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

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Abstract

This study estimates the firm-level employment effects of investment grants in Germany. In addition to the average treatment effect on the treated, we examine discrimination in the funding rules as potential source of effect heterogeneity. We combine a staggered difference-in-differences approach that explicitly models variations in treatment timing with a matching procedure at the cohort level. The findings reveal a positive effect of investment grants on employment development in the full sample. The subsample analysis yields strong evidence for heterogeneous effects based on firm characteristics and the economic environment. This can help to improve the future design of the program.

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

JEL classification: A11, D61, H20, Z0

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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

This study analyzes 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 program rules: the establishments' characteristics and the economic environment.[1]

Place-based policy schemes are common globally. For example, a considerable share of the overall budget in the European Union (EU), is allocated to such policy schemes amounting to €278 billion in the 2007-2013 funding period (Ciani and de Blasio 2015). Beyond that, almost all EU member states have implemented national and regional policy programs that include investment grants (Criscuolo et al. 2019). The total expenditure for the German program within this study's analysis period amounted to approximately €9 billion (BAFA 2016), while approximately one-third of all investments in eligible regions are funded by the analyzed program (BAFA 2016).

This type of policy is primarily designed to foster economic development in structurally weak regions by enhancing employment and income (Neumark and Simpson 2015); however, the intention of such interventions has been discussed ambiguously. In principle, imperfect markets may justify the introduction of such programs. Previous research primarily highlights externalities, indivisible production factors, imperfect labor mobility, financial constraints due to asymmetric information, and regional equality issues as rationales for implementing such policy schemes (Calmfors et al. 2002, Neumark and Simpson 2015). The literature also discusses potential side-effects and difficulties of implementing place-based policy programs, for example 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 investigations that address these arguments considering specific regional features. Recent studies predominantly 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. 2024, Wardenburg and Brenner 2019). However, for selected place-based investment policies in (southern) Italy, the literature also provides evidence revealing negative

[1] Throughout this study, we use the terms establishment and firm synonymously, which also applies to treated and subsidized.

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

effects on regional economic performance (e.g., Accetturo et al. 2020, Accetturo and de Blasio 2012, Andini and de Blasio 2016).

Besides, the past 20 years have witnessed a rising number of empirical studies on the effect of investment grants, at the establishment level. The most analyzed examples are the Italian Law 488/1992 and the British Regional Selective Assistance; however, we also find evidence for research on such policies in other European countries. In summary, previous research largely suggests that investment grants positively influence overall firm-level employment, investments, turnover, output, and firm survival (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).

The majority of studies examine the average overall effect of the subsidy and only some research also addresses diverging funding effects due to firm heterogeneity. Thus far, the most frequent focus is on firm size. For example, Bade (2012) and Grunau et al. (2024) in Germany and Criscuolo et al. (2019) in the United Kingdom find that the effects of investment subsidies are higher for smaller firms. Previous studies lack a systematic analysis of heterogeneous effects due to firm characteristics and the local economic environment. Based on a meta analysis of the empirical literature on public grants in the EU, Dvouletý et al. (2020) acknowledge the need to dive further into the heterogeneity of effects caused by observable firm characteristics and propose to address research questions such as "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).

This study fills 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 program. Our contribution to the literature is a systematic analysis of heterogeneous program effects based firm characteristics and the local economic environment. Since the rules of the analyzed German program differentiate funding based on specific establishment characteristics and the economic environment, we leverage this treatment discrimination as a guideline for our heterogeneity analysis. Using an exceptionally rich dataset that includes detailed information on establishments and economic environments, we can compare the magnitude of the employment effect in different subsamples. Our results provide valuable insights for optimizing 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 with previous evidence for Germany, these calculations provide a more realistic impression of the real costs connected to created or safeguarded jobs via investment grants, for the sample as a whole, as well as the analyzed subsamples.

Finally, our study contributes to the contemporary empirical discussion regarding 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 staggered difference-in-differences (DID) approach of Callaway and Sant'Anna (2021) that explicitly models variation in treatment timing with a ties matching procedure that aligns the most diverging relevant characteristics at the cohort level.

The remainder of the paper is organized as follows: The next section describes the legal framework for investment grants in Germany, section 3 provides an overview of our data sources and the sample analyzed. Section 4 explains the construction of subsamples based on the treatment discrimination of the program. Section 5 introduces our estimation approach, and we present our results for the full sample and subsamples in section 6. Section 7 presents some quality and robustness checks, and 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 primary goal of the program is to reduce regional disparities across Germany, particularly in terms of (un)employment and income by providing investment grants in poor regions.[4] Since investment grants distort competition in the Common Market, EU must approve the program rules for a programming period, which is typically seven years.

A key feature of place-based policies is spatially limited program access. Therefore, only firms in structurally weak regions can access the funding. Regional eligibility is based on a structural weakness score that includes several single indicators (regional unemployment, gross wages and salaries, quality of infrastructure, and employment projection).[5] Consequently, mainly regions in East Germany, those bordering the Czech Republic, some regions in the north and some former industrial regions that have undergone serious structural change are eligible in the analyzed funding period. In Appendix Figure A.2, we present different maps illustrating the regional eligibility at the district level and the distribution of the number of approved projects and the GRW funding intensity. The maps reveal a positive correlation between the severity

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

[4]The program 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 program's legal framework and the funding rules are provided in Appendix Tables A.1 and A.2.

[5]Appendix Figure A.1 provides a detailed description of GRW score components and calculation.

of structural weakness (i.e., eligibility category) and funding intensity in terms of the number of approved projects and the volume of the subsidies paid.

The GRW program has an implicit sectoral scope in the analyzed funding period. Applicants must satisfactorily demonstrate supra-regional sales, which is defined as sales that are 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 fulfill this criterion, predominantly listing manufacturing-sector industries,[6] and firms operating in the service sector are also eligible if they meet the aforementioned criteria.

The application process follows a normalized procedure, that is managed by the responsible federal state. An applying firm must describe the planned investment project and provide a business plan, 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 dataset combines information from multiple sources. The treatment information is obtained from the Federal Office for Economic Affairs and Export Control (BAFA), establishment-level employment information 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 includes project-level information, e.g., the start and end of the subsidized project, as well as location of the applicant and the investment, in addition to monetary information.

This study considers 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 realized and received financial support. Overall, 13,384 projects are included in the treatment data which corresponds to 11,031 treated establishments. As shown

[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.

in Table 1, the distribution of the total investments of €33.5 billion is very imbalanced among the treated. The funded projects last two years on average, the average subsidy rate is about one-third. The funding costs amount to €5.5 billion, and the distribution of the funding per project is highly skewed.

Table 1: **Key facts of GRW funding (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 amount of project subsidy	*€ thousand*	409.70
Median amount of project subsidy	*€ thousand*	134.28
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).

Unfortunately, the database contains no information on rejected applicants and projects. To obtain information on non-treated establishments, we use the employment history data provided by the IAB for 2002 to 2016, aggregated at the establishment level. The IAB data comprise information on an firms' number of employees and employee structure in terms of age, gender and professional qualifications. We summarize size information based on the EU definition of micro establishments with up to 9 employees, small establishments with 10-49 employees, medium-sized establishments with 50-249 employees, and large establishments with 250 or more employees. We also use information on vocational qualifications to characterize firms' 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, and the share of low-skilled employees represents the proportion of employees without vocational education. The age structure is described by the share of young employees (under 30 years of age), and the share of older employees, which is defined as the proportion of persons aged 50 years or older.

Our data also include information on the establishments' date of foundation, location, and economic sector. Economic sector information is based on the German Classification of Economic Sectors, which is consistent with the Nomenclature of Economic Activities (NACE) classifi-

cation system. Referencing this information, we can restrict the sample to establishments operating in economic sectors that are formally eligible for GRW.[8]

Establishments' location enables us to enrich the data with regional information from the INKAR database of the BBSR. We include districts' unemployment rate, GDP per inhabitant, tax revenue per 1,000 inhabitants, and gross wages and salaries per employee. We also consider the BBSR's definition of districts' settlement-structure,[9] constructing two broad categories: urban regions (cities and urban districts) and rural regions.

The result is a rich, unbalanced, panel dataset with quarterly information from 2002 to 2016 with 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 10,015 are treated establishments located in eligible regions.[10]

When selecting non-treated establishments as potential controls, we face a tradeoff between two sources of potential estimation distortion. The *first source* is selection bias due to unobserved characteristics of the non-treated establishments in eligible regions: The GRW is a demand-driven program in which all establishments in eligible sectors in eligible regions can apply to the GRW program, and we cannot determine why some establishments applied for grants and others did not. The *second source* concerns the non-treated establishments located in non-eligible regions that 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 development exhibits 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 concern. To control for unobservable selection, we exclude non-treated establishments in eligible regions from the sample, only considering firms 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. We also conduct comprehensive

[8]The BAFA provided detailed sector-specific information on eligibility for investment grants at the 4-digit level for WZ2003 and 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), which is based on population share in large and medium-sized cities, population density, and population density excluding large and medium-sized cities. See BBSR (2018) [in German].

[10]Out of 11,031 establishments that receive GRW funding, we were able to assign information for 10,015 firms using record linkage techniques.

robustness tests regarding the choice of potential controls. We specify four alternative samples focusing on non-treated firms located in the treated firm's neighborhood to minimize the probability of different regional developments, see section 7.

Table 2 summarizes potentially relevant firm and environment-related characteristics that may influence the firms' employment development and (successful) investment grant application.[11] Since the panel data are unbalanced, the descriptive statistics in Table 2 provide a snapshot of the establishments in the sample at the start of the funding period (the first quarter of 2007).

The descriptive statistics for the sample at the start of the funding period in Table 2 suggest that the GRW program is highly selective. We observe substantial differences concerning some firm characteristics. The vast majority of both, treated and non-treated firms are small or very small, but the treated establishments are larger, on average. Compared with the non-treated establishments, the share of treated 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 subsidized firms operate in fabricated metal products (16 percent) and machinery and equipment (9 percent), whereas non-subsidized establishments primarily operate in business-related activities (14 percent)[12] or construction (11 percent). A remarkable divergence in the firms' location also emerges. Only one-third of the subsidized establishments are located in urban areas, compared with approximately four-fifths of the non-treated establishments.

In contrast, we observe a very similar distribution of firms in the age groups and employee structure, which exhibit only minor differences. Despite the similar employee structure, differences in the median monthly salary are observable: treated establishments pay €1,900 per month on average, and non-treated establishments pay approximately €300 more. This may be partially explained by different economic environments. As expected, the (non-treated) establishments in regions that are not eligible for investment grants benefit from better economic conditions that include significantly lower unemployment rates, substantially higher GDP per capita, and higher tax revenue. The difference in regional gross wages and salaries per employee is remarkable at approximately €450. Finally, the descriptive statistics reveal 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 with approximately 70 percent of the treated establishments, but no non-treated firms. This is surprising at first sight; how-

[11] Excluding non-treated establishments in eligible regions causes missing values for non-treated establishments in Bremen, Berlin, Brandenburg, Mecklenburg Pomerania, Saxony, Saxony Anhalt, and Thuringia in Table 2.

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

Table 2: **Descriptive statistics for treated and non-treated firms in the sample (2007)**

	treated*			**non-treated****		
	N	mean/share	std.dev.	N	mean/share	std.dev.
total number of establishments		7,402			683,966	
establishment characteristics						
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	
young establishment	1,665	22.49		162,833	23.81	
settled establishment	5,737	77.51		521,133	76.19	
sector (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 revenue 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 data in this Table are from the first quarter of 2007. * treated firms are located in eligible regions; ** non-treated firms are located in non-eligible regions.
(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; authors' calculations.

ever, the map in Appendix Figure A.2 reveals that all districts in East Germany are eligible for investment grants.[13]

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

In addition to the general funding rules described above, the program allows some degree of variation in treatment intensity. We next examine some theoretical impact channels and the expected effects linked to these rules. Since the impact on the success of the program cannot be clearly predicted from theory, empirical research will shed some light. We use variations in treatment intensity as a guideline for our heterogeneity analysis and construct subsamples according to the differentiation.

Regarding the firms' *regional economic environment*, the program allows for higher maximum aid in structurally weaker regions. Neo-classical growth theory (Solow 1956, Swan 1956) argues that decreasing marginal factor productivity will yield higher returns to GRW funding in more disadvantaged regions; however, the endogenous growth theory (Romer 1986, Lucas 1988) suggests the opposite. Tto analyze the influence of disadvantages, we separate 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 program. 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, the preferential allocation of funds in regions endowed with specific location conditions is the 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 whether the employment effect of investment grants is influenced by agglomeration, we consider urban and rural regions in our analysis.

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

While GRW funding rules do not explicitly favor some economic sectors over others, the program has an implicit sectoral scope due to the interregional sales criterion that is automatically

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

fulfilled if an applying firm is classified in a sector that belongs to a 'whitelist'. Moreover, the distribution of investment grants reveals a clear concentration of funding in some sectors. Since the economic sector implies distinct production processes based on specific technology and equipment that require different types of employees, we presume that heterogeneous employment effects will be evident in different sectors. We broadly aggregate the eligible sectors in five sector groups based on the 2-digit level of the German Classification of Economic Activities system.[14] See Appendix Table A.3 for more details.

Maximum subsidy rates are provided for investment projects that may have a *special structural impact*, e.g., investments that strengthen regional innovative capacities or business start-ups. A firm's age represents some aspects 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). Futhermore, young firms have a higher probability of market exit as newcomers must adapt to rules, routines, and skills in a new economic environment (Fackler et al. 2013). To capture this potential source of heterogeneity, we divide the sample into young establishments (less than five years old) and mature establishments that are five years old and above.

An important precondition for innovative capacity is the composition of an establishment's workforce. Since the seminal work of Mincer (1962), workforce qualification level and work experience serve as standard proxies for a firm's human-capital endowment, or labor quality. Recent empirical studies confirm a positive relationship between labor quality and firm performance; e.g., Conlon et al. (2023), Galindo-Rueda and Haskel (2005), Morris et al. (2020). We investigate the influence of establishment's labor quality on the employment effect of investment grants. Variations in workforce composition are represented by different proportions of employee types in an establishment. We differentiate between high and low shares of at least

[14]The aggregation scheme references the IAB Establishment Panel's aggregation scheme and represents the best possible solution to the tradeoff 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.
For example, the sector group "manufacturing of products for private consumption" contains 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, where production processes require large machinery and are comparatively less labor-intensive. In the classification "machinery and equipment for industrial production", we summarize manufacturing sectors that produce electrical equipment, machinery, vehicles, metal products, and construction. This group represents the core of the GRW treatment in terms of the number of treated establishments and the subsidy amounts (see Appendix Table A.4 for more details). It includes strongly supported sectors such as those that manufacture fabricated metal products and machinery and equipment. The sector group "services and health care" summarizes all treated establishments in the service sectors and health services. It incorporates relatively labor-intensive sectors. Therefore, we expect comparatively large effects on the number of employees. We also find strongly subsidized sectors in this group, such as wholesale trade and accommodation. The "exploitation of natural resources" group contains sectors such as agriculture, forestry, mining, and basic supply, and represents sectors that are not central to the GRW program. We combine less supported economic sectors in terms of the number of treated establishments and the total amount of the subsidy in this group (see Appendix Table A.4 for more details).

medium-skilled employees, low-skilled employees, and young and old employees as proxies for experience.[15]

5 Identification strategy

For reliable estimation results, we must consider potential sources of bias. The selection bias due to differences in the relevant observable characteristics discussed above is a serious issue for our analysis, as shown in the sample description (Table 2). In addition, 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 following a treatment can affect the current effect (Jacobson et al. 1993), and changes of the economic situation may have an impact on the effectiveness of a program (Bergemann et al. 2009). This is of particular importance for our study as our observation period covers the period of the 2008 financial crisis and remarkable economic changes in subsequent years. Therefore, 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.

To consider the noted sources of bias in our analysis, we take up the idea of simultaneous control for selection bias resulting from observable and unobservable heterogeneity (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 that 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. Since the conditional parallel-trend assumption is not testable, we regard the firms' pre-treatment employment development as a proxy for development in the absence of treatment and employ placebo tests for different periods prior to the treatment to verify whether this assumption is fulfilled (see section 7).

[15] To define of a low share, we use the 30th percentile of the distribution of the respective variable among the treated establishments, while the 70th 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.

As emphasized 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), where overlap is required for each treated unit and each time period included in the analysis. We only consider establishments that fulfill the common-support condition in the estimation approach.

The *no-anticipation assumption* requires that the treatment must not have a causal influence on the outcome prior to its implementation. Otherwise the change 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 related to the 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 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 possibility that a treatment may have an impact on an outcome (current or future) even when it is completed. In our estimations, firms are considered to be treated from the start of the first treatment and all subsequent time periods.

In addition, our identification strategy requires *no uncontrolled carryover* and *no spillover effects*.[16] The potential outcome of an observed establishment neither depends on its own previous treatments (or they can be controlled for) nor on other establishments' current treatment status. We exclude all (treated and non-treated) establishments that received GRW investment subsidies in the years prior to the analyzed funding period to address potential carryover effects.[17]

The requirement of no spillover effects means in the context of our study that there must be no influence of the treatment of other firms on the individual treatment effect (in the sense of the *stable unit treatment value assumption* for matching). As described in section 2, the GRW investment subsidies are provided on application for a particular investment project, and all applications are subject to assessment by the funding authorities. Since the number of applicants is much lower than that of eligible establishments, we regard the assumption of no spillover effects to be fulfilled.

[16] This assumption replaces the *random sampling assumption* of the parametric DID approach.

[17] Germany offers a wide range of potential funding alternatives for firms; however, these alternative funding options do not have an explicit spatial scope and can be accessed by establishments all over Germany. Additional place-based policies (e.g., those implemented by states or municipalities) are explicitly forbidden as they would violate the EU rules.

5.2 The estimation approach

The core of our estimation strategy is a semiparametric DID approach that considers time-varying treatments and potential time dependence of the 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), or partial average treatment effects in each cohort at each time. A group, or cohort, is defined according to the first treatment start date, and the time refers to the number of periods following 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 is treated for the first time at time G. The average treatment effect in 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 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:

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

Heere, 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.[18]

The estimator weights the controls by a combination of regression adjustment and inverse probability weighting based on the propensity score (which is regarded as "doubly robust approach", see Callaway and Sant'Anna (2021)). Pre-treatment employment is found to be the most important aspect; therefore we include covariates representing the employment development prior to treatment.

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

The descriptive analysis (Table 2) indicates that the treated firms are a rather selective group. In addition, the estimation process described is extremely computing-time and memory-intensive for large datasets. Therefore, we implement a matching procedure to reduce the size of the dataset by focusing on those non-treated establishments that are actually comparable to the treated firms. This data preprocessing also adjusts the data in that it reduces potential inefficiency and model dependency in subsequent estimations (Ho et al. 2007).

5.3 Data preprocessing

To identify the best comparable potential controls for treated establishments, we use a two-step ties matching at the cohort level. In the first step, an exact matching of the most divergent 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".[19] Exact matching regarding the 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 characterize the establishments' employee structures and economic environments. We include the proportion of high-skilled employees, of low-skilled employees and young employees to characterize the establishments' employee structure. Since the regional economic environments of treated on non-tretaed firms differ '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 regional development prior to the treatment start and match establishments in regions that exhibit similar development in the pre-treatment years.[20] We use three regional characteristics to capture the regional development of the compared establishments: GDP per capita and tax revenue per 1,000 inhabitants as broad proxies for regional prosperity, and gross wages and salaries per employee as a proxy for the regional economy's competitiveness.

[19] Classes are the most detailed categories of the NACE classification system and correspond to a very detailed description of the production processes 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". Eurostat (2008) p.21.

[20] 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 consider the mean development over the last two years as "optimal" to resolve this conflict. In section 7, we present the results of estimations considering longer pre-treatment developments as robustness tests.

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 2007-2013 GRW funding period. The control group includes only non-treated establishments located in non-eligible regions to exclude unobservable selection.

As noted in the introduction, we differentiate between the average treatment effect and heterogeneous effects in selected subsamples. The following tables present the effects in absolute and normalized figures. For normalization, we relate the estimated effect to the amount of the subsidies awarded to firms in the (sub)sample(s). The resulting employment per €100,000 subsidy allows us to compare the results across different subsamples.[21] 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 randomized). Therefore, it is not possible to conduct ceteris paribus comparisons between the subsamples in the sense of interpreting the coefficients of parametric models, for example. This means that we cannot identify the influence of a single covariate on the employment effect. Nevertheless, we can draw reliable conclusions concerning the extent to which the normalized effects differ in the subsamples.[22] This means, that the subsamples in our analysis must be interpreted independently from one another. The only reference for comparing the effect size is the entire sample. As a result, we can identify subsamples where the GRW investment grants were particularly successful or worked less well.

The results for the full sample reveal a positive effect of investment grants on employment development (Table 3). On average, treated establishments grew by 7.3 employees more than the controls. Considering the total number of subsidized firms (8,079) we observe a total employment effect of 58,967 additional jobs. Relating this to the amount of the subsidies paid (€5.21 billion), the normalized effect is 1.1 employees per €100,000 subsidy in current prices. In other words, an additional job is subsidized by approximately €88,300 (Appendix Table A.4). This result aligns with the empirical evidence for positive employment effects of investment grants e.g., in Italy (Bernini and Pellegrini 2011, Cerqua and Pellegrini 2014), the UK (Criscuolo et al. 2019), Belgium (Decramer and Vanormelingen 2016), and Northern

[21] As additional information, we present the total amount of subsidies in the subsamples and the costs per additionally created job in Appendix Table A.4.

[22] We observe diverging trends in employment development prior to the treatment for some of the subsamples (see Table 7 in section 7). Since they do not fulfil the identifying assumption of the estimation approach, we cannot interpret the results exclusively as a consequence of the funding. The corresponding entries in Tables 3 and 4 are marked in gray.

Ireland (Harris and Trainor 2007).[23] Also Grunau et al. (2024) report (slightly higher) positive employment effects for Germany.

6.1 Effect heterogeneity based on economic environment and firm characteristics

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

The findings reveal that treated establishments in strongly disadvantaged regions grew significantly stronger than the controls with an ATT of 5.9 workers. The normalized effect amounts to 0.7 employees per €100,000 subsidy, which is lower than the average effect of 1.1.[24] The sample of treated establishments in most disadvantaged regions exclusively includes firms in East Germany reflecting the highest aid intensities possible in the GRW program. Given these beneficial program incentives, the highest share of GRW funds in absolute terms is allocated to this German macroregion. Our findings suggest that the program has achieved its goals in terms of safeguarding existing jobs and creating new jobs, particularly in East Germany, which is mostly targeted by this policy. Our result is consistent with the findings of Grunau et al. (2024) and Siegloch et al. (2024) who present positive effects at the micro and the regional level, respectively.[25] 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 that of control establishments, despite less favourable economic environmental conditions. The effect on establishments in rural regions is significant and positive. At 0.6 employees per €100,000 subsidy, the strength of the effect in rural areas is below the average effect of 1.1. Initiating employment growth in rural areas seems to be more expensive than the average cost, which is also confirmed by the substantially higher amount of subsidy per additional job (€159,200; see Appendix Table A.4). Although not explicitly announced in the GRW targets, balancing out spatial disparities between urban and rural areas is also on Germany's policy agenda. Our

[23] The costs per job in our analysis are higher than those presented in previous empirical studies. Criscuolo et al. (2019) report costs per job of €20,000 for the UK, Cerqua and Pellegrini (2014) report costs between €46,000 and €77,500 for Italy, and Grunau et al. (2024) present costs per job of about €25,000 for Germany. The extremly skewed distribution of the GRW funding in Germany (see Table 1) provides an explanation for this difference: the reported mean costs are strongly influenced by the sample drawn from the population of all subsidized firms, and the sample composition of Grunau et al. (2024) differs considerably from our sample.

[24] The subsidy per additionally created job in treated establishments in those regions is €134,200, which is substantially higher than the average (€88,350).

[25] Siegloch et al. (2024) calculate costs per job of €19,935. The remarkable difference to our study can also be explained by the different sample compositions of both studies.

Table 3: **Heterogeneity based on 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: 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 (Appendix Table A.3 in the appendix; [(2)]production of food, fabrics, other goods; [(3)]petroleum processing, manufacturing 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); authors' calculations.

findings suggest that the GRW program contributes to this policy goal in a positive manner. The results for the subsample of urban regions is not interpretable.

As noted previously, 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, finding significant positive employment effects for the sectors "chemicals and pharmaceutics", "equipment for industrial production" and "services and health care" (see Table 3).

The "equipment for industrial production" sector group includes the most strongly treated sectors, "manufacturing of fabricated metal products" and "manufacturing of machinery and equipment". The total amount of funding is with €2 billion larger than the average funding (see Appendix Table A.4). Nevertheless, the effect in absolute numbers is with about five jobs below the average. This is also true for the normalized effect with 0.8 employees per €100,000 compared with1.1.

Comparatively large employment effects may be expected for labor-intensive sectors, which is the case for the sector group "services and health care", in particular.[26] Surprisingly, the effect in absolute terms, with an ATT of three jobs, is rather small. However, the normalized

[26]In this group, we also find strongly subsidized sectors such as wholesale trade and accommodation.

effect is with 0.9 employees per €100,000 slightly below the average. In the rather capital-intensive sectors summarized in the "chemicals and pharmaceutics" group, the ATT and the normalized effect are with three jobs and 0.4 employees per €100,000, respectively, clearly below average. This result aligns with the findings of Grunau et al. (2024) for capital-intensive sectors. The effect is insignificant for the subsample "exploitation of natural resources", and not interpretable for the sector group "manufacturing of products for private consumption".

6.2 Effect heterogeneity based on the GRW special structural impacts rule

Regarding the criterion of *special structural impacts* in the GRW 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, the results reveal a positive effect of 1.2 employees per €100,000, which is above the average 1.1 job per €100,000. Obviously, the program is highly effective for the group of mature establishments. The result for the subsample of young establishments is not interpretable.

Table 4: **Heterogeneity based on GRW special structural impact rule**

	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); authors' calculations.

A second source of special structural impacts of firms may be attributable to human capital endowment. We construct subsamples based on employees' qualification and experience structure (see section 4). From theoretical considerations, we expect above average effects of the GRW program for establishments employing more highly qualified and more experienced employees. The estimation results only partially confirm our presumptions.

As is described in section 4, our subsamples represent the top and the bottom 30 percent of the distribution of the respective qualification and experience proxies. The findings confirm the expected positive employment effect at the top of the distribution (i.e., in firms with a high share of at least medium-skilled employees and those with a low share of low-skilled employees), at 1.5 and 1.4 employees per €100,000, respectively, which 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, 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 also positive at 1.3 in both subsamples, which is slightly above average.

As expected, employees' experience also influences the strength of the treatment effect. Among establishments with a low share of young workers, we find a positive effect on employment development; the normalized 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 insignificant, indicating that control firms create the same number of jobs as subsidized firms. Unfortunately, the results for the subsamples with respect to the share of experiences workers are not interpretable.

7 Quality and robustness tests

7.1 Reliability of full sample estimations

We next 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 is correlated with diverging developments between the treated and control firms over time. In other words, equal employment trends should be observed for treated and control group in the absence of a treatment. We use the establishments' pre-treatment employment development as a proxy for this development to determine whether differences in the employment trends in the treated and control groups are evident before treatment. Figure 1 presents the result of our placebo test for the five-year period (which corresponds with 20 quarters) prior to treatment. The figure reveals no significant differences between the employment development in treated and control establishments over the of five-year period prior to the treatment. Threfore, the parallel trend assumption can be considered as fulfilled.

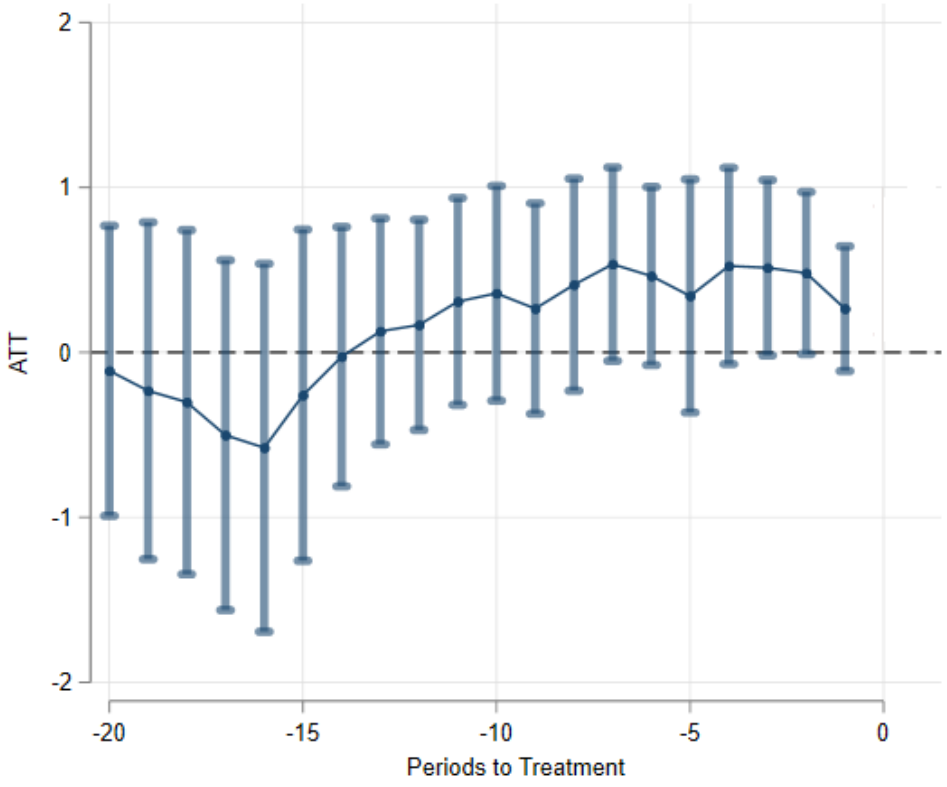

Figure 1: *Placebo test of conditional parallel trend assumption*

Note: The graph presents the estimated pre-treatment effect in the sample for 20 quarters prior to treatment. Sources: Employment History (IAB), GRW treatment data (BAFA), INKAR data (BBSR); authors' calculation and illustration.

The results presented in Table 5 also confirm this conclusion. The first panel of the table presents the results of pre-treatment placebo tests to validate the conditional parallel trend assumption for different time periods related to the treatment start. No significant treatment effect is observed in any of the analyzed periods in three or five years prior to treatment, and the whole pre-treatment period.

The second panel of Table 5 presents the results of short-term placebo tests for detecting anticipation effects (de Chaisemartin and D'Haultfœuille 2021). These period-to-period effects are also insignificant for the pre-treatment periods. Therefore, the assumption of no anticipation can be considered as fulfilled.

These test results also indicate that the potentially different economic environments of treated and control firms do not result in different trends in the employment development and do not distort the results. Nevertheless, we conduct various robustness tests including the verification of our sample definition.

Table 5: **Placebo tests for conditional parallel trends and 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); authors' calculations.

We first verify the choice of the control sample. To consider regional economic environments in an alternative way, we include only neighboring regions of the treated establishments as a possible location of potential controls differentiating three cases: *First*, neighboring regions can be eligible and non-eligible districts, which will change the composition of the control group compared to the initial analysis, because the pool of potential controls is limited to establishments in the neighborhood, but not only in non-eligible districts. The results are subject to both potential sources of distortion, 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, which is presumably due to our 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, it is smaller than in the initial sample.

Second, the neighboring regions must be exclusively eligible districts. This requirement reduces the pool of potential controls for treated establishments at the border between eligible and non-eligible districts in particular, 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) the sample is reduced. 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. From a theoretical point of view, this is the best alternative definition since it avoids potential selection bias due to unobservable characteristics, and the similarity of the economic environment should be rather high. Unfortunately, the regional distribution of eligible districts (see Appendix Figure A.2) does not allow the identification of non-eligible neighboring regions, particularly for many of the most strongly treated establishments in East Germany. The consequence is a considerable

loss of observations, and the estimated effect is only based on 3,000 observations. The effect amounts to 0.3 additional jobs per €100,000, only one-third of the effect based on the initial sample.

We also examine how the results change if we require potential controls to be located in the same region as the treated firm. This approach defines the pool of potential controls ensuring that the economic environmental conditions are as similar as possible, and accepts potential selection bias distorting the results. In this case, the majority of the treated establishments is lost for the analysis due to the lack of comparable establishments, and the results are based on only 229 observations, with virtually zero effect.

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

We next examine the assumption of no uncontrolled carryover effects. Because Germany has a wide range of alternative funding options, while there is no information available on similar treatments for the establishments analyzed, we cannot completely exclude the influence of potential treatments prior to the observed GRW funding period. However, we can control for the influence of previous GRW investment grants. Based on the initial sample, in the first step, we exclude all (treated and non-treated) establishments that received GRW investment subsidies in the years directly before start of the analyzed funding period (in 2005 or 2006). In the second step, all establishments that received GRW treatment in the entire observed pre-funding period are excluded to verify carryover effects. The second panel of Table 6 shows that the number of firms is considerably reduced by approximately 2,000 and 6,000 establishments for the two steps, 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 do not influence the effect of current GRW investment subsidies on the employment development.

Also the duration of the observed pre-treatment trends of regional characteristics in the matching procedure may influence the estimation results. The third panel of Table 6 presents the results of estimations applying longer pre-treatment developments, revealing that the employment effect remains positive in the three analyzed cases. The number of observations is slightly reduced, and the employment effect becomes somewhat larger (1.8 to 2.4 employees per €100,000).

The fourth to sixths panels of Table 6 present the results of variations in the estimation approach and the data preprocessing. The third panel contains different covariate combinations

Table 6: **Robustness: Variation in the sample composition and the estimation approach**

	number	estimated effect		
	unique obs.	ATT	(std.err.)	normalized[1]
initial model	16,379	7.30***	(1.82)	1.13
sample: choice of potential control establishments from ...				
neighboring regions, eligible and non-eligible	11,928	4.89***	(0.66)	0.91
only eligible neighboring regions	11,188	3.70***	(0.60)	0.65
only non-eligible neighboring regions	2,908	7.69***	(1.12)	0.28
the same region as the treated ones	229	7.00*	(3.60)	0.02
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
estimation: pre-treatment development of regional variables ...				
over 3 years	16,327	6.40***	(1.52)	2.01
over 4 years	16,316	7.60***	(1.96)	2.38
over 5 years	16,300	5.61***	(1.54)	1.76
estimation: 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
estimation: 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: The inital model represents the estimations of 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 per capita; [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 per capita.

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

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 examining combinations of the pre-treatment outcome with firm characteristics and regional development, respectively. The fourth panel presents our results regarding the influence of variations in the data preprocessing, varying the covariates included in the second step of the approach described in section 5.3. 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. The fifth panel presents the results of variations in the development of regional characteristics. Alternatively to the observation of regional development over the last two years before treatment, we estimate the effects considering the development over three, four and five years prior to the GRW funding. Overall, the variations in the estimation yield very similar results. The effect remain positive and ranges in most variations between 1.2 and 0.8 employees per €100,000. An interesting exception emerges considering only firm characteristics in the preprocessing, with an insignificant effect, confirming the importance of considering the development of the economic environment when choosing potential controls. In addition, considering levels of pre-treatment outcome instead of the pre-treatment development in the weighting process (unexpectedly) influences the estimation, reducing the treatment effect is reduced by approximately half. Another potential impact

In summary, based on the results of the quality and robustness tests for the sample, we conclude that our results presented in chapter 6 are reliable, not distorted by different trends or influenced by anticipation or carryover effects. They are also robust to the choice of different potential control samples and the variation of the estimation process and the data preprocessing.

7.2 Reliability of subsample estimations

We also examine the estimation results for the subsamples. In this subsection, we present the results of the tests for the conditional parallel trend assumption and the assumption of no anticipation. As for the full sample, we consider the pre-treatment employment trends as proxies for the employment development in the absence of treatment. The following Table 7 presents the results of placebo tests for the five-year period prior to treatment.

Columns (3)-(5) provide the results of the placebo tests to verify the conditional parallel trend assumption for the subsamles. The assumption is fulfilled for most of the analyzed subsamples, with the exception of subsamples representing different firm size categories. The placebo effects significantly differ from zero for micro, small and medium-sized firms. We do not include the corresponding results in the Table 3, since they are not interpretable; therefore, no information about the influence of size on the treatment effect is available. In addition, two subsamples representing different employee experience levels exhibit significant placebo effects, which indicates a violation of the parallel trend assumption. Therefore, we cannot interpret the corresponding results presented in section 6 as causal effect, and the results Table 4 are marked in gray. The same is true for the subsamples of firms in less strong lagging and urban regions

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-year period prior to treatment; [2]estimated period-to-period treatment effects for the five-year period prior to treatment.

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

as well as firms in the sector group "'roducts for private consumption" and young firms (see Tables 3 and 4). In columns (7)-(8), we present the results of the period-to-period placebo tests to verify the assumption of no anticipation. The results reveal a violation of the assumption in three cases; namely in two size categories and the subsample of firms with a high share of experienced employees.

In addition, a graphical illustration of the pre-treatment placebo tests regarding the conditional parallel trend assumption is presented in Appendix Figures A.3 and A.4, and Appendix Figures A.5 and A.6 provide placebo tests for the assumption of no anticipation.

Overall, the results demonstrate that it is not possible to create subsamples that fulfil the identification assumption of the estimation approach in every case. The main rationale for this is the selectivity of treated firms. However, the results also reveal that the estimation strategy is appropriate for producing reliable results for the majority of subsamples.

8 Conclusions

This study analyzes the establishment-level employment effects of GRW investment grants issued in the funding period from 2007 to 2013 in Germany. The funding regime typically allows for flexible application times and varying start dates for investment projects. Therefore, we apply the staggered DID approach of Callaway and Sant'Anna (2021) that explicitly models variations in treatment timing. Since the program is highly selective, we combine this approach with a ties matching at the cohort level.

Beyond analyzing the average program effect, this study places specific emphasis on effect heterogeneity guided by the program's discrimination rules. The political intention behind the variation of program generosity is not always clear cut. It can address various goals like compensation for disadvantages and picking the winners. Therefore, we translate program rules into different subsamples reflecting specific firm characteristics and regions' economic environment. Due to the detailed information in the dataset, we can normalize the estimated absolute effects to compare the results for the subsamples.

Overall, GRW investment grants are effective. The results reveal a stronger growth in the treated establishments amounting to 7.3 employees. Relating this effect to the amount of the subsidy awarded, the normalized employment effect is 1.1 employees per €100,000. This effect remains robust against variations in the control group composition and the estimation approach.

Concerning the subsamples representing different economic sectors, in addition to different magnitudes of the employment effect, the results reveal substantial variation in the costs of additional employment across industries.
Regarding the influence of firms' 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 qualification. However, the estimated effects are also positive and (although slightly smaller) above the average at the bottom of the distribution. Additional employment is more costly in this case than for firms that are well-endowed with human capital. It is doubtful whether this is consistent with the aim of the GRW program, i.e., subsidizing investments that contribute to the regional economic development.

In addition, we demonstrate that the program is effective in the mostly targeted regions, applying to firms located in most disadvantaged regions, i.e., East Germany, as well as establish-

ments located in rural areas. Although the effect is below the average in these two subsamples (rendering additional employment more costly), the results indicate that allocating the majority of GRW subsidies in these areas is money well spent and contributes to reducing spatial disparities.

Although we contribute to empirically answering the rarely addressed question about the conditions under which investment grants work best, our study raises further research questions. In particular, it would be promising to analyze the quality of the subsidized employment in more detail. Another interesting consideration that has not yet been aspect addressed concerns treated establishments' recruitment channels. Do they hire staff from among the unemployed or does substantial job mobility occur between establishments?

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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: Authors' illustration on the basis of Eckey (2008).

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

Type of the program	Non-repayable grants for investment projects (co-funding)
Set-up of the program	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 program	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: Authors' compilation and illustration.

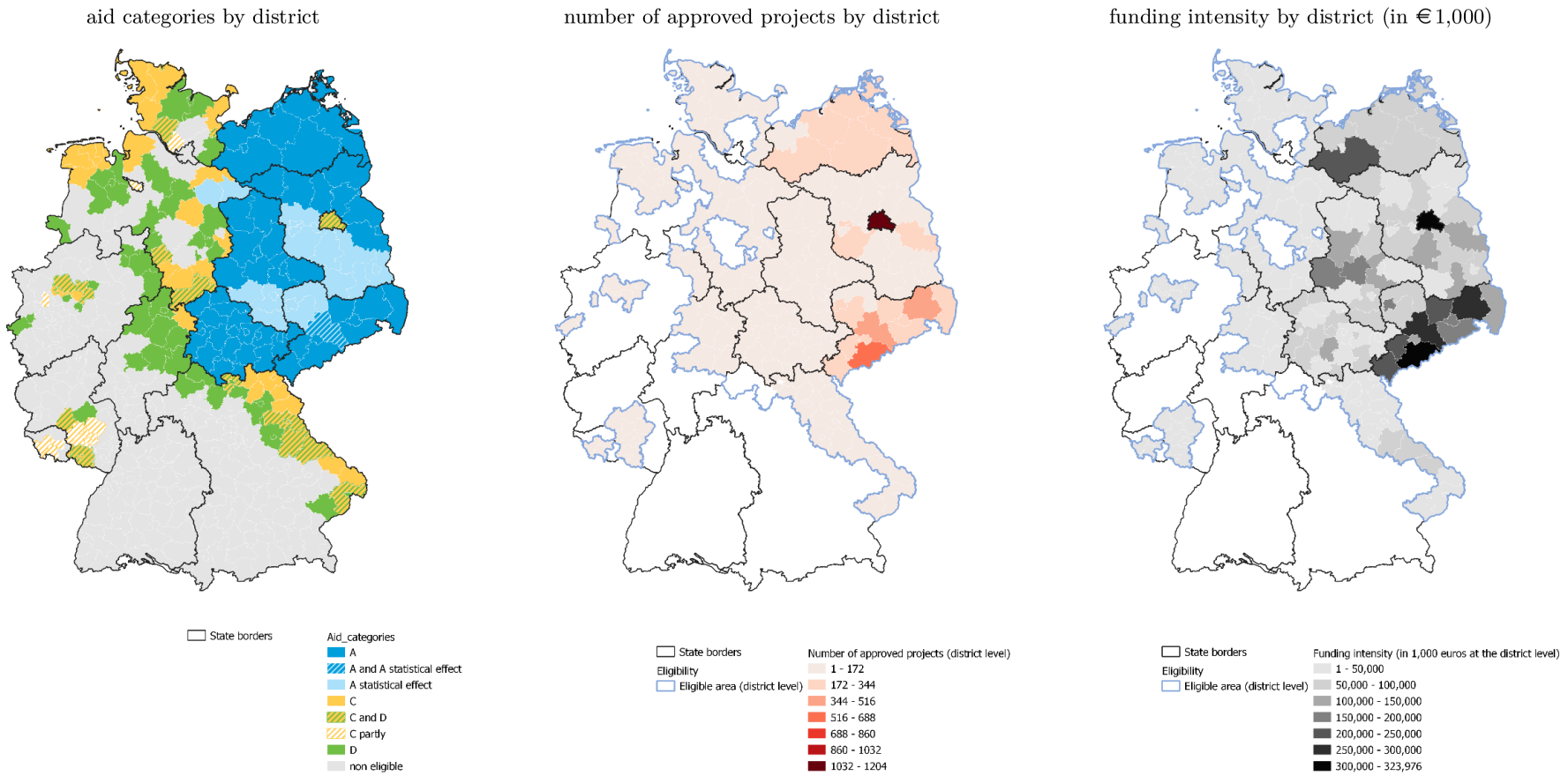

Figure A.2: *The Joint Task 'Improvement of Regional Economic Structure' (GRW) 2007-2013 – Regional distribution of eligibility, number of projects and funding intensity*

Note: Formal eligibility is determined at the municipality level. We aggregate this information to the district level. Districts with varying aid categories are shown as shaded areas.

Source: GRW treatment data of BAFA; authors' calculations and illustration.

Table A.2: **GRW funding rules for the analyzed program 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 program or overhauling the entire process of production ∘ purchasing an inoperative establishment *Sectoral scope of the program* ∘ 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: Authors' 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: Authors' 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 Appendix 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, manufacturing 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; authors' calculations.

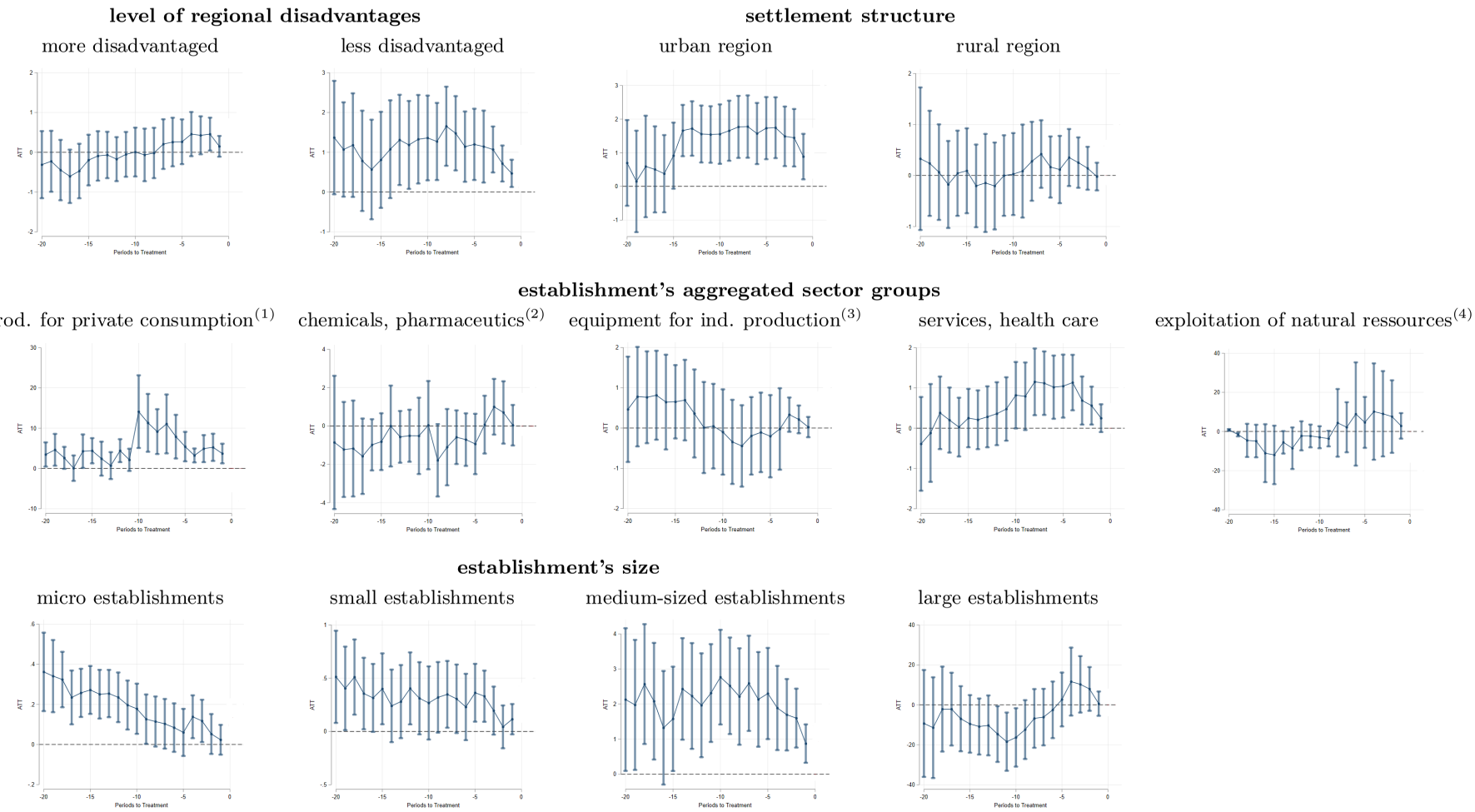

Figure A.3: *Conditional parallel trend assumption in the subsamples (economic environment and establishment's characteristics)*

Note: The graph gives the estimated pre-treatment effects in the subsamples for the five-years-period prior to treatment.
(1)production of food, fabrics, other goods; (2)petroleum processing, manufacturing 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); authors' calculations and illustration.

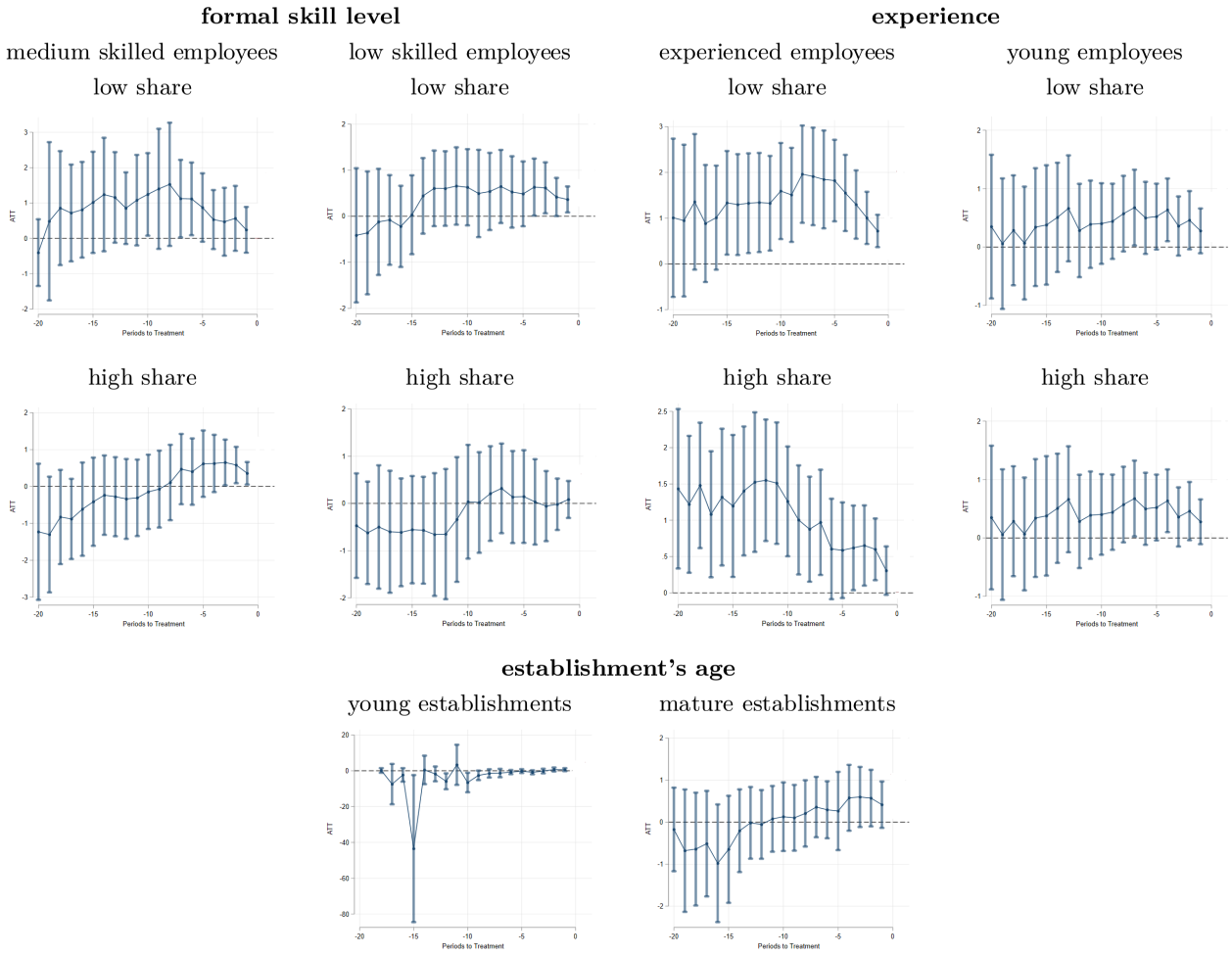

Figure A.4: *Conditional parallel trend assumption in the subsamples (human capital endowment and establishment's age)*

Note: The graph gives the estimated pre-treatment effects in the subsamples for the five-years-period prior to treatment.

Sources: Employment History (IAB), GRW treatment data (BAFA), INKAR data (BBSR); authors' calculations and illustration.

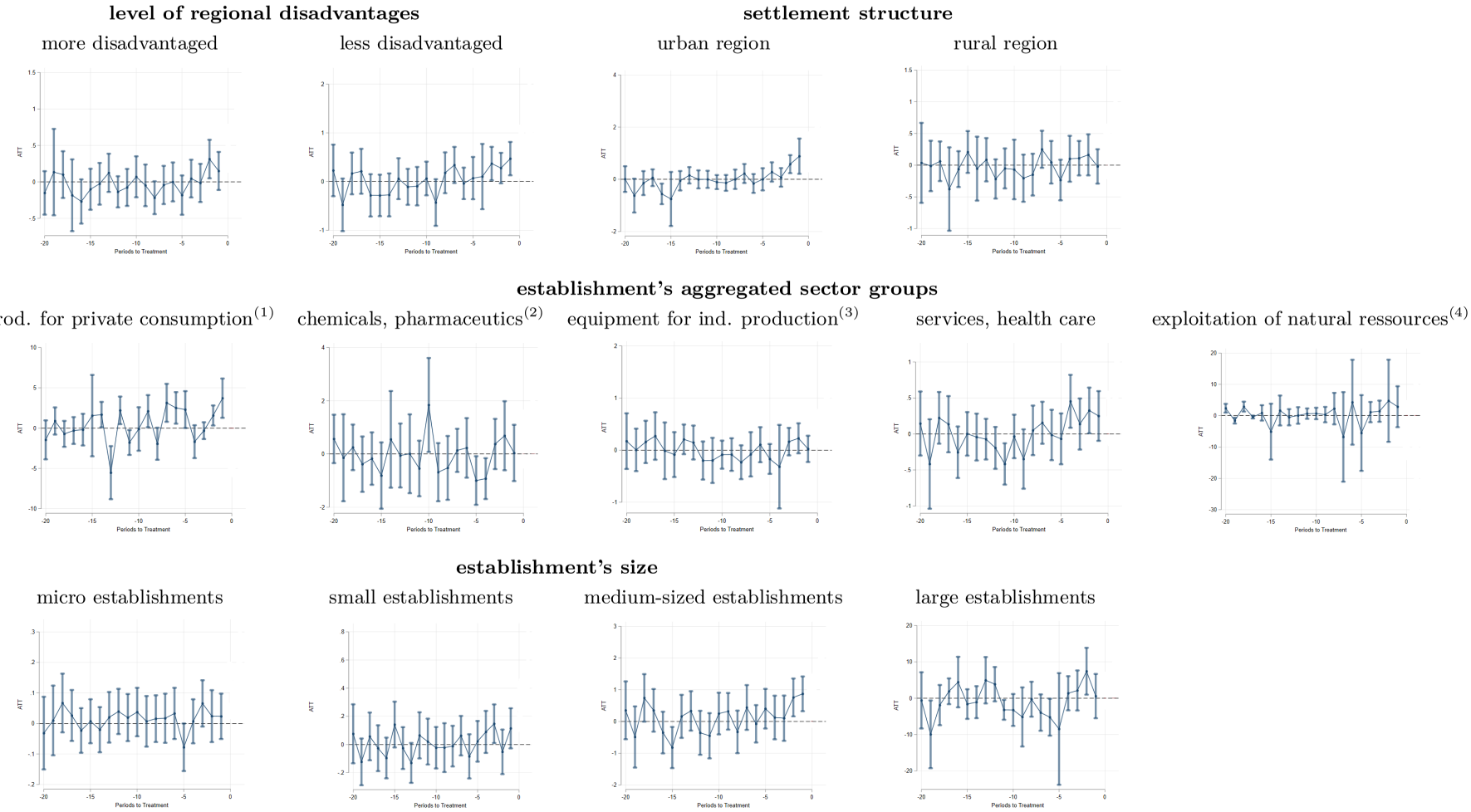

Figure A.5: *No anticipation assumption in the subsamples (economic environment and establishment's characteristics)*

The graph gives the estimated period-to-period treatment effects in the subsamples for the five-years-period prior to treatment.
(1)production of food, fabrics, other goods; (2)petroleum processing, manufacturing 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); authors' calculations and illustration.

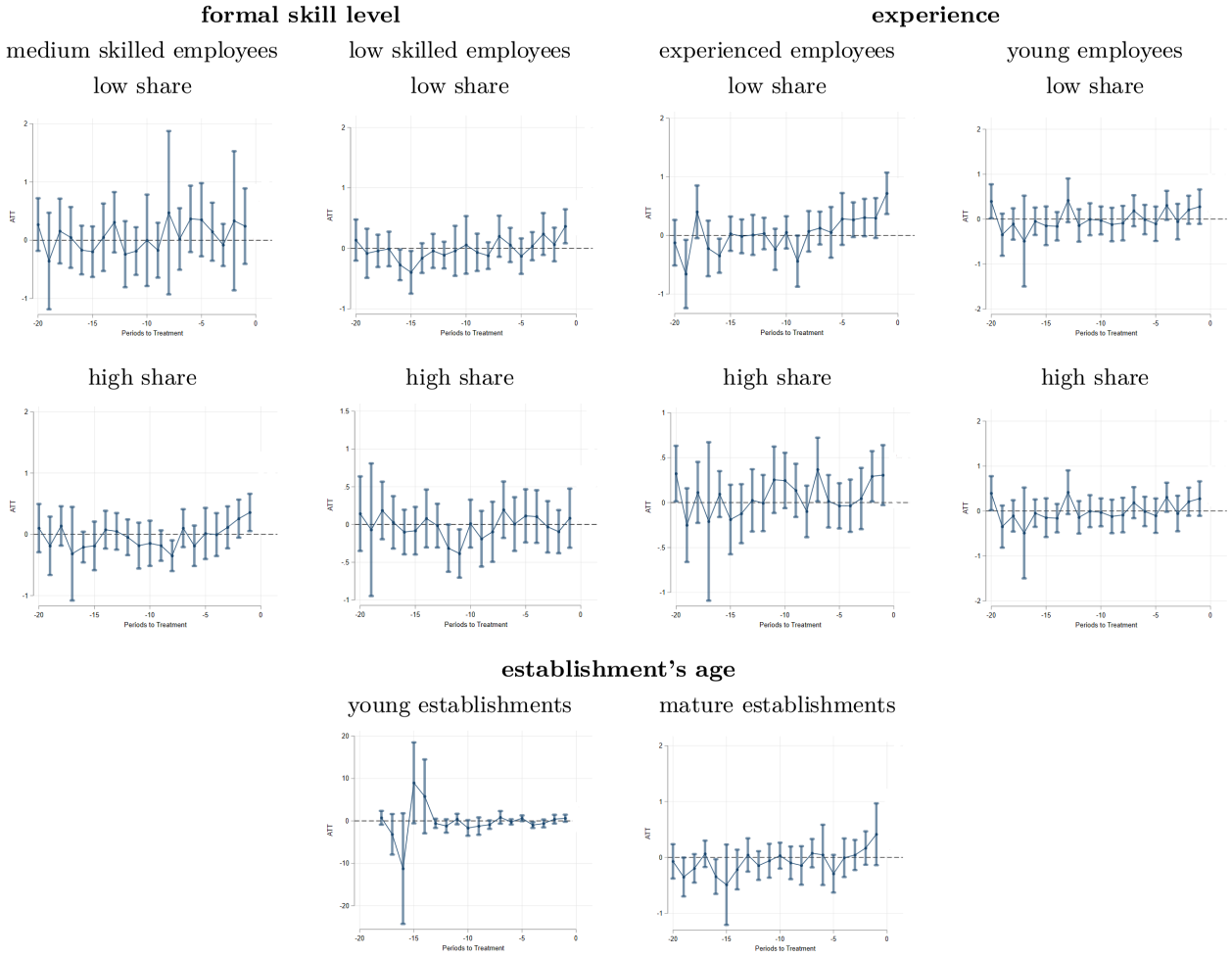

Figure A.6: *No anticipation assumption in the subsamples (human capital endowment and establishment's age)*

Note: The graph gives the estimated period-to-period treatment effects in the subsamples for the five-years-period prior to treatment.

Sources: Employment History (IAB), GRW treatment data (BAFA), INKAR data (BBSR); authors' calculations and illustration.

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