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**Working Paper**

## Investment grants: Curse or blessing for employment?

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## Investment Grants: Curse or Blessing for Employment?

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# Investment Grants: Curse or Blessing for Employment?\*

## Abstract

In this study, establishment-level employment effects of investment grants in Germany are estimated. In addition to the quantitative effects, I provide empirical evidence of funding effects on different aspects of employment quality (earnings, qualifications, and job security) for the period 2004 to 2020. The database combines project-level treatment data, establishment-level information on firm characteristics and employee structure, and regional information at the district-level. For the estimations, I combine the difference-in-differences approach of Callaway and Sant'Anna (2021) with ties matching at the cohort level. The estimations yield positive effects on the number of employees, but point to contradicting effects of investment grants on different aspects of employment quality.

*Keywords: causal inference, employment quality, place-based policy, staggered treatment adoption*

*JEL classification: C14, D04, D61, H20*

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# 1 Introduction

GRW investment grants are the key instrument of German place-based policy.<sup>1</sup> Since the programme was launched in 1969, 81 billion€ have been spent to subsidise over 155,000 investment projects with a volume of approximately 388 billion€ (BMWK 2024). However, place-based policy schemes are common globally. In the European Union, for example, a considerable share of the overall budget is allocated to such policy schemes: 942 billion€ in the analysed funding periods (European Investment Bank 2024). In addition, almost all member states offer national and regional investment subsidy programmes to foster the economic development in structurally weak regions (Criscuolo et al. 2019). The advantages and drawbacks are continually discussed in the empirical literature,<sup>2</sup> but are also the subject of controversial debates among economists, politicians and the public.<sup>3</sup> Such debates highlight the demand for credible empirical evidence of the effects on a variety of outcomes.

Recent studies have shown positive influences of investment grants on the key figures of regional economic development, such as private-sector investments, employment, and productivity in disadvantaged regions (Brachert et al. 2019, de Castris and Pellegrini 2012, Criscuolo et al. 2019, Eberle et al. 2019, Sieglösch et al. 2025, Wardenburg and Brenner 2019). Empirical studies at the establishment level have drawn a more differentiated picture. In summary, the empirical results suggest that investment grants positively influence overall firm-level investments, turnover, output, and survival (see e. g. Bernini and Pellegrini 2011, Brachert et al. 2018, Cerqua and Pellegrini 2014, Criscuolo et al. 2019, Decramer and Vanormelingen 2016, Harris and Trainor 2007, Pellegrini and Muccigrosso 2017). The effects on total factor productivity and location choice are rather negative or negligible (see Alexandre et al. 2024, Bernini and Pellegrini 2011, Bergström 2000, Devereux et al. 2007, Moffat 2014). Empirical literature also provides (rather short- and midterm) evidence for a positive effect of investment grants on the number of employees (Bernini and Pellegrini 2011, Brachert et al. 2018, Cerqua and Pellegrini 2014, Criscuolo et al. 2019, Decramer and Vanormelingen 2016) and a positive or insignificant effect on labour productivity (Alexandre et al. 2024, Brachert et al. 2018, Muraközy and Telegdy 2023).

However, beyond employment rates and productivity measures, the nature and quality of the provided jobs can provide insights into individual wealth and potential for the future development of establishments and regions. My contribution to the empirical literature is a comprehensive joint analysis of the influence of investment grants on employment quantity and different aspects of employment quality described below for 3 consecutive funding periods in the

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<sup>1</sup>The abbreviation GRW refers to the German title for the programme, 'Gemeinschaftsaufgabe Verbesserung der Regionalen Wirtschaftsstruktur'.

<sup>2</sup>See e.g. Barca et al. (2012) and Neumark and Simpson (2015) for systematic overviews of the main arguments related to this type of policy intervention, and Südekum (2025) for a current discussion.

<sup>3</sup>One of the most recent examples in Germany is the set of discussions around massive subsidies to attract foreign business investments in semiconductor factories e. g. in Saxony-Anhalt (Intel) and Saxony (TSMC) (see e. g. Eddy 2023).

time 2004 to 2020. In addition to average effects, I observe the development of the treatment effects over a period of approximately 15 years after the treatment start. This period enables me to distinguish short- and long-term effects and to understand whether the effects of GRW investment grants are persistent.

In addition to being a proxy for labour productivity, *earnings* are one of the primary and most obvious aspects of employment quality. In Germany, labour income comprises almost two-thirds of a household's disposable income (Destatis 2024), playing a pivotal role for the personal standard of living for the majority of individuals. Thus, wages and salaries not only reflect the monetary compensation for individual labour, but are also fundamental determinants of household economic welfare, influencing various aspects of daily life, financial security and the capacity to invest in personal and professional development.

The type of professional activity and the *skill level required for a job task* can serve as indicators for employment quality in terms of individual job satisfaction (Millán et al. 2013). Positions that imply specialised knowledge, expertise and creativity often offer individuals a sense of autonomy and self-determination, leading to greater job satisfaction and work that is more productive. On the other hand, tasks that are monotonous, repetitive or do not fully utilise employees' potential can lead to dissatisfaction. At the establishment level, the workforce composition is an important precondition, e. g., for firm performance and innovative capacity. Recent empirical studies confirm the positive relationship between labour quality and firm performance (see e. g. Conlon et al. 2023, Galindo-Rueda and Haskel 2005, Morris et al. 2020).

Another aspect of employment quality is *job security*. One of the key indicators of job security is the type of employment contract. For example, permanent contracts are positively correlated to job satisfaction (Clarke and Postel-Vianey 2008, Millán et al. 2013). They provide employees with a sense of predictability and financial security, reducing anxiety about potential job loss. Moreover, permanent contracts offer access to training programs and opportunities for career development (Albert et al. 2005, Booth et al. 2002). However, the (particularly neoclassical) literature suggests a trade-off between individual job security and firms' labour market flexibility (Hamermesh 1993, Hogan and Ragan 1995).

During the observation period of 2000-2020, remarkable changes in economic conditions, unexpected social events, and modifications in the funding rules occurred. Since I presume that the (economic) environment influences the employment effects of investment grants, and thus, the treatment effect changes over time, I apply the difference-in-differences (DID) approach of Callaway and Sant'Anna (2021), which explicitly controls for variation in treatment timing. I combine the DID with a ties matching procedure at the cohort level to focus on the best comparable potential controls for the treated establishments, and to align the most diverging relevant characteristics in the sample.

The analysis yields contradictory results. The effect of GRW investment grants on the number of employees is positive and persistent, in particular, the mid-term results provide no hints of

windfall profits. Additionally, the treatment effect on job security is positive, although I find hints of a short-term anticipation of future funding. On the other hand, the labour income in treated firms increase less than in the control firms. Also this effect is persistent. Furthermore, the effect on employees' qualification levels is rather negligible.

The remainder of the paper is organised as follows. The next section describes the legal framework for investment grants in Germany. Section 3 provides an overview of the data sources and the analysed sample. Section 4 introduces the estimation approach, the estimation results for employment quantity and quality are presented in Section 5. Section 6 provides quality and robustness checks, and Section 7 concludes.

## 2 Institutional framework

The Joint Federal Task for the Improvement of Regional Economic Structures (GRW) is the main instrument of German place-based policy. It is jointly coordinated by the federal government and the governments of the federal states. The main goal is to reduce spatial disparities across Germany, particularly in terms of (un)employment and income. The programme provides investment grants to less developed regions.<sup>4</sup> Since investment grants distort competition in the EU Common Market, the programme rules must be approved by the European Commission in advance.

A key feature of place-based policies is spatially limited programme access. Regional eligibility relies on a structural weakness score consisting of several single indicators describing the labour market situation and the infrastructure quality<sup>5</sup> at the level of labour market regions. All districts in a labour market region are assigned the same eligibility status; exceptions are possible, and are fixed in the respective GRW coordination framework. In the analysed funding periods, mainly regions in East Germany, regions bordering the Czech Republic, and some regions in the north are eligible (see Figure A.2).

Moreover, the GRW programme has an implicit sectoral scope: Applying firms must satisfactorily prove supra-regional sales, namely, sales entailing more than 50 km from the place of production.<sup>6</sup> For simplicity, the funding rules include a whitelist that announces all industries expected to automatically fulfil this criterion. The whitelist predominantly includes manufac-

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<sup>4</sup>The programme provides investment grants for establishments and municipalities in disadvantaged regions. In my analysis, I focus on investment grants for establishments.

<sup>5</sup>Figure A.1 in the appendix provides a detailed description of the score components and calculation in the analysed funding periods.

<sup>6</sup>Throughout this study, I use the terms 'establishment' and 'firm' synonymously. The same is true for 'treated' and 'subsidised'.

turing industries.<sup>7</sup> Establishments operating in the service sector are generally eligible if they meet the aforementioned criteria.<sup>8</sup>

The application process follows a standardised procedure, which is managed by the responsible institution of the 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. The applicant may start the project at its own risk while the application is being processed by the funding authority.

The subsidised firm is subject to a detailed monitoring process during the entire treatment and investment process. Once the project is finished the firm must prove that it has created or safeguarded the planned number of jobs and must maintain the jobs over a commitment period of 5 years.

Although these general rules are (more or less) constant over time, the observation period of 2000-2020 experienced some changes in the funding rules. Most importantly, before 2007, the structural weakness score was calculated separately for East and West Germany to cover specific economic conditions, and East German regions received comparatively large amounts of funding. The introduction of a common calculation scheme for the scores resulted in a shift in the eligibility of some regions (see Figure A.2). Moreover, reductions in the maximum aid intensity (see Table A.2) and the overall amount of GRW funding changed the conditions for GRW applicants over time.

### 3 Data and sample description

The database combines information from multiple sources. The treatment information is obtained from the Federal Office for Economic Affairs and Export Control (BAFA). Employment and establishment characteristics are provided by the Institute for Employment Research (IAB) of the Federal Employment Agency, whereas 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 for the analysis

The BAFA treatment database consists of the reports of the federal governments responsible for implementing the GRW investment grants. The data contain project-level information,

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<sup>7</sup>Meanwhile, the EU's legal framework contains a blacklist with industries that are excluded from this type of state aid. This exclusion 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.

<sup>8</sup>A more detailed description of the programme's legal framework and the funding rules are provided in Tables A.1 and A.2 in the Appendix.



e. g. the start and end of the subsidised project, and the investment location. In addition, monetary information is available. For the analysis, I consider projects that were subsidised according to the coordination frameworks for GRW funding from 2004 to 2020. This period includes three GRW funding periods, i.e., 2000-2006, 2007-2013, and 2014-2020.<sup>9</sup>

Overall, I observe approximately 20,000 projects of 15,000 treated establishments in the GRW treatment data; see Table 1. An average investment project amounts to 2.5 million €, but the variation is enormous. Overall, total investments of 51 billion € are subsidised. The funded projects last on average two years, and the average subsidy rate is approximately one third of the eligible costs and almost one fourth of the investment costs. The funding costs amount to 11.7 billion €.

Table 1: **Key facts of GRW funding in the period 2004-2020**

Number of projects	20,101
Number of establishments	15,151
Total amount of investments ( <i>million €</i> )	50,948.90
Mean investment costs ( <i>thousand €</i> )	2,534.65
Variation of investment costs ( <i>thousand €</i> )	
min.	2.52
max.	711,053.75
Total amount of funds ( <i>million €</i> )	11,687.18
Mean treatment intensity nominal <sup>(1)</sup> ( <i>percent</i> )	33.01
Mean treatment intensity real <sup>(2)</sup> ( <i>percent</i> )	22.94
Mean treatment duration ( <i>months</i> )	24

Notes: <sup>(1)</sup> as percentage of the eligible costs; <sup>(2)</sup> as percentage of the investment costs.

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 nontreated establishments, I use the employment history data provided by the IAB for 2000-2020, aggregated at the establishment level.<sup>10</sup>

The IAB data provide information on the establishments' foundation date, location, and economic sector. Using the foundation date, I define two categories of establishment age: following e. g. Decker et al. (2020), I regard an establishment whose foundation year falls within the previous five years as a young establishment; mature establishments are five years old or more. 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.

<sup>9</sup>Table A.2 provides an overview of the coordination frameworks valid during this period.

<sup>10</sup>The IAB data consist of detailed information on all establishments with at least one employee subject to the German social insurance system.

In addition, the data contain employment information: an establishment’s number of employees, the employee structure, wages and salaries, and the type of employment contract. I summarise the size information following the EU definition: micro establishments are those with less than 10 employees, small establishments have 10-49 employees, medium-sized establishments have 50-249 employees, and large establishments have 250 or more employees. The firm’s human capital endowment is characterised via information on the employee’s vocational qualifications.<sup>11</sup> The share of high-skilled employees is defined as the proportion of employees with a university or university of applied sciences degree. Accordingly, the share of at least medium-skilled employees is the proportion of employees with vocational qualifications or higher formal degrees, and the share of low-skilled employees is 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.

The data enable me to create proxies for the analysed aspects of employment quality. I use the information on the median salary of full-time employees as a proxy for *labour income*. The shares of high and low-skilled employees are proxies for the *skill level required for a job task*, and the share of permanent full-time contracts is used as a proxy for *job security*. In addition, the number of employees is used to estimate the quantitative employment effect.

The database is enriched by district-level regional information from the INKAR database of the BBSR. I include indicators for regional labour market flexibility (unemployment rate), productivity (GDP per employee) and wealth (GDP per capita, and fiscal power). Additionally, I consider the BBSR definition of the districts’ settlement structure to capture different agglomeration levels.<sup>12</sup> I summarise the information in two categories: urban regions (which consist of cities and urban districts) and rural regions.

The result is a rich, unbalanced panel dataset with information for 2000-2020. It consists of detailed information on the treatment, establishment and regional characteristics.

### 3.2 Sample selection

I regard firms as treated if their observed projects were approved under the funding rules valid from 2004 to 2020 and started no earlier than 2004. I only consider establishments whose projects were actually realised and who received financial support. I focus on firms with only one treatment during the observation period, and I exclude outliers in terms of the subsidy amount.<sup>13</sup> In addition, I exclude all establishments (treated as well as nontreated) that were

<sup>11</sup>Since the seminal work of Mincer (1962), the workforce’s qualification level and work experience have been used as standard proxies for a firm’s human-capital endowment, or labour quality.

<sup>12</sup>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-sized cities, population density, and population density excluding large and medium-sized cities. See BBSR (2018) [in German].

<sup>13</sup>I exclude the firms in the largest one percent percentile regarding the subsidy.

treated before 2004 to eliminate the influence of previous treatments on outcome development during the observation period.

Regarding the nontreated establishments, I face two further sources of potential distortion. The *first source* is selection bias due to unobserved characteristics: The GRW is a demand-driven programme in which all establishments in eligible regions (in the eligible sectors) have access to funding, and I cannot observe why some establishments apply for grants and others do not. The influence of unobserved characteristics may bias the results in both directions.<sup>14</sup> The *second source* is the influence of the economic environment on the development of establishments and the estimated effect (Heckman et al. 1997, 1999): Compared with the treated establishments located in disadvantaged regions, establishments located in noneligible regions benefit from a more favourable environment in economically stronger regions.

I consider unobservable selection as the more serious problem since neither the direction nor the amount of bias can be assessed. To control for unobservable selection, I exclude all nontreated establishments in eligible regions from the sample. Thus, only establishments in eligible sectors from regions that do not have access to GRW funding are potential controls. This decision also means that I accept a potential underestimation of the treatment effect. To reduce the risk of underestimation, I exclude nontreated firms from particularly prosperous regions (in terms of fiscal power and GDP per employee).<sup>15</sup> Overall, I expect rather conservative estimation results of the treatment effect. On the other hand, this design permits an assessment whether GRW investment grants are suitable to (over)compensate for economic disadvantages in lagging regions and contribute to the reduction of spatial disparities.

As a result, my sample consists of approximately 1.1 million establishments, of which 8,649 are treated and located in eligible regions. A total of 1,135,823 establishments are not treated and are located in noneligible regions.

### 3.3 Sample description

In Table 2, I summarise potentially relevant firm and environment-related characteristics. They may influence establishments' employment development and (successful) application for investment grants. Since I have unbalanced panel data, the descriptive statistics in Table 2 provide a 'snapshot' of the establishments in the sample at the beginning of the first funding period, 2004, and the last funding period, 2014.

The descriptive statistics in Table 2 suggest that the GRW programme is very selective. I observe substantial differences regarding some firm characteristics and the regional distribution

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<sup>14</sup>If, for example, more establishments with efficient/good management are among the applicants, the effect tends to be overestimated. If, on the other hand, predominantly less productive firms are subsidised, the effect tends to be underestimated.

<sup>15</sup>I conduct comprehensive robustness checks on the basis of alternative choices of the location of potential controls. Section 6 presents the results of the respective estimations.

Table 2: Sample descriptive statistics, 2004 and 2014

	2004		2014	
	non-treated	treated	non-treated	treated
<b><i>establishment characteristics</i></b>				
micro establishment ( <i>percent</i> )	59.4	34.2	55.8	29.7
small establishment ( <i>percent</i> )	33.2	40.7	36.1	47.7
medium establishment ( <i>percent</i> )	6.3	21.1	6.8	20.0
large establishment ( <i>percent</i> )	1.1	3.9	1.2	2.7
settled establishment ( <i>percent</i> )	74.9	72.1	84.2	73.8
young establishment ( <i>percent</i> )	25.1	27.9	15.8	26.2
<i>classification of economic activity: largest sectors (2-digit level NACE Rev.1.1, and NACE Rev.2), percent</i>				
manufacture of fabricated metal products <sup>(1)</sup>	3.1	13.6	3.2	15.8
manufacture of machinery and equipment	2.1	8.9	1.6	7.6
business-related services <sup>(2)</sup>	13.1	7.4		
hotels and restaurants	8.8	6.8		
accommodation			1.8	6.2
food and beverage service activities			7.8	1.5
wholesale trade <sup>(3)</sup>	7.3	4.7	6.4	6.7
construction	11.5	4.2		
specialised construction activities			9.8	4.1
retail trade <sup>(4)</sup>	9.4	2.2	8.9	3.3
share high-skilled employees ( <i>percent</i> )	0.069	0.139	0.106	0.175
share low-skilled employees ( <i>percent</i> )	0.164	0.103	0.133	0.084
share young employees ( <i>percent</i> )	0.251	0.244	0.224	0.235
share experienced employees ( <i>percent</i> )	0.236	0.230	0.338	0.290
share secure contracts <sup>(5)</sup> ( <i>percent</i> )	0.511	0.766	0.425	0.715
median monthly salary FTE <sup>(6)</sup> (€)	2,142.855	1,844.473	2,561.072	2,328.737
<b><i>regional characteristics (district level)</i></b>				
located in rural district ( <i>percent</i> )	20.9	67.8	21.2	57.0
located in urban district ( <i>percent</i> )	79.1	32.2	78.8	43.0
unemployment rate ( <i>percent</i> )	7.747	16.293	5.166	8.871
GDP per employee ( <i>thousand €</i> )	57.890	45.884	68.598	58.308
GDP per capita ( <i>thousand €</i> )	28.913	19.719	37.207	28.378
fiscal power (€ per inhabitant)	565.059	304.362	847.376	580.672
<i>located in (federal state) ...</i>				
Schleswig Holstein ( <i>percent</i> )	2.0	0.9	0.0	2.0
Hamburg ( <i>percent</i> )	4.3	0.2	0.0	0.3
Lower Saxony ( <i>percent</i> )	6.2	13.9	7.2	3.4
Bremen ( <i>percent</i> )	0.0	0.3	0.0	1.4
Northrhine Westphalia ( <i>percent</i> )	27.4	1.0	27.4	12.0
Hesse ( <i>percent</i> )	8.3	3.2	8.5	3.1
Rhineland Palatinate ( <i>percent</i> )	6.7	2.5	7.4	2.3
Baden Wuerttemberg ( <i>percent</i> )	23.3	0.0	25.9	0.4
Bavaria ( <i>percent</i> )	21.3	2.3	23.0	6.5
Saarland ( <i>percent</i> )	0.5	0.9	0.8	2.3
Berlin ( <i>percent</i> )	0.0	7.6	0.0	11.5
Brandenburg ( <i>percent</i> )	0.0	8.3	0.0	9.9
Mecklenburg Pomerania ( <i>percent</i> )	0.0	11.0	0.0	11.0
Saxony ( <i>percent</i> )	0.0	26.2	0.0	19.0
Saxony Anhalt ( <i>percent</i> )	0.0	7.3	0.0	7.6
Thuringia ( <i>percent</i> )	0.0	14.5	0.0	7.4
<i>number establishments</i>	<i>574,506</i>	<i>913</i>	<i>538,338</i>	<i>1,150</i>

Notes: <sup>(1)</sup> manufacture of fabricated metal products (except manufacture of machinery and equipment); <sup>(2)</sup> other business activities, incl. accounting, advertising, consultancy, recruitment, security, industrial cleaning; <sup>(3)</sup> wholesale and commission trade (except of motor vehicles and motorcycles); <sup>(4)</sup> retail trade (except of motor vehicles and motorcycles), repair of personal and household goods; <sup>(5)</sup> share of permanent full-time contracts; <sup>(6)</sup> salary of full-time employees.

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

of the establishments. The vast majority of both treated and nontreated firms are small or very small (e. g. in 2004, approximately 75 percent of the treated and 90 percent of the nontreated firms). However, the share of subsidised medium-sized establishments is about three times higher than that of nonsubsidised establishments. The two subsamples also differ in terms of the distribution of economic activities: many of the treated establishments in 2004 operated in the manufacturing of fabricated metal products (14 percent) and machinery and equipment (9 percent), whereas the nonsubsidised establishments operated mainly in business-related activities (13 percent) or construction (12 percent). Until 2014, the distribution of economic activities changes in the subsample of treated firms. While the manufacturing of fabricated metal products and machinery and equipment remains the predominant economic sectors, the funding focus in the service sector shifts from business related services to wholesale trade and accommodation.

Regarding the employees' qualification structure, I observe differences at the high and low ends of the distribution: in 2004, the share of high-skilled employees in the treated firms was approximately twice as high as was that in the nontreated firms (14 vs. 7 percent), and the share of low-skilled employees was considerably lower (10 vs. 16 percent). The employees' qualifications slightly improve in both the treated and nontreated firms over time. The share of secure jobs is markedly higher in subsidised establishments, but it declines over the observation period.

In addition, the median monthly salary differs between both subsamples: in 2004, treated establishments paid, on average, 1,850€ per month, whereas non-subsidised establishments pay approximately 300€ more. The pay gap was still considerable in 2014. This gap may be partially explained by the different economic environments; as expected, the (nontreated) establishments in regions not eligible for investment grants benefit from better economic conditions. Here, I observe a significantly lower unemployment rate, a substantially higher GDP per employee, higher GDP per capita, and remarkably greater fiscal power. In addition, the majority of the nonsubsidised establishments are located in urban regions (80 percent), compared with only one third of the subsidised firms in 2004, and approximately 40 percent in 2014.

Finally, I observe different distributions of the treated and nontreated firms across the federal states. In 2004, the difference for the East German federal states (including Berlin)<sup>16</sup> is most striking, where I find approximately 75 percent of the treated establishments, but no nontreated establishments. This observation is surprising at first sight; however, the map in Figure A.2 shows that all districts in East Germany are eligible for investment grants.<sup>17</sup> In 2014, only 66 percent of the treated firms were still located in East German federal states. Similarly, the

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<sup>16</sup>The East German federal states are Brandenburg, Mecklenburg Pomerania, Saxony, Saxony Anhalt and Thuringia.

<sup>17</sup>I exclude nonsubsidised establishments in eligible regions from the sample for selectivity reasons, as mentioned above.

share of firms located in Lower Saxony decreases by approximately ten percentage points in the subsample of treated firms. In contrast, e. g., the share of treated firms located in Northrhine Westphalia increases to twelve percent. These changes presumably reflect the adaptation to the variation in the funding rules (see section 2).

Overall, the composition of the treated and nontreated establishments in the sample changes over time. Most obvious is the change in the regional distribution of the treated firms. When looking at the development of employment quality and quantity in the subsidised and nonsubsidised firms over the observation period (Figure 1), I observe more or less similar patterns, but at different levels. In both the treated and nontreated firms, the earnings increase, similar to human capital endowments (increasing shares of high-skilled employees, and decreasing shares of low-skilled employees), whereas the share of permanent full-time contracts decreases. The number of employees slightly increases.

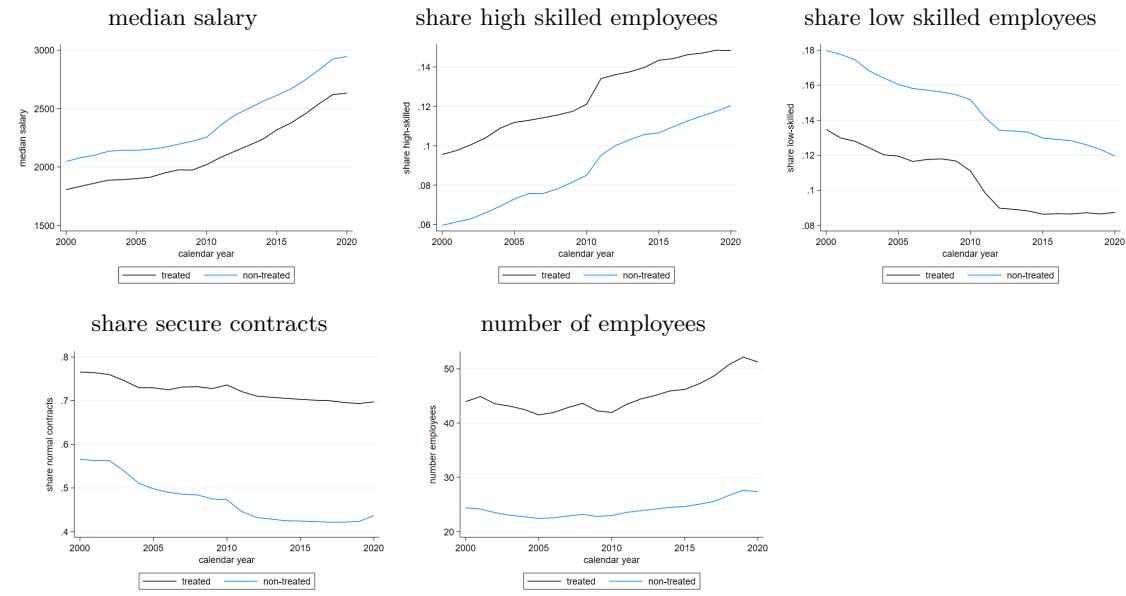


Figure 1: *Outcome development in the sample*

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

## 4 Identification strategy

### 4.1 Assumptions

The key assumption of a combined matching and DID approach, the *conditional parallel-trend assumption*, implies that unobservable individual characteristics must be invariant over time for units with equal observed characteristics. This assumption also means, that covariate-specific

trends are allowed (Heckman et al. 1997, 1998).<sup>18</sup> The conditional parallel-trend assumption is not testable. I regard the pretreatment 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 whether this assumption is fulfilled (see Section 6).

*Common support* is a necessary condition for matching (Heckman et al. 1997, 1998). In a panel-data context, this condition is required for each treated unit at each time period included in the estimation (Callaway and Sant’Anna 2021). In the analysis, I consider only establishments that fulfil the common-support condition at each observed time period.

In addition, the *assumption of no anticipation* rules out an influence of the treatment on the outcome prior to its implementation. Otherwise the changes in the outcome for the treated group between pre- and posttreatment could reflect not only the causal effect but also the effect of behavioural 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), I conduct year-by-year placebo tests to verify the assumption (see Section 6).

The strict interpretation of the *irreversibility of treatment or staggered treatment adoption* assumption is that units adopt the treatment 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 a potential impact of a treatment on a (current or future) outcome even when it is completed. In my estimations, an establishment is regarded as treated from the treatment start for all of the following time periods.

Applying a combined matching and DID approach relies on the assumption of *no uncontrolled carryover and spillover effects*:<sup>19</sup> the potential outcome of an observed establishment must not depend on its own previous treatments, or on the current treatment status of other establishments. In the analysis, I exclude all (treated and nontreated) establishments that received GRW investment subsidies in the years prior to the analysed funding period to address potential carryover effects. Additional place-based policy programmes (e. g., those implemented by the states or municipalities) are explicitly forbidden as they would violate the EU common market rules.<sup>20</sup>

Moreover, 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

<sup>18</sup>Thus, the combination of matching and DID methods in a panel context relaxes the *assumption of sequential ignorability* for matching (Robins et al. 2000) and the *common trend assumption* required for DID.

<sup>19</sup>This assumption replaces the *random sampling assumption* of the ‘pure DID approach’ for the combination of matching and DID methods.

<sup>20</sup>Although a wide range of potential funding alternatives for establishments exist in Germany, I do not expect carryover effects since these alternative funding options do not have an explicit spatial scope; these programmes can be used by (treated and nontreated) establishments all over Germany.

of applicants is much less than is that of eligible establishments in the eligible sectors, I also consider the assumption of no spillover effects as fulfilled.

## 4.2 Estimation approach

The core of my estimation strategy consists of a semiparametric difference-in-differences approach that considers time-varying treatments and the 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): 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 definition refers to the number of periods after the treatment start. The average effect of units in a particular group at a particular time is estimated as the comparison of two outcome developments:

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

where  $G_g = 1$  means that group  $g$  is initially treated at time  $G$ . The average treatment effect in group  $g$  at time  $t$ ,  $ATT(g, t)$ , is estimated by comparing the outcome development from pretreatment to time  $t$  in the case of treatment  $Y_t(g)$  and in the case of non-treatment  $Y_t(0)$ . This method requires at least one pretreatment 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:

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

where  $\theta$  denotes the aggregated effect of all group-time-average treatment effects  $ATT(g, t)$  over all groups  $g \in G$  and all time periods  $t = 2, \dots, T$ ,  $w(g, t)$  is the weighting function. I estimate the overall average treatment effect that corresponds to the ‘conventional’ average treatment effect for the treated using the following weighting function:<sup>21</sup>

$$w_o(g, t) = \frac{1\{t \geq g\} P(G = g | G \leq T)}{(T - g + 1)}. \quad (3)$$

---

<sup>21</sup>In principle, this is a weighted average of all partial effects; the weights are the respective numbers of observations in each cohort. It is equivalent to the Equation 3.11 of Callaway and Sant’Anna (2021).



In addition, I apply the weighting scheme for an event study analysis to explore the development of the treatment effect:

$$w_e(g, t) = \frac{1 \{g + e \leq T\} \mathbf{1} \{t - g = e\} P(G = g | G + e \leq T)}{(T - g + 1)}, \quad (4)$$

where  $e$  denote the event-time, i. e. the time elapsed since treatment adoption ( $e = t - g$ ). The event-time estimator gives the average effect of being treated for all treated units  $e$  time periods after the treatment was adopted.<sup>22</sup>

For the second part of the study, I apply the weighting scheme for cohort-specific effects to estimate the effects in the different funding periods:

$$w_{\tilde{c}}(g, t) = \frac{1 \{t \geq g\} \mathbf{1} \{g = \tilde{g}\}}{(T - g + 1)}, \quad (5)$$

where  $\tilde{c}$  denotes the respective cohort.<sup>23</sup>

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 a doubly robust approach, see Callaway and Sant’Anna (2021)).

As I know from the descriptive analysis (see Table 2), I am faced with a rather selective group of treated firms. In addition, the pool of potential controls is large compared with the number of treated. Since the described estimation process is very computing time and memory intensive in large datasets, I implement a matching procedure to focus on those nontreated establishments that are actually comparable to the treated firms and reduce the size of the dataset. This data preprocessing also adjusts the data by reducing potential inefficiency and model dependency of the subsequent estimations (Ho et al. 2007).

### 4.3 Data preprocessing

I apply a ties matching at the cohort level to identify the best comparable potential controls for the treated establishments. An obvious idea is to construct one matched sample and analyse all the outcomes in one and the same sample. Unfortunately, I find no sample fulfilling the conditional parallel trend assumption for all outcomes at the same time. Therefore, I use different, outcome-specific matched samples for the estimations.

For preprocessing, I consider the covariates characterising the establishments, their employee structure and their economic environment described in Section 3.3 one year before the treatment starts. Since the regional economic environment of treated on non-treaed firms is different ‘by definition’ (treated firms in eligible, structurally weak regions vs. nontreated firms in

<sup>22</sup>The estimator is equivalent to the Equation 3.4 of Callaway and Sant’Anna (2021).

<sup>23</sup>The estimator is equivalent to Equation 3.7 of Callaway and Sant’Anna (2021).

stronger, noneligible regions, see Section 3.2), it is not useful to match the level values of regional characteristics. Instead, I consider regional development prior to start of the treatment and match establishments in regions that exhibit similar developments in the pretreatment years.<sup>24</sup> In Table A.3 of the Appendix, I summarise the covariates used to construct the outcome-specific matched samples for each estimation.

## 5 Results

This section presents the results of the above described estimation approach. The units of observation are establishments in Germany located in eligible regions that were treated during three consecutive GRW funding periods from 2004 to 2020. The control group consists of nontreated establishments located in noneligible regions to exclude unobservable selection.

The study focuses on two main questions. *First*, an event study analyses the effects of GRW investment grants on employment quality in terms of earnings, employees' qualification levels and job security, and the number of employees. The database enables me to estimate the treatment effect over a comparatively long observation period of up to 15 years post treatment start and thus to distinguish between short-term effects (up to two years after treatment start), medium-term effects (seven to eight years after treatment start), and long-term effects (at least ten years post treatment start).<sup>25</sup> The long-term results may indicate whether investment grants can initiate sustainable employment and income growth in treated establishments, which is the main goal of the GRW programme.

Moreover, the event study results for the number of employees can shed some light on potential windfall profits:<sup>26</sup> A decrease in the effect size after the expiry of the five-year-commitment period might be a hint of large-scale windfall profits from the GRW investment subsidies. When the average project duration of two years for the subsidised projects is considered, the mid-term results are of special interest in this context.

The *second* question is whether changing economic conditions, unexpected social events and changes in funding rules during the observation period influenced the treatment effect. To answer this question, I distinguish the effects of GRW investment grants between the three analysed funding periods in cohort specific estimations.

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<sup>24</sup>Figure A.3 of the appendix describes the development of the considered proxies for regional wealth (GDP per capita, and fiscal power), productivity (GDP per employee) and labour market flexibility (unemployment rate) in the eligible and noneligible regions over the observation period. The proxies for regional productivity and wealth exhibit different levels (with higher GDP per employee, GDP per capita and fiscal power in the noneligible districts), but similar trends over the observation period. In contrast, the unemployment rate is higher in the eligible regions and declines more strongly than it does in the noneligible regions. Since this might particularly influence the quantitative employment effect and the income effect, the development of the unemployment rate is included in the matching procedures for these outcomes.

<sup>25</sup>This differentiation of the observed effects follows the suggestion of Dvouletý et al. (2020).

<sup>26</sup>A prominent example for so-called 'subsidy grasshoppers' in Germany is the Finnish cell phone manufacturer Nokia, which closed its plant in Bochum in Northrhine Westphalia rather unexpectedly in 2008 after receiving approximately 60 million €GRW subsidies (see Hage 2008).

The selection of potential controls exclusively among establishments in noneligible regions (see Section 3.2 for more details) might result in rather conservative estimations. However, it is the best available sample design to answer the question of whether GRW investment grants contribute to a reduction in spatial disparities. In addition, a comprehensive robustness check using alternative requirements regarding the location of potential controls reveals that the results presented below are not sensitive to the sample design.

## 5.1 Average treatment effects and event study results

Table 3 presents the estimation results for the overall treatment effects on employment quality in terms of labour income (the median monthly salary of full-time employees), employees' qualification structure (the share of high-skilled and low-skilled employees, respectively), and job security (the share of permanent full-time contracts), as well as employment quantity (the number of employees). In addition, the table provides event-study results on the development of the treatment effect over a period of 15 years.

***Labour income*** The average treatment effect for the treated (ATT) on earnings is negative. Although the median wage of full-time employees increases in the treated and the nontreated firms over time (see Figure 1), the growth for the employees in the treated firms is approximately €32 lower than that in the control firms. While the contemporaneous effect is insignificant, I observe a negative short-term effect (from one year before treatment to two years after treatment start) of approximately €17. The effect increases continuously to approximately €−45 after eight years and to €−65 after 14 years.

The results suggest that the objective of reducing spatial disparities in income by GRW investment grants is not achieved at the individual level; rather, funding reinforces the imbalance of individual earnings in eligible and noneligible regions. One might accuse different economic environmental conditions for that result, but the effects are very similar if the control firms are located in the same district or labour market region as the treated firms are, see Section 6.

The estimated negative earnings effects differ from the findings in recent empirical studies. Alexandre et al. (2024) find positive wage effects of the European Regional Development Fund in Portugal, particularly for multiply funded firms, Grunau et al. (2024) estimate positive mid-term wage effects of GRW investment grants for incumbent workers in treated German firms; Muraközy and Telegdy (2023) find positive effects of the European Union's Cohesion and Structural Funds on labour productivity in Hungarian firms, driven by increasing wages for skilled workers. The results of Brachert et al. (2018) yield insignificant wage effects of investment grants in Saxony Anhalt.

***Qualification*** Regarding the qualification structure of the employees, I observe a very small but negative effect of GRW investment grants on both proxies. If the general development

Table 3: **Effects of GRW investment grants on the quality and quantity of employment**

	median monthly salary <sup>(*)</sup>	share high skilled	share low skilled	share secure contracts <sup>(**)</sup>	no. of employees
average effect <sup>(***)</sup>	-32.711*** (7.842)	-0.004** (0.002)	0.005* (0.003)	0.037*** (0.003)	5.881*** (1.264)
<i>effect from 1 year before treatment to ...</i>					
... treatment start	1.673 (3.403)	0.001 (0.001)	-0.004*** (0.001)	0.004** (0.002)	1.123*** (0.227)
... 1 year later	-14.299*** (4.768)	-0.002 (0.001)	-0.001 (0.002)	0.013*** (0.002)	3.285*** (0.403)
... 2 years later	-17.380*** (6.749)	-0.003* (0.002)	-0.0002 (0.002)	0.018*** (0.003)	3.979*** (0.619)
... 3 years later	-30.693*** (7.576)	-0.003 (0.002)	0.003 (0.003)	0.025*** (0.003)	4.974*** (0.903)
... 4 years later	-34.572*** (8.429)	-0.003 (0.002)	-0.001 (0.003)	0.034*** (0.003)	5.410*** (1.140)
... 5 years later	-41.303*** (9.315)	-0.004* (0.002)	0.003 (0.003)	0.039*** (0.003)	5.821*** (1.381)
... 6 years later	-44.407*** (9.973)	-0.005** (0.002)	0.003 (0.003)	0.046*** (0.003)	7.799*** (1.676)
... 7 years later	-49.509*** (10.693)	-0.005* (0.003)	0.004 (0.004)	0.049*** (0.004)	7.643*** (1.942)
... 8 years later	-44.682*** (11.865)	-0.006** (0.003)	0.007* (0.004)	0.055*** (0.004)	7.049*** (2.213)
... 9 years later	-45.185*** (13.324)	-0.008*** (0.003)	0.008* (0.005)	0.061*** (0.005)	7.252*** (2.388)
... 10 years later	-51.213*** (15.016)	-0.008*** (0.003)	0.008* (0.004)	0.061*** (0.005)	6.438** (2.891)
... 11 years later	-50.462*** (16.706)	-0.008*** (0.003)	0.013*** (0.005)	0.069*** (0.005)	7.726** (3.741)
... 12 years later	-51.016*** (19.515)	-0.006* (0.003)	0.020*** (0.006)	0.074*** (0.006)	10.893** (4.617)
... 13 years later	-75.150*** (24.135)	-0.006* (0.004)	0.028*** (0.007)	0.078*** (0.007)	13.728** (6.064)
... 14 years later	-65.008** (31.807)	-0.005 (0.005)	0.038*** (0.010)	0.093*** (0.009)	15.364** (7.337)
<i>number establishments</i>	<i>10,578</i>	<i>10,878</i>	<i>11,300</i>	<i>12,346</i>	<i>10,825</i>

Notes: <sup>(\*)</sup> median monthly salary of full-time employees; <sup>(\*\*)</sup> share of permanent full-time contracts; <sup>(\*\*\*)</sup> estimated effect corresponds to 'conventional' average treatment effect for the treated.

Standard errors in parantheses; results significant on the level: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

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

over time (Figure 1) is considered, the estimated effects mean that the share of high-skilled employees increases by 0.004 percentage points less in the treated firms than it does in the control firms, and that the share of low-skilled employees decreases by 0.005 percentage points less than it does in the control firms. The event study results show that the effects occur mainly in the medium and long term: in the case of the share of high-skilled employees, the effect becomes significant five years after treatment start, while the effect on the share of low-skilled employees is significant beginning eight years post treatment start.

However, the described results suggest rather negligible effects on the quality of employment in terms of employees' qualification structure. Investment grants have no noticeable effects on employees' job satisfaction and negligible effects on establishment potential for future innovations and growth.

**Job security** The ATT in terms of job security is positive. The share of permanent full-time jobs decreases over time in general (Figure 1), however, the decrease in the treated firms is by 0.04 percentage points lower than it is in the control firms. The event study results reveal a continually increasing effect. While the mid-term effect (seven years after the start of the treatment) is 0.05 percentage points, the share of secure jobs in the treated firms declines from one year before treatment to 14 years after treatment start, which is 0.09 percentage points less than it is in the control firms.

Another interesting finding is the short-term anticipation effect that is detected when placebo tests are conducted prior to the treatment. One year before the treatment starts, the share of secure jobs is 0.004 percentage points larger in the treated firms than it is in the control firms (see Table 5 in Section 6). This difference suggests that, after having applied for GRW investment grants, some firms might maintain some of the permanent full-time jobs instead of eliminating them, as the firms anticipate future funding for safeguarding those jobs.<sup>27</sup>

The results suggest a rather persistent positive impact of investment grants on job security: the treated firms do not reduce the share of permanent full-time contracts as much as the control firms do. This finding can be regarded as a contribution of GRW investment grants to safeguarding secure jobs in eligible regions. At the same time, it suggests that this programme reduces the flexibility of the funded firms regarding personnel adjustments.

**Number of employees** While the number of employees generally increases over time (see Figure 1), the growth in the treated firms is by approximately six employees stronger than is that in the control firms. Starting from a rather small positive contemporaneous effect of one employee, the effect becomes stronger over time: compared with the pretreatment period, treated establishments grow by seven employees more than their controls do in medium term and by 15 employees more until 14 years after the treatment start.

The estimated effect on employment quantity suggests a persistent reduction in spatial disparities in terms of (un)employment between establishments in eligible and noneligible regions. One of the main goals of GRW investment grants is achieved. Moreover, the results reveal no evidence of windfall profits. The observed mid-term effects (seven and eight years post treatment start) do not exhibit a sudden decline in the employment effect.

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<sup>27</sup>The significant short-term placebo effect prevents a causal interpretation of the contemporaneous effect and short-term effects on secure jobs.

The results correspond to the empirical evidence showing positive employment effects in Germany (see e. g. Brachert et al. 2018, Grunau et al. 2024) and other European countries such as Italy (Bernini and Pellegrini 2011, Cerqua and Pellegrini 2014), the UK (Criscuolo et al. 2019), Belgium (Decramer and Vanormelingen 2016) and Northern Ireland (Harris and Trainor 2007).

Overall, the analysis provides empirical evidence for contradictory effects of GRW investment grants. While funding reduces spatial disparities in the number of employed persons and helps to safeguard secure jobs in eligible regions, it maintains, or slightly widens, the gap in average individual labour income. Thus, one might conclude that GRW grants predominately foster low-skilled, lower paid jobs. However, a noticeable effect on employees' qualification structure is not evident.

However, the results of the study are based on average effects, and the range of effects might be wide. For example, certain groups of people may benefit in terms of income or suffer from worse employment prospects, and the effects may differ depending on the firm type. So, e. g., Dettmann et al. (2023) find stronger employment effects of GRW funding in firms in the service and health care sector than in capital-intensive sectors such as chemistry and pharmacy, and the results of Grunau et al. (2024) reveal positive income effects for incumbent workers but no effects for young hired workers.

## 5.2 Cohort specific effects of investment grants

During the observed time period (2000-2020), several socially and economically relevant events occurred that might have influenced the treatment effect. For example, in the first analysed funding period, 2000-2006, the common European currency was launched, the dot-com crisis took place, and (particularly in East Germany) unemployment rates were high (see, e. g., Figure A.3). In the second funding period, 2007-2013, the German economy was characterised by the global financial crisis and the recovery phase, as well as a comprehensive reform of the labour market policy including rules on unemployment benefits that were more restrictive. The third funding period, 2014-2020, corresponds to a phase of stable economic upswing and the start of the Coronavirus crisis in 2020. In addition, the amount of money available for the GRW program continually decreased, the vast majority of regions were exposed to a decline in funding; and, for some regions, the eligibility status completely changed during the observation period.

In the second step, I therefore verify whether the estimated treatment effects are time dependent, i.e., whether different economic conditions, modifications in the funding rules, or other (presumably unobserved) events influence the observed effects. The focus is not on the influence of specific events, but on the question whether and how much the treatment effects vary over time. To answer this question, I conduct cohort-specific estimations for cohorts that

coincide with the three analysed funding periods. The results presented in Table 4 are the mean effects for each of the cohorts.

Table 4: **Cohort-specific effects of GRW investment subsidies on the quantity and quality of employment**

	median monthly salary <sup>(*)</sup>	share high skilled	share low skilled	share secure contracts <sup>(**)</sup>	no. of employees
average effect <sup>(***)</sup>	-32.711*** (7.842)	-0.004** (0.002)	0.005* (0.003)	0.037*** (0.003)	5.881*** (1.264)
<i>number establishments</i>	<i>10,578</i>	<i>10,878</i>	<i>11,300</i>	<i>12,346</i>	<i>10,825</i>
FP 2004-2006	-29.577** (13.315)	-0.002 (0.003)	0.011** (0.005)	0.026*** (0.005)	8.159*** (2.425)
<i>number establishments</i>	<i>6,730</i>	<i>7,386</i>	<i>11,300</i>	<i>7,987</i>	<i>6,911</i>
FP 2007-2013	-37.467*** (10.540)	-0.006*** (0.002)	-0.005 (0.004)	0.040*** (0.004)	4.712** (1.505)
<i>number establishments</i>	<i>8,461</i>	<i>8,742</i>	<i>9,164</i>	<i>9,963</i>	<i>8,689</i>
FP 2014-2020	-12.354 (18.410)	-0.002 (0.004)	0.0002* (0.005)	0.018** (0.007)	6.143*** (0.909)
<i>number establishments</i>	<i>5,465</i>	<i>5,700</i>	<i>6,122</i>	<i>6,538</i>	<i>5,647</i>

Notes: <sup>(\*)</sup> median monthly salary of full-time employees; <sup>(\*\*)</sup> share of permanent full-time contracts; <sup>(\*\*\*)</sup> estimated effect corresponds to 'conventional' ATT presented in Table 3.

Standard errors in parantheses; results significant on the level: \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

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

The cohort specific effects presented in the table differ from the estimated average effects on the analysed outcomes. The deviations show some interesting patterns. In the first funding period of 2004-2006, the strongest positive effect of GRW funding on the number of employees (8 employees) is observed, and the treatment effect on the share of low-skilled employees is nearly twice as high as the overall effect (0.01 vs. 0.005 percentage points). Presumably, the high unemployment in this period might have fostered the creation of rather low-skilled jobs. In contrast, the lowest positive effect on the number of employees (4.7 employees) and the strongest negative treatment effects on earnings (€ -37) and the share of high-skilled employees are observed in the second funding period, 2007-2013. This is the period of financial crisis and recovery, and the first funding period with common eligibility rules for East and West Germany. The observed effects might suggest that funding is less successful in times of economic upswing. In the last analysed funding period, 2014-2020, the effect on the number of employees is near the average, the earnings effect is insignificant, and I observe the smallest positive effect on job security (0.02 percentage points).

Overall, the results presented in Table 4 suggest that changes in the economic environment and modifications of the funding rules have noticeable influences on the treatment effect. For

reliable results, it is therefore important to consider the influence of treatment timing in the estimation approach.

## 6 Quality and robustness checks

In the following, I present the results of different quality and robustness tests for the analysis. As argued in Section 4, the impact of observable and unobservable heterogeneity is problematic if it causes different outcome developments in the treated and control groups. Absent treatment, equal trends should be observed in the treated and control groups. I regard the pretreatment development of the observed outcomes in the establishments as a proxy for development in the absence of treatment and conduct placebo tests for the time prior to treatment to verify the validity of the parallel trend assumption and the assumption of no anticipation.

Figure 2 shows the results of the placebo parallel trend tests for each year 15 years before treatment for each analysed outcome. The figure reveals no significant differences between treated and controls prior to treatment in the case of labour income, the share of high-skilled and low-skilled employees and the number of employees. The parallel trend assumption can be considered fulfilled. Also the share of secure contracts does not differ prior to treatment, except for one year before the treatment starts.

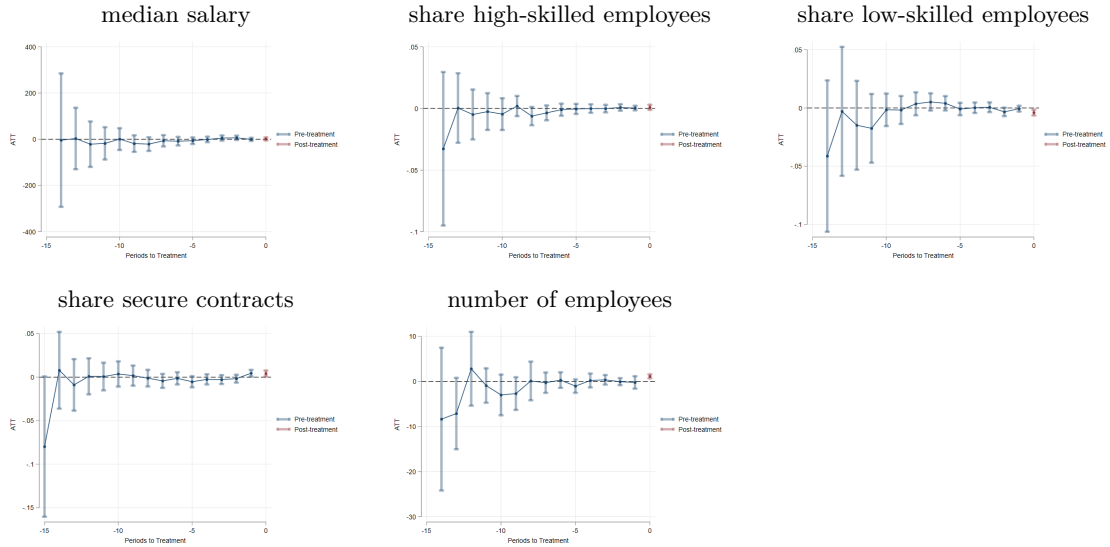


Figure 2: *Placebo test of conditional parallel trend assumption*

Note: The graph gives the estimated pre-treatment effects for each analysed outcome in the respective samples for the period of 15 years prior to treatment at the significance level  $p < 0.05$ .

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

Table 5 contains the results of the placebo tests for the parallel-trend assumption and the assumption of no anticipation for different time periods related to the start of the treatment.



The *first panel* of the table presents the results of the pretreatment placebo tests to validate the conditional parallel trend assumption for one year, three years, five years, and the full observed pretreatment period. In the case of labour income, the qualification structure of employees and the number of employees, the placebo tests yield insignificant funding effects prior to treatment. The parallel trend assumption can be considered fulfilled. As an exception, the test for the share of secure contracts reveals a significant placebo effect one year before the treatment starts. This finding might reflect anticipatory effects. As a consequence, the contemporaneous and short-term results for job security are not interpreted.

In the *second panel* of Table 5, I present short-term results of year-to-year placebo tests that can be regarded as a detection for anticipation effects (de Chaisemartin and D’Haultfoeuille 2021). These effects are insignificant for both one and three years prior to treatment for the following outcomes: labour income, the qualification structure of employees, and the number of employees. For these outcomes, the assumption of no anticipation can also be regarded as fulfilled. In the case of the share of secure contracts, the anticipation test for one year prior to treatment confirms the presumed short-term anticipation of future funding.

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

	median monthly salary <sup>(*)</sup>	share high skilled	share low skilled	share secure contracts <sup>(**)</sup>	no. of employees
<i>pre-treatment effects</i>					
1 year	-0.184 (3.453)	0.0004 (0.001)	-0.006 (0.007)	0.004** (0.002)	-0.222 (0.705)
3 years	3.918 (3.843)	0.0004 (0.001)	-0.001 (0.001)	0.00002 (0.002)	0.028 (0.404)
5 years	0.982 (4.200)	0.0001 (0.001)	-0.001 (0.002)	-0.021 (0.002)	-0.148 (0.481)
full pre-period	-6.234 (18.255)	-0.004 (0.004)	-0.005 (0.004)	-0.009 (0.007)	-1.418 (1.095)
<i>year-to-year effects prior to treatment</i>					
1 year	-0.184 (3.453)	0.0004 (0.001)	-0.006 (0.007)	0.004** (0.002)	-0.222 (0.705)
3 years	-2.315 (2.038)	0.0002 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.041 (0.301)
<i>number establishments</i>	<i>10,578</i>	<i>10,878</i>	<i>11,300</i>	<i>12,346</i>	<i>10,825</i>

Notes: <sup>(\*)</sup> median monthly salary of full-time employees; <sup>(\*\*)</sup> share of permanent full-time contracts.  
Sources: Employment History (IAB), GRW treatment data (BAFA), INKAR data (BBSR); own calculations.

The presented test results also indicate that the potentially different economic environments of treated firms located in regions eligible for GRW and control firms in noneligible regions (see Section 3.2 for more details) do not cause different trends prior to treatment; thus, they do not distort the results. Nevertheless, I present robustness checks verifying the influence of the location requirements for the control firms.

For the results presented in Section 5, I control for selection bias due to unobservables by excluding nontreated firms in eligible regions. The converse idea, in which the pool of potential controls is defined requiring the economic environmental conditions to be as similar as possible, accepts a potential selection bias due to unobservables. The *first* alternative sample definition is the selection of controls from the same district as the treated firm; the *second* alternative is a location in the same labour market region. In both cases, the potential controls are (predominantly) selected from regions eligible for GRW. As a *third* alternative, I require contiguous regions of the treated establishments as a possible location of the potential controls. Neighboring regions can be eligible or noneligible districts. This distinction changes the composition of the control group compared with the initial analysis, and also compared with the above-mentioned alternatives. The results are subject to both sources of distortion: an unobservable selection bias and the impact of different economic environments. Table 6 presents the results of the described robustness checks.

Table 6: **Robustness checks: location of potential controls**

	median monthly salary <sup>(*)</sup>	share high skilled	share low skilled	share secure contracts <sup>(**)</sup>	no. of employees
initial model	-32.711***	-0.004**	0.005*	0.037***	5.881***
<i>number establishments</i>	<i>10,578</i>	<i>10,878</i>	<i>11,300</i>	<i>12,346</i>	<i>10,825</i>
same district	-28.479***	-0.001	-0.0004	0.021***	7.214***
<i>number establishments</i>	<i>11,089</i>	<i>11,137</i>	<i>13,468</i>	<i>12,301</i>	<i>11,027</i>
same labor market region	-32.031***	-0.0004	0.0002	0.019***	6.983***
<i>number establishments</i>	<i>11,083</i>	<i>11,122</i>	<i>13,381</i>	<i>12,283</i>	<i>11,019</i>
contiguous districts	-45.102***	0.0001	0.005***	0.025***	6.770***
<i>number establishments</i>	<i>99,316</i>	<i>93,826</i>	<i>93,826</i>	<i>92,841</i>	<i>58,931</i>

Notes: Initial model means the overall results in table 3. Grey records indicate non-interpretable results due to pre-treatment trends of the outcome. Results significant on the level: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

(\*) median monthly salary of full-time employees; (\*\*) share of permanent full-time contracts.

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

The results in Table 6 reveal no large or systematic deviations from the results of the initial analysis. The funding effect on labour income is still negative and comparable in size.<sup>28</sup> The effect on the share of both, high and low skilled employees as proxies for the qualification structure is insignificant in most of the robustness checks, confirming the negligible impact of GRW funding on qualification. The treatment effect on job security is still positive, but slightly smaller. The quantitative employment effect is also positive and slightly larger.<sup>29</sup> Overall, the results of the alternative definitions of the location of potential controls do not

<sup>28</sup>One exception is the comparison to firms in contiguous districts. However, the placebo test for this specification exhibits a significant pretreatment trend, and the result cannot be interpreted as a causal treatment effect. The respective result is marked in grey in Table 6.

<sup>29</sup>The robustness checks for the number of employees reveal significant pretreatment trends if the controls are located in the same district or the same labour market region. The results in 6 are marked in grey.

indicate a distortion of the estimation results due to the applied definition of potential control firms exclusively from noneligible regions.

In the next step, I check the robustness of the results with respect to variations in the estimation approach and the data preprocessing. The *first panel* of Table 7 presents the results of variations in the specification of the DID approach; the data preprocessing and the covariate combinations for the weighting procedure remain unchanged. The results reveal a rather small influence of the specification of the DID approach; the estimations yield very similar results.<sup>30</sup>

Table 7: **Robustness checks: variation in the estimation approach**

	median monthly salary <sup>(*)</sup>	share high skilled	share low skilled	share secure contracts <sup>(**)</sup>	no. of employees
initial model	-32.711***	-0.004**	0.005*	0.037***	5.881***
<i>number establishments</i>	<i>10,578</i>	<i>10,878</i>	<i>11,300</i>	<i>12,346</i>	<i>10,825</i>
<i>variation in the did specification</i>					
only never treated controls	-32.942***	-0.005**	0.005	0.038***	5.790***
regression adjustment	-37.529***	-0.003**	0.007***	0.035***	6.174***
inverse probability weighting	-10.062	-0.016***	-0.006	0.036***	4.455
<i>number establishments</i>	<i>10,578</i>	<i>10,878</i>	<i>11,300</i>	<i>12,346</i>	<i>10,825</i>
<i>variation in the covariates for outcome weighting</i>					
no covariates	-58.681***	-0.004***	0.006***	0.028***	7.409***
outcome <sup>1</sup> + firm <sup>2</sup>	-15.141*	0.001	-0.006***	0.039***	6.241***
outcome <sup>1</sup> + region <sup>3</sup>	-39.552***	-0.003	-0.008***	0.043***	5.966***
<i>number establishments</i>	<i>10,578</i>	<i>10,878</i>	<i>11,300</i>	<i>12,346</i>	<i>10,825</i>
<i>variation in data preprocessing</i>					
firm <sup>2</sup>	-50.786***	-0.002	0.014***	0.031***	4.471***
<i>number establishments</i>	<i>12,229</i>	<i>12,229</i>	<i>12,229</i>	<i>12,229</i>	<i>12,229</i>
region <sup>3</sup>	-44.918***	-0.001	0.007***	0.050***	6.062***
<i>number establishments</i>	<i>629,714</i>	<i>629,714</i>	<i>629,714</i>	<i>629,714</i>	<i>629,714</i>
firm <sup>2</sup> + region <sup>3</sup>	-49.838***	-0.002	0.010***	0.028***	4.759***
<i>number establishments</i>	<i>10,817</i>	<i>10,817</i>	<i>10,817</i>	<i>10,817</i>	<i>10,817</i>

Notes: Initial model means the overall results in table 3. Grey records indicate non-interpretable results due to pre-treatment trends of the outcome. Results significant on the level: \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

(\*) median monthly salary of full-time employees; (\*\*) share of permanent full-time contracts.

<sup>1</sup> pre-treatment outcome in (t-1), outcome differences  $(t) - (t-1)$  and  $(t-1) - (t-2)$ ; <sup>2</sup> firm characteristics: classification of economic activity (4-digit level), firm size category, median of the monthly salary of full-time employees, share of high-skilled employees, regional settlement structure; <sup>3</sup> mean growth of regional factors over the last two years before treatment: unemployment rate, taxpower, and GDP per capita.

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

The *second panel* of the table contains different covariate combinations for the weighting procedure of the DID approach; the data preprocessing and the specification of the DID approach correspond to the initial analysis. The results presented in the *third panel* of Table 7 give an impression on the influence of variations in the data preprocessing. For the estimations, one

<sup>30</sup>In case of inverse probability weighting of the controls, I observe a significant pre-treatment trend for the monthly salary of the full-time employed; the same applies in case of regression adjustment for the number of employees. Both results are marked in grey in Table 7.

set of covariates is used for the matching process for all outcome estimations; the DID specification and the covariate combinations for the weighting procedure correspond to the initial analysis.

The results of the robustness tests in the second and the third panels illustrate the sensitivity of the estimation results to changes in the considered covariates. In particular, the results for labour income are not interpretable in most cases since the pretreatment placebo tests suggest significant pretreatment differences between the treated and the control firms. This problem also applies to some of the robustness tests for the other analysed outcomes.<sup>31</sup> However, the deviation of the interpretable robustness checks for qualification, the share of secure jobs and the number of employees is rather small.

Summarising the information revealed by the quality and robustness tests for the analysis, I conclude that the results presented in Section 5 are reliable. They are not distorted by different trends, nor are they influenced by anticipation or carryover effects. The results are also robust to the choice of different potential control samples. The robustness checks regarding data preprocessing and the weighting of the controls for DID confirm the selectivity of the sample of treated firms and the importance of the selection of relevant characteristics for the estimation.

## 7 Summary and Conclusion

The Joint Federal Task for the Improvement of Regional Economic Structures provides investment grants for firms in less developed regions. The main goal of the programme is to reduce spatial disparities across Germany, particularly in terms of (un)employment and income. Although the programme aims at fostering regional development, grants are provided to establishments, and subsidised establishments are expected to induce, or at least contribute to, sustainable wealth in the region.

In this study, I analyse the establishment-level effects of GRW investment grants on employment for three consecutive funding periods. In addition to the number of employees, my focus is on employment quality in terms of labour income, qualification structure and job security. Since the panel database covers 2000-2020, I can provide empirical evidence for short- and long-term developments. For the analysis, I combine the difference-in-differences approach of Callaway and Sant’Anna (2021), which explicitly considers the influence of time on the treatment effect with a ties matching at the cohort level. I compare treated establishments with nontreated firms located in noneligible regions. This research design results in rather conservative estimations. However, it enables me to answer the question of whether the GRW programme can help reduce spatial disparities.

The analysis yields contradicting results: The estimated effect of the investment grants on employment development is with 6 employees positive and persistent. In particular, the mid-

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<sup>31</sup>The affected results are marked in grey.

term results provide no hint of windfall profits. Additionally, job security in terms of the share of permanent full-time jobs is positively influenced by investment subsidies. On the other hand, the average earnings of the treated firms increase by approximately €32 less than those of the control firms. Also this effect is persistent. No noticeable effect on the qualification structure is evident. Overall, the study results show that the stated goal of reducing spatial disparities in employment and income through GRW investment grants is not reached at the firm level. However, this finding is true for the analysed average treatment effects, the findings for certain groups of employees or firms may deviate.

The question of whether investment grants are a curse or a blessing for employment can be answered: "It depends ...". Since the results reveal a trade-off between different aspects of employment, the answer depends on which aspect is most important for the funding authority. If the focus is on an increase in the pure number of employees, or on safeguarding individual job security, investment grants are rather a blessing. On the other hand, job security also means limited labour market flexibility for the funded firms. However, in terms of the average labour income of employees, the programme seems to act more like a curse. Furthermore, GRW investment grants have rather negligible effects on the employees' qualification structure, which means that neither individual job satisfaction nor firm capacity for innovation and future performance are influenced.

In addition, the results point to an impact of time on the treatment effect. The event study results show increasing funding effects over time, and the cohort specific analysis reveals cohort-specific variations in the treatment effects for the analysed outcomes. For unbiased estimation results, it seems necessary to consider time-varying treatment effects in the estimation approach.

The deviations in the cohort-specific effects suggest that certain events, economic conditions, the design of the funding rules, and other factors have an impact on the treatment effect, both in case of employment quantity *and* quality. It might be interesting for further research to disentangle the influence of different factors on the funding effect.

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## 8 Appendix

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

where  $S_r$  - overall structural weakness score for region  $r$   
 $w_m$  - weight of weakness indicator  $m$   
 $V_{mr}$  - standardized value of weakness indicator  $m$  in region  $r$   
 $\mu_m$  - mean value of weakness indicator  $m$   
 $\sigma_m$  - standard deviation of weakness indicator  $m$

weakness indicators and their weight in the analysed funding periods			
$m$	weight in percent		
	2000-2006	2007-2013	2014-2020
average unemployment rate ( $t - 4$ to $t - 2$ )*	40	—	—
average unemployment rate ( $t - 5$ to $t - 2$ )	—	50	45
gross wages and salaries per capita ( $t - 3$ )	40	40	—
gross wages and salaries per capita ( $t - 4$ )	—	40	—
gross wages and salaries per employee ( $t - 4$ )	—	—	40
quality of infrastructure	10	5	7.5
employment projection ( $t - 3$ to $t + 4$ )	10	5	7.5

Notes: \*in East Germany: average underemployment rate. Time related to the start of the programme period ( $t = 2000 / 2007 / 2014$ ).

Figure A.1: *Calculation of the structural weakness score for the analysed programme periods*  
Source: Own illustration.

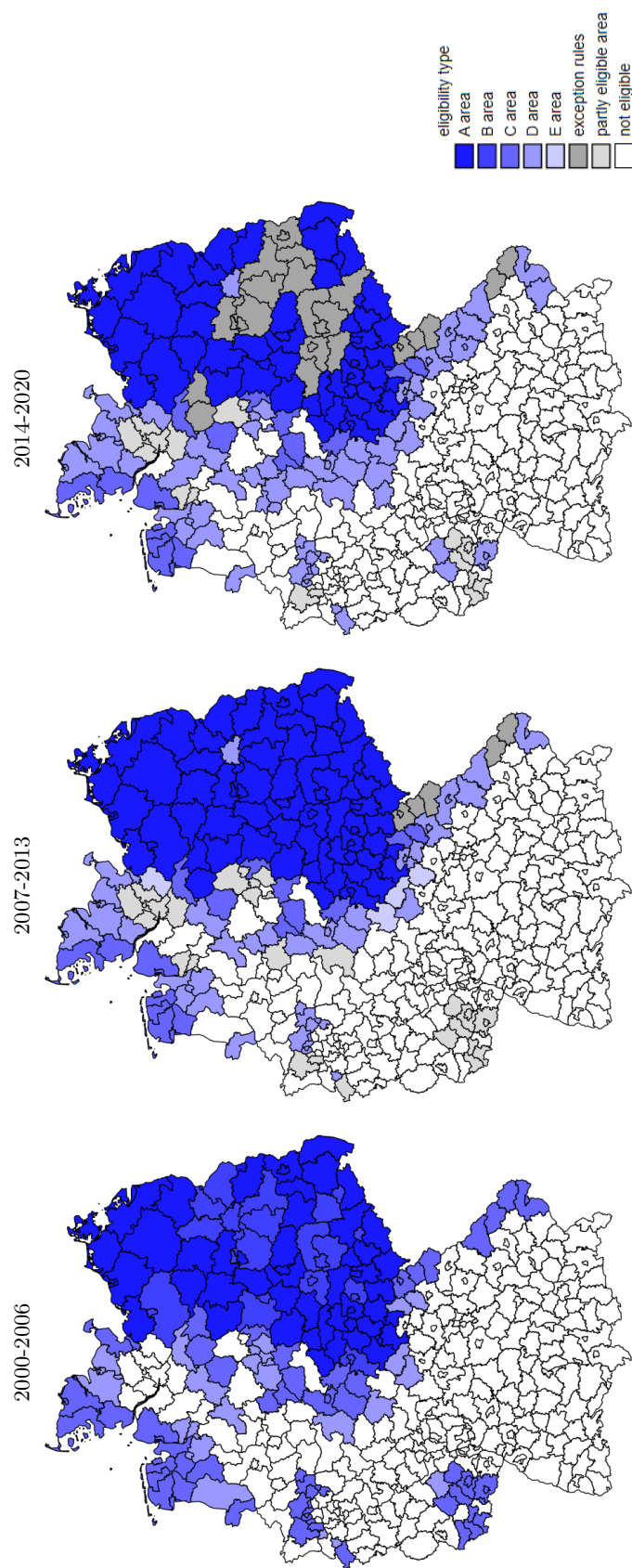


Figure A.2: *The Joint Federal Task for the Improvement of Regional Economic Structures (GRW) – Map of eligible regions for the analysed programme periods.*  
Source: Own illustration on the basis of BAFA data.

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

Type of the programme	Non-repayable grants for investment projects (co-funding)
Set-up of the programme	1970
Targets	<p>Reduction of regional disparities</p> <p>Increase of regional income and employment in assisted regions</p> <p>Catching-up of assisted regions to the general economic development</p>
Regional scope of the programme	Eligible labor market regions in the Federal Republic of Germany
Influence of EU	<p>→ Eligibility depends on the degree of the structural weakness of a region</p> <p>Limitation of the number of assisted regions according to a certain population share</p> <p>funding period linked to the funding period of EU Structural funds</p>
Legislation	<p>Treaty of the European Union (Articles 87 and 88)</p> <p>German Basic Law (Articles 72(2) No. 2, 91a)</p> <p>Joint Task Law (GRW-Gesetz)</p> <p>GRW Coordination frameworks</p> <ul style="list-style-type: none"> <li>◦ 2000-2006: No. 29 (2000-2004), 33 (2004-2006)</li> <li>◦ 2007-2013: No. 36 (2007-2008), 361 (2008-2011), 362 (2011-2014)</li> <li>◦ 2014-2020: No. 36310 (2014-2016), 36311 (2017), 36320 (2018-2020)</li> </ul> <p>Legislation of the Federal States</p>
Application process	<p>normalized application forms</p> <p>assessment by the Federal government and the responsible institution</p>
Granting authority	Government of the federal state where the investment project is planned

Source: Own compilation.

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

Recipients	Establishments in eligible areas	Municipalities in eligible areas			
Subject of funding	Investments in fixed assets <ul style="list-style-type: none"><li>o starting a new establishment</li><li>o capacity expansion of existing establishments</li><li>o changing the production programme or overhauling the entire process of production</li><li>o purchasing an inoperative establishment</li></ul> <ul style="list-style-type: none"><li>o industries with supra-regional sales</li><li>o exclusion of industries according to EU state aid legislation and further economic intentions</li></ul>	Investments in local commerce-related infrastructure <ul style="list-style-type: none"><li>o building of business parks</li><li>o revitalizing of business parks</li><li>o building and expansion of transport infrastructure</li><li>o building and expansion of associated utilities infrastructure (water, electricity, telecommunication etc.)</li><li>o building and expansion of sewage plants</li><li>o building and expansion of touristic infrastructure</li><li>o building and expansion of workforce training centers</li><li>o building and expansion of technology parks</li></ul>			
Sectoral scope					
Maximum aid intensity (as percent of eligible costs)*	small**	medium-sized**	large firms**		
2000-2006	CF 29	15-50	7.5-50	0-35	80
	CF 33	15-50	7.5-50	0-35	90
	CF 36	20-50	10-40	0-30	90
2007-2013	CF 361	20-50	10-40	0-30	60
	CF 362	20-50	10-40	0-30	60
	CF 36310	20-40	10-30	0-20	60
2014-2020	CF 36311	20-40	10-30	0-20	60
	CF 36320	20-40	10-30	0-20	60
					60

Notes: \* deviations from the standard rule are possible; \*\* dependent on the eligibility status of the region; CF = coordination framework.  
Source: Own illustration.

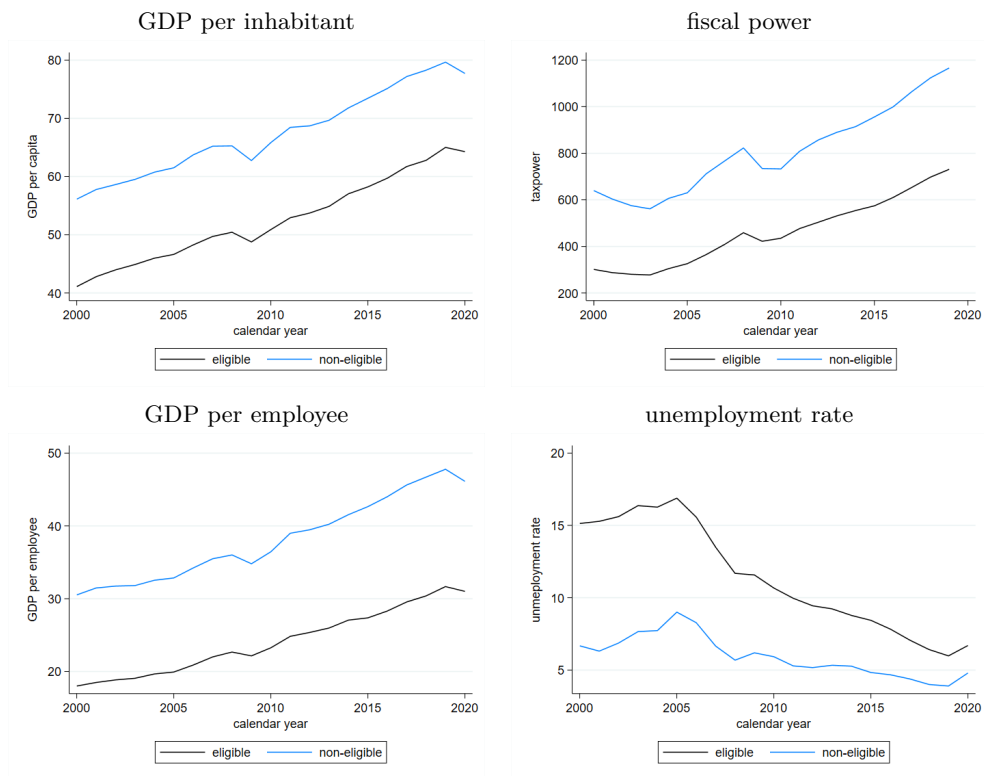


Figure A.3: *Development of regional indicators for labor market flexibility, productivity, and wealth in eligible and non-eligible regions at the district level*

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

Table A.3: **Covariates used for the preprocessing**

outcome	considered covariates for matching
<i>median salary</i>	classification of economic activity (4-digit level) firm size firm age category settlement structure development of median salary (2 years prior treatment) share high skilled empl. development of unemployment rate (2 years prior treatment) development of GDP per capita (2 years prior treatment)
<i>share high skilled empl.</i>	classification of economic activity (4-digit level) firm size category firm age category settlement structure median salary development of GDP per employee (2 years prior treatment) development of fiscal power (2 years prior treatment)
<i>share low skilled empl.</i>	median salary development of GDP per employee (2 years prior treatment) development of fiscal power (2 years prior treatment)
<i>share secure contracts</i>	classification of economic activity (4-digit level) share of full-time employees median salary
<i>number employees</i>	classification of economic activity (4-digit level) firm size category firm age category settlement structure median salary share high skilled empl. development of number of employees (2 years prior treatment) development of unemployment rate (2 years prior treatment) development of GDP per capita (2 years prior treatment)

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

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