effect in the sample for the period of 20 quarters prior to treatment (blue = pre-treatment effect, red = post-treatment effect).
Sources: Employment History (IAB), GRW treatment data (BAFA), INKAR data (BBSR); own calculation and illustration.

observe no significant treatment effect – neither for three or five years prior to treatment, nor the whole pre-treatment period.

In the second panel of table 5, we present the results of short-term placebo tests that can be regarded as a detection for anticipation effects (de Chaisemartin and D'Haultfœuille 2021). Also these period-to-period effects are insignificant for the analysed periods prior to treatment. Thus, also the assumption of no anticipation can be regarded as fulfilled.

Table 5: **Placebo tests for conditional parallel trends and no anticipation**

	Number unique obs.	ATT	std.err.	$P > \|z\|$
pre-treatment effects				
3 years	16,379	0.39	0.24	0.112
5 years	16,379	0.14	0.28	0.621
whole observation period	16,379	0.01	0.50	0.982
period-to-period effects prior to treatment				
3 years	16,379	0.02	0.03	0.556
5 years	16,379	-0.05	0.03	0.115
whole observation period	16,379	0.09	0.06	0.183

Sources: Employment History (IAB), GRW treatment data (BAFA), INKAR data (BBSR); own calculations.

The presented test results also mean that the potentially different economic environments of treated and control firms do not lead to different trends in the employment development – and so, do not distort the results. Nevertheless, in the following, we present the results of various robustness checks including the verification of the sample definition.

First of all, the choice of the control sample is verified. To consider the regional economic environment in an alternative way, we require only neighboring regions of the treated establishments as a possible location of the potential controls. We differentiate three cases: *First*, neighboring regions can be eligible and non-eligible districts. This will change the composition of the control group compared to the initial analysis, because one the one hand, the pool of potential controls is limited to establishments in the neighborhood. On the other hand, the pool of potential controls is not limited to establishments located in non-eligible districts. The results are subject to both sources of distortion, an unobservable selection bias and the impact of different economic environments (see subsection 3.2 for more detailed explanations). Although, the number of treated observations should not be affected, the results in table 6 reveal a loss of observations, presumably due to the rather strict matching criteria (see section 5.3). The estimation results are based on a sample of approximately 12,000 observations. The estimated employment effect is still highly significant and positive, and with 0.9 additional jobs per €100,000 smaller than in the initial sample.

Second, the neighboring regions must be exclusively eligible districts. This requirement reduces the pool of potential controls especially for treated establishments at the border between eligible and non-eligible districts, i.e. in the north and the few western eligible regions, and may result in a lower quality of the control group. Also in this case, both sources of distortion may influence the estimations. The number of treated observations is not affected by construction, but (again, due to the strict matching criteria) we observe a reduction of the sample. The considerably smaller effect of 0.7 additional jobs per €100,000 is based on approximately 11,000 observations.

Third, the considered neighboring regions must be non-eligible districts. This is the best alternative definition, since a potential selection bias due to unobservable characteristics is avoided, and the similarity of the economic environment should be rather high. Unfortunately, the regional distribution of eligible districts (see figure A.2) does not allow to identify non-eligible neighboring regions especially for many of the most strongly treated establishments in East Germany. The consequence is a massive loss of observations: the estimated effect is based on 3,000 observations only, and with 0.3 additional jobs per €100,000 only one third of the effect based on the initial sample.

We also check how the results change if we require that potential controls are located in the same region like the treated firm. This is the 'opposite' idea of defining the pool of potential controls: for the economic environmental conditions to be as similar as possible, we accept

a potential selection bias distorting the results. In this case, the lion's share of the treated establishments is lost for the analysis due to the lack of comparable establishments: the results are based on only 229 observations; the effect is virtually zero.

All in all, we conclude that the required type of neighboring region influences the size and the quality of the pool of potential controls. Only the first case represents a possible alternative for our choice of the control sample. The presented checks also confirm that the initial sample is best suited for the purpose of our analysis since we retain as many observations as possible and thus, are able to reliably analyse various subsamples.

Next, we verify the assumption of no uncontrolled carryover effects. Because we have a wide range of alternative funding options in Germany, while there is no information available on similar treatments for the analysed establishments, we cannot completely exclude an influence of potential treatments prior to the observed GRW funding period. But at least the influence of prior GRW investment grants we can control for. Based on the initial sample, we exclude, in the first step, all (treated and non-treated) establishments that received GRW investment subsidies in the years directly before the analysed funding period starts (in 2005 or 2006) and, in the second step, all establishments that received GRW treatment in the entire observed pre-funding period to verify carryover effects. The second panel of table 6 shows that the number of establishments is strongly reduced by approximately 2,000 and 6,000 establishments for the two periods, respectively. Nevertheless, the estimated employment effects of 1.2 and 1.7 employees per €100,000 for the restricted samples confirm that a carryover effect does not influence the effect of current GRW investment subsidies on the employment development.

In the third and fourth panel of table 6, we present the results of variations in the estimation approach and the data preprocessing. The third panel contains different covariate combinations for the weighting procedure of the DID approach: starting with no covariates, we include only firm characteristics and regional development in the next step – ignoring the pre-treatment outcome, and then check combinations of the pre-treatment outcom with firm characteristics and regional development, respectively. The fourth panel presents our results regarding the influence of different data preprocessings. Here, we vary the included matching covariates: based on the exact matching, we first include only firm characteristics, second only the regional development, and then combine level values of pre-treatment outcome with firm characteristics and the regional development, respectively. These variations yield by and large very similar results: the effect remain positive and ranges in most variations between 1.2 and 0.8 employees per €100,000. But we observe some interesting exceptions: Considering only firm characteristics in the preprocessing changes the estimation: the effect is not significant. This result confirm the importance of taking into account the development of the economic environment when choosing potential controls. Also considering levels of pre-treatment outcome instead of the pre-treatment development in the weighting process influences the estimation (unexpectedly):

the treatment effect is reduced by approximately half.

Table 6: **Robustness checks**

	number	estimated effect		
	unique obs.	ATT	(std.err.)	normalized[1]
initial model	16,379	7.30***	(1.82)	1.13
variation in the sample: choice of potential control establishments from ...				
neighbor regions, eligible and non-eligible	11,928	4.89***	(0.66)	0.91
only eligible neighbor regions	11,188	3.70***	(0.60)	0.65
only non-eligible neighbor regions	2,908	7.69***	(1.12)	0.28
the same region like the treated ones	229	7.00*	(3.60)	0.02
variation in the sample: GRW funding prior to treatment				
without GRW funding in 2005-2006	14,644	6.48***	(1.74)	1.18
without any GRW prior to treatment	10,285	7.02***	(2.15)	1.66
variation in the estimation approach: weighting procedure				
no covariates	16,379	7.82***	(0.66)	1.21
firm characteristics and regional development[2]	16,379	6.97***	(0.58)	1.08
pre-treatment outcome and firm characteristics[3]	16,379	3.28***	(0.98)	0.51
pre-treatment outcome and regional development[4]	16,379	2.74***	(0.72)	0.42
variation in the estimation approach: data preprocessing				
only firm characteristics[5]	139,726	-0.63	(1.96)	./.
only regional development[6]	38,559	7.86***	(1.17)	1.22
pre-treatment outcome and firm characteristics[3]	21,977	5.67***	(1.08)	0.85
pre-treatment outcome and regional development[4]	13,289	5.39***	(1.12)	0.80

Notes: Inital model is the full sample in tables 3 and 4. Results significant on the level: *** p<0.01, ** p<0.05, * p<0.1.
[1] effect per €100,000.
[2] sector (4-digit NACE), share of high-skilled employees, share of low-skilled employees, reg. settlement structure, mean development of regional gross wages and salaries per employee, mean development of regional tax revenues per 1,000 inhabitants, mean development of regional GDP p.c.; [3] sector (4-digit NACE), share of high-skilled employees, share of low-skilled employees, reg. settlement structure, pre-treatment outcome in the years -1, -2, -3; [4] mean development of regional gross wages and salaries per employee, mean development of regional tax revenues per 1,000 inhabitants, mean development of regional GDP p.c., pre-treatment outcome in the years -1, -2, -3.
[5] sector (4-digit NACE), share of high-skilled employees, share of low-skilled employees, reg. settlement structure; [6] mean development of regional gross wages and salaries per employee, mean development of regional tax revenues per 1,000 inhabitants, mean development of regional GDP p.c..

Sources: Employment History (IAB), GRW treatment data (BAFA), INKAR data (BBSR); own calculations.

Summing up the information gained by the quality and robustness tests for the sample, we conclude that the results presented in chapter 6 are reliable. They are not distorted by different trends and not influenced by anticipation or carryover effects. They are also robust to the choice of different potential control samples as well as the variation of the estimation process and the data preprocessing.

7.2 Reliability of the estimations in the subsamples

Also for the subsamples, we verify the reliability of the estimation results. In this subsection, we present the results of the tests for the conditional parallel trend assumption and the assumption of no anticipation. Like for the sample as a whole, we regard the pre-treatment employment trends as proxies for the development in the absence of treatment. The following table 7 contain the results of placebo tests for the period of five years prior to treatment.

Table 7: **Placebo tests for conditional parallel trends and no anticipation in the subsamples**

	no.	cond. parallel trends[1]			anticipation[2]		
	unique obs.	ATT	std.err.	$P > \|z\|$	ATT	std.err.	$P > \|z\|$
subsamples: level of regional disadvantage							
strong	10,240	-0.03	(0.25)	0.920	-0.03	(0.02)	0.256
less strong	6,284	1.11	(0.62)	0.004	0.02	(0.04)	0.503
subsamples: settlement structure							
urban	6,091	1.27	(0.32)	0.000	-0.13	(0.04)	0.749
rural	10,263	0.09	(0.32)	0.768	-0.02	(0.03)	0.529
subsamples: aggregated sector class							
products for private consumption	1,913	5.27	(1.44)	0.000	0.38	(0.24)	0.118
chemicals and pharmaceutics	1,132	-0.58	(0.64)	0.365	-0.03	(0.04)	0.492
equipment for industrial production	6,359	0.22	(0.39)	0.577	0.01	(0.05)	0.978
services and health care	6,306	0.51	(0.32)	0.107	0.01	(0.03)	0.968
exploitation of natural resources	315	-0.44	(3.33)	0.896	0.32	(0.35)	0.363
subsamples: establishment's size							
micro	5,339	0.19	(0.05)	0.000	0.01	(0.01)	0.017
small	7,061	0.31	(0.13)	0.012	0.02	(0.01)	0.347
medium-sized	2,904	2.06	(0.53)	0.000	0.12	(0.05)	0.027
large	380	-5.35	(6.04)	0.376	-0.89	(0.56)	0.114
subsamples: establishment's age							
young firms	3,539	-3.85	(1.43)	0.007	-0.19	(0.21)	0.349
mature firms	12,697	-0.02	(0.34)	0.962	-0.08	(0.04)	0.053
subsamples: employees' formal skill level							
low share at least medium-skilled	4,883	0.85	(0.53)	0.111	0.07	0.05	0.186
high share at least medium-skilled	5,697	-0.14	(0.42)	0.736	-0.04	(0.03)	0.255
low share low-skilled	5,119	0.32	(0.35)	0.347	-0.02	(0.03)	0.558
high share low-skilled	6,199	-0.23	(0.46)	0.609	-0.02	(0.03)	0.464
subsamples: employees' experience							
low share experienced	4,730	1.35	(0.48)	0.005	0.03	(0.03)	0.340
high share experienced	5,588	1.06	(0.32)	0.001	0.06	(0.03)	0.014
low share young	5,199	0.41	(0.30)	0.179	-0.01	(0.03)	0.953
high share young	5,683	-0.11	(0.43)	0.805	0.03	(0.03)	0.355

Notes: [1] estimated pre-treatment effects for the five-years-period prior to treatment; [2] estimated period-to-period treatment effects for the five-years-period prior to treatment.

Sources: Employment History (IAB), GRW treatment data (BAFA), INKAR data (BBSR); own calculations.

Columns three to five give the results of the placebo tests to verify the conditional parallel trend assumption in the subsamles. The assumption can be regarded as fulfilled for most of the analysed subsamples. An exception are the subsamples representing different firm size categories. Here, the placebo effects are significantly different from zero in the three main categories, micro, small and medium-sized firms. We do not include the corresponding results in the table 3, since we cannot interpret any of the results. Thus, no information about the influence of size on the treatment effect is available. Also two subsamples representing different experience levels of the employees exhibit significant placebo effects. Since this indicates a violation of the parallel trend assumption, we cannot interpret the corresponding results presented in section 6 as causal effect; the results are marked in gray, see table 4. The same is true for the subsamples of firms in less strong lagging regions and in urban regions as well as firms in the sector group products for private consuption and young firms, see table 3 and 4. In columns seven to eight, we present the results of the period-to-period placebo tests to verify the assumption of no anticipation. Here, we observe a violation of the assumption only in three cases, namely in two size categories and the subsample of firms with a high share of experienced employees.

In addition, a graphical description of the pre-treatment placebo tests regarding the conditional parallel trend assumption is given in figures A.3 and A.4. Similar, figures A.5 and A.6 present placebo tests for the assumption of no anticipation.

All in all, the results show that it is not possible in every case to create subsamples that fulfil the identification assumption of the estimation approach. The main reason we see in the selectivity of the treated firms. But the results also reveal that the estimation strategy is appropriate to find reliable results for the majority of subsamples.

8 Conclusions

The study analyses the establishment-level employment effects of investment grants issued in the funding period from 2007 to 2013 in Germany. The funding regime typically allows for flexible application times and varying starts of investment projects. Thus, we apply the difference-in-differences approach of Callaway and Sant'Anna (2021) that explicitly models variation in treatment timing. Since the programme is highly selective, we combine this approach with a ties matching at the cohort level.

Beyond the analysis of the average programme effect, the study places specific emphasis to effect heterogeneity guided by the discrimination rules of the programme. The political intention behind the variation of programme generosity is not always clear cut. It addresses various goals like compensation for disadvantages and picking the winners. Against this backdrop, we translate programme rules into different subsamples reflecting specific firm characteristics and

the economic environment. Due to the detailed information in the data set, we can normalise the estimated absolute effects and thus, are able to compare the results in the subsamples.

Overall, investment grants are effective: The results reveal a stronger growth in the treated establishments that amounts to 7.3 employees. Relating this effect to the amount of the subsidy paid, the normalised employment effect is 1.1 employee per €100,000. This effect is robust against variations regarding the composition of the control group and the estimation approach.

With respect to the subsamples representing different economic sectors, the results show – besides different magnitudes of the employment effect – that there is substantial variation in the costs of additional employment across industries.
Regarding the influence of a firm's human-capital endowment the analysis partially confirms the expectations. As expected, we find positive and above average employment effects for firms at the top of the distribution of employee's qualification. However, also at the bottom of the distribution, the estimated effects are positive and – although slightly smaller – above the average. Additional employment is more expensive here than in firms well-endowed with human capital. It is doubtful whether this is consistent with the aim of the GRW programme, i. e. subsidising investments that contribute to the regional economic development.

In addition, we show that the programme is effective in the mostly targeted regions. This applies to firms located in most disadvantaged regions, i.e. East Germany, as well as establishments located in rural areas. Although the effect is below the average in these two subsamples (and thus the additional employment is more expensive), the results shows that spending the lion's share of GRW subsidies in these areas is money well spent and contributes to the reduction of spatial disparities.

Although we contribute to answer the empirically rarely addressed question about the conditions under which investment grants work best, our study raises further research questions. Especially, it is promising to analyse the quality of the subsidised employment in more detail. Another interesting aspect not yet addressed is linked to the treated establishments' recruitment channels: Do they hire staff from among the unemployed or is there substantial job mobility between establishments?

References

Abbring, J.H. and van den Berg, G.J. (2003). The nonparametric identification of treatment effects in duration models. *Econometrica* 71(5):1491–1517

Accetturo, A., Albanese, G., and D'Ignazio, A. (2020). A new phoenix? Large plants regeneration policies in Italy. *Journal of Regional Science* 60:878–902

Accetturo, A. and de Blasio, G. (2012). Policies for local development: An evaluation of Italy's patti territoriali. *Regional Science and Urban Economics* 42:15–26

Andini, M. and de Blasio, G. (2016). Local development that money cannot buy: Italy's Contratti di Programma. *Journal of Economic Geography* 16:365–393

Athey, S. and Imbens, G.W. (2022). Design-based Analysis in Difference-In-Differences Settings with Staggerd Adoption. *Journal of Econometrics* 226:62–79

Bade, F.J. (2012). Die Förderung gewerblicher Investitionen durch die GemeinschaftsaufgabeVerbesserung der regionalen Wirtschaftsstruktur: Wie erfolgreich sind die geförderten Betriebe? *Raumforschung und Raumordnung* 70(1):31–48

BAFA (2016). *Bewilligte GRW-Mittel nach Jahren; Zeitraum 1991 bis 2015*. URL `http://www.bafa.de/SharedDocs/Downloads/DE/Wirtschafts_Mittelstandsfoerderung/grw_bewilligte_mittel_jahre.pdf?__blob=publicationFile&v=2`. Access: 2017-01-30

Barca, F., McCann, P., and Rodriguez-Pose, A. (2012). The case for regional development intervention: place-based versus place-neutral approaches. *Journal of Regional Science* 52(1):134–152

Bechmann, S., Tschersich, N., Ellguth, P., Kohaut, S., Baier, E., and Florian, C. (2021). Technical Report on the IAB Establishment Panel - Wave 27 (2019). techreport 01/2021, Research data Center at the Institute for Employment Research (IAB). URL `http://doku.iab.de/fdz/reporte/2021/MR_01-21_EN.pdf`

Bergemann, A., Fitzenberger, B., and Speckesser, S. (2009). Evaluating the dynamic employment effects of training programs in East Germany using conditional difference-in-differences. *Journal of Applied Econometrics* 24(5):797–823

Bergström, F. (2000). Capital subsidies and the performance of firms. *Small Business Economics* 14:183–193

Bernini, C., Cerqua, A., and Pellegrini, G. (2017). Public subsidies, TFP and efficiency: A tale of complex relationships. *Research Policy* 46(4):751–767

Bernini, C. and Pellegrini, G. (2011). How are growth and productivity in private firms affected by public subsidy? Evidence from a regional policy. *Regional Science and Urban Economics* 41:253–265

Brachert, M., Dettmann, E., and Titze, M. (2018). Public Investment Subsidies and Firm Performance – Evidence from Germany. *Journal of Economics and Statistics* 238(2):103–124

Brachert, M., Dettmann, E., and Titze, M. (2019). The regional effects of a place-based policy – Causal evidence from Germany. *Regional Science and Urban Economics* 79. doi: https://doi.org/10.1016/j.regsciurbeco.2019.103483

Caliendo, M. and Künn, S. (2011). Start-up subsidies for the unemployed: Long-term evidence and effect heterogeneity. *Journal of Public Economics* 95:311–331

Callaway, B. and Sant'Anna, P.H.C. (2021). Difference-in-Differences with Multiple Time Periods. *Journal of Econometrics* 2252,(2):200–230. doi:https://doi.org/10.1016/j.jeconom.2020.12.001

Calmfors, L., Forslund, A., and Hemström, M. (2002). Does Active Labour Market Policy Work? Lessons from the Swedish Experiences. Seminar Paper 700, Institute for International Economic Studies, Stockholm University, Institute for International Economic Studies, Stockholm University, Stockholm

Cerqua, A. and Pellegrini, G. (2014). Do subsidies to private capital boost firms' growth? A multiple regression discontinuity design approach. *Journal of Public Economics* 109:114–126

Ciani, E. and de Blasio, G. (2015). European structural funds during the crisis: evidence from Southern Italy. *IZA Journal of Labor Policy* 4:20

Conlon, G., Dohler, G., Lee, S.M., and Patrignani, P. (2023). Skills and UK productivity: Estimating the contribution ofeducational attainment to productivity growth. Government social research report, Uk government, department for education. URL `https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/1137822/Skills_and_UK_productivity.pdf`

Criscuolo, C., Martin, M.R., Overman, H.G., and Van Reenen, J. (2019). Some Causal Effects of an Industrial Policy. *American Economic Review* 109(1):48–85

de Castris, M. and Pellegrini, G. (2012). Evaluation of Spatial Effects of Capital Subsidies in the South of Italy. *Regional Studies* 46:525–538

de Chaisemartin, C. and D'Haultfœuille, X. (2021). Difference-in-DifferencesEstimators of Intertemporal Treatment Effects. discussion paper 3731856, Social Science Research Network (SSRN)

Decramer, S. and Vanormelingen, S. (2016). The effectiveness of investment subsidies: evidence from a regression discontinuity design. *Small Business Economics* 47:1007–1032

Devereux, M., Griffith, R., and Simpson, H. (2007). Firm location decisions, regional grants and agglomeration externalities. *Journal of Public Economics* 91:413–435

Dhawan, R. (2001). Firm size and productivity differential: theory and evidence from a panel of us firms. *Journal of Economic Behavior and Organization* 44:269–293

Dvouletý, O., Srhoj, S., and Pantea, S. (2020). Public SME grants and firm performance in European Union: A systematic review of empirical evidence. *Small Business Economics* doi:https://doi.org/10.1007/s11187-019-00306-x

Eberle, J., Brenner, T., and Mitze, T. (2019). A look behind the curtain: Measuring the complexeconomic effects of regional structural funds in Germany. *Papers in Regional Science* 98:701–735

Eckey, H.F. (2008). Verbesserung der regionalen Wirtschaftsstruktur. Gesetz über die Gemeinschaftsaufgabe vom 6. oktober 1969. In H.H. Eberstein, H. Karl, and G. Untiedt (ed.), *Handbuch der regionalen Wirtschaftsförderung*, vol. 1, ch. V. Köln: Otto Schmidt Verlag

Eurostat (2008). *NACE Rev. 2 – Statistical classification of economic activites in the European Community*. European Commission. URL `https://ec.europa.eu/eurostat/documents/3859598/5902521/KS-RA-07-015-EN.PDF`

Fackler, D., Schnabel, C., and Wagner, J. (2013). Establishment exits in Germany: the role of size and age. *Small Busines Economics* 41:683–700

Fujita, M., Krugman, P., and Venables, A. (1999). *The spatial economy. Cities, regions, and international trade*. Cambridge (Mass.): MIT Press

Galindo-Rueda, F. and Haskel, J. (2005). Skills, Workforce Characteristics and Firm-Level Productivity: Evidence from the matched ABI/Employer Skills Survey. Discussion Paper 1542, Institute for the Study of Labor (IZA)

Girma, S., Görg, H., and Strobl, E. (2007). The effects of government grants on plant survival: a micro-econometric analysis. *International Journal of Industrial Organization* 25:701–720

Guerzoni, M. and Raiteri, E. (2015). Demand-side vs. supply-side technology policies: Hidden treatment and new empirical evidence on the policy mix. *Research Policy* 44(3):726–747

Gustafsson, A., Stephan, A., Hallman, A., and Karlsson, N. (2016). The 'sugar rush' from innovation subsidies: a robust political economy perspective. *Empirica* 43:729–756

Harris, R. and Trainor, M. (2007). Impact of government intervention on employment change and plant closure in Northern Ireland, 1983–97. *Regional Studies* 41:51–63

Heckman, J.J., Ichimura, H., Smith, J.A., and Todd, P.E. (1998). Characterizing Selection Bias Using Experimental Data. *Econometrica* 66(5):1017–1098

Heckman, J.J., Ichimura, H., and Todd, P.E. (1997). Matching As An Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme. *Review of Economic Studies* 64(4):605–654

Heckman, J.J., LaLonde, R.J., and Smith, J.A. (1999). The Economics and Econometrics of Active Labor Market Programs. In O. Ashenfelter and D.E. Card (ed.), *Handbook of Labor Economics*, vol. III, pp. 1865–2097. Amsterdam: Elsevier Science B.V.

Henderson, J., Kuncoro, A., and Turner, M. (1995). Industrial Development in Cities. *Journal of Political Economy* 103(5):1067–1085

Ho, D.E., Imai, K., King, G., and Stuart, E.A. (2007). Matching as Nonparametric Preprocessing for Reducing Model Dependence in Parametric Causal Inference. *Political Analysis* 15:199–236

Holl, A. (2018). Local employment growth patterns and the Great Recession: The case of Spain. *Journal of Regional Science* 58:837–863

Jacobson, L.S., LaLonde, R.J., and Sullivan, D.G. (1993). Earnings Losses of Displaced Workers. *The American Economic Review* 83(4):685–709

Krugman, P. (1991). *Geography and trade*. Cambridge (Mass.): MIT Press

Lucas, R.E. (1988). On the mechanics of Economic Development. *Journal of Monetary Economics* 22:3–42

Malani, A. and Reif, J. (2015). Interpreting pre-trends as anticipation: Impact on esti-mated treatment effects from tort reform. *Journal of Public Economics* 124:1–17

Mincer, J. (1962). On-the-job Training: Costs, Returns, and some Implications. *Journal of Political Economy* 70(5):50–79

Müller, S. and Stegmaier, J. (2015). Economic failure and the role of plant age and size. *Small Business Economics* 44:621–638

Moffat, J. (2014). Regional Selective Assistance in Scotland: Does it make a difference to plant productivity? *Urban Studies* 51(12):2555–2571

Morris, D., Vanino, E., and Corradini, C. (2020). Effect of regional skill gaps and skill shortages on firm productivity. *Environment and Planning A: Economy and Space* 52(5):933–952

Neumark, D. and Simpson, H. (2015). Place-Based Policies. In G. Duranton, J.V. Henderson, and W. Strange (ed.), *Handbook of Regional and Urban Economics*, vol. 5, ch. 18, pp. 1197–1287. Elsevier Science B. V

Pagano, P. and Schivardi, F. (2003). Firm Size Distribution and Growth. *Scandinavian Journal of Economics* 105(2):255–274

Pellegrini, G. and Muccigrosso, T. (2017). Do subsidized new firms survive longer? Evidence from a counterfactual approach. *Regional Studies* 51(10):1483–1493

Robins, J.M., Hernán, M.n., and Brumback, B. (2000). Marginal Structural Models and Causal Inference in Epidemiology. *Epidemiology* 11(5):550–560

Romer, P.M. (1986). Increasing Returns and Long-Run Growth. *Journal of Political Economy* 94:1002–1037

Rosenthal, Stuart an Strange, W. (2004). Evidence on the Nature and Sources of Agglomeration Economies. In J. Henderson and J. Thisse (ed.), *Handbook of Regional and Urban Economics*, vol. 4, ch. 49, pp. 2119–2171. Els

Saito, H. and Wu, J.J. (2015). Agglomeration, congestion, and U.S. regional disparities in employment growth. *Journal of Regional Science* 56(1):53–71

Siegloch, S., Etzel, T., and Wehrhöfer, N. (2021). Efficiency and Equity Effects of Place-Based Policies: Evidence from Capital Subsidies in East Germany. Discussion paper 21-038, Leibniz Centre for European Economic Research (ZEW)

Solow, R. (1956). A Contribution to the Theory of Economic Growth. *Quarterly Journal of Economics* 70:65–94

Swan, T.W. (1956). Economic growth and capital accumulation. *Economic Record* 32(2):334–361

Wardenburg, S. and Brenner, T. (2019). How to improve the quality of life in peripheral and lagging regions by policy measures? Examining the effects of two different policies in Germany. *Journal of Regional Science* 60:1047–1073

9 Appendix

Calculation of the structural weakness score

$$S_r = \prod_m {V_{mr}}^{w_m}$$

$$\textit{with} \quad V_{mr} = \begin{cases} 100 - \frac{m_r - \mu_m}{\sigma_m} & \textit{if} \quad m = 1 \\ 100 + \frac{m_r - \mu_m}{\sigma_m} & \textit{if} \quad m = 2, 3, 4 \end{cases}$$

and
- S_r – *overall score for region r*
- V_{mr} – *standardized value of indicator m in region r*
- μ_m – *mean value of indicator m*
- σ_m – *standard deviation of indicator m*

Indicators for structural weakness

Indicator (m)		Weight (w_m)
1	Average unemployment rate (2002-2005)	0.50
2	Annual gross salary (2003)	0.40
3	Quality of business-oriented infrastructure (2005)	0.05
4	Employment projection for the period (2004-2011)	0.05

Figure A.1: *Calculation of the structural weakness score for the funding period 2007–2013*

Source: Own illustration on the basis of Eckey (2008).

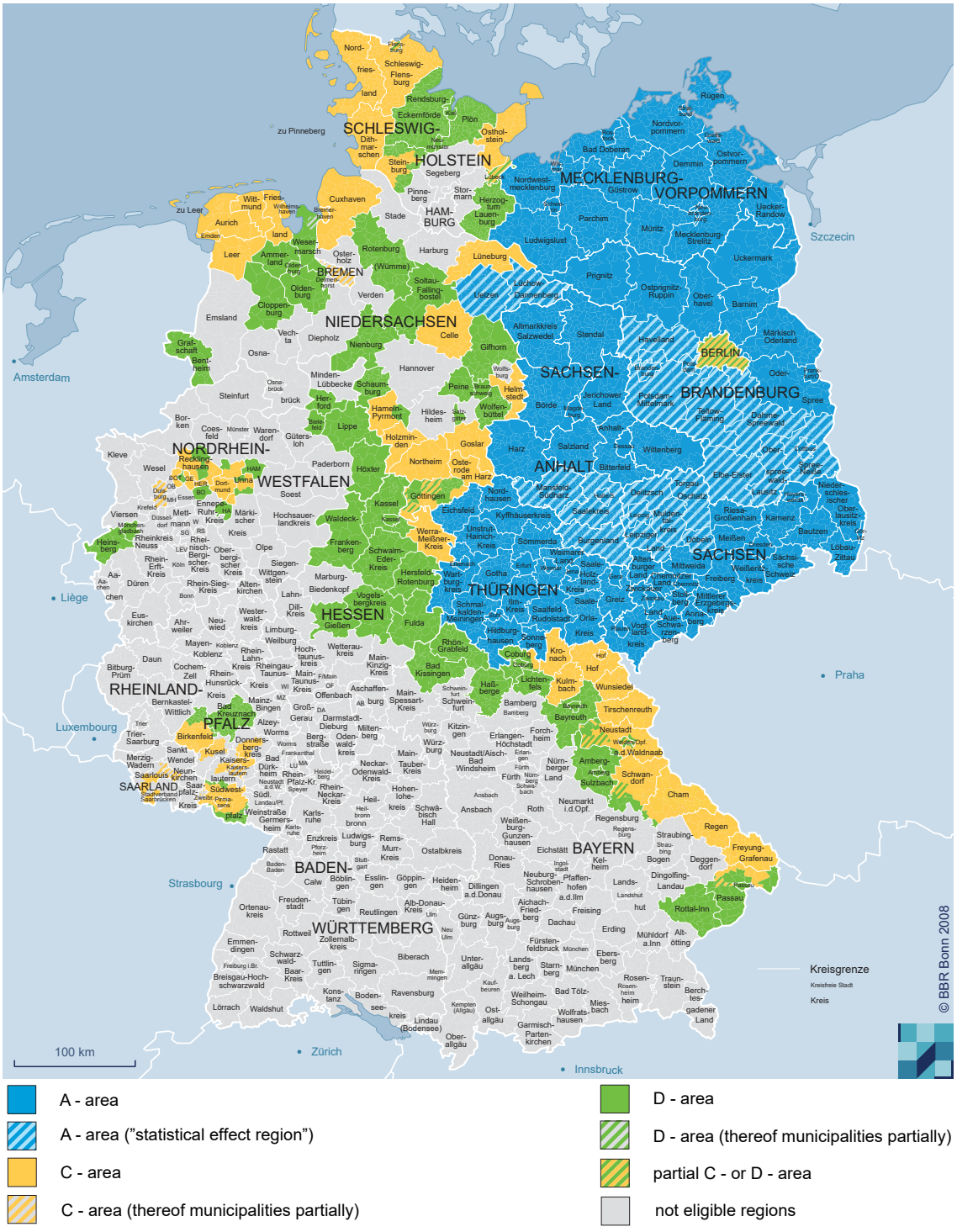

Figure A.2: *The Joint Task 'Improvement of Regional Economic Structure' (GRW) – Map of Eligible Regions 2007-2013.*

Source: BBSR.

Table A.1: **Legal framework for GRW in Germany for the analyzed programme period**

Type of the programme	Non-repayable grants for investment projects (co-funding)
Set-up of the programme	1970
Targets	Reduction of regional disparities Increase of regional income and employment in assisted regions Catching-up of assisted regions to the general economic development
Regional scope of the programme	Eligible labor market regions in the Federal Republic of Germany → Eligibility depends on the degree of the structural weakness of a region
Influence of EU	Limitation of the number of assisted regions according to the population share (40.2% of German population) funding period linked to the funding period of EU Structural funds
Legislation	Treaty of the European Union (Articles 87 and 88) German Basic Law (Articles 72(2) No. 2, 91a) Joint Task Law (GRW-Gesetz) Coordination framework ◦ No.36, 361, 362 Legislation of the Federal States
Application process	normalized application forms assessment by the Federal government and the responsible institution
Granting authority	Government of the federal state where the investment project is planned

Source: Own compilation and illustration.

Table A.2: **GRW funding rules for the analyzed programme period**

Recipients	Establishments in eligible areas			Municipalities in eligible areas
Subject of funding	Investments in fixed assets ∘ starting a new establishment ∘ capacity expansion of existing establishments ∘ changing the production programme or overhauling the entire process of production ∘ purchasing an inoperative establishment *Sectoral scope of the programme* ∘ industries with supra-regional sales ∘ exclusion of industries according to EU state aid legislation and further economic intentions			Investments in local commerce-related infrastructure ∘ building of business parks ∘ revitalizing of business parks ∘ building and expansion of transport infrastructure ∘ building and expansion of associated utilities infrastructure (water, electricity, telecommunication etc.) ∘ building and expansion of sewage plants ∘ building and expansion of touristic infrastructure ∘ building and expansion of workforce training centers ∘ building and expansion of technology parks
Maximum aid intensity (as percent of eligible costs)	A-areas[(1)]	small	50	90
		medium-sized	40	
		large	30	
	C-areas[(2)]	small	35	
		medium-sized	25	
		large	15	
	D-areas	small	15	
		medium-sized	7.5	
		large	7.5, max. 200 000 €	

Notes: [(1)] In 'statistical-effect regions', the maximum state-aid is reduced to 40, 30 and 20 percent, respectively. [(2)] Deviations from the standard rule are possible.

Source: Own compilation and illustration.

Table A.3: **Aggregated economic sectors for the analysis of subsamples**

	sector codes included	
name of the sector group	NACE Rev.1.1	NACE Rev.2
manufacturing of products for private consumption	15, 16, 17–19, 21, 36, 37	10–12, 13–17, 31, 32
chemicals and pharmaceutics	23–26	19–23
machinery and equipment for industrial production	27–35, 45	24–30, 33, 41–43
services and health care	22, 50–52, 55, 60–67, 70–74, 85, 92, 93	18, 45–47, 49–56, 58–66, 68–82, 86–88, 90–93, 95, 96
exploitation of natural resources	1, 2, 5, 10–14, 40, 41, 90	1–3, 5–9, 35–39

Note: The aggregation uses the German Classification of Economic Activities system that corresponds to the NACE classification system and is oriented on the aggregation scheme of the IAB Establishment panel, see Bechmann et al. (2021). Eligibility to apply for GRW investment grants is defined at the 4-digit sectoral level, so excluded sectors cannot be identified in this aggregation scheme.

Source: Own summary.

Table A.4: **Amount of the subsidy in the subsamples, total and per job additionally provided**

	number of treated establishments	total subsidy (million €)	subsidy per add. job* (€)
full sample	8,079	5,210	88,300
subsamples: employee structure			
low share of at least med.-skilled[(1)]	2,457	1,070	78,290
high share of at least med.-skilled[(2)]	2,861	1,840	67,923
low share of low-skilled[(3)]	2,498	1,350	69,314
high share of low-skilled[(4)]	3,139	1,520	76,094
low share of experienced[(5)]	2,346	1,340	91,867
high share of experienced[(6)]	2,808	1,440	140,828
low share of young[(7)]	2,507	1,190	100,217
high share of young[(8)]	2,907	1,650	126,012
subsamples: economic environment			
strongly disadvantaged regions	5,053	4,000	134,190
less disadvantaged regions[(9)]	3,020	1,190	51,556
urban region	2,862	1,610	39,290
rural region	5,204	3,560	159,192
*subsamples: establishment's aggregated economic sector groups***			
products for private consumption[(10)]	982	670	878,849
chemicals and pharmaceutics[(11)]	584	437	256,216
equipment for industrial production[(12)]	3,247	2,060	120,738
services and health care	2,890	1,090	112,593
exploitation of natural resources[(13)]	160	63.1	85,303
subsamples: establishment's size			
micro est. (< 10 employees)	2,297	360	239,106
small est. (>= 10 and < 50 employees)	3,664	1,320	196,874
medium-sized est. (>= 50 and < 250 employees)	1,537	1,870	199,536
large est. (>= 250 employees)	200	772	48,843
subsamples: establishment's age			
young establishments (< 5 years)	1,695	608	396,883
mature establishments (>= 5 years)	6,305	4,200	84,825

Notes: *Costs are calculated per additionally provided job in treated establishments compared to the controls (the absolute treatment effect); **sector aggregation follows the aggregation scheme of the IAB Establishment panel, see table A.3.
[(1)]share below or equal to 85 percent; [(2)]share above 93 percent; [(3)]share below or equal to 5 percent; [(4)]share above 10 percent; [(5)]share below or equal to 20 percent; [(6)] share above 33 percent; [(7)] share below or equal to 17 percent; [(8)] share above 27 percent; [(9)] west Germany including Berlin; [(10)]production of food, fabrics, other goods; [(11)]petroleum processing, manufacture of chemicals, pharmaceutics and ceramics; [(12)]production and maintenance of electrical equipment, machinery and vehicles, metal production, construction; [(13)]agriculture, forestry, mining, basic supply.

Sources: Employment History of IAB, GRW treatment data of BAFA, INKAR data of BBSR; own calculations.

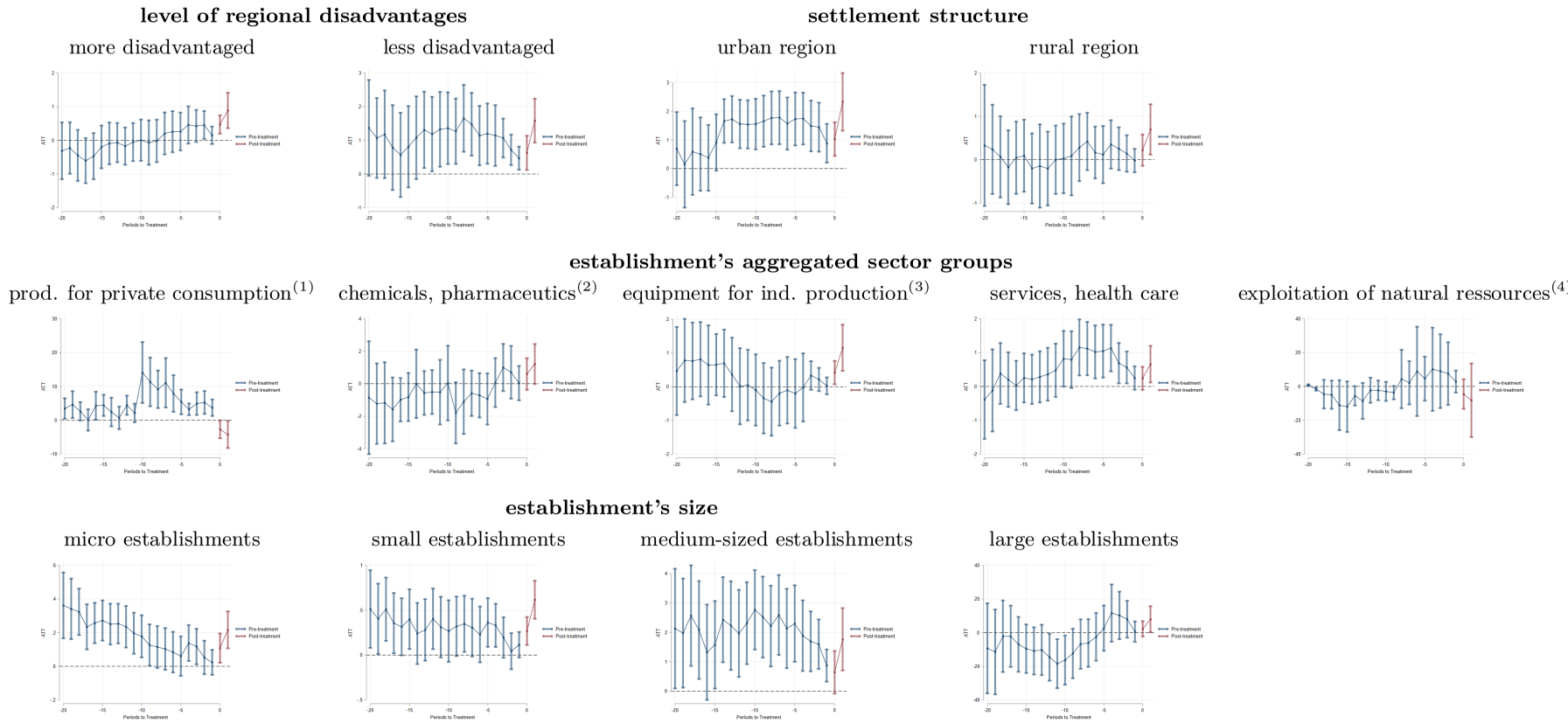

Figure A.3: *Conditional parallel trend assumption in the subsamples (economic environment and establishment's characteristics)*

Note: The graph gives the estimated pre-treatment effects in the subsamples for the five-years-period prior to treatment (blue = pre-treatment effect, red = post-treatment effect).
(1)production of food, fabrics, other goods; (2)petroleum processing, manufacture of chemicals, pharmaceutics and ceramics; (3)production and maintenance of electrical equipment, machinery and vehicles, metal production, construction; (4)agriculture, forestry, mining, basic supply.

Sources: Employment History (IAB), GRW treatment data (BAFA), INKAR data (BBSR); own calculation and illustration.

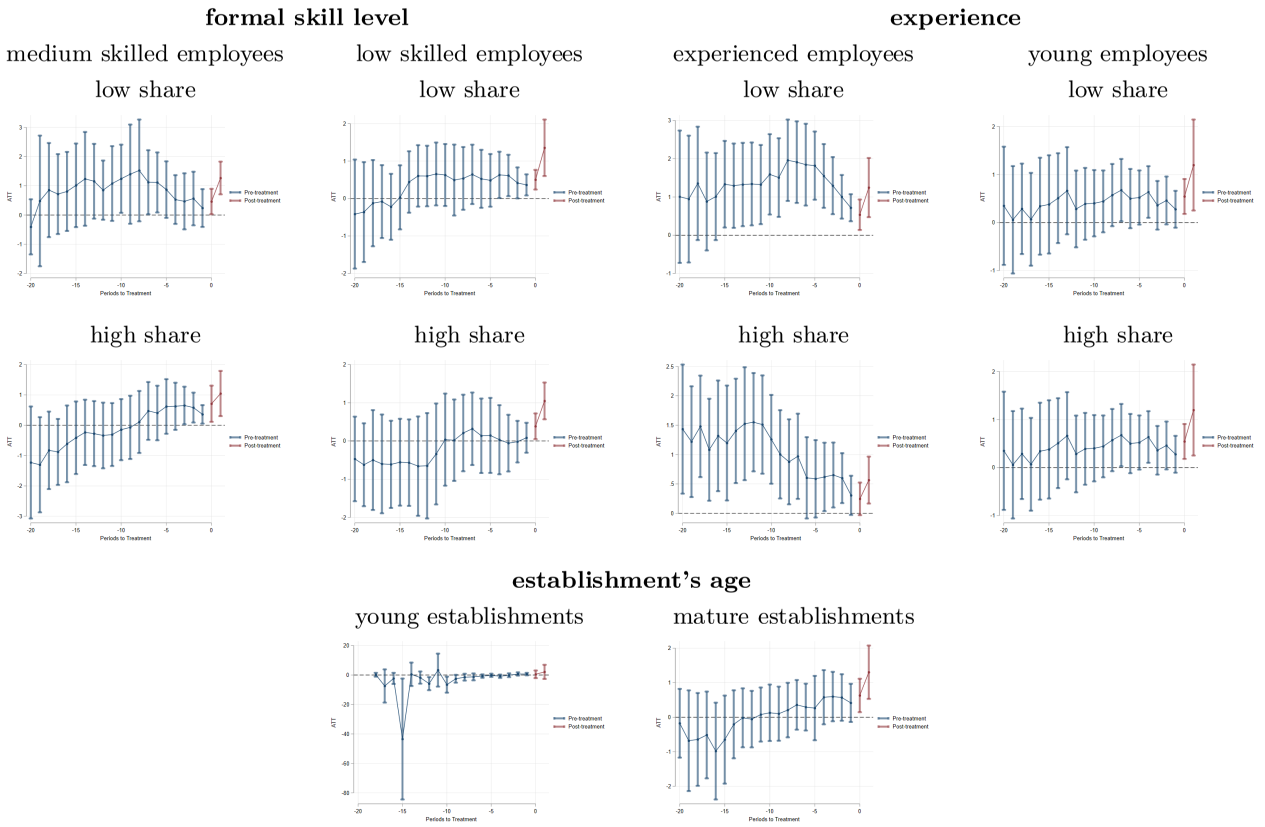

Figure A.4: *Conditional parallel trend assumption in the subsamples (human capital endowment and establishment's age)*

Note: The graph gives the estimated pre-treatment effects in the subsamples for the five-years-period prior to treatment (blue = pre-treatment effect, red = post-treatment effect).

Sources: Employment History (IAB), GRW treatment data (BAFA), INKAR data (BBSR); own calculation and illustration.

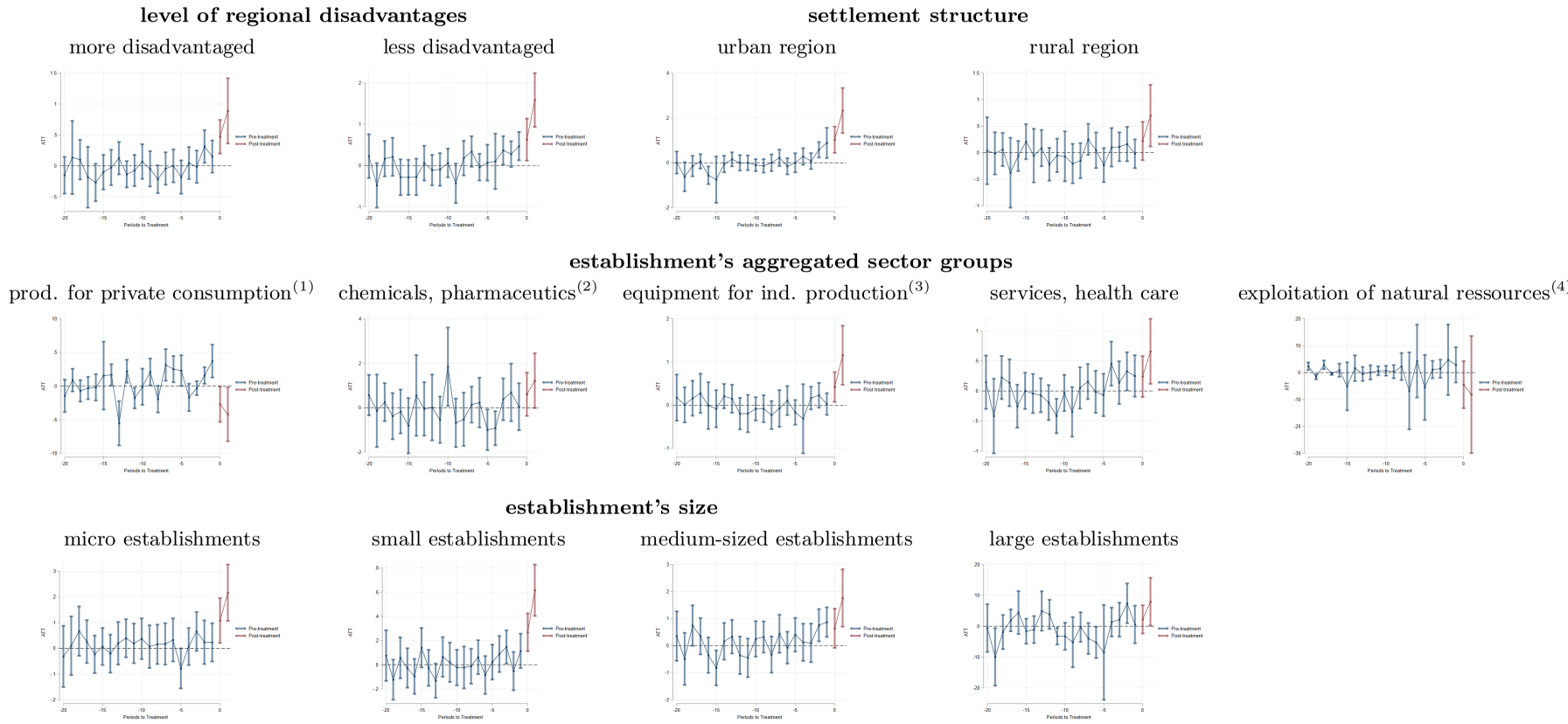

Figure A.5: *No anticipation assumption in the subsamples (economic environment and establishment's characteristics)*

The graph gives the estimated period-to-period treatment effects in the subsamples for the five-years-period prior to treatment (blue = pre-treatment effect, red = post-treatment effect).
(1)production of food, fabrics, other goods; (2)petroleum processing, manufacture of chemicals, pharmaceutics and ceramics; (3)production and maintenance of electrical equipment, machinery and vehicles, metal production, construction; (4)agriculture, forestry, mining, basic supply.

Sources: Employment History (IAB), GRW treatment data (BAFA), INKAR data (BBSR); own calculation and illustration.

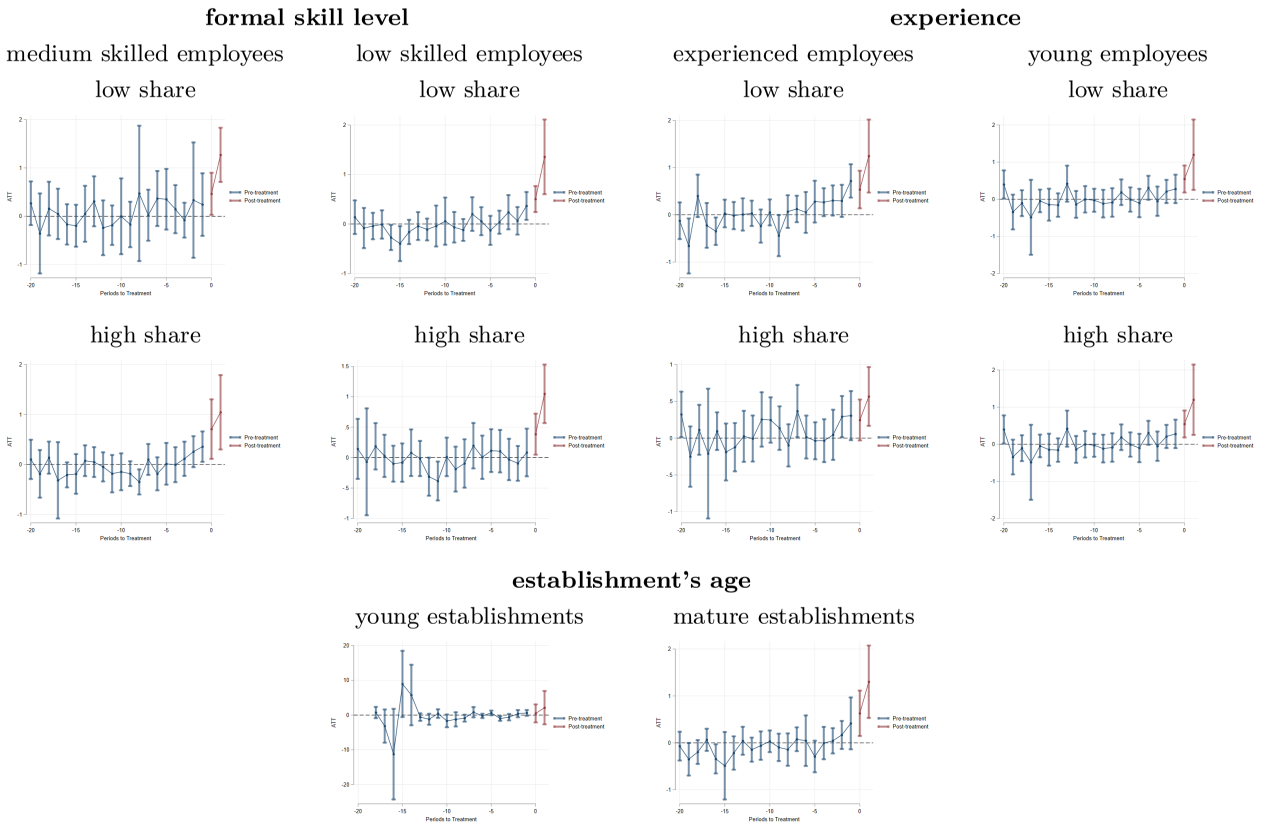

Figure A.6: *No anticipation assumption in the subsamples (human capital endowment and establishment's age)*

Note: The graph gives the estimated period-to-period treatment effects in the subsamples for the five-years-period prior to treatment (blue = pre-treatment effect, red = post-treatment effect).

Sources: Employment History (IAB), GRW treatment data (BAFA), INKAR data (BBSR); own calculation and illustration.

Halle Institute for Economic Research –
Member of the Leibniz Association

Kleine Maerkerstrasse 8

D-06108 Halle (Saale), Germany

Postal Adress: P.O. Box 11 03 61

D-06017 Halle (Saale), Germany

Tel +49 345 7753 60

Fax +49 345 7753 820

www.iwh-halle.de

ISSN 2194-2188

The IWH is funded by the federal government and the German federal states.