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Philipp Grunau, Florian Hoffmann, Thomas Lemieux, Mirko Titze

Authors

Philipp Grunau
Institute for Employment Research (IAB)
E-mail: philipp.grunau@iab.de

Florian Hoffmann
Corresponding author
University of British Columbia and
Halle Institute for Economic Research (IWH) –
Member of the Leibniz Association,
Department of Structural Change and
Productivity
E-mail: florian.hoffmann@ubc.ca

Thomas Lemieux
University of British Columbia
E-mail: thomas.lemieux@ubc.ca

Mirko Titze
Halle Institute for Economic Research (IWH) –
Member of the Leibniz Association,
Centre for Evidence-based Policy Advice
(IWH-CEP), and Martin Luther University
Halle-Wittenberg
E-mail: mirko.titze@iwh-halle.de

Editor

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

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

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

www.iwh-halle.de

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Who Benefits from Place-based Policies? Evidence from Matched Employer-Employee Data*

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Abstract

We study the wage and employment effects of a German place-based policy using a research design that exploits conditionally exogenous EU-wide rules governing the program parameters at the regional level. The place-based program subsidizes investments to create jobs with a subsidy rate that varies across labor market regions. The analysis uses matched data on the universe of establishments and their employees, establishment-level panel data on program participation, and regional scores that generate spatial discontinuities in program eligibility and generosity. These rich data enable us to study the incidence of the place-based program on different groups of individuals. We find that the program helps establishments create jobs that disproportionately benefit younger and less-educated workers. Funded establishments increase their wages but, unlike employment, wage gains do not persist in the long run. Employment effects estimated at the local area level are slightly larger than establishment-level estimates, suggesting limited spillover effects. Using subsidy rates as an instrumental variable for actual subsidies indicates that it costs approximately EUR 25,000 to create a new job in the economically disadvantaged areas targeted by the program.

Keywords: local labor market, matched employer-employee data, place-based policies

JEL classification: D04, H25, J21, J31, J61

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1. Introduction

In most countries, governments grant subsidies or tax advantages to attract and financially support private firms in particular geographical areas. The typical motivation behind these "place-based" policies is to create jobs in economically disadvantaged regions. Despite the significant resources invested in place-based policies, their economic benefits remain controversial since they may create distortions or help finance investments that firms would have undertaken anyway.[1] These are significant concerns in light of the vast sums of money invested in these programs. Bartik (2020) estimates that the United States spends $60 billion annually on place-based policies. Since reunification in 1990, the German government has spent over a trillion Euros supporting firms, individuals, and local governments in economically disadvantaged Eastern Germany.

While many empirical studies suggest that place-based policies help increase employment in targeted areas, it remains largely unknown how they translate into firm-level labor-market decisions. Who precisely benefits from these policy interventions therefore remains an open question.[2] Do subsidized firms hire unemployed workers living in disadvantaged areas or recruit more skilled workers living elsewhere? Do other local firms hire more workers thanks to positive spillover effects – e.g., if they are suppliers of the subsidized firms –, or cut back employment in response to increased competition for a limited set of workers? Estimating the incidence of place-based policies on different groups of individuals is essential for evaluating their overall welfare impact but represents a major empirical challenge for two key reasons. First, place-based policies are often implemented in response to the declining economic fortunes of the targeted areas, making it challenging to estimate their causal impact on employment outcomes. Second, highly detailed data on workers and their employers are required for evaluating how subsidies to targeted firms affect the employment outcomes of different groups of individuals. Still, these types of data are rarely available.

We tackle these issues by studying the effect of a large place-based policy in Germany called the *Joint Federal Task for the Improvement of Regional Economic Structures* (henceforth GRW).[3] A first important feature of GRW is that local disbursement of the funds is highly constrained by pro-competition laws of the European Union. As in Criscuolo et al. (2019) and Siegloch, Wehrhöfer, and Etzel (2022), we leverage specific program rules set by the EU to estimate the causal impact of GRW on local outcomes. The scoring

[1] See Glaeser and Gottlieb (2008) for a review of the arguments in favor and against place-based policies.
[2] See the reviews by Kline and Moretti (2014b) and Neumark and Simpson (2015).
[3] GRW is the acronym for "**G**emeinschaftsaufgabe Verbesserung der **R**egionalen **W**irtschaftsstruktur"

model used by the program generates spatial discontinuities in policy parameters, most notably in the subsidy rate on the amount of investment establishments of a certain region are eligible for. Furthermore, since the EU operates and periodically negotiates the program's design with its member states, local governments are limited in their ability to manipulate how the subsidy borders are drawn or how generous the program parameters are. This sharply contrasts with the situation in other countries like the United States, where different levels of government have limited constraints on subsidizing firms in different local areas.

Another significant advantage of studying the GRW is the richness of the data available for estimating the wage and employment effects of the program at the establishment level and its incidence on different groups of individuals. Unlike existing studies that typically rely on measures of employment and related outcomes aggregated at the local level, we use the universe of German administrative data on employees liable to social security contributions and marginal employees, aggregated to the establishment level, to measure establishment-level outcomes, including the employment dynamics in stocks and flows, wages, and the skill structure. An important feature of these data is that, starting in 1999, they record both the place of residence and the place of work for each employee. This information is used to identify whether workers hired by subsidized establishments come from the local community or elsewhere. Note that we refer to “establishments” instead of “firms” from now on since our data are defined at the establishment level and do not provide firm identifiers.

Using record linkage on establishment names, postal addresses, and administrative establishment identifiers, we match establishment-level information on program participation, funding period, and the amount of subsidies received to the administrative employment data. These data are further matched to the complete set of parameters describing the GRW program on the regional level, including eligibility, subsidy rates for small, medium, and large establishments, and scores from the EU-approved GRW scoring models for the period 1999-2020. To the best of our knowledge, the resulting data set is the first in the literature on place-based policies to contain the universe of establishments and their employees, establishment-level panel data on program participation, and regional scores that generate spatial discontinuities in program eligibility.

With a few exceptions discussed in Section 2, the GRW parameters are set at the labor market region (LMR) level. LMRs are geographically connected groups of municipalities (Gemeinden) – the finest administrative units in Germany. Although neighboring municipalities located on different sides of LMR borders have much in common, they may be exposed to different GRW subsidy rates due to differences in funding scores of the LMR where they are located. We leverage this spatial variation in program parameters using matched difference-in-differences and event-study designs for pairs of municipalities or establishments located on different sides of LMR borders with different program parameters.

For the sake of comparison with the existing literature, we first present estimates of the impact of GRW at the municipality level. We next look at establishments that receive GRW subsidies, showing who gets hired when these establishments expand in response to government financial support (e.g., unemployed individuals, local workers previously employed by other firms in the same municipality, or commuters living in other municipalities), and what happens to their wages. This detailed establishment-level analysis, together with a credible causal design, are the main contributions of the paper. We also look at possible spillover effects on the employment decisions of other establishments located nearby.

In the case of the municipality-level analysis, we can directly implement a research design that leverages the spatial variation in GRW subsidy rates induced by EU policy rules. We present reduced-form (intent-to-treat) estimates of the effects of the subsidy rate on employment outcomes and instrumental variables (IV) estimates that show the impact of an extra euro of funding. In the latter case, the subsidy rate is used as an IV for total funding.

Developing a credible research design for causal effects at the establishment level is more difficult because the GRW is a discretionary program, so that selection into treatment is inherent. We address the selection issue using an event-study design where funded establishments are matched to comparable establishments in “donor” municipalities on the other side of the LMR border. Since we observe that employment in funded establishments grows faster than average in the years prior to receiving the GRW subsidy, we match establishments based on pre-funding employment trends and more standard covariates, such as industry affiliation and the average level of employment in the pre-funding period.

One disadvantage of matching on pre-trends is that we cannot test the validity of the event-study design by comparing these pre-trends for treatment and control establishments. We instead propose three alternative tests of the validity of our research design. First, since our data have a large set of outcomes that are not mechanically linked to the evolution of employment, such as hiring and separation rates, the skill structure of employees, or wages, we compare pre-trends in these additional outcomes for treatment and control establishments. Second, we compare the estimated establishment-level estimates to the municipality-level estimates that do not require matching on pre-trends. Third, we perform a placebo test using establishment in treated municipalities that do not self-select into treatment. All three tests support the validity of our research design.

The main findings of the paper are as follows. Consistent with existing studies like Criscuolo et al. (2019), our municipality-level estimates indicate that GRW subsidies increase employment. The IV estimates suggest it costs approximately EUR 20,000 in GRW investment subsidies to create one more job in a municipality. Establishment-level estimates of the cost per job are slightly larger (EUR 25,000), indicating that nearly all municipality-level employment gains come from subsidized establishments. Employment in non-subsidized establishments in the same municipality grows at about the same rate as control establishments in neighboring municipalities, indicating no large positive or negative spillover effects within the municipality. Quantitatively, the effect on treated establishments is large. Starting from an average of about 20 employees, treated establishments funded at the average subsidy rate add about seven more jobs over the post-event period relative to control establishments.

Turning to the incidence of the GRW program, we find that most of the employment increase is attributable to commuters living in other municipalities. Close to half of new hires also come from non-employment. These proportions are relatively similar to their baseline levels, indicating that commuters and previously non-employed workers do not disproportionately benefit from the GRW program. In contrast, GRW-induced employment expansion disproportionately benefits younger and less-educated workers. In terms of outcomes besides the level and composition of employment, labor churn slightly increases in subsidized establishments as both hiring and separations grow in the years after establishments receive their funding. Importantly, incumbent workers in funded establishments experience significant wage growth relative to workers in control firms in the medium run (up to five years after the funding event).

Our paper contributes to several important literatures. First, we contribute to the large empirical literature on the effect of place-based policies by providing causal estimates of the impact of these policies. Generally speaking, the empirical literature distinguishes two types of programs: The first one encompasses large groups of different interventions designated to larger geographical areas (e.g., Structural Policy in the EU or the *Zonenrandgebiet* program in West Germany [40km-band adjacent to the Iron Curtain during the Cold War]). The second type of program is targeted at relatively smaller geographical areas. It includes a narrower set of interventions such as Enterprise Zones (e.g., Tennessee Valley Authority and California Enterprise Zone) in the US, Enterprise Zone Policy in the UK, Zones Franches Urbaines in France), Empowerment Zones (e.g., Federal Empowerment Zone Program or Federal

Enterprise Community Program in the US), and other discretionary programs (e.g., Regional Selective Assistance in the UK, Law 488 in Italy, Joint Task for the Improvement of Regional Economic Structures in Germany). This literature is broadly discussed by Neumark and Simpson (2015) and What Works Centre (2016a,b).[4]

A significant limitation of many of these studies is that interventions are endogenously targeted to depressed areas, making it challenging to find a good comparison group. This is particularly an issue in the United States, where different levels of government have a lot of freedom in picking subsidized firms. Greenstone, Hornbeck, and Moretti (2010) use a creative "runner-up" design to tackle this issue, though their approach is only applicable to large "million dollar" plants. As in some of the above-cited European studies (e.g., Bronzini and de Blasio, 2006; Becker, Egger, and von Ehrlich, 2010; Criscuolo et al., 2019; Brachert, Dettmann, and Titze, 2018 & 2019; and Siegloch, Wehrhöfer, and Etzel, 2022), we can use a compelling research design that leverages program variation induced by EU-approved GRW scores. An important advantage of our paper is that we directly observe these GRW scores, while others, such as Criscuolo et al. (2019), have to infer the corresponding UK program scores indirectly.

Our paper also contributes to the growing literature looking at the impact of firm-level demand shocks on wage and employment outcomes. For example, Kline et al. (2019) estimate the effect of successful patents among small innovative firms, Garin and Silverio (2023) study the impact of export shocks, and Kroft et al. (2022) look at what happens to winning and losing construction firms that are bidding for procurement auctions. A key focus of these papers is to estimate whether firms need to pay higher wages to attract more workers in response to positive shocks. Such firm-specific labor supply curves are consistent with monopsony power in the labor market. In our case, the GRW subsidy is the source of the shock, and our wage estimates suggest that funded establishments have some market power.

[4] Recent papers include Becker, Egger and von Ehrlich (2010, 2012, 2013) for the EU Cohesion Policy, von Ehrlich and Seidel (2018) for the West-German Zonenrandgebiet program, Kline and Moretti (2014a) for the Tennessee Valley Authority program, Neumark and Kolko (2010) for the California Enterprise Zone program, Busso, Gregory and Kline (2013), Hanson and Rohlin (2013), Reynolds and Rohlin (2014) for US Empowerment Zones, Givord, Rathelot and Sillard (2013), Briant, Lafourcade and Schmutz (2015), Mayer, Mayneris and Py (2017) for the Zones Franches Urbaines program in France, Devereux, Griffith, Simpson (2007), Criscuolo, Martin, Overman, van Reenen (2019) for the Regional Selective Assistance Program in the UK, Bronzini and de Blasio (2006), Bernini and Pellegrini (2011), de Castris and Pellegrini (2012), Cerqua and Pellegrini (2014) for the Law 488 in Italy, Brachert, Dettmann and Titze (2018, 2019) and Siegloch, Wehrhöfer, and Etzel (2022) for the GRW program in Germany.

At a broader level, our findings on the incidence of place-based policies for different groups of individuals help illustrate where these policies fit relative to other redistribution policies (income assistance for poor households, earned income tax credits, etc.) used by governments around the world. In a recent theoretical contribution, Gaubert, Kline, and Yagan (2021) argue that since poor households are geographically concentrated, place-based policies redistributing income from one place to another can yield equity gains despite generating some economic distortions. Critical to this argument is who precisely benefits from place-based policies in economically depressed areas, an issue we can tackle thanks to the richness of our data.[5]

2. Institutional Context

2.1 Overview

This study focuses on the GRW program, the largest place-based policy measure in Germany, and its central instrument of regional economic policy. In this section we describe the design of the GRW for our sample period, which ends in 2020.[6] The program has two components: investment subsidies for establishments and municipality-level subsidies for business-related infrastructure, with two-thirds of the total budget going to the former.[7] The program's generosity is determined by subsidy rates that vary across three establishment-size categories and multiple eligibility groups. Only establishments with some supra-regional sales are eligible for the program.

Since its inception in 1969, the explicit goal of the GRW has been to close the gap in socio-economic outcomes between structurally weak regions and the rest of Germany. As a member state of the European Union (EU), Germany does not have complete autonomy over the design of the policy. Place-based policies in EU member states violate Article 107 of the "Treaty on the Functioning of the EU," which interprets state aid as distorting competition. On the other hand, economic, social, and territorial cohesion represent important goals and core values of the EU. As a compromise between these two competing goals, the EU introduced a rule-based process for the extent and the structure of state-level regional policies.

[5] Bartik (1996) uses longitudinal data to study who benefits from local demand shocks, though he does not explicitly focus on demand shocks induced by place-based policies.
[6] The GRW has undergone a major reform in 2022. This reform does not affect our study since it was implemented after our sample period and since it was unanticipated in the last year of our sample, 2020. We therefore continue to use the present tense.
[7] Strictly speaking, municipality-level subsidies can also be used for non-investment-related activities, as long as they help boost an area's competitiveness.

Three features of the GRW program are particularly important for our study. First, municipalities do not have any control over eligibility and the generosity of the GRW funds they are entitled to. Second, municipalities are explicitly forbidden to operate their own place-based policies.[8] Third, program eligibility and generosity are determined by a scoring model that generates spatial discontinuities.

2.2 The Geography of the GRW Program

Eligibility and program generosity of the GRW program varies at the level of "labor market regions" (LMRs), which are geographically connected groups of municipalities (Gemeinden) most comparable to U.S. commuting zones. There are many more municipalities than LMRs, and municipal borders cannot cross LMR borders.[9] Consequently, the GRW program design generates sharp regional discontinuities at the municipal level. Importantly, LMRs are not strongly related to the regional demarcation of the German public administration on the next higher levels, the counties (Kreise) and states (Bundeslaender).

There are, however, special cases where the EU allows variation in subsidy rates within LMRs. For example, states can argue that socioeconomic disparities within an LMR are too significant to be addressed by a common subsidy rate. The case of municipalities bordering Poland or the Czech Republic is practically more relevant. Here, the EU Commission allows adjustments to avoid excessively large disparities in program generosity across contiguous borders of its member countries. In our empirical analysis, we exclude these municipalities. This has a negligible impact on our results since most municipalities granted exceptions are not located on continuous borders of LMRs *within* Germany.

Figure 1 presents heat maps of program eligibility in the top panel and subsidy rates in the bottom panel around our sample's beginning (January 2000 to January 2004) and end (year 2017). Thin borders are for LMRs, and thick borders are for the 16 federal states. Several spatial patterns are worth highlighting. First, every LMR in Eastern Germany is eligible for GRW funds in both periods. Variation in subsidy rates in this part of Germany thus comes from program generosity, not program eligibility.[10] Second, in both periods, many LMRs in West Germany are eligible but are surrounded by non-eligible LMRs. Municipalities located

[8] Municipalities do have some discretion over the corporate tax rate (e.g. Fuest, Peichl and Siegloch (2018)) and some other small-scale economic activities, as long as they are sufficiently small not to violate EU competition law on "State Aid" ("EU-Beihilferecht").

[9] In 2017, the year we use for normalizing the geographic classification in our data, there were 258 LMRs and 11,052 municipalities, excluding unincorporated areas. Municipalities, whose administrative borders were determined historically, cannot manipulate on which side of a border of an LMR they are located.

[10] Siegloch, Wehrhöfer, and Etzel (2022) study the impact of the GRW using time variation in subsidy rates in Eastern Germany.

along each side of their borders are the primary "donors" for our treatment and control establishments. Third, for 2017, we observe several green-yellow "speckled" LMRs in the lower right corner of Germany. These are LMRs with within-variation of subsidy rates due to their location on the border with the Czech Republic, as discussed above.

Appendix Table 1 lists the share of GRW funds for each of the 16 federal states, separately for the three EU funding periods covered by our sample. East Germany received the lion's share of the funds for all three periods: 87 percent from 2000 to 2006, 86 percent from 2007 to 2013, and 80 percent from 2014 to 2020. The secular increase in the share of funds going to West Germany is primarily due to the increasingly poor relative performance of former industrial and coal regions in the states of North Rhine-Westphalia and Saarland. Furthermore, there is a substantial number of municipalities in East Germany whose eligibility status has been downgraded over time.

2.3 Legislative Framework

The legislative underpinning of the GRW program operates at two levels: the EU through its EU Commission and the German federal government in conjunction with its 16 states.[11] Besides reviewing place-based policy programs of its member states, the EU Commission sets, for periods of typically seven years ("funding periods"), a limit on the EU population share that is covered by such programs. The EU Commission also sets a simple rule: any region with a PPP-adjusted per capita GDP of less than 75% of the EU average qualifies automatically and is eligible for the highest subsidy rate.[12] In the German context, a one-dimensional score is used to further divide regions that satisfy the 75% rule into "A" regions that receive the maximum subsidy, and "B" regions where the subsidy is slightly lower (See Figure 1). This score is a weighted average of four indicators of regional economic strength: unemployment, average gross wages and salaries, quality of infrastructure, and employment projection.[13]

An EU member state can expand coverage beyond regions with an A- and B-status as long as it is formally reviewed and approved by the EU Commission. Germany added a first set of "C" regions with a lower subsidy rate meeting the EU-wide population share rule mentioned above.[14] Germany also added a

[11] Key legislative documents are listed in Appendix Table 3.
[12] These EU policies are described in the "Guidelines on National Regional Aid". For references, see the last column of Appendix Table 3.
[13] Weights by funding period are listed in Appendix Table 2.
[14] Due to the EU enlargement over our sample period and the relative poverty of countries that have joined since 2000, the EU has increased this share from 42.7% for the first funding period (2000 to 2006) to 47% for the third

further set of covered regions (typically D-regions) with an even lower subsidy rate with the approval of the EU Commission. Importantly, "A" to "D" regions (and subcategories within this broad set of regions in later periods) are ranked according to the same one-dimensional score. Cutoffs are set such that the population share falling into each eligibility group hits its targets. The remaining policy parameters, such as subsidy rates by establishment size and eligibility group, are determined by each member state, subject to EU rules for regional policies, and are written into "funding plans" (Rahmenplaene/ Koordinierungsrahmen).[15]

2.4 Implementation of subsidies

Subsidies are paid as shares of capital expenditures incurred by funded establishments or municipalities, and applications are only considered if they involve investment projects that pass a certain lower threshold for projected costs.[16] Establishments, municipalities, or firms with multiple establishments can file multiple applications per funding period. For establishments, funding can be used for expanding- or for opening a business. Typical examples for municipal projects qualifying for the GRW are road construction, infrastructure for industrial parks, or technology equipment for vocational schools, provided they are business related or otherwise help improving competitiveness. Conversations with the ministry administering the funding data indicated that the evaluation process is strict and rigorous. Yet, rejection rates are low because applicants deemed marginal tend to go through personal consultations with a local administrator of the GRW until the project is considered acceptable under formal eligibility criteria.

Since the primary goal of the GRW is improving employment rates in economically disadvantaged regions, funded establishments must guarantee that funding leads to job creation or helps avoid job destruction. The latter introduces substantial flexibility in how to interpret the employment effect of the funds from an administrative and legal point of view. In particular, it allows funded establishments to claim they need

funding period (2014 to 2020) to avoid reducing eligibility too drastically in richer countries. Despite this adjustment, Germany's eligible population share has decreased over these two periods from 34.89% to 25.85%.

[15] Legislative decisions are made by a coordination committee ("Koordinierungsausschuss") consisting of the Federal Minister for Economic Affairs and Energy, the Federal Minister of Finance and one Minister or Senator for Economic Affairs from each of the 16 Federal States. The committee votes on the program parameters, and decisions are reached by majority rule. For the duration of a funding plan, policy parameters remain constant. Until 2008, these funding plans were drafted annually. In many cases, changes between annual funding plans were so tiny that the coordination committee decided to change the administrative process and draft funding plans only in the case of substantial changes. For details, see Alm and Fisch (2014) and the references in Appendix Table 2.

[16] The planned investment expenditures have to exceed the threshold of 50% of the average amount of depreciation over the last 3 years before the application is filed. This goal is also achieved if the firm makes a self-commitment to increase the number of jobs by 15%.

government funding to avoid cutting jobs while treating subsidies as a pure windfall. In light of these issues, projects and the corresponding employment are monitored by public administrators of the GRW for violation of the program rules for up to five years after finishing the project. Withdrawal or payback of subsidies are enforced and do happen.

2.5 Some Descriptive Statistics of the GRW Policy

Table 1 provides key descriptive statistics of the GRW policy for each funding plan covered by our data. There were eight funding plans in total, two for the first EU funding period (2000 to 2006) and three each for the next two EU funding periods. Columns 4 to 8 describe the key policy parameters. Column 4 shows the list of eligibility groups, starting with the four groups (A to D) discussed earlier in the first funding plan. As explained in the table footnote, additional groups were added over time and no region of Germany qualified for the highest funding groups (A and B) satisfying the 75% rule after the EU expansion (2014-2020 funding period).

Columns 5 to 7 show the generosity of the subsidy program, which varies by eligibility group and establishment size. Rather than showing a complete list of the subsidy rates for each eligibility group, we display their ranges for each establishment size category. For example, the first funding plan offered a subsidy rate of 50% on capital costs for small establishments in A-areas, defined as those with less than 50 employees at the time of application. The corresponding numbers for medium-sized establishments (between 50 and 249 employees) and large establishments (at least 250 employees) were 50% and 35%, respectively. Generally speaking, the program is becoming less generous over time, especially for establishments located in labor market regions with the highest eligibility status. For example, by the end of the sample period, the highest subsidy rates for small, medium, and large establishments had decreased to 40%, 30%, and 20%, respectively. Interestingly, regardless of eligibility status, the GRW program offers the same subsidy rates for business-related infrastructures to any eligible municipality. The rate was 80% during the first funding plan, increased to 90% for the next two funding plans, and decreased to 60% after that unless a project is deemed to be of extraordinary importance.

The last four columns show several statistics that summarize the program's generosity. Column 9 contains the total budget in *current* Euro (EUR) for each funding plan. Since funding plans have differing lengths, numbers are not directly comparable across rows. When aggregating them to the funding periods instead, the total budget was 14.9 Billion euros from 2000 to 2006, 11.5 Billion euros from 2007 to 2013, and only 5.5 Billion euros from 2014 to 2020. Adjusting for inflation would yield an even larger decline in the total budget allocated to the GRW program. As discussed above, the main reason for this decline is the expansion of the EU. This is also reflected in the decrease in the number of projects funded for firms and municipalities, as shown in columns 10 and 11.

On the other hand, conditional on receiving funds, the program has not become less generous over time, as shown in the last two columns. The program paid EUR 16,710 per employee at the beginning of the sample period and increased to almost EUR 25,000 per employee for the most recent funding plan.[17] This is an increase in generosity even when accounting for inflation. Similarly, average funding per establishment has not displayed any apparent trends, ranging from approximately EUR 218,000 per year for a project for the funding plan beginning in January 2007 to EUR 283,000 per year for a project for the funding plan starting in February 2011. The GRW policy is thus comparatively generous. For example, the British Regional Selective Assistance Program evaluated by Criscuolo et al. (2019) paid only £56,000 (EUR 92,000) per project in the late 1990s and £36,000 (EUR 59,000) per project in the 2000s.

2.6 Other Programs

A major issue for evaluating place-based policies is that economically disadvantaged regions may qualify for multiple support programs or that some localities that do not receive federal funding create their own subsidy programs. An advantage of studying the GRW is that EU-level pro-competition laws do not allow other German place-based policies. Nonetheless, several other programs may confound the impact of the establishment-based GRW program.

First, the GRW also provides subsidies for municipal investment projects governed by the same rules as those for establishment-level projects. In the case of the establishment-level analysis, we control for municipal subsidies in our econometric models of establishment-level outcomes. Our results are not affected significantly by including this variable since spending on establishments and infrastructure projects are only weakly correlated in our data. Likewise, in the municipal-level analysis, we present results using either total GRW municipal-level funding with or without municipal investment projects.

A second program that channels funds into economically disadvantaged regions and generates a regional discontinuity is "Aufbau Ost," the federally funded economic policy established in the aftermath of the German Reunification in 1990 and attempts to close the gap in socio-economic outcomes between East-

[17] The subsidy per employee is computed by dividing the granted project-level subsidy by the projected employment at the end of the subsidy period (current employment plus the number of jobs committed to be created by the project).

and West Germany. With annual transfers between 60- and 80 billion euros since its inception in 1990, this vast program generates a regional discontinuity along the former “inner-German border”. Similarly, in the German context the policy parameters of the so-called EU Cohesion Policy vary, with very few exceptions, only across these two large regions. We show below that excluding municipalities along this border has little impact on our results.

Lastly, while the GRW explicitly targets relatively large investment projects carried out by establishments with some supra-regional business activity, the so-called “ERP-Regionalfoerderprogramm” provides regional aid to small establishments through below-market interest rate loans. Although the program relies on the same regional allocation mechanism as the GRW, it is unlikely to affect our results as it is much smaller in scope than the GRW.[18]

3. Empirical Strategy

To evaluate the labor market impact of the GRW program, we implement two complementary approaches at the municipality and establishment levels. In the case of municipalities, we estimate the effect of the GRW subsidy rate using a difference-in-differences (DiD) design. We next use an event-study design at the establishment level to see how the subsidy rate affects the employment choices of the treated establishments that receive subsidies. In both cases, we estimate the “pass-through” of an additional euro of GRW subsidies in terms of employment and related outcomes, using subsidy rates as an IV for total funds received by a municipality or an establishment. This provides a common metric for comparing our municipality and establishment-level estimates.

3.1 Municipality-Level Estimation Strategy

The main empirical challenge when estimating the effect of GRW subsidies on labor market outcomes is that eligibility and generosity of the program depend on LMR-wide economic conditions. This potentially confounds the causal effect of GRW funding if the subsidy rate increases after a negative LMR-wide shock. We address this challenge by leveraging the variation in subsidy rates induced by changes in the funding plans over the different periods. As discussed above, funding has become less generous over time in terms of the fraction of eligible LMRs and the level of subsidies. A critical aspect of our research design is that, besides the few exceptions discussed in Section 2, LMR eligibility and subsidy rates are solely a function of the funding scores we observe in our data.

[18] The same does in fact apply to the EFRE/ESF-programs, i.e. those programs funded by EU Cohesion policy.

These issues are best illustrated using a potential outcomes framework. Let y_{mt} represent an outcome of interest observed in municipality *m* in year *t*, such as employment per capita. The potential outcomes $y_{mt}(r)$ for different subsidy rates *r* are typically referred to as the dose-response function in the treatment effect literature (see, e.g., Imbens 2000). The observed subsidy rate R_{mt} is solely a function of the LMR funding score, $score_{mt}$, in which municipality *m* is located (we use the score rank in the empirical analysis). R_{mt} can be formally expressed as $R_{mt} = R_t(score_{mt})$, where the step function $R_t(\cdot)$ summarizes the parameters of the funding plan in year *t*. For instance, if LMRs with a score of 0.7 or above are eligible for a 50% subsidy while LMRs with a score below 0.7 are ineligible, we have $R_t(score) = 0$ if $score < 0.7$ and $R_t(score) = .5$ if $score \geq 0.7$.

In treatment effect models, the unconfoundedness or conditional independence assumption (CIA) states that conditional on observables, potential outcomes are independent of the treatment variable. In our setting, the assumption states that for each funding period *t,* we have:

$$y_{mt}(r) \perp R_{mt} \mid score_{mt} \text{ for all values of } r.$$

The CIA is typically viewed as a strong assumption since unobservable factors influencing treatment assignment may also be correlated with potential outcomes. In our setting, however, the CIA is trivially satisfied since the subsidy rate is a deterministic function $R_{mt} = R_t(Score_{mt})$ of the observable score. Conditional on, R_{mt} is a constant that is independent, by design, from the potential outcomes. This situation is similar to a regression discontinuity (RD) design where the CIA is also trivially satisfied (Imbens and Lemieux, 2008). Intuitively, this means that the causal effect of the subsidy rate can be obtained as long as we adequately control for the funding score.

We model the potential outcomes as a flexible function $g(\cdot)$ of the score in each funding period *t*:

$$E[y_{mt}(r) \mid score_{mt}] = \beta r + g(score_{mt}) + \mu_t \,, \tag{1}$$

where we assume, for the sake of simplicity, that the dose-response function is linear in the funding score. Year fixed effects μ_t are included to control for aggregate economic conditions that affect outcomes and could be correlated with the overall generosity of funding plans. Provided that the functional form restrictions in equation (1) are accurate, the CIA guarantees that β is interpretable as the causal effect of the subsidy rate on the outcome variable y.

Conditional on the observed subsidy rate and funding score, equation (1) leads to the following estimating equation:

$$y_{mt} = \beta R_{mt} + g(score_{mt}) + \alpha_m + \mu_t + \varepsilon_{mt}, \tag{2}$$

where α_m is a municipality-specific fixed effect, and ε_{mt} is an idiosyncratic error term. Under the CIA, α_m and ε_{mt} are independent of the treatment variable R_{mt} conditional on the funding score. Nonetheless, we include municipality-level fixed effects in the model to improve precision by absorbing time-invariant variation in outcomes across municipalities. Empirically, we model $g(\cdot)$ as a flexible function of the funding score that includes a quartic function of the score and a set of decile bin dummies.

Equation (2) imposes two important restrictions: *i)* homogeneity in the treatment effect across space and time, and *ii)* restrictions on the relationship between the funding score and outcomes. One way of relaxing these restrictions is to introduce interactions between the score and the subsidy rate and let the function $g(\cdot)$ vary over time. This is similar to the approach in the dose-response literature, where outcomes are modeled as a flexible function of the treatment variable and the propensity score.[19]

Instead, we pursue a second approach that takes advantage of the spatial discontinuity induced by the GRW program. As discussed in Section 2, since the funding score is set at the LMR level, two municipalities on different sides of the LMR border are often eligible for different subsidy rates even if they are plausibly experiencing similar economic conditions linked to the state of the local economy. Matching treated municipalities to their neighbors on the other side of the LMR helps relax the restrictions imposed by equation (2) in two ways. First, equation (2) imposes a parallel trend assumption by stating that, conditional on the score, all municipalities follow the same trend over time that is captured by the time effects, μ_t. The assumption would be violated if, for instance, poorer regions with a high funding score were growing faster than richer regions with a low funding score. Matching to a neighboring municipality ensures that treated and control municipalities follow similar trends.

Likewise, there may be heterogeneity in the treatment effect depending on the state of the local economy. For example, GRW subsidies may help create jobs in economically depressed regions but not in regions already close to full employment. This poses a challenge to the interpretability of the estimated parameter β in a dose-response model. Matching treated municipalities to neighboring municipalities

[19] For example, Hirano and Imbens (2004) parametrize the dose-response function using a quadratic function and an interaction with the propensity score, which is a useful single-dimensional measures of how covariates affect the probability of treatment. In our case, the propensity score can be replaced by the funding score which, by design, is the sole determinant of the probability of treatment. Using the propensity score would not be useful as it is a degenerate function of the score in our setting.

with similar economic conditions and (arguably) treatment effects ensures that the estimated β is interpretable as an average treatment effect.[20]

Appendix Figure 1 visualizes our spatial matching procedure using, as an example, the municipality "Hamm," which was eligible for funding in 2000-2004, while neighboring municipalities in different LMRs were not. Of Hamm's ten neighboring municipalities, six are contained in ineligible LMRs and are, therefore, used as controls. This defines the stratum shown in panel C, with Hamm colored in red as the treatment unit and the six contiguous control municipalities colored in pink.[21]

Although we view the establishment-level analysis as the paper's main contribution, comparing the two sets of estimates –at the municipality and establishment level– helps assess the validity of the establishment-level approach that relies on an arguably less compelling research design since establishments self-select into treatment. The municipality-level estimates are also more directly comparable to other studies that have exploited the structure of EU programs to estimate the effect of subsidies on local economic outcomes. For example, Criscuolo et al. (2019) studied the UK version of the program at the Ward level (roughly comparable to a German municipality), while Siegloch, Wehrhöfer, and Etzel (2022) studied the same GRW program at the county level in East Germany.[22]

The estimated effect of the subsidy rate (equation 2) is an intend-to-treat (ITT) effect that depends on the take-up of GRW funds and the causal effect of these funds on the outcomes of interest. We also estimate the treatment effect of a euro of subsidies using an instrumental variables approach where total per capita subsidies at the municipality level are instrumented with the subsidy rate. These estimates are easier to interpret than the ITT estimates as they indicate how GRW spending measured in euros (per capita)

[20] Consider two municipalities A and B where the subsidy rate decreases by 0.2 and 0.1, respectively, due to a decline in the generosity of the GRW program. If the treatment effects are $\beta_A = 1$ and $\beta_B = 3$, respectively, comparing changes in outcomes in the two municipalities will suggest a negative treatment effect since outcomes in municipality A will decline less (-0.2 x 1 = -0.2) than in municipality B (-0.1 x 3 = -0.3). In contrast, if both municipalities share the same treatment effects $\beta_A = \beta_B = 2$, the difference-in-differences will correctly yield an estimated treatment effect of 2.

[21] Panel D shows that case of a municipality, Ahlen, located on the other side of the LMR border. Other than Hamm, this municipality only has one other control municipality (Lippetal) located on the other side of the LMR border. Hamm and Lippetal are, therefore, used as controls for Ahlen.

[22] An important advantage of our study is that we directly observe the score (and weights on unemployment rate, etc.). In contrast, Criscuolo et al. (2019) have to construct proxies and use an IV approach instead of just controlling for the score. Siegloch, Wehrhöfer, and Etzel (2022) use a standard DiD design that relies on changes in the generosity of the GRW that we also exploit here, though they don't control for the funding score.

translates into employment growth and related outcomes. They are also directly comparable to corresponding estimates at the establishment level. We estimate the following model:

$$y_{mt} = \gamma F_{mt} + g(score_{mt}) + \alpha_m + \mu_t + \varepsilon_{mt}, \quad (3)$$

where GRW funding per capita, F_{mt}, is instrumented using R_{mt}. As in the ITT models, we flexibly control for the funding score, control for municipality and year effects, and match treated municipalities to control municipalities located on the other side of the LMR border. Note that we estimate equation (3) using a narrower measure of funding that only includes subsidies to private establishments and a broader one that also includes public business-related infrastructure subsidies.

3.2 Establishment-Level Event Studies

The paper's main contribution is to use an establishment-level event-study design to estimate the effect of GRW funding on establishment size, hiring rates, separation rates, the number of commuters among employees, the number of marginal employees, and the wage structure. [23] Unlike the municipality-level approach, we can measure in a direct and detailed way which pool of workers funded establishments hire from. This helps answer the long-standing question of whether establishment growth due to place-based policies comes at the expense of non-funded establishments and non-eligible regions or mainly consists of hiring non-employed individuals.

The establishment-level research design relies on a comparison between funded establishments in a treated municipality and unfunded establishments in neighboring municipalities where the GRW program is less generous. An important empirical challenge is that establishments self-select into treatment. We confront this challenge by matching funded and neighboring unfunded establishments based on pre-funding employment trends in addition to more standard covariates, such as industry affiliation and the average level of employment in the pre-funding period.[24]

An important difference relative to the municipality-level approach is that we can only perform an event-study analysis on establishments already operating before the funding event. In contrast, the estimated

[23] Examples of recent empirical research in labor economics with similar research designs are Kline, Petkova, Williams and Zidar (2019) and Jäger and Heining (2022), who estimate respectively the effect of patent allowance or worker deaths on various firm-level outcomes.

[24] From a program evaluation point of view, the matching procedure aims at identifying "complier" establishments in control municipalities that would have received subsidies had they been eligible, and estimating average treatment effects by comparing compliers in treatment and control municipalities. In contrast, municipality-level estimates can be interpreted as intent-to-treat (ITT) estimates that represent the fraction of complier establishments multiplied by the average treatment effect among compliers.

impact of the GRW at the municipality level includes the effect of the subsidies on the creation of new establishments (the extensive margin) and the impact on existing establishments (the intensive margin).

3.2.1. Econometric Approach

We limit the event study to establishments operating for at least nine consecutive years around treatment, including four years before the funding event.[25] This enables us to compare pre-trends of treated and untreated establishments for a long enough period. Let *i* index a funded or matched control establishment, and D_i indicate whether the establishment received funding. In terms of notation, it is convenient to divide establishments into a set of strata *s* that include a unique funded establishment and its matched controls. Let $t^e(s)$ represent the calendar year when the funding was awarded, and τ indicate the number of years since the funding event occurred. Consider the following event study model for a balanced sample of treatment and control establishments with four years of pre-treatment and five years of post-treatment observations:

$$y_{ist} = \sum_{\tau=-4}^{4} \mathbf{1}\{t = t^e(s) + \tau\} * \theta_\tau + D_i * R_{i,t^e(s)} * \sum_{\tau=-4}^{4} \mathbf{1}\{t = t^e(s) + \tau\} * \beta_\tau + \alpha_i + \mu_t + \varepsilon_{ist}, \quad (4)$$

where y_{ist} is the outcome variable of interest (employment, hiring, wages, etc.), α_i is an establishment-specific fixed effect, and μ_t is a calendar year effect. The coefficients of interest, $\{\beta_\tau\}$, represent the effect of the GRW subsidy rate on outcomes. The coefficients are normalized relative to $\tau = -1$, the last year before the treatment establishment received funding. We don't include control variables in the model besides a dummy variable indicating whether the municipality where an establishment is located received public business-related infrastructure subsidies.

As in most other studies based on an event-study design (see, for example, Miller, 2023, for a survey of recent work), the model in equation (4) contains a set of time effects μ_t at the national level that imposes a common trend assumption on all strata. Following Dube, Lester, and Reich (2010), our preferred specification is a highly flexible version of the model where μ_t is replaced with an unrestricted set of time effects μ_{st} at the strata level:

$$y_{ist} = D_i * R_{i,t^e(s)} * \sum_{\tau=-4}^{4} \mathbf{1}\{t = t^e(s) + \tau\} * \beta_\tau + \alpha_i + \mu_{st} + \varepsilon_{ist}, \quad (5)$$

where the first part of equation (4), representing the evolution of outcomes in control establishments, is captured by the strata-level time effects and has dropped out of the model.

[25] For expositional clarity we assume that funding is received in the year in which the project starts. We provide a more detailed discussion of this point in Section 4.

As for the municipality-level models, the $\{\beta_\tau\}$ are ITT estimates that depend on the causal effects of a euro of subsidies on outcomes and how the subsidy rate affects the subsidy amount. In addition to the ITT estimates obtained from equation (5), we also estimate the effect of the annual subsidy (in euros) on outcomes using the subsidy rate as an instrumental variable. To simplify the exposition, we present the IV estimates for the difference-in-differences version of equation (5) where the $\{\beta_\tau\}$ are assumed to be zero in the pre-treatment period ($\tau < 0$) and constant in the post-treatment period ($\tau \geq 0$).

3.2.2 Control establishment matches

Control establishments are selected in two steps. First, as in the case of municipalities, we select municipalities on the other side of the LMR border with a lower subsidy rate R_{mt}.[26] Second, we match treated establishments to all "donor" establishments in control municipalities based on two-digit industry, employment in the baseline period, and employment growth in the pre-period.

More specifically, we define baseline employment as the average establishment employment 3 and 4 years before the funding event, and employment growth as the difference between average employment 1 and 2 years before the funding event and baseline employment. We use these two-year employment averages to improve the matching quality by reducing the role of year-to-year employment variation. In the case of baseline employment, we keep all control establishments with baseline employment within 20 percent of the baseline employment of the funded establishment. [27] In the case of employment growth, we use two complementary approaches. First, based on the evidence that will be presented in Section 4 that the average growth rate of funded establishments is similar to the average growth rate on the top half of the control establishment distribution, we only keep control establishments from the top half of the distribution. Second, we use a more conventional "pairwise matching" approach where we keep control establishments with a growth rate within 20 percent of the growth rate of funded establishments. In cases where multiple control establishments satisfy all these matching requirements, we average outcomes over all matched control establishments. This yields a balanced sample with one treated and one control establishment observed over nine years in each stratum.

[26] In principle, we could limit our analysis to cases where neighboring municipalities are not eligible to the program (subsidy rate of zero). In practice, this substantial reduces the number of observations, and limits the analysis to West Germany since all municipalities in East Germany are eligible to the program.

[27] More details about the matching procedure, including how we modify the 20 percent matching criteria for very small establishment, are provided in Appendix 2.

Under the assumption of parallel trends conditional on matching, the $\{\beta_\tau\}$ can be interpreted as the causal effect of the subsidy rate. One challenge when matching on pre-trends, as we do here, is that the validity of the parallel trends assumption can no longer be verified by comparing pre-trends for treated and control establishment. We address this challenge in three ways. First, since we are matching on pre-trends in employment, we can still test for pre-trends in other outcomes, such as hiring or wages. Second, subject to the issues raised above (intensive vs extensive margin), we can compare the establishment-level estimates to the municipality-level ones that rely on an alternative research design. Third, we perform placebo tests by matching unfunded establishments in the treated municipality to control establishments in municipalities located on the other side of the LMR border. Since the unfunded establishments do not self-select into the GRW, a failed placebo test would suggest that treated and control municipalities are not on the same time trends, violating the parallel trends assumption.

Note that the second and third specification tests could also fail when establishment funding has positive spillover effects on other establishments in the same municipality. Under this scenario, municipal-level estimates should be larger than the establishment-level estimates since unfunded establishments benefit from the GRW subsidies awarded to the funded firms. Likewise, unfunded placebo firms would also be expected to benefit from GRW subsidies relative to control establishments.

4. Data and Descriptive Statistics

We use the universe of German administrative data on employees liable to social security contributions and marginal employees to construct an establishment-level panel data set that records the evolution of labor market variables such as (different types of) employment, worker flows, wages, and the number of commuters. We match the establishment data to the complete administrative records of the GRW policy, which provide detailed information for each project that was granted GRW subsidies since 1997. Data on the municipality-year level policy parameters come from the funding plans, which we digitized. Finally, various administrative reforms during our sample period, especially in East Germany, make a careful mapping between past and present regional codes necessary. To this end, we have constructed regional concordance tables. We discuss the main features of these data and provide some descriptive statistics in the rest of the section. More details are provided in Appendix 1.

4.1 Administrative Employment Data

We use the universe of German administrative social security data administered by the Institute for Employment Research (IAB) of the German Federal Employment Agency (BA) to construct our establishment-level panel data. The data come from mandatory notifications of wages submitted by employers for their entire workforce at least once per calendar year. Apart from wages, they contain worker and establishment identifiers and detailed information on worker and establishment characteristics, such as daily wages, age, educational attainment, and industry. Excluded are civil servants and individuals who are self-employed or short-term employed. We follow the convention in the empirical literature based on the same data and measure our establishment-level variables on June 30th of each year. A detailed data description can be found in Card, Kline, and Heining (2013) and Dauth, Findeisen, Moretti, and Suedekum (2017).

The establishment data are processed as follows. First, we construct our sample from the universe of establishments that reported a positive number of employees at least once between 2000 and 2016. These data are further aggregated at the municipality level to implement the municipality-level design discussed in Section 3.1. The sample starts in 2000 because our commuter variables in stocks and flows are constructed from two variables, the place of residence and the place of work, and the former is available in the IAB data only since 1999 (inflows- and outflows of commuters can only be computed starting in 2000). It stopped in 2016 because we needed four years of pre- and post-funding data in the event-study sample; 2020 is the last year for which the IAB data were available at the date of writing.

We limit our analysis to establishments located in counties that are (a) either eligible for the GRW policy at some point between 2000 and 2016 or that are (b) ineligible every year but share a border with a county in (a). The share of counties that satisfy one of these conditions is relatively high, approximately 65 percent. Counties excluded from the analysis are located in richer parts of Germany that would not be appropriate controls for the disadvantaged areas eligible for GRW funding.[28] We do not otherwise restrict the sample of workers used to construct the establishment-level data.

From these data, we construct a set of key outcome variables at the establishment level, including employment, the number of commuters, the number of "marginal jobs," worker inflows and outflows, and average daily wages. Workers are classified as holding a marginal job when their monthly earnings do not exceed EUR 450 per month. Commuters are employees who live and work in different municipalities. We also compute these outcomes by various subgroups (e.g., by education, commuters from other LMRs, etc.). Further details on these variables are provided in Appendix 1.

[28] Using a subsample of counties also help meet recent legal changes to usage of IAB data that prohibit unrestricted usage of the data.

4.2 Administrative GRW Project Data

Information on GRW funding comes from the Federal Office for Economic Affairs and Export Control (BAFA).[29] These data are recorded on the project level rather than the establishment level. For each project, we observe the name of the firm and the establishment identifier, the location of the investment, the date of application, and the date of acceptance. There are also multiple funding variables, including the funds applied for and the funds received.[30] An establishment may apply for multiple projects in the same funding period.[31] In our establishment-level event study, we treat these overlapping project periods as one "event" and use the year of project initiation as the event time.[32] In cases where an establishment receives subsidies for multiple projects with non-overlapping project dates, we follow Kline, Petkova, Williams, and Zidar (2020) and only keep first-time projects (first-time patent applicants in their context).

We next match the project data to our IAB sample via record linkage. Two key features of the data greatly simplify the matching process. First, the GRW and IAB data sets report information at the establishment level. Furthermore, starting in 2004, GRW data systematically report the administrative establishment identifiers of the German Federal Employment Agency.[33] In cases where we fail to match projects to IAB establishments using identifiers, we instead match based on an establishment's name and the location of its branch. As described below, we managed to match over 80% of the projects to the corresponding establishment. More details on the matching procedure are provided in Appendix 1.

4.3 Digitized Policy Parameters

The policy parameters governing eligibility and subsidy rates are available at the municipality level for each funding plan. [34] As such, they can be easily matched to our other data sources based on the German

[29] The BAFA has the status of a federal agency and is subordinated to the Federal Ministry for Economic Affairs and Climate Action.
[30] We computed funds as eligible cost * subsidy rate (both in actual numbers, as opposed to the target numbers).
[31] For example, an existing establishment may file two funding applications to (i) buy new equipment for an existing building, and (ii) expand by constructing a new building later on.
[32] On average, a project starts 55 days after proposal submission. At that point, establishments have already gone through a detailed consultation process with the funding agency. This also actively involves the bank through which the funds are remitted. A curious feature of this process is that almost all projects start before the state government formally gives a final approval for project GRW-funding. It is exactly because the bank lends to the establishment against future funding payments that we use the time of project initiation as event time.
[33] Establishment ID's are created and administered by the German Federal Employment Agency (BA). Other administrative units may adopt them, which is the case for the BAFA starting in 2004. For establishments that have filed multiple applications, the BAFA has carried out a backward-imputation for establishment identifiers.
[34] Subsidy rates in the published documents are gross values. This is slightly different from the UK context in Criscuolo et al. (2019) where the net grant equivalent (NGE) is published instead.

classification of municipalities ("Gemeindeverzeichnis"). The information obtained from digitizing the funding plans is summarized in columns 1 to 8 of Table 1. Regional scores are provided by the Federal Ministry for Economic Affairs and Climate Action (formerly the Federal Ministry for Economic Affairs and Energy). These are published at the level of LMRs. We use historical IAB data to generate, year by year, the mapping from municipalities to LMRs. For example, for the 2000-2004 funding plan, we used 1999 regional codes for municipalities and labor market regions.[35]

One challenge in constructing a consistent data set of policy parameters at the municipality level is that the definition of municipalities has changed over time due to (primarily) mergers and splits. We explain in Appendix 1 how we use municipal identifiers for 2017 as our baseline regional codes. If two municipalities merged before 2017, we combine them in these prior years and use a weighted average of their policy parameters in cases where they were located on different sides of LMRs. The problem is not as acute for the establishment-level analysis since we can assign policy parameters based on the municipality (based on historical codes) where the establishment was located at the time of funding.

4.4 Sample Sizes and Descriptive Statistics

Table 2 reports the sample sizes for our main data sets before and after imposing various restrictions and performing record linkages. Starting with the administrative employment data from which our establishment panel is built, Panel A shows that we have almost 7.7 million unique establishments, 61.7 million workers, and 208.5 million establishment-worker observations from 2000 to 2016. These numbers drop by about a third when we only keep border counties. As noted earlier, excluded counties are located in richer parts of Germany, which would not be a good counterfactual for eligible regions.

Turning to the GRW data, Panel B shows sample sizes at various stages of the matching process between the GRW data and the IAB data. The GRW funded 40,790 projects in 28,603 establishments over the 2000 to 2016 period. There are more projects than establishments since some establishments received funding for multiple GRW-funded projects. Panel B next shows that 80 percent of establishments that received GRW funding can be matched to the IAB data. The matching rate increases after establishment identifiers were introduced in the GRW data in 2004.[36]

[35] This approach takes into account that the composition of labor market regions can change over time, for example, if territorial administrative reforms lead to mergers of municipalities that were formerly located on different sides of a LMR border. In our sample, territorial reforms of this kind took place mostly in East Germany.

[36] Matching rates are also higher for projects than establishments. This is not surprising since such establishments have multiple entries in the GRW data base and thus more information for linkage.

The remaining rows of Panel B show that only a small fraction of funded establishments satisfy all requirements to be included in the event study. After dropping establishments with unusual characteristics that prevent matching to GRW data and narrowing the sample to border counties (row a) or municipalities (row b) that are used for the main part of our empirical analysis, we are left with 4.3 (2.8) million establishments, of which over 21 (13) thousand received GRW funding (the "treated" establishment in parentheses) in border counties (municipalities).[37] Row c next shows that 7,879 of these GRW-funded establishments are located in counties that share a border with a control county where the subsidy rate is lower. While all these establishments can be matched to control establishments in the same 2-digit industry (row d), we lose over half of the treated establishments that are not observed for nine consecutive years centered around the funding year or don't have matched control establishments that satisfy this requirement (row e). New establishments funded by the GRW –that are not observed in the pre-funding period– are dropped at that stage. We are left with 1,816 treated establishments after matching on baseline employment and employment growth (row f) and 286 treated establishments when only looking at the subsample of border municipalities (row g).

The first two columns of Panel C compare the evolution of pre-treatment average employment for the final event-study sample (row g of Panel B). As expected, the level and trends in average pre-treatment employment are similar for treated and control establishments since we match based on employment growth and levels. Given the relatively small number of treated establishments in the final sample, we also estimate the event study for the larger border counties sample (row f of Panel B) and when only matching on employment growth. The third and fourth rows of Panel C show the evolution of employment in the pre-period for the latter. Average employment among treated and especially control firms increases substantially as it is easier to find matches for larger firms when we no longer need to match on employment levels. As expected, the employment trends prior to treatment are similar in treated and control establishments since we are matching on employment growth.

[37] The approximately 500,000 establishments that are dropped between panel A and panel B are establishments we flagged as having "unlikely" histories in the establishment panel. The most frequent examples of such histories are ones including a change in 1-digit industry or a change in county of location. It is likely that such histories represent a recycling of establishment id's for two different establishments, one that ceases to exist and one that is newly created. In principle, giving the same id for different establishments is not allowed, but such histories in the data suggest that it happens sometime. Given how important geographical information is to our project we decided to drop these establishments altogether.

5. Municipality-level Results

In this section, we first report reduced-form estimates of the effect of the subsidy rate on labor market outcomes at the municipality level based on equation (2). We next estimate the effect of subsidy amounts on labor market outcomes using the subsidy rates as IV for subsidy amounts. As discussed in Section 3, our main estimates are obtained using a DiD approach where treated municipalities are matched to neighboring municipalities with a different subsidy status on the other side of the LMR border. We also report results for a broader sample where all municipalities located in border counties are included in the analysis, and counties on the other side of the LMR border are used as controls. Although treatment and control municipalities located on the two sides of an LMR border may be experiencing very similar economic circumstances, one concern is that the GRW may have spillover effects on the economic activity in control municipalities. The spillover effects could be negative (displacement effects) if firms in control municipalities relocate to take advantage of the subsidies, or positive if increased economic activity in treated municipalities generates additional demand in control municipalities.[38] These spillover effects, however, are less likely to occur when using broader geographic regions (LMRs instead of municipalities). For all labor market outcomes besides wages, we control for differences in population by normalizing outcomes relative to municipal population in the base periods. Standard errors in all models are clustered at the municipality level.

5.1 Reduced Form Estimates

Table 3 shows the (reduced form) effects of the subsidy rate when progressively adding richer controls to the regression specifications. The subsidy rate is normalized on a scale of 0 to 1 to simplify the interpretation of the results. Results for the sample of contiguous municipalities are reported in Panel A. In Panel B, contiguous and non-contiguous municipalities are aggregated at the LMR level. As expected from the design of the GRW that targets areas with poorer labor market outcomes, column (2) shows that the raw correlation between the subsidy rate and labor market outcomes is, in most cases, negative and statistically significant. The estimated effects increase and become statistically insignificant when we estimate a conventional DiD model with municipality (or LMR) and year fixed effects in column 3. Adding controls for the funding rank (columns 4-6) further increases the estimated effects, suggesting that conventional DiD estimates are severely biased due to area-specific shocks that affect labor market

[38] These spillover effects violate the stable unit treatment value assumption (SUTVA) used in equation (1) where potential outcomes do not depend on subsidy rate in neighboring municipalities.

outcomes and the subsidy rate (via the program rules). Comparing the results for the different specifications shows that controlling for funding ranking is critical for estimating the causal effects of the GRW program.

In light of these findings, we focus the remainder of the discussion on the richest specification with a quartic in the funding rank and a set of decile rank fixed effects (column 6). To help with interpretation, column 7 rescales the estimated effects in percentage points relative to the mean (column 1) when evaluated at the average value of the subsidy rate. For example, the estimated effect of 0.040 for employment in column 6 (first row of Panel A) indicates that switching the subsidy rate from 0 to 1 would increase the employment-population ratio by 4 percentage points, or 18 percent relative to the mean of 0.221.[39] Multiplying by the average subsidy rate of 0.34 yields a 6.16 percent increase relative to the mean, as reported in column 7.

The following two rows of Panel A show that the subsidy rate increases hiring but has no significant impact on separations. As per-capita hiring and separation rates shown in column 1 are approximately equal, these estimates translate into a clear increase in the raw number of hires relative to separations. Hence, the policy seems to meet its intended goal of increasing employment via job creation. The results at the LMR level (Panel B) show a similar impact on hiring but, surprisingly, a large effect on separations. This suggests that subsidies increase labor market churn, as workers hired in subsidized firms may be coming from other local firms (separating workers moving to another firm or into non-employment). We return to this issue when discussing the results at the establishment level in the next section of the paper.

A primary concern in urban economics is that place-based policies may have unintended regional spillovers that attenuate positive employment effects in the targeted areas. This would happen if the program created jobs that attract workers from non-eligible areas. A unique feature of our data is that we observe both the place of work and the place of residence of workers. We use this information to compute the establishment-level number of commuters who live and work in different municipalities. Column 1 indicates that about two-thirds of workers are commuters, though most of them live in the same LMR, the local area the GRW program is targeted at. Column 6 shows that the subsidy rate has a large and significant effect on the number of commuters employed in treated municipalities. When expressed

[39] The employment rate of 0.221 is low for several reasons. Most importantly, border municipalities are often commuting cities where residents work somewhere else. This explains why the employment rate is substantially larger (0.321) when looking at border LMRs instead (panel B). Furthermore, we are dividing by the total population instead of the working age population. Public servants and the self-employed workers are also not part of the employment count, as discussed in Section 4.1.

relative to the baseline mean in column 7, the estimates imply that commuter employment increases more (6.93 percent) than total employment (6.16 percent). Although the difference looks small, it gets larger when comparing commuters to non-commuters who live and work in the same municipality and for whom employment increases by 5.05 percent (not shown in the table).

These findings suggest that it may be challenging to target place-based policies at a very local level since most workers commute from elsewhere, confirming similar concerns raised in the case of employment zones in the United States (Busso et al., 2013). Targeting is less of an issue in the case of a place-based policy like the GRW that is set at the LMR level and covers a wide range of disadvantaged areas. Indeed, the averages reported in column 1 indicate that most commuters live in the same LMR. Although the effect of the subsidy rate (5.69 percent) is not as large as for all commuters, the pattern is reversed in the analysis at the LMR level presented in Panel B.

The remainder of the table shows that marginal employment (workers on part-time "mini-jobs") increases relatively less than total employment, suggesting that GRW subsidies increase job quality. However, this finding is not robust, as we find the opposite in the analysis at the LMR level. The next row shows that earnings per capita increase by 8.6 percent (5.8 percent at the LMR level). This is primarily due to employment effects, as the impact on earnings per worker in the last row of the table is small and negative. This negative wage effect may be due to composition effects if newly hired workers are younger and less educated than currently employed workers. We investigate this important issue in more detail in the empirical analysis at the establishment level.

As discussed earlier, the main advantage of estimating the model for border municipalities is that matched control municipalities on the other side of the LMR likely share similar local trends. A potential weakness of the approach is that the estimates may be biased because of spillover effects on adjacent control municipalities that cause a violation of SUTVA. Since these issues are less likely to prevail at the broader LMR level, the similarity of the results in Panel A and B suggests that spillover effects are, at best, very small. The overall employment effect is only slightly larger at the municipality level (6.2 percent) than at the LMR level (5.6 percent), and the difference is well within standard errors.

5.2 Instrumental Variable Estimates

We now turn to the IV estimates, where we estimate the effect of GRW funding on labor market outcomes, using the subsidy rate as an instrument. The first-stage estimates are reported in Table 4, both for the amount of subsidies paid to establishments with- and without the amount of subsidies for public business-related infrastructure projects. As in Table 3, we show estimates for various specifications but focus our discussion on the case with the richest set of controls for the funding rank reported in column 6. All specifications, including those with a very limited set of controls, indicate that higher subsidy rates translate into higher subsidy amounts per capita. The estimated effects are smaller for specifications with limited controls, suggesting that negative shocks that trigger higher subsidy rates reduce the number of investment projects. As in the case of the reduced form estimates, controlling for the funding rank corrects for this negative bias.

Our preferred estimates in column (6) suggest that an increase of the subsidy rate from 0 to 34 percent leads to a rise in total funding amounts to firms by approximately EUR 60 (=177*.34) per capita. The corresponding number for total funding amounts, including infrastructure projects, is EUR 67 per capita. These are sizeable amounts given that the unconditional sample averages of these variables, including zeros, are EUR 25.5 and EUR 29.2, respectively. Even with conservative clustering, these coefficients are precisely estimated, with t-statistics of over 9. Weak instrument tests, though not included in this table, indicate no evidence for a weak IV issue.

Comparing coefficient estimates in panels A and B indicates that the data aggregation level has little impact on first-stage coefficients for subsidies to firms only. In contrast, the estimates at the LMR level (Panel B) are substantially larger when including public infrastructure spending in our funding measure. A possible explanation is that the development of business-related infrastructure projects such as business parks is more likely to take place in the larger "centroids" of LMRs, which are located away from an LMR border.

The primary purpose of the IV (second stage) estimates reported in Table 5 is to show how euros of GRW spending translate into local labor market impacts. To help with interpretation, we express total subsidies in thousands of euros per capita. Looking at the effects on hiring (second row in the table), the estimated coefficient indicates that increasing per capita subsidies by EUR 1,000 increases the hiring rate by about 0.05, implying that it takes EUR 20,000 of subsidies to create an additional job. Note that we only report our findings for our preferred specification, corresponding to column (6) in the two preceding tables.

Columns (1) and (2) present the IV estimates for the baseline funding variable, which excludes spending on public infrastructure projects, both for the municipality-level and the LMR-level analysis. Columns (3) and (4) repeat the same exercise using total funding as instrumented variable. Looking at total employment effects first, the results indicate that EUR 1,000 of funding increases employment by slightly more than one-fifth of a job, with estimates ranging from 0.146 to 0.238 depending on specifications. Stated differently, it takes roughly EUR 5,000 of funding to increase employment by one worker. As employment is a stock variable, the EUR 5,000 amount should be interpreted as the yearly cost of sustaining an additional job.

As discussed above, the effect of EUR 1,000 of funding on hires is about 0.05, ranging from 0.043 to 0.070 depending on the specification. Focusing on a flow variable like hires arguably provides a more straightforward way of computing the cost of creating a new job, with a one-time infusion of EUR 20,000 of subsidies (EUR 1,000 ÷ 0.05) resulting in an additional hire. One potential complication when working at the municipality level is that aggregate hiring may lead to more labor market churn. For instance, if a funded establishment hires a worker from a competitor, another firm may have to hire another worker as a replacement. This suggests that net hiring (effect on hires minus the effect on separations) may provide a more accurate measure of the flow of new jobs created by the GRW funding. We return to this issue in the establishment-level analysis presented in the next section.

Most of the other results reported in the other rows of Table 5 have already been hinted at in the discussion of reduced-form estimates (the IV estimates are a rescaled version of these estimates). However, the IV estimates are particularly insightful in the case of earnings per capita reported in the second to last row in the table. Depending on the specifications, the estimates range from 9.14 to 17.43, for an average of about 14.26. Since the outcome variable is daily earnings, scaling it up at the annual level implies that a EUR 1,000 subsidy increases annual earnings by EUR 5,200. This large effect relative to the size of the subsidy is mainly driven by the employment effect. Multiplying the employment effect of 0.2 by average annual earnings per worker yields an effect of about EUR 3,900, which is close to the EUR 5,200 directly estimated using earnings per capita as the outcome variable.[40]

Although the municipality-level estimates are interesting for their own sake, we mostly view them as benchmark estimates based on a compelling research design against which we can compare our establishment-level estimates to which we turn next. It is also re-assuring to find that our preferred estimate of the cost of creating a job based on the IV estimates for hires (EUR 20,000) is in the same range as those of Criscuolo et al. (2019) and Siegloch, Wehrhöfer, and Etzel (2022) who study the impact of similar programs at the local area level.

[40] Since daily earnings are averaged over calendar days, the average daily earnings in Table 4 are annualized by multiplying by 365. This yields annual earnings of EUR 19,540 when using the average of workers daily earnings at the municipality (EUR 48.42) and LMR (58.67) level.

6. Establishment-Level Results

In this section, we use the event-study design in equation (5) to estimate the impact of the GRW on a rich set of outcomes. As explained in Section 3.2, all treated establishments are matched to control establishments located in municipalities that share a common LMR border with municipalities where treated establishments are located but that qualify for lower, often zero GRW subsidy rates. Establishments are matched on the basis of 2-digit industry and, depending on specifications, baseline employment and/or employment growth in the pre-treatment period. We also report estimates based on a broader sample of establishments located in border counties instead of just border municipalities.

The rationale for matching based on the level and growth in employment in the pre-period is that establishments may be self-selecting into the GRW program. Figure 2 indeed shows that, even after matching on baseline employment, treated establishments grew faster than matched control establishments in the pre-period.[41] The figure compares average employment in treated establishments to average employment in control establishments for each of the four quartiles of pre-treatment employment growth (average employment in 1 and 2 years before treatment minus average employment 3 and 4 years before treatment). The figure shows that treated establishments (the dotted line) were growing at about the same rate as the two top quartiles of control establishments –faster than Q3 but slower than Q4—prior to treatment. Employment in treated establishments diverges up, however, when GRW funding starts being received at event time 0. Although Figure 2 provides clear evidence that GRW funding positively affects employment, it also suggests that the employment effect may be overstated if we fail to control for differences in pre-trends. For the sake of transparency, we first report event-study estimates where we only match based on employment level and then turn to our preferred estimates where we match based on the level and growth in employment in the pre-period.

6.1 Estimates: Matching on Initial Employment

Figure 3 shows the event-study estimates based on equation (5) and 95% confidence intervals for six key outcomes when we only match based on baseline employment (and 2-digit industry). The estimated coefficients $\{\beta_\tau\}$ displayed in the figure represent the effect of the GRW subsidy rate on outcomes and are normalized relative to $\tau = -1$, the last year before treated establishments received funding. As in

[41] The sample used in Figure 2 includes 316 treated establishment and 12,729 control establishments in the border municipality sample. Matching is based on the average employment 3 and 4 years prior to the funding event (Section 3.2). See Appendix 3 for more details.

the municipality-level analysis, the subsidy rate is normalized on a scale of 0 to 1. The coefficients need to be divided by about 3 to be interpreted as the impact of the average subsidy rate (0.37 in this sample). We later report these impacts in percentage terms in Table 6 to facilitate interpretation. No additional control variables are included in the model besides a dummy variable indicating whether the municipality in which an establishment is located received public business-related infrastructure subsidies. As noted earlier, we average employment for all control establishments matched to treated establishments. With 316 treated pairs observed over nine years that satisfy the matching requirements, the estimation sample consists of $5{,}688 = 316 * 2 * 9$ observations. The standard errors are clustered at the strata (treatment pair) level.

The first panel of Figure 3 plots the evolution of $\hat{\beta}_\tau$ for total employment. The estimated effects grow steadily over time and are significantly different from zero except in the transition year (event time zero) when GRW funding starts being received. In terms of magnitude, the coefficient of 20 four years after treatment means that about seven jobs are created at the average subsidy rate of 0.37. This is a large effect relative to a baseline employment of about 20 one year before treatment. However, the results have to be interpreted with caution due to the statistically significant pre-trends in total employment.

The next two panels show estimates of equation (5) for labor market flows rather than stocks. Consistent with the analysis at the municipality level, we find that funded establishments increase their size by intensifying recruitment activity rather than lowering worker separation rates, at least initially. Overall, worker turnover at the establishment level starts to grow two years after the funding event when separation rates begin to catch up, likely because of composition effects (separation rates are generally higher at lower levels of tenure). Interestingly, there is less evidence of pre-trends in hiring and separation rates than in total employment. This suggests that funded establishments start recruiting more aggressively after getting the GRW funding and that growth in overall employment is not just a windfall effect.

The next panel presents the event-study estimates for the number of commuters. As in the aggregate analysis, a large share of employment growth comes from hiring additional commuters. Slightly more than one half of the additional workers in the treated establishments are commuters. The event-study estimates are otherwise similar to the overall effects and exhibit a clear pre-trend. Likewise, there is a strong pre-trend in the number of workers in marginal jobs (fifth panel) and log daily earnings (last panel). In both cases, the growth in outcomes after treatment is no larger than before treatment.

A potential concern with earnings is that the composition of workers may be systemically changing over time. We show in Table 6 below that, as expected, newly-hired workers are younger and have, by definition, less tenure, two factors associated with lower earnings. The first panel of Figure 4 shows event-study estimates that control for composition by limiting the analysis to workers with at least five year of tenure. There is no longer evidence of pre-trends and the estimated treatment effects revolve around 0.1 in this alternative specification. Scaling the estimates using the average subsidy rate of 0.37 means that GRW funding increases the wages of incumbent workers by about four percentage points.

6.2 Estimates: Matching on Pre-Trends

6.2.1 Event-Study Estimates

Based on the evidence reported in Figure 2, a simple way of controlling for pre-trends is to only keep matched control establishments with employment growth above the median in the pre-period. The rationale for the approach is that since, on average, treated establishments grow as fast as above-median control establishments, keeping the latter set of control establishments should achieve balance in growth rates. We only keep treated establishments with at least two matched control establishments to have at least one above the median. As shown below, we find similar results using a conventional "pairwise matching" approach where control establishments with a pre-treatment growth rate within 20 percent of the pre-treatment growth rate of treated establishments are used in the estimation.[42]

The event-study estimates when matching on pre-trends using the above-median approach are reported in Figure 5. As expected, there is no longer a pre-trend for total employment in the first panel of the figure. Interestingly, the estimated treatment effects (0 to 4 years after the funding event) are very similar to those obtained when only controlling for the baseline employment levels in Figure 3. Likewise, except for log daily earnings, the treatment effects for other outcomes shown in Figure 5 are similar to those without controls for employment pre-trends. Furthermore, since we match based on employment pre-trends, there is no mechanical reason why there should not be pre-trends in outcomes besides employment reported in the figure. Thus, testing for pre-trends in other outcomes can be considered a specification test for the matching procedure. The lack of pre-trends for most of the other outcomes reported in Figure 5 helps bolster the validity of the establishment-level research design. While there are substantial pre-

[42] A potential downside of pairwise matching is that we are potentially "matching on noise" since there is considerable sampling variation in employment growth rates. Fortunately, these matching errors should cancel out when averaging over all treated pairs. Our two matching approaches are different ways of selecting control firms that grow faster than average in the pre-period. The fact the two approaches yield similar results is re-assuring.

trends for log daily earnings, they vanish once we control for composition effects by limiting the analysis to workers with at least five years of tenure in the second panel of Figure 4. The similarity in the earnings estimates obtained using the two matching approaches reported in Figure 4 provides strong evidence that an important consequence of GRW is to raise the earnings of incumbent workers.

6.2.2 Difference-in-Differences Estimates

Having graphically established that the GRW subsidy rate increases employment and earnings, we next report in Table 6 DiD estimates for a broader set of outcomes and matching strategies. Using a DiD approach reduces the dimensionality of the estimates by averaging the dynamic treatment effects over the five post-treatment years. Our preferred estimates based on the same contiguous municipalities sample used in the event study are reported in column 2, along with the sample means for that sample (column 1) and the estimated effects expressed in percentage terms (column 6). The next two columns report the estimates for larger samples obtained by broadening the set of establishments in border counties (column 3) and only matching on employment growth (column 4). The estimates based on the more conventional pairwise matching (on pre-trends) are reported in column 5.

Generally speaking, the estimates obtained using the various samples and matching procedures are similar except for employment outcomes when using the broader sample of contiguous border counties. Those estimates are systematically smaller than for other specifications. As will be discussed in the heterogeneity section below, the source of this difference is that estimated effects are substantially smaller for establishments in East Germany that are over-represented in the border county relative to the border municipality design. In light of this, we focus our discussion on the preferred sample of contiguous municipalities reported in column 2. The other results are discussed in more detail in Appendix 4.

As expected, the estimated effect of the subsidy rate on total employment of 13.11 (first row of Table 6) is more or less equal to the average of the corresponding five post-treatment coefficients plotted in Figure 5. Column 6 shows that this translates into a 21 percent increase in employment relative to the baseline when using the average GRW subsidy rate of 0.37. The next three rows report the employment effects separately by education group. As shown in column (1), about 20% of employees have no secondary degree, 70% have a secondary degree, including an apprenticeship, and the remaining 10% have a post-secondary degree. While the estimated effects for the first two groups more or less correspond to their share in the population, the effect is smaller for workers with a post-secondary degree. This translates into a 9 percent employment increase in column 6, compared to over 20 percent for the two other groups.

Thus, the first important finding is that more educated workers benefit relatively less from the GRW program than their less-educated counterparts.

Next, we decompose total hiring flows into three different origin states: hires from establishments in the same municipality, establishments in a different municipality, and non-employment.[43] The results indicate that treated firms increase their hiring rates uniformly by about 40 percent for each of these three origin states. If anything, the hiring effect is a bit lower for non-employed workers, indicating that the GRW is not disproportionately targeted at out-of-employment individuals. We also conduct the same decomposition for separations, where the three categories above are now the destination states. As in the municipality-level analysis, the overall effect of the subsidy rate on separations is small and not statistically significant. Looking across the three destination states, the effect on separation flows to employment in other municipalities is the only one that is statistically significant. In percentage terms, however, this effect remains considerably smaller than the hiring effects.

Although the DiD estimates for hires and separations indicate that the former largely dominates the latter in the medium run (average effect over five years after treatment), the event-study estimates suggest these average effects may be hiding interesting dynamics. Recall that separations gradually increase over time and approach the level of hires by the end of the sample period. We further explore this issue in Figure 6 when limiting the event study to establishments we observe for at least ten years after the funding event. As we further discuss in the next section, these longer-term estimates indicate that separations fully catch up with hires and the overall employment effect stabilizes in the 5 to 10-year period after the funding event.

As indicated in column 1 of Table 6, over half of the workers are commuters. The estimated effect of the GRW for all commuters (22 percent at the average subsidy rate) is more or less proportional to the effect for all workers (21 percent). The next three rows show that commuters living in the same LMR disproportionately benefit from the program (31 percent effect) relative to commuters from other LMRs. Since subsidy rates are set at the LMR level, this evidence indicates the GRW is relatively well-targeted since commuting does not dilute the effect of the policy by benefiting workers in non-targeted areas.

[43] In our data, individuals are classified as non-employed when they are either not working (with or without unemployment benefits) or hold a job that is not subject to social security contributions. The latter group is a relatively small share of the total workforce that is unlikely to represent the majority of cases we refer to as "non-employed". Another measurement issue is that since we do not observe the place of residence for the "non-employed", we cannot perform a decomposition by geographic origin.

Likewise, results reported in the next three rows indicate that, consistent with the findings for overall employment, more educated workers do not particularly benefit from the program. Commuter flows grow the fastest (34 percent effect) for workers without a secondary degree. The next row shows that the effect for workers holding marginal jobs is proportional to the overall employment effect, indicating that GRW funding does not affect this dimension of workforce composition.

Consistent with the evidence from the event study, the next set of results indicates that the GRW subsidy rate has a positive and significant impact of about 4 percent (at the average subsidy rate of 0.37) on daily wages. Although the effect is mainly driven by offsetting pre-trends (last panel of Figure 5), the composition-adjusted effect for workers with at least five years of tenure, for which there are no pre-trends, is only slightly smaller at about 3 percent. The next set of rows explores the heterogeneity in the estimated effects based on education, age, and whether the worker is a commuter or a marginally employed individual. We find no wage effects for less-skilled workers who hold a marginal job or do not have a secondary educational degree. A likely explanation for this finding is that less-skilled workers experience high unemployment and have a fairly elastic labor supply response in the depressed economic areas targeted by the GRW program. Commuters do not experience wage increases either. In contrast, we find positive wage effects for workers with higher educational attainment, particularly those with a post-secondary degree. Although the effects by education groups are imprecisely estimated in our preferred continuous municipality sample, we find similar and more precise results in the contiguous county sample in column 3. The wage effect is positive and significant for more educated workers and particularly high for those with a post-secondary degree. Finally, limiting the sample to workers aged 30 and above has little impact on the findings.

The final two rows of the table show that GRW funding reduces workers' average age and tenure in treated firms. Although these findings are not surprising, they provide additional evidence on the validity of the research design. If treated firms were on a steeper growth path regardless of funding, there is no particular reason why tenure would decline after the funding is received.

6.3 Additional Evidence: Long-Term Effects and Heterogeneity

Figure 6 shows longer-term estimates where we follow establishments up to 10 years after the funding event. The number of treated establishments drops from 286 to 164 as only establishments that receive funding up to 2010 can be followed for ten years since our sample ends in 2020. The event-study estimates for total employment reported in the first panel show that employment grows until about five years after the funding event and stabilizes after that. Importantly, there is no evidence of mean reversion after establishments stop receiving financial support a few years after the initial funding event (subsidies are typically paid over 2-3 years). This finding is consistent with the county-level event-study estimates of Siegloch et al. (2022), who show that GRW funding has a permanent impact in East Germany.

As discussed earlier, the next two panels in Figure 6 show that hiring sharply increases following the funding event but eventually stabilizes and decreases slightly until a new steady state is reached where hires are more or less equal to separations. The event-study estimates in the fourth (commuters) and fifth (marginal employment) panels follow the same dynamic pattern as for total employment. They grow steadily in the first five years post-treatment and stabilize after that. As in Table 6, the magnitude of the estimated effects is roughly proportional to the fraction of workers who commute or hold marginal jobs, indicating that employment gains induced by GRW subsidies do not substantially change the composition of employment. The evidence for wages in the last panel is noisier but suggests that the early wage gains are transitory. Wages revert to their pre-treatment level after the significant ramp-up in hiring is over about five years after the treatment. Note that due to the composition effects uncovered earlier, we are reporting the wage estimates for workers with at least five years of tenure in Figure 6.

We next explore in Table 7 how the DiD estimates of the effect of the subsidy rate vary for subgroups of establishments based on geography and establishment characteristics (industry and establishment size). Panels A and B report the findings for the sample of contiguous municipalities and counties, respectively. Estimates for a more detailed industry breakdown for the larger contiguous county sample are shown in Panel C. We summarize the main findings here and provide a more detailed discussion in Appendix 5.

The first important finding is that the estimated effects remain positive and significant for most subgroups shown in the table. Consistent with the findings for all establishments (first column in Panels A and B), the one exception is that the impact on separations is small and insignificant in most cases. The second important finding is that the estimated effects in East- and West-Germany are substantially different. The GRW subsidy rate has a considerably lower impact in the East than in the West for both the contiguous municipality and county samples. Interestingly, most of the difference between the average effect between these two samples is due to composition effects. Only 30 percent (85 out of 286) of treated establishments are from the East in the contiguous municipality sample, compared to 55 percent (1,007 out of 1,816) in the contiguous county sample. The relatively small fraction of treated establishments in Eastern Germany may be surprising since over 80 percent of the GRW funding goes to that part of the country (Appendix Table 1). However, there are two challenges in finding matched control establishments in Eastern Germany. First, the lack of spatial variation in funding rates (Figure 1) means that most treated counties do not have a donor neighboring county with a lower funding rate. Second, fewer control establishments are available in neighboring areas because East Germany is less economically dense than West Germany. The problem is particularly acute at the municipality level.

Two possible explanations for the lower effect of the GRW policy in the East are that, compared to West Germany, establishments are smaller and more concentrated in manufacturing. This results in substantial composition effects since columns (6) to (9) show that the estimated effects are generally lower in manufacturing (non-service sector) and for small establishments. Note, however, that although the employment effect is lower for small establishments, it is larger in relative terms since baseline employment is only about 5 workers for small establishments, compared to more than 30 workers for larger establishments.[44] Another possible explanation is that recruiting commuters may be more challenging due to the East's lower population density. We leave a more detailed investigation of East-West differences in the effectiveness of place-based policies to future work since the lack of policy variation in Eastern Germany limits what can be learned from our research design.

The last panel of Table 7 shows how the estimated effect of the subsidy rate varies across six main industry groups. Over half of the funded establishments are in the manufacturing sector. The estimated effects in manufacturing are slightly lower than average, likely because larger subsidies are required to create the same number of jobs in this more capital-intensive sector. The estimated effect in other sectors is relatively close to the average except for “Trade and Transportation” where it is larger. The effects are small and insignificant in the “Communications, Finance, Insurance, Real Estate,” although these results have to be interpreted with caution due to small sample sizes.

6.4 Placebo Analysis

Our main empirical strategy for dealing with pre-trends is to match treated establishments to control establishments with similar employment growth in the pre-period. Although testing for employment pre-trends in this setting is no longer possible, we presented earlier two pieces of evidence in support of our identification strategy. First, there are no pre-trends in outcomes besides wages after matching on total

[44] Establishments are divided into a small and large group depending on whether baseline employment is below or above the median in the baseline period. Average employment for the two groups is 5.7 and 31.7 in Panel A, and 5.4 and 30.6 in Panel B. The larger relative employment effect for small establishments is consistent with the wage effect, which is also larger for small establishments.

employment pre-trends. Second, treatment effects are very similar with and without controls for pre-trends.

However, one remaining concern is that municipality-level trends affecting all establishments may systematically differ in treated and control municipalities. We formally test for this using placebo tests where treated establishments are replaced with untreated establishments in the same municipalities, using the same control establishments as before. Under the assumption of parallel municipality-level trends, none of the placebo treatment effects should be statistically significant. Interestingly, these placebo tests could fail if GRW funding has spillover effects on non-funded establishments. In the extreme case where the funded establishment hires all its additional workers from other establishments in the same municipality, treatment effects would be negative for placebo establishments. In contrast, if GRW funding has positive spillovers on other establishments due to agglomeration effects, placebo treatment effects would be positive.

Results from the placebo tests are shown in Figure 7 for the same outcomes as in Figure 3 and displayed in the same order. Relative to the analysis sample underlying Figure 3, we keep the same control establishments but replace the treated establishment with establishments located in the same municipality, belonging to the same 2-digit industry, and starting from the same initial level of employment. Since the typical treated municipality is relatively small, we only have placebo establishments for 168 of the 316 treated establishments. We use two alternative strategies to increase the number of placebo establishments. In Figure 8, we report results after dropping the requirement that either control establishments or placebos are matched on initial employment to the treated establishment. In Figure 9, we further drop the requirement that they are part of the same 2-digit industry and use all non-treated establishments as placebos.

As discussed in detail in Appendix 6, the placebo treatment effects reported in Figure 7-9 are small and rarely statistically significant. One interesting exception is wages (log daily earnings) that systematically increase after the funding event. Relative to the 10 percent benchmark effect for treated establishments (Figure 3), wages in the 2-digit industry increase by about 5 percent (Figures 7 and 8) and by about 1 percent for all establishments (Figure 9). These results are consistent with wage spillovers linked to competition where competitors in the same 2-digit industry have to respond more aggressively to the wage increases of funded firms than establishments in other sectors. Interestingly, unlike Siegloch et al. (2022), we do not find significant employment spillovers in other sectors, perhaps due to differences in the research design and the aggregation level (establishments here vs counties in Siegloch et al. 2022).

6.5 IV Estimates and the Cost of Job Creation

We end this section by presenting DiD estimates of the effect of (actually granted) subsidy amounts on establishment-level outcomes. As in the municipality-level analysis, we instrument subsidy amounts using the subsidy rate. We also normalize each establishment's employment outcomes relative to baseline employment (4 years before treatment) to make the results as comparable as possible to the municipality-level IV estimates that are normalized relative to the baseline population. Based on the event-study findings that most of the hiring in response to GRW funding takes place in the first five years and that the resulting employment increase is permanent, we focus the IV analysis on hiring rates.

Table 8 shows the first-stage and IV results for the same four specifications reported in the reduced form analysis in Table 6. The first-stage estimates are large and statistically significant. The outcome variable in the first-stage model is total subsidies per baseline worker annualized over the five years of the post-treatment period.[45] The estimated coefficient of 15.73 in column (1) implies that the annual flow of subsidies per worker is around EUR 5,000-6,000 at the average subsidy rate of 0.37. Summing up the flow over five years yields a total subsidy amount close to the mean reported at the bottom of the table. The IV estimates are all statistically significant and range from 0.026 to 0.066 depending on the specification. The average effect across all four specifications is 0.040, which is slightly lower than the corresponding estimates of 0.053 at the municipality level (average over the four specifications in Table 5). The similarity in the estimated effects at the establishment and municipality levels is reassuring since they are obtained using very different research designs.

Taken at face value, the larger effects of subsidies estimated at the municipality level are consistent with modest spillover effects on non-funded establishments. The difference in average coefficients suggests that for each job directly created by the GRW program, a third of a job ((0.053-0.040)/0.040 = 0.33) is created by non-funded establishments. This finding is consistent with Criscuolo et al. (2019) and Siegloch et al. (2022), who found local spillover effects using a different approach.[46] However, these results should be interpreted with caution for several reasons. First, our establishment-level analysis only looks at

[45] Strictly speaking, we allocate the full value of the subsidy in the first year of treatment since actual payments are differently staggered over time (typically over 2-3 years) for different funded establishments. Although we could instead divide this amount equally among all post-treatment periods, doing so would not matter in a conventional DiD model where treatment effects are averaged out over the post-period. In our setting where we include strata-specific time trends the two approaches yield very similar, though not identical, estimates.

[46] Criscuolo et al. (2019) and Siegloch et al. (2022) measure spillovers by looking at employment effects in industries that are not eligible for funding.

intensive margin effects for existing establishments. Employment effects may be different at the extensive margin (newly created establishments). Second, while we focus on average effects over five years at the establishment level, it is not clear what the corresponding period is for the DiD estimates at the municipality level. Third, we did not detect positive spillovers in the placebo analysis, perhaps because we didn't have enough statistical power to detect small spillover effects. Furthermore, the job creation cost is not very different when using municipality (EUR 1,000 / 0.053 = EUR 19,000) or establishment-level (EUR 1,000 / 0.040 = EUR 25,000) estimates.[47] Given our focus on the establishment-level estimates of who benefits from place-based policies, we leave a more detailed reconciliation of establishment and local area estimates for future work.

7. Taking Stock: Who Benefits from Place-Based Policies?

We end by returning to the main question asked at the beginning of the paper: Who benefits from place-based policies? At a high level, our findings suggest that most groups benefit from new employment opportunities created by the GRW program. This includes previously non-employed workers, local workers in the municipality where funded establishments are located, and commuters living in other municipalities. While the positive impact on the latter group suggests that individuals living in non-targeted areas may be benefiting from the program, most of these commuters come from a broader area (the LMR) directly targeted by the program. Furthermore, employment growth among commuters and other groups mentioned above is roughly proportional to their baseline share. One notable exception is workers with a lower or average level of education who disproportionately benefit from the GRW program relative to more highly educated workers. Another group that disproportionately benefits from the program is younger workers. Viewed under this angle, the GRW program achieves its goal by supporting new jobs in economically disadvantaged areas with little evidence these jobs go to more advantaged groups of workers. Furthermore, longer-term estimates suggest that employment gains linked to GRW funding last long after (at least 10 years) the initial funding event. In quantitative terms, employment increases by about 20 percent in treated relative to control establishments.

Although the GRW's primary aim is to support employment, we also find that wages in funded establishments increase by about 4 percent. Interestingly, the wage and employment effects are

[47] These amounts are consistent with recent international evidence, such as LaPointe and Sakabe (2022), who evaluate an indirect place-based subsidy on capital expenditures in Japan.

consistent with a labor supply elasticity of 6-7, which is similar to recent estimates in the monopsony literature (e.g., Lamadon et al., 2022). This suggests that funded establishments need to bid up wages to attract workers away from competitors in an imperfectly competitive labor market. Unlike the employment effects, however, the wage effects disproportionately benefit highly educated workers, perhaps due to labor market slack for less-skilled workers in the economically depressed local labor markets targeted by the GRW program. Although the wage effects partly offset distributional benefits on the employment side, wages revert back to their initial level after the initial period of employment expansion supported by the GRW program. Less-skilled workers still disproportionately benefit from the program in the medium and long run since employment effects persist while wage effects don't.

While conducting a full cost-benefit analysis of the GRW program is beyond the scope of the paper, our findings suggest that the program achieves desirable distributional goals at a relatively low cost of no more than EUR 25,000 per new job.[48] A policy consequence of our findings is that the declining generosity of the GRW program linked to EU enlargement has likely contributed to an increase in the spatial dispersion of labor market opportunities across Germany. Given the large differences in labor market conditions across space, it is unclear whether other policies can offset the GRW program's declining generosity.

[48] See Siegloch et al. (2022) for a detailed discussion of the costs and benefits of the GRW program in East Germany.

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TABLE 1 - KEY DESCRIPTORS OF THE GRW-PROGRAM, BY FUNDING PERIOD

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Funding Period (Europ. Union)	**Rahmenplan (Period)**	**Municipality Codes**	**Eligibility Groups**	**Subsidy Rates in Percentages of Investment Volume (Ranges)**				**Planned GRW Budget (Mill. €)**	**Number of Projects**		**Paid Subsidies per Employee**	**Av. subsidy of a establ. per year**
				Small Firms	***Med Firms***	***Lrg Firms***	***Municipal***		***Firms***	***Municipal***		
2000 - 2006	29 (Jan 2000 - Jan 2004)	1999	A - D	15 - 50	7.5 - 50	0 - 35	80	9,400.2	14,571	1,270	16,710	227,127.2
	33 (Feb 2004 - Dec 2006)	2003	A - E; B1 - C1	15 - 50	7.5 - 50	0 - 35	90	5,523.4	7,428	689	17,380	260,917.8
2007 - 2013	36 (Jan 2007 - Sep 2008)	2006	A - D; C1 - C5	20 - 50	10 - 40	0 - 30	90	2,682.5	4,564	345	17,369	217,804.2
	361 (Oct 2008 - Jan 2011)	2008	A - D; C1 - C5	20 - 50	10 - 40	0 - 30	60	4,067.0	6,549	583	21,006	232,342.7
	362 (Feb 2011 - Jun 2014)	2010	A - D; A1; C1 - C5	20 - 50	10 - 40	0 - 30	60	4,717.0	6,410	555	22,142	283,407.6
2014 - 2020	36310 (Jul 2014 - Dec 2016)	2013	C1p; C2p; Cnp; D	20 - 40	10 - 30	0 - 20	60	1,269.5	2,861	192	19,376	236,641.3
	36311 (Jan 2017 - Dec 2017)	2013	C1p; C2p; Cnp; D	20 - 40	10 - 30	0 - 20	60	1,027.8	1,843	34	26,129	259,504.4
	36320 (Jan 2018 - Dec 2020)	2013	C1p; C3p; Cnp; D	20 - 40	10 - 30	0 - 20	60	309.3	840	6	24,692	189,668.6

NOTES: This table shows key descriptive features of the GRW program, split by funding period ("Rahmenplan"). Column (3) shows the historical municipality codes used administratively for delineating geographical units. Columns (4) to (8) show the eligibility groups and the corresponding range of subsidy rates, expressed in percentages of the planned investment volume. The original four eligibility regions in 2000-2004 (A to D) were gradually expanded to include additional small categories (e.g., E, B1, and C1) with slightly different subsidy rates set on the basis of the one-dimensional score. Following the EU expansion, no region of Germany met the 75% rule required to qualify as an "A" or "B" region in the 2014-2020 funding period. "C" regions were divided into three subgroups, with "C1p" regions receiving the largest subsidy rate of 40 percent (less than the EU-allowed maximum of 50 percent for "A" regions). Rates differ by firm size and whether they fund private or public investments. Small firms are defined administratively as those with up to 49 employees, medium firms as those with 50 to 249 employees, and large firms as those with at least 250 employees. Subsidy rates of zero occur for large firms that are located in eligibility groups that provide positive rates to small- and medium, but not large firms. Data used for calculating the statistics in columns (9) to (13) come from the administrative GRW data and thus condition on funding. To calculate the variable in column (12), we use granted project-level subsidies and projected employment, the latter of which is the sum of employment at the time of the application plus additional jobs committed to being created by the project. Column (13) shows the average total funds paid to subsidized establishments per year of funding. All monetary variables are listed in current prices.

TABLE 2 - SAMPLE SIZES

PANEL A - MATCHED EMPLOYER-EMPLOYEE DATA, 2000-2016

	Unrestricted	Border Counties
Establishments	7,679,294	4,784,775
Workers	61,653,576	41,812,286
Establishments - Workers	208,500,000	124,600,000
Counties (2017 codes)	402	247
Municipalities (2017 codes)	11,053	7,468

PANEL B - GRW PROJECT DATA, 2000-2016

		Establishments (treated)	Projects
	Number of funded projects	28,603	40,790
Matching Rate *	***2000-2016***	80.3%	83.8%
	2000-2003	77.7%	80.8%
	2004-2016	80.8%	85.4%
Record-Matched Sample **	***a) Total in Border Counties***	4,285,630 (21,345)	32,678
	b) Total in Border Municipalities	2,820,966 (13,317)	20,035
	c) Treated Counties with lower subsidy rate control county	2,225,887 (7,879)	8,121
	d) c+ matching on 2dig Industry	749,751 (7,879)	8,121
	e) d+ 9 consecutive years around event time	214,284 (3,251)	3,288
	f) e + Balance on Empl. Levels and Empl. Growth	31,724 (1,816)	1,833
	g) f + Border Municipalities	7,093 (286)	290

NOTES:
* establishment ID systematically collected since 2004
** treated firms in brackets
*** This is also the sample from which the municipality-IV data are aggregated from.

PANEL C - EVENT-STUDY SAMPLES

	Balance on: Pre-Event Employment Levels- and Growth		Balance on: Pre-Event Employment Growth only	
	Treated	*Controls*	*Treated*	*Controls*
Establishments	286	6,807	744	39,792
Establishment-Year	2,574	69,867	6,696	590,625
Number of Employees				
4 years pre-event	17.9	17.2	23.8	36.3
3 years pre-event	18.5	19.0	24.7	38.5
2 years pre-event	20.0	20.7	26.1	40.4
1 year pre-event	21.6	21.9	27.6	41.6

TABLE 3 - REDUCED FORM ESTIMATES OF THE EFFECT OF THE SUBSIDY RATE AT THE MUNICIPALITY LEVEL

PANEL A: MUNICIPALITY-LEVEL

	Avg. (Std.)	**Effect of the subsidy rate (Avg = .34; Std = .12, conditional on eligibility)**					**% effect at average**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Employment	0.221	-0.082	-0.001	0.016	0.039	0.040	6.16
	(0.189)	*(0.0173)*	*(0.0077)*	*(0.0090)*	*(0.0104)*	*(0.0104)*	(1.60)
Hires	0.044	-0.019	0.002	0.005	0.009	0.009	7.21
	(0.038)	*(0.0031)*	*(0.0022)*	*(0.0024)*	*(0.0035)*	*(0.0035)*	(2.70)
Separations	0.041	-0.015	-0.001	0.001	0.004	0.003	2.76
	(0.036)	*(0.0028)*	*(0.0021)*	*(0.0024)*	*(0.0032)*	*(0.0032)*	(2.65)
Commuters	0.137	-0.037	0.003	0.015	0.027	0.028	6.93
	(0.158)	*(0.0139)*	*(0.0066)*	*(0.0080)*	*(0.0094)*	*(0.0095)*	(2.34)
from same LMR	0.083	0.018	0.003	0.007	0.013	0.014	5.69
	(0.088)	*(0.0079)*	*(0.0035)*	*(0.0042)*	*(0.0042)*	*(0.0043)*	(1.77)
from adjacent LMR	0.043	-0.048	0.001	0.006	0.006	0.006	5.09
	(0.082)	*(0.0065)*	*(0.0028)*	*(0.0034)*	*(0.0031)*	*(0.0032)*	(2.54)
from non-adjacent LMR	0.011	-0.007	-0.001	0.002	0.008	0.008	23.22
	(0.029)	*(0.0024)*	*(0.0023)*	*(0.0027)*	*(0.0058)*	*(0.0056)*	(16.99)
Marginally Employed	0.047	-0.064	-0.004	-0.002	0.006	0.005	3.77
	(0.033)	*(0.0028)*	*(0.0022)*	*(0.0022)*	*(0.0029)*	*(0.0028)*	(2.02)
Earnings per Capita	12.22	-6.61	-0.39	1.14	2.84	3.08	8.58
	(15.06)	*(1.38)*	*(0.70)*	*(0.91)*	*(0.91)*	*(0.94)*	(2.62)
Earnings per Worker	48.42	-3.22	-3.13	-3.09	-1.56	-1.59	-1.11
	(14.26)	*(1.39)*	*(0.83)*	*(0.89)*	*(0.94)*	*(0.96)*	(0.67)
Rank Control	-	*No*	*No*	*Linear*	*No*	*Quartic*	*Quartic*
Rank-Percentile FE	-	*No*	*No*	*No*	*Yes*	*Yes*	*Yes*
FE for Mun and Year		*No*	*Yes*	*Yes*	*Yes*	Yes	Yes
Observations				48,024			

PANEL B: LMR (LABOR MARKET REGION)-LEVEL

	Avg. (Std.)	Regression Coefficient on NGE (Avg = .35; Std = .12, conditional on eligibility)					% effect at average
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Employment	0.321	-0.060	-0.014	0.029	0.050	0.052	5.64
	(0.090)	*(0.035)*	*(0.014)*	*(0.018)*	*(0.013)*	*(0.013)*	(1.42)
Hires	0.063	-0.012	0.002	0.011	0.015	0.015	8.42
	(0.020)	*(0.007)*	*(0.004)*	*(0.004)*	*(0.005)*	*(0.005)*	(2.50)
Separations	0.060	-0.007	0.000	0.010	0.012	0.013	7.53
	(0.019)	*(0.007)*	*(0.004)*	*(0.004)*	*(0.004)*	*(0.004)*	(2.45)
Commuters	0.174	-0.038	-0.005	0.025	0.031	0.032	6.41
	(0.055)	*(0.020)*	*(0.009)*	*(0.012)*	*(0.009)*	*(0.009)*	(1.78)
from same LMR	0.093	0.016	-0.005	0.005	0.018	0.019	6.97
	0.040	*(0.014)*	*(0.005)*	*(0.005)*	*(0.005)*	*(0.005)*	(1.99)
from adjacent LMR	0.063	-0.048	0.001	0.016	0.013	0.013	7.26
	0.031	*(0.011)*	*(0.004)*	*(0.007)*	*(0.004)*	*(0.005)*	(2.50)
from non-adjacent LMR	0.018	-0.007	-0.001	0.004	0.001	0.000	0.70
	0.012	*(0.004)*	*(0.002)*	*(0.002)*	*(0.002)*	*(0.002)*	(4.48)
Marginally Employed	0.063	-0.067	-0.002	0.003	0.012	0.013	7.39
	(0.024)	*(0.0065)*	*(0.0035)*	*(0.0037)*	*(0.0036)*	*(0.0037)*	(1.97)
Earnings per Capita	19.38	-6.97	-2.00	2.61	2.84	3.23	5.84
	(7.74)	*(2.79)*	*(1.10)*	*(2.52)*	*(1.26)*	*(1.40)*	(2.45)
Earnings per Worker	58.67	-7.21	-4.85	-1.86	-4.25	-3.92	-2.34
	(8.74)	*(2.85)*	*(1.13)*	*(2.11)*	*(1.40)*	*(1.55)*	(0.90)
Rank Control	-	*No*	*No*	*Linear*	*No*	*Quartic*	*Quartic*
Rank-Percentile FE	-	*No*	*No*	*No*	*Yes*	*Yes*	*Yes*
FE for LMR and Year		*No*	*Yes*	*Yes*	*Yes*	Yes	Yes
Observations				3,200			

Notes: This table shows regression estimates of the effect of the subsidy rate on municipality-level outcomes. This is the reduced form of the IV specification. "LMR" stands for labor market region. All outcomes, with the exception of earnings per worker, are normalized by municipality-level population size in the initial sample year. The employment variables include all types of jobs, in particular full-time-, part-time- and mini-jobs. Commuters are workers whose municipality of employment differs from their municipality of residence. Earnings are aggregated over all employment spells in a sample year. Each coefficient comes from a separate regression. In panel A, the level of observation is municipality-year. Panel B aggregates further to the LMR level. The specification for the function in "rank" is allowed to vary freely between East- and West Germany before 2006 and the entire Germany after 2006. Column 7 shows the impact (in percentage terms relative to the average in column 1) of increasing the subsidy rate from zero to its average value among treated municipalities under the most general specification reported in column 6. Standard errors (in parentheses) are clustered by level of geographic aggregation (e.g municipalities in Panel A).

TABLE 4 - MUNICIPALITY-LEVEL FIRST-STAGE ESTIMATES OF THE EFFECT OF THE SUBSIDY RATE ON GRW FUNDING

	Avg. in EUR	Effect of the subsidy rate on GRW funding (1000s of EUR)				
	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A: MUNICIPALITY-LEVEL						
Subsidies to Firms	25.5	0.115	0.143	0.159	0.181	0.177
	(252)	*(0.011)*	*(0.016)*	*(0.019)*	*(0.021)*	*(0.020)*
Subsidies to Firms and Public Infrastructure	29.2	0.131	0.158	0.174	0.203	0.197
	(268)	*(0.013)*	*(0.018)*	*(0.021)*	*(0.024)*	*(0.022)*
Observations	48,024					
PANEL B: LMR (LABOR MARKET REGION)-LEVEL						
Subsidies to Firms, in 1000s of EUR (per capita)	25.5	0.113	0.135	0.147	0.217	0.218
	(69)	*(0.011)*	*(0.015)*	*(0.015)*	*(0.032)*	*(0.032)*
Subsidies to Firms and Public Infrastr. (per capita)	48.2	0.218	0.228	0.258	0.364	0.354
	(153)	*(0.035)*	*(0.036)*	*(0.041)*	*(0.070)*	*(0.069)*
Observations	3,200					
Rank Control	-	*No*	*No*	*Linear*	*No*	*Quartic*
Rank-Percentile FE	-	*No*	*No*	*No*	*Yes*	*Yes*
FE for Mun/LMR and Year		*No*	*Yes*	*Yes*	*Yes*	Yes

Notes: This table shows estimates from regressing municipality-level subsidy amounts in EUR on the subsidy rate. This is the first stage of our IV specification. We consider two measures for subsidies. The first is the sum over all GRW subsidies paid to any establishment in a municipality-year cell. The second adds to this the total amount GRW subsidy amount for public infrastructure projects. We normalize both measures by year-2000 population size. Each coefficient comes from a different regression. In panel A, the level of observation is municipality-year. Panel B aggregates further to the LMR-level. The specification for the function in "rank" is allowed to vary freely between East- and West Germany before 2006 and the entire Germany after 2006. Standard deviations are reported in parentheses for the means in column 1. Standard errors (in parentheses) are clustered by level of geographic aggregation (e.g., municipalities in Panel A) in the regression models reported in columns 2-6.

TABLE 5 - MUNICIPALITY-LEVEL IV ESTIMATES: CAUSAL IMPACTS OF 1,000 EUR OF ADDITIONAL GRW FUNDS

	Instrumented: Total Subsidies to Firms		**Instrumented: Total Subsidies to Firms and Public Infrastructure**	
	(1)	(2)	(3)	(4)
	Municipality-Level	LMR-Level	Municipality-Level	LMR-Level
Employment	0.227	0.238	0.203	0.146
	(0.065)	*(0.073)*	*(0.058)*	*(0.049)*
Hires	0.052	0.070	0.047	0.043
	(0.020)	*(0.021)*	*(0.018)*	*(0.014)*
Separations	0.019	0.059	0.017	0.037
	(0.018)	*(0.019)*	*(0.016)*	*(0.012)*
Commuters	0.158	0.147	0.142	0.090
	(0.057)	*(0.049)*	*(0.051)*	*(0.033)*
from same LMR	0.078	0.085	0.070	0.052
	(0.027)	*(0.027)*	*(0.024)*	*(0.018)*
from adjacent LMR	0.037	0.060	0.033	0.037
	(0.019)	*(0.024)*	*(0.017)*	*(0.016)*
from non-adjacent LMR	0.043	0.002	0.039	0.001
	(0.032)	*(0.011)*	*(0.029)*	*(0.007)*
Marginally Employed	0.030	0.061	0.027	0.038
	(0.016)	*(0.018)*	*(0.014)*	*(0.011)*
Earnings per Capita	17.43	14.85	15.63	9.14
	(5.79)	*(7.20)*	*(5.20)*	*(4.57)*
Earnings per Worker	-8.97	-18.01	-8.04	-11.08
	(5.45)	*(5.99)*	*(4.88)*	*(3.65)*
Observations	48,024	3,200	48,024	3,200

Notes: This table shows 2SLS estimates of GRW subsidy amounts on municipality-level outcomes. This is the 2nd-stage of our IV specification, where we use the subsidy rate as IV. All outcomes are normalized by year-2000 population size. Each coefficient comes from a different regression. The table only displays results for our most flexible specification, corresponding to column 6 in tables 3 and 4. They include fixed effects for municipalities and years together with rank decile fixed effects and a quarter in rank. The specification for the function in "rank" is allowed to vary freely between East- and West Germany before 2006 and the entire Germany after 2006. Standard errors (in parentheses) are clustered by level of geographic aggregation.

TABLE 6 - ESTABLISHMENT-LEVEL DIFFERENCE-IN-DIFFERENCES ESTIMATES OF THE EFFECT OF THE SUBSIDY RATE

	Avg. (Std.)	Matching on Initial Level and Pre-Event Employment Growth		Alternative Matching Approaches (both using contiguous municipalities)		% effect at average
	(1)	(2)	(3)	(4)	(5)	(6)
	Main Sample (Contiguous Municipalities)	Contiguous Municipalities	Contiguous Counties	No Matching on Initial Employment Levels	"Pairwise Match" on Pre-Event Employment Growth	Contiguous Municipalities (column 2)
Employment	23.2	13.11	7.71	8.95	12.55	20.87
	(29.54)	*(2.929)*	*(0.912)*	*(2.658)*	*(1.584)*	(4.66)
no secondary degree	4.3	2.76	1.31	2.34	2.52	23.53
	(8.23)	*(0.730)*	*(0.226)*	*(0.356)*	*(0.434)*	(6.23)
secondary degree	16.4	9.73	6.13	6.17	9.08	21.93
	(21.26)	*(2.318)*	*(0.656)*	*(2.362)*	*(1.215)*	(5.22)
post-secondary degree	2.5	0.62	0.28	0.44	0.96	9.23
	(5.37)	*(0.360)*	*(0.234)*	*(0.325)*	*(0.336)*	(5.34)
Hires	4.4	5.15	2.63	4.38	3.60	43.36
	(7.71)	*(1.057)*	*(0.501)*	*(0.510)*	*(0.562)*	(8.90)
same municipality	0.81	1.00	-0.37	0.60	0.37	45.74
	(3.52)	*(0.534)*	*(0.519)*	*(0.203)*	*(0.272)*	(24.53)
different municipality	1.6	1.69	1.21	2.25	1.79	40.32
	(2.95)	*(0.396)*	*(0.134)*	*(0.315)*	*(0.270)*	(9.42)
non-employment	2.1	2.20	1.40	1.97	1.72	38.62
	(3.69)	*(0.471)*	*(0.139)*	*(0.326)*	*(0.381)*	(8.25)
Separations	3.0	1.00	-0.03	1.14	2.16	12.22
	(4.60)	*(0.585)*	*(0.250)*	*(0.384)*	*(0.504)*	(7.16)
same municipality	0.48	-0.10	-0.06	-0.01	0.08	-7.94
	(1.10)	*(0.143)*	*(0.070)*	*(0.148)*	*(0.120)*	(11.00)
different municipality	1.09	0.75	0.26	0.85	1.15	25.49
	(2.03)	*(0.290)*	*(0.095)*	*(0.229)*	*(0.294)*	(9.87)
non-employment	1.6	0.43	0.13	0.30	0.74	10.14
	(2.59)	*(0.315)*	*(0.103)*	*(0.232)*	*(0.265)*	(7.39)
Commuters	12.4	7.39	5.60	5.26	8.09	22.06
	(17.49)	*(1.588)*	*(0.776)*	*(2.285)*	*(1.024)*	(4.74)
from same LMR	4.6	3.82	3.82	2.89	4.27	30.74
	(7.11)	*(0.893)*	*(0.476)*	*(1.518)*	*(0.534)*	(7.18)
from adjacent LMR	6.0	2.59	1.35	1.82	3.18	15.89
	(10.61)	*(0.715)*	*(0.288)*	*(0.798)*	*(0.594)*	(4.38)
from non-adjacent LMR	1.7	0.97	0.44	0.55	0.64	20.49
	(4.34)	*(0.497)*	*(0.199)*	*(0.262)*	*(0.210)*	(10.54)
no secondary degree	1.9	1.73	1.19	1.27	1.36	33.96
	(4.31)	*(0.455)*	*(0.327)*	*(0.229)*	*(0.285)*	(8.91)
secondary degree	8.9	5.07	4.03	3.62	5.86	21.11
	(12.82)	*(1.240)*	*(0.541)*	*(1.947)*	*(0.785)*	(5.16)
post-secondary degree	1.6	0.58	0.39	0.37	0.87	13.33
	(3.74)	*(0.211)*	*(0.119)*	*(0.281)*	*(0.223)*	(4.83)
Marginally Employed	3.2	1.63	0.94	1.15	1.41	18.67
	(6.38)	*(0.490)*	*(0.379)*	*(0.316)*	*(0.401)*	(5.62)

TABLE 6 - CONTINUATION

	Avg. (Std.)	Matching on Initial Level and Pre-Event Employment Growth		Alternative Matching Approaches (both using contiguous municipalities)		% effect at average
	(1)	(2)	(3)	(4)	(5)	(6)
	Main Sample (Contiguous Municipalities)	Contiguous Municipalities	Contiguous Counties	No Matching on Initial Employment Levels	"Pairwise Match" on Pre-Event Employment Growth	Contiguous Municipalities (column 2)
log Daily Earnings	4.0	0.10	0.13	0.18	0.14	3.77
	(0.48)	*(0.033)*	*(0.013)*	*(0.027)*	*(0.034)*	(1.22)
no secondary degree	3.6	-0.01	0.03	-0.03	-0.13	-0.55
	(0.59)	*(0.081)*	*(0.029)*	*(0.050)*	*(0.066)*	(2.99)
secondary degree	4.2	0.03	0.05	0.05	0.04	1.10
	(0.34)	*(0.027)*	*(0.009)*	*(0.016)*	*(0.022)*	(1.01)
post-secondary degree	4.4	0.14	0.09	0.18	0.14	5.32
	(0.47)	*(0.080)*	*(0.027)*	*(0.042)*	*(0.061)*	(2.96)
older 30	4.3	0.08	0.06	0.07	0.07	2.89
	(0.37)	*(0.029)*	*(0.010)*	*(0.018)*	*(0.021)*	(1.07)
tenure > 5 years	4.3	0.08	0.06	0.06	0.09	3.07
	(0.38)	*(0.038)*	*(0.012)*	*(0.023)*	*(0.027)*	(1.40)
commuters	4.2	-0.01	0.03	0.02	0.06	-0.52
	(0.39)	*(0.036)*	*(0.014)*	*(0.023)*	*(0.034)*	(1.32)
marginal	2.2	0.01	0.11	0.04	0.02	0.26
	(0.34)	*(0.068)*	*(0.029)*	*(0.046)*	*(0.063)*	(2.51)
Age (in Years)	41.0	-1.89	-1.82	-3.12	-3.21	-1.71
	(5.10)	*(0.618)*	*(0.223)*	*(0.385)*	*(0.501)*	(0.56)
Firm tenure (in Years)	4.91	-1.65	-1.22	-1.77	-1.87	-12.44
	(2.480)	*(0.232)*	*(0.079)*	*(0.143)*	*(0.197)*	(1.75)
Nr of Strata		*286*	*1,816*	*744*	*468*	
Nr of Cells		*5,148*	*32,688*	*13,392*	*8,424*	

Notes: This table reports difference-in-differences estimates of the effect of the subsidy rate on treated establishment relateive to matched control establishments. Treatment and control establishments are compared over a period of 9 years. There are 4 pre-funding and 5 post-funding observations for each matched pair. Each coefficient comes from a different regression. All specifications include strata-specific time trends and establishment fixed effects. Columns in the table differ by how we carry out the matching. In column 2 we match establishments in border municipalities on initial establishment size and pre-event employment growth. In column 3 we broaden the sample to border counties rather than municipalities. The specification in column 4 only matches on pre-event employment growth. Column 5 shows results for a "pair-wise" matching-like approach by using control establishments in contiguous municipalities whose pre-event employment growth is contained in a symmetric window of +/-5% around the pre-event growth of the treated establishment. We show results for our 6 core outcomes and for additional outcomes that decompose them further. Column 7 shows the impact (in percentage terms relative to the average in column 1) of increasing the subsidy rate from zero to its average value among treated municipalities under the most general specification reported in column 6. Standard errors are clustered at the strata-level.

TABLE 7 - EVENT STUDY: DID-ESTIMATES, ROBUSTNESS AND TREATMENT HETEROGENEITY

PANEL A: CONTIGUOUS MUNICIPALITIES - SUBSAMPLE ANALYSIS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Full Sample	**Exclude Inner-German Border**	**East Germany**	**West Germany**	**Exclude Berlin**	**Non-Service Sector**	**Service Sector**	**Small Establ.**	**Large Establ.**
Employment	13.11	13.26	4.34	21.88	15.52	7.80	19.26	10.49	16.73
	(2.929)	*(3.130)*	*(1.685)*	*(5.657)*	*(3.621)*	*(2.595)*	*(5.564)*	*(2.434)*	*(6.547)*
Hires	5.15	5.39	1.43	8.93	6.29	3.58	6.98	3.48	7.53
	(1.057)	*(1.131)*	*(0.499)*	*(2.070)*	*(1.324)*	*(0.938)*	*(2.014)*	*(0.833)*	*(2.415)*
Separations	1.00	1.04	0.08	1.86	1.23	0.33	1.76	1.56	0.26
	(0.585)	*(0.624)*	*(0.543)*	*(1.038)*	*(0.684)*	*(0.540)*	*(1.084)*	*(0.612)*	*(1.155)*
Commuters	7.39	7.35	4.09	10.42	7.88	4.70	10.34	6.13	9.06
	(1.588)	*(1.679)*	*(1.477)*	*(2.751)*	*(1.902)*	*(1.840)*	*(2.626)*	*(1.539)*	*(3.243)*
Marginally Employed	1.63	1.68	-0.17	3.18	2.10	0.88	2.46	1.35	1.83
	(0.490)	*(0.518)*	*(0.357)*	*(0.862)*	*(0.614)*	*(0.403)*	*(0.916)*	*(0.388)*	*(1.028)*
log Daily Earnings	0.10	0.10	0.08	0.13	0.14	0.09	0.12	0.15	0.03
	(0.033)	*(0.035)*	*(0.042)*	*(0.051)*	*(0.039)*	*(0.034)*	*(0.059)*	*(0.051)*	*(0.032)*
Number of Strata	*286*	*275*	*85*	*201*	*249*	*139*	*147*	*149*	*137*
*Avg baseline employment**	*18.2*	*18.5*	*10.5*	*21.4*	*19.1*	*20.4*	*16.1*	*5.7*	*31.7*

PANEL B: CONTIGUOUS COUNTIES - SUBSAMPLE ANALYSIS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Full Sample	**Exclude Inner-German Border**	**East Germany**	**West Germany**	**Exclude Berlin**	**Non-Service Sector**	**Service Sector**	**Small Establ.**	**Large Establ.**
Employment	7.71	8.47	4.41	17.21	9.06	5.95	10.88	4.31	11.92
	(0.912)	*(1.005)*	*(0.992)*	*(2.037)*	*(0.915)*	*(1.188)*	*(1.416)*	*(0.861)*	*(1.740)*
Hires	2.63	2.75	1.24	6.62	3.28	1.69	4.35	1.29	4.31
	(0.501)	*(0.610)*	*(0.631)*	*(0.666)*	*(0.341)*	*(0.692)*	*(0.659)*	*(0.787)*	*(0.556)*
Separations	-0.03	0.19	-0.31	0.79	0.09	-0.16	0.25	0.10	-0.17
	(0.250)	*(0.215)*	*(0.297)*	*(0.458)*	*(0.275)*	*(0.196)*	*(0.606)*	*(0.334)*	*(0.379)*
Commuters	5.60	6.17	3.52	10.76	6.14	3.82	8.68	3.69	7.86
	(0.776)	*(0.670)*	*(0.911)*	*(1.458)*	*(0.882)*	*(0.978)*	*(1.285)*	*(0.718)*	*(1.466)*
Marginally Employed	0.94	0.73	0.60	1.77	1.05	0.36	1.89	1.09	0.74
	(0.379)	*(0.250)*	*(0.500)*	*(0.455)*	*(0.435)*	*(0.194)*	*(0.972)*	*(0.610)*	*(0.410)*
log Daily Earnings	0.13	0.12	0.12	0.13	0.13	0.10	0.17	0.17	0.07
	(0.013)	*(0.015)*	*(0.015)*	*(0.026)*	*(0.014)*	*(0.015)*	*(0.024)*	*(0.021)*	*(0.012)*
Number of Strata	*1,816*	*1,456*	*1,007*	*809*	*1,645*	*1,118*	*690*	*913*	*903*
*Avg baseline employment**	*17.9*	*18.9*	*13.4*	*23.5*	*18.1*	*19.0*	*16.2*	*5.4*	*30.6*

PANEL C: CONTIGUOUS COUNTIES - DO EFFECTS VARY ACROSS INDUSTRIES?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Full Sample	**Manufacturing**	**Construction**	**Trade & Transportation**	**Hospitality**	**Communications, Finance, Insurance, Real Estate**	**Other Services, Public Admin**
Employment	7.71	5.67	7.16	16.42	5.26	-1.29	10.78
	(0.912)	*(1.411)*	*(1.508)*	*(2.708)*	*(1.266)*	*(1.834)*	*(3.290)*
Hires	2.63	1.78	1.30	5.45	2.87	0.77	5.15
	(0.501)	*(0.841)*	*(0.338)*	*(0.971)*	*(0.567)*	*(0.703)*	*(2.375)*
Separations	-0.03	-0.25	0.23	1.27	0.21	-1.20	-1.42
	(0.250)	*(0.229)*	*(0.316)*	*(0.689)*	*(0.434)*	*(0.484)*	*(2.558)*
Commuters	5.60	3.57	4.98	10.83	4.44	1.61	12.51
	(0.776)	*(1.157)*	*(1.324)*	*(1.702)*	*(0.936)*	*(1.106)*	*(5.024)*
Marginally Employed	0.94	0.37	0.29	0.67	1.36	0.26	5.69
	(0.379)	*(0.234)*	*(0.175)*	*(0.768)*	*(0.717)*	*(0.555)*	*(5.519)*
log Daily Earnings	0.13	0.11	0.08	0.18	0.24	-0.09	0.11
	(0.013)	*(0.164)*	*(0.032)*	*(0.033)*	*(0.044)*	*(0.094)*	*(0.056)*
Number of Strata	*1,816*	*929*	*189*	*307*	*213*	*32*	*138*
*Avg baseline employment**	*17.9*	*20.3*	*12.5*	*21.4*	*11.4*	*9.7*	*13.4*

Notes: This table shows estimates corresponding to table 6, but for various subsamples, and only for our 6 core outcomes. Each coefficient comes from a different regression. We only consider the specification where we match on initial firm size and pre-event employment growth. Panel A shows results when treated firms and their controls are located in adjacent municipalities, while panel B shows the corresponding results for the specification in which they are located in adjacent counties. For this sample we also document results in panel C when estimating the event-study models for 6 different industries. In columns (8) and (9) of the first two panels we split the sample by establishment size. Specifically, we calculate the median of initial employment ***among treated firms*** . Strata with "small firms" are those in which the treated firm has initial employment equal- or below the median. The median is 10.5 employees in panel A and 10 employees in panel B. The split into small vs. large firms is not exactly equal because there is a mass point at the median. Standard errors of regression estimates are clustered on the strata-level.

* of treated establishments in t-4 and t-3

TABLE 8 - FIRM-LEVEL IV-ESTIMATES OF FUNDING AMOUNTS (1000s of EUR) ON THE NUMBER OF HIRES

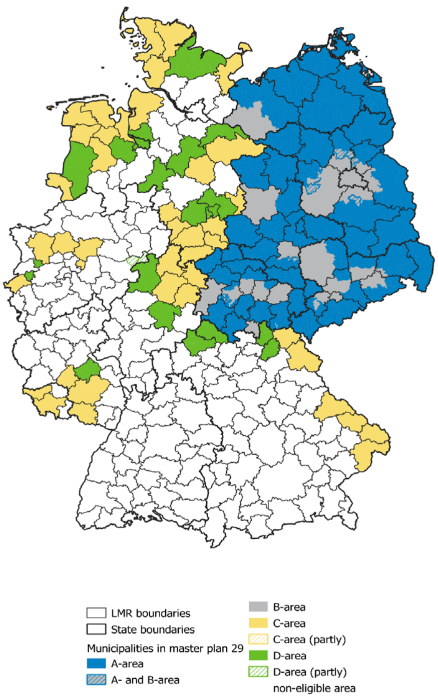
	Regression Coefficients			
	(A)	(B)	(C)	(D)
	Matching on Initial Level and Pre-Event Growth	Contiguous Counties Sample	Matching on Pre-Event Growth	"Pairwise Matching" on Pre-Event Growth
First Stage Coefficient				
	15.73	13.37	15.53	12.38
	(1.90)	*(0.85)*	*(1.90)*	*(1.75)*
Second Stage Coefficient				
	0.066	0.036	0.031	0.026
	(0.021)	*(0.009)*	*(0.006)*	*(0.005)*
Summary Statistics: Treated Establishments				
Hires Per Employee, pre-treatment	0.39	0.34	0.36	0.23
Total Subsidies per Employee, Conditional on Treatment	23,392	23,148	25,333	19,812
Subsidy rate	0.37	0.42	0.39	0.38
Nr. of Strata	*286*	*1,816*	*744*	*468*

Notes: This table shows first-stage and second-stage results when estimating the DiD models where the subsidy rate is used as IV for the actual amount of subsidies. All variables are normalized relative to baseline employment to make the models comparable to those estimated on a per capita basis at the municipality level. The outcome variable in the first-stage model is equal to total funding per worker at funding time (time 0 in the event studies) and zero otherwise. As such, the first stage coefficient represents the effect of the subsidy rate on per-worker average funding in each of the five years of the post-treatment period. The outcome variable in the second stage is annual hires per worker. The notes from Table 6 otherwise apply.

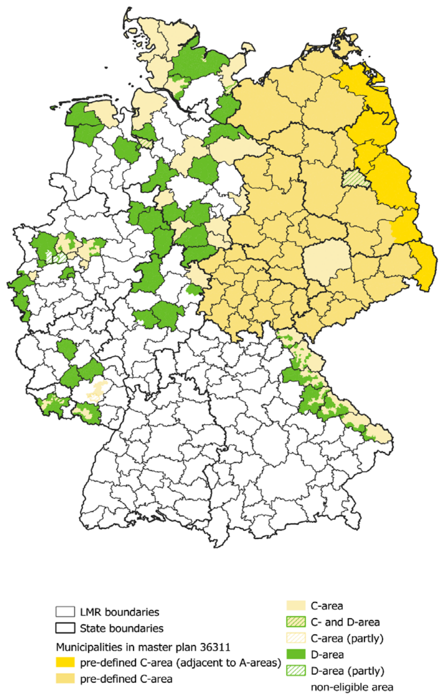
FIGURE 1 - MAPS OF ELIGIBILITY STATUS AND SUBSIDY RATES OF THE GRW PROGRAM: EXAMPLES FROM TWO FUNDING PLANS

PANEL A - ELIGIBILITY STATUS FOR FUNDING PLANS 29 (JAN 2000 to JAN 2004) AND 36311 (YEAR 2017), BY LABOR MARKET REGION (LMR)

(a) Funding Plan 29 (Jan 2000 to Jan 2004)

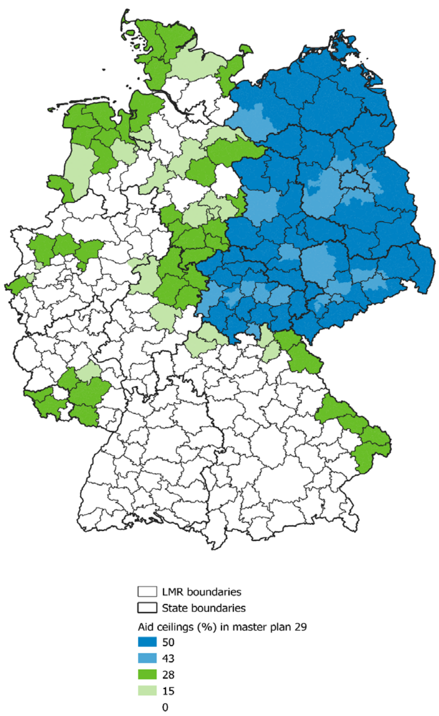


(b) Funding Plan 36311 (Jan 2017 to Dec 2017)



PANEL B - SUBSIDY RATES FOR FUNDING PLANS 29 (JAN 2000 to JAN 2004) AND 36311 (YEAR 2017), BY LABOR MARKET REGION (LMR)

(a) Funding Plan 29 (Jan 2000 to Jan 2004)



(b) Funding Plan 36311 (Jan 2017 to Dec 2017)

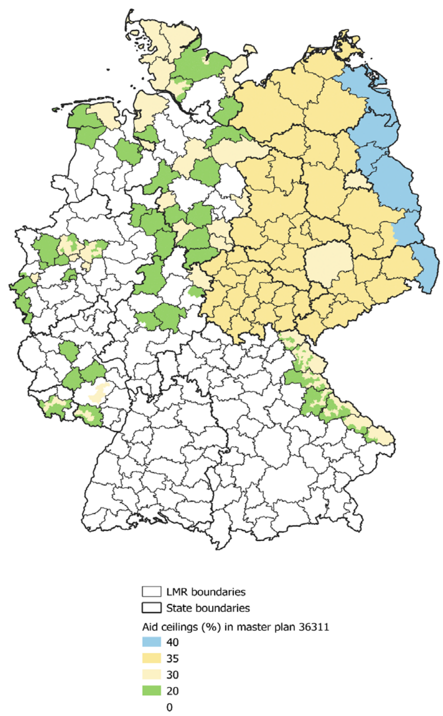
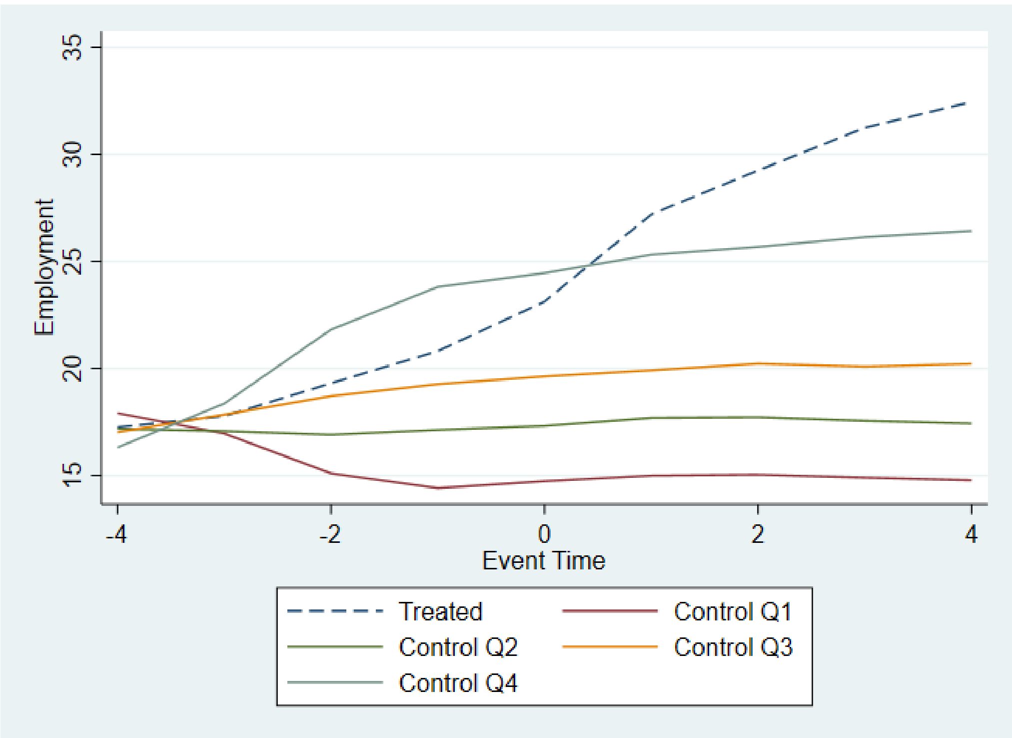


FIGURE 2 - EMPLOYMENT GROWTH: DESCRIPTIVE STATISTICS

PANEL A: AVERAGE GROWTH RATES AND MEAN-REVERSION

	All Strata		Strata with at least 4 Control Firms	
	Treated	Control	Treated	Control
Number of Establishments	316	12,729	237	12,572
Pre-Event Employment Growth Rate	0.307	0.099	0.301	0.099
	(0.812)	(1.191)	(0.850)	(1.198)
Post-Event Employment Growth Rate	0.139	0.030	0.136	0.030
	(0.317)	(0.350)	(0.299)	(0.351)
Regression Coefficient: Post-Event- on Pre-Event Employment Growth Rate	-	-0.002	-	-0.002
		(0.003)		(0.003)

PANEL B: EVOLUTION OF EMPLOYMENT OVER 9 YEARS; TREATED ESTABLISHMENTS VS. CONTROLS

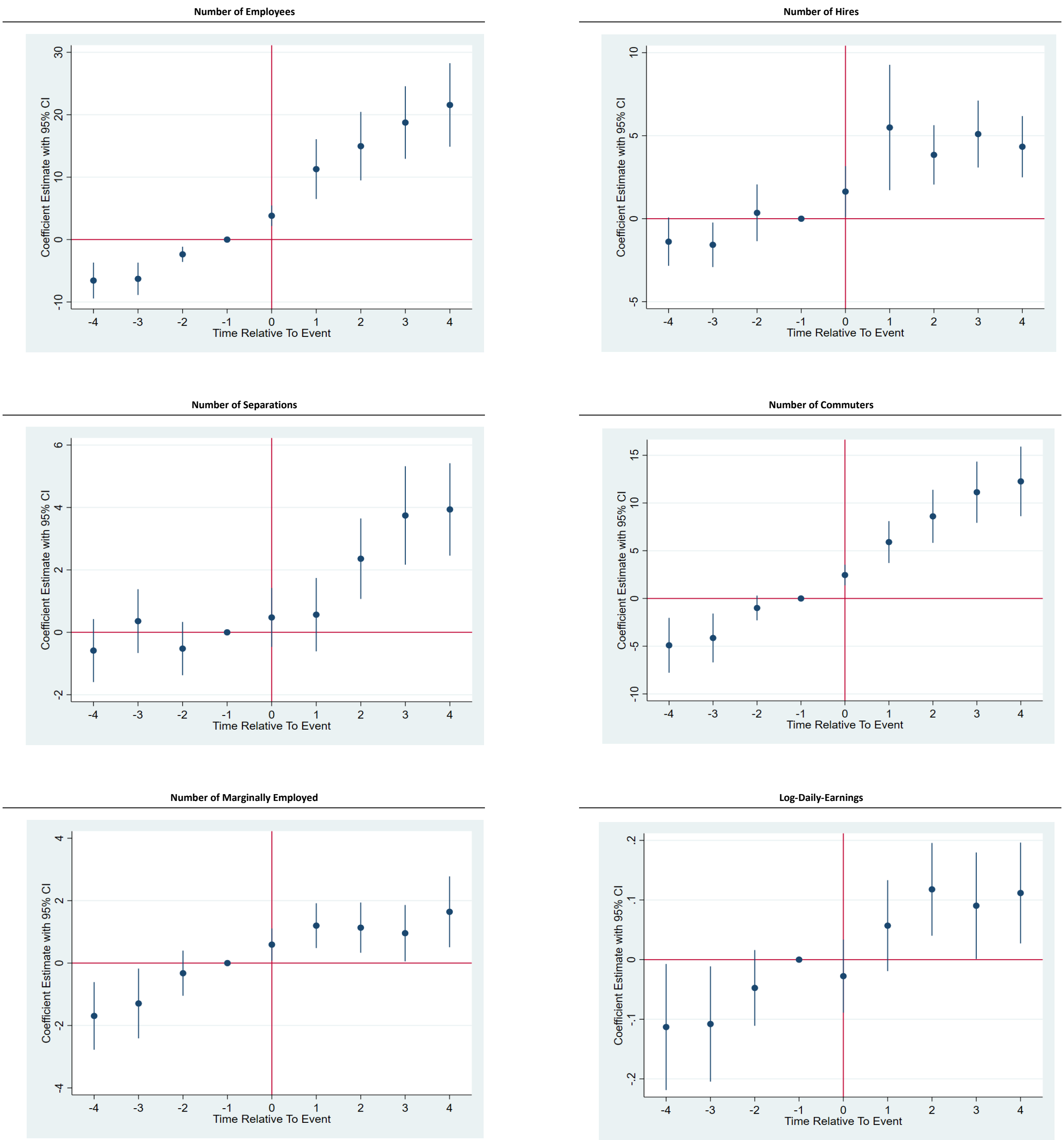


Notes: This figure shows descriptive statistics of employment growth for treated- and control-firms in the baseline sample (figure 3). Panel A shows average employment growth rates in the 4 years before and after the event, together with the regression coefficient of post-event growth rates on pre-event growth rates. Standard errors are in parantheses. Panel B shows the evolution of average employment over the 9 years used in the event studies, separately for treated firms and their controls. The latter are split into 4 groups defined by their standing in the pre-event within-strata employment growth. The figure is computed from group-specific event-time dummies, net of strata fixed effects.

FIGURE 3 - EVENT-STUDY RESULTS FOR CORE OUTCOMES, MATCHING ON INITIAL EMPLOYMENT LEVELS

Number of Observations (after collapse): 5,688

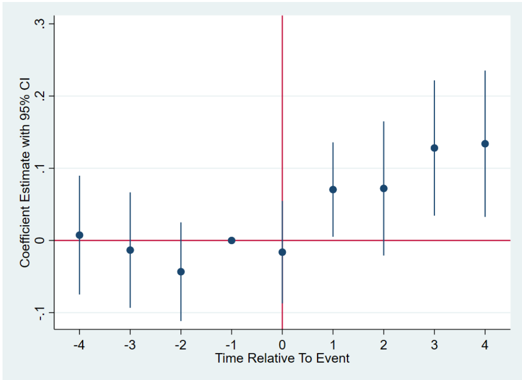
Number of Strata: 316



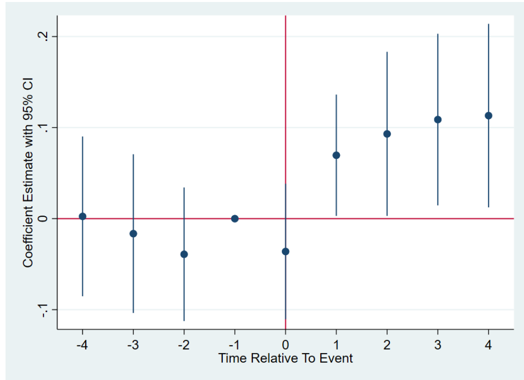
Notes: The figures show point estimates and their 95% confidence intervals for an event study that tracks differences in outcomes between treatment- and control groups over a period of 9 years. Point estimates are coefficients on the interaction between event-time dummies and the GRW subsidy rate. Within each strata, the two groups are perfectly matched on initial employment. Point estimates displayed in the figures are differences in outcomes relative to its difference one year prior to the event, scaled by the GRW subsidy rate. This difference is normalized to zero in the baseline period. Increasing (decreasing) point estimates imply that the outcome is growing faster (slower) in the treatment- than in the control group. We show results for our 6 core outcomes. Standard errors are clustered on the strata-level.

FIGURE 4 - EVENT-STUDY RESULT: LOG-DAILY EARNINGS, TENURE >= 5 YEARS

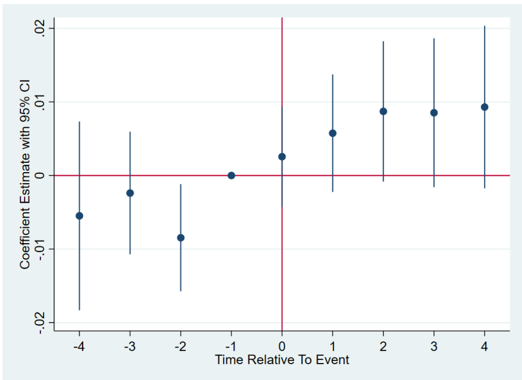
Matching on Initial Employment-Levels



Matching on Initial Employment-Levels- and Growth



Placebo Regressions: All Establishments

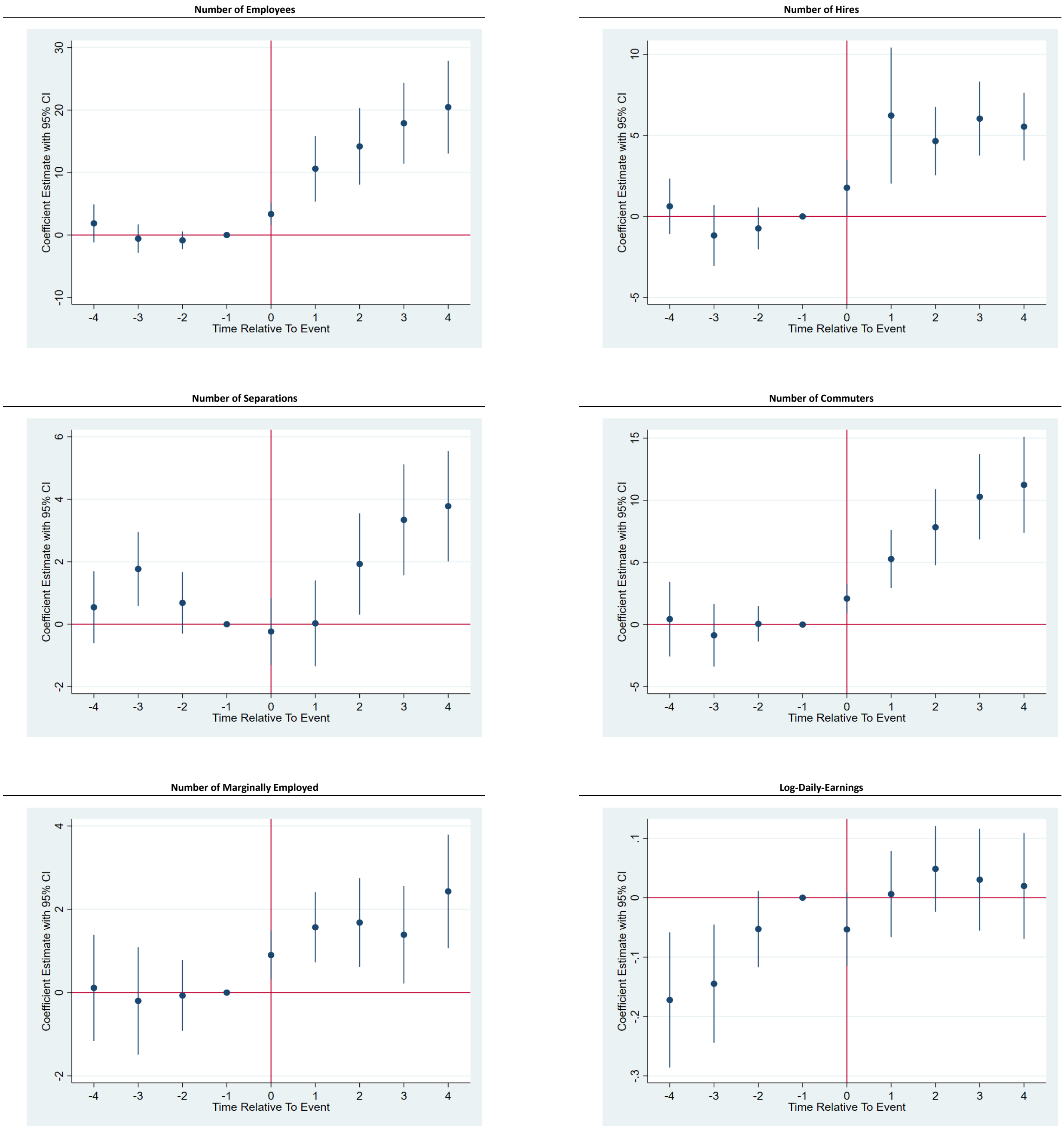


Notes: The figures show point estimates and their 95% confidence intervals for an event study that tracks differences in log-daily earnings of employees with at least 5 years of tenure at the time of the event between treatment- and control groups. Point estimates are coefficients on the interaction between event-time dummies and the GRW subsidy rate. We show results for three specifications: Matching on initial employment levels, matching on initial employment levels and pre-event growth, and placebo regressions that include all non-treated establishments in both treated- and control municipalities. Point estimates displayed in the figures are differences in outcomes relative to its difference one year prior to the event, scaled by the GRW subsidy rate. This difference is normalized to zero in the baseline period. Increasing (decreasing) point estimates imply that the outcome is growing faster (slower) in the treatment- than in the control group. Standard errors are clustered on the strata-level.

FIGURE 5 - EVENT-STUDY RESULTS FOR CORE OUTCOMES, MATCHING ON INITIAL LEVEL AND PRE-EVENT GROWTH OF EMPLOYMENT

Number of Observations: 5,148

Number of Strata: 286



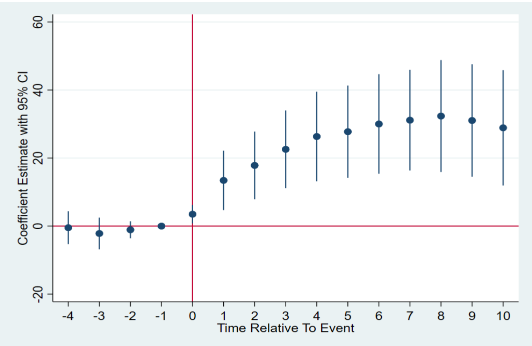
Notes: The figures show point estimates and their 95% confidence intervals for an event study that tracks differences in outcomes between treatment- and control groups over a period of 9 years. Point estimates are coefficients on the interaction between event-time dummies and the GRW subsidy rate. Within each strata, the two groups are perfectly matched on initial employment. Control firms only include those with above-median pre-event employment growth in their municipality-industry cell. Point estimates displayed in the figures are differences in outcomes relative to its difference one year prior to the event, scaled by the GRW subsidy rate. This difference is normalized to zero in the baseline period. Increasing (decreasing) point estimates imply that the outcome is growing faster (slower) in the treatment- than in the control group. We show results for our 6 core outcomes. Standard errors are clustered on the strata-level.

FIGURE 6 - EVENT-STUDY RESULTS FOR CORE OUTCOMES, LONG-RUN EFFECTS (10 YEARS POST TREATMENT)

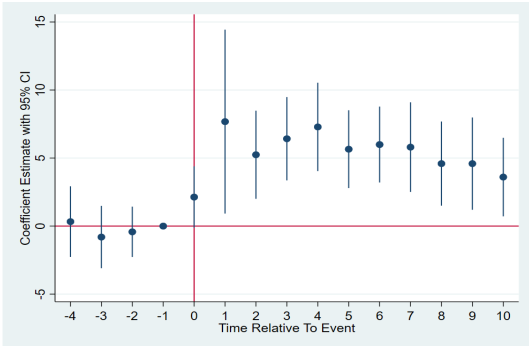
Number of Observations: 4,920

Number of Strata: 164

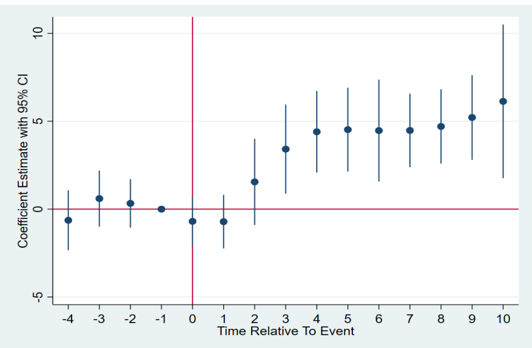
Number of Employees



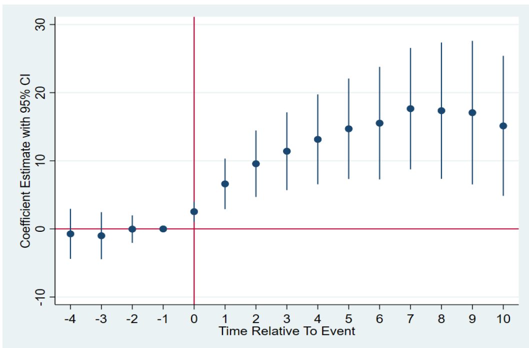
Number of Hires



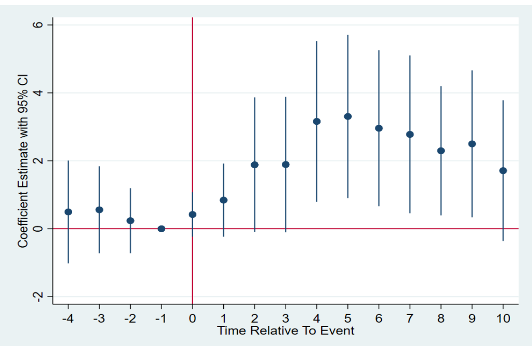
Number of Separations



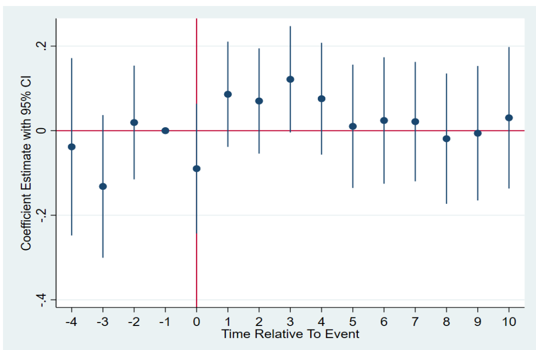
Number of Commuters



Number of Marginally Employed



Log-Daily-Earnings (tenured workers)

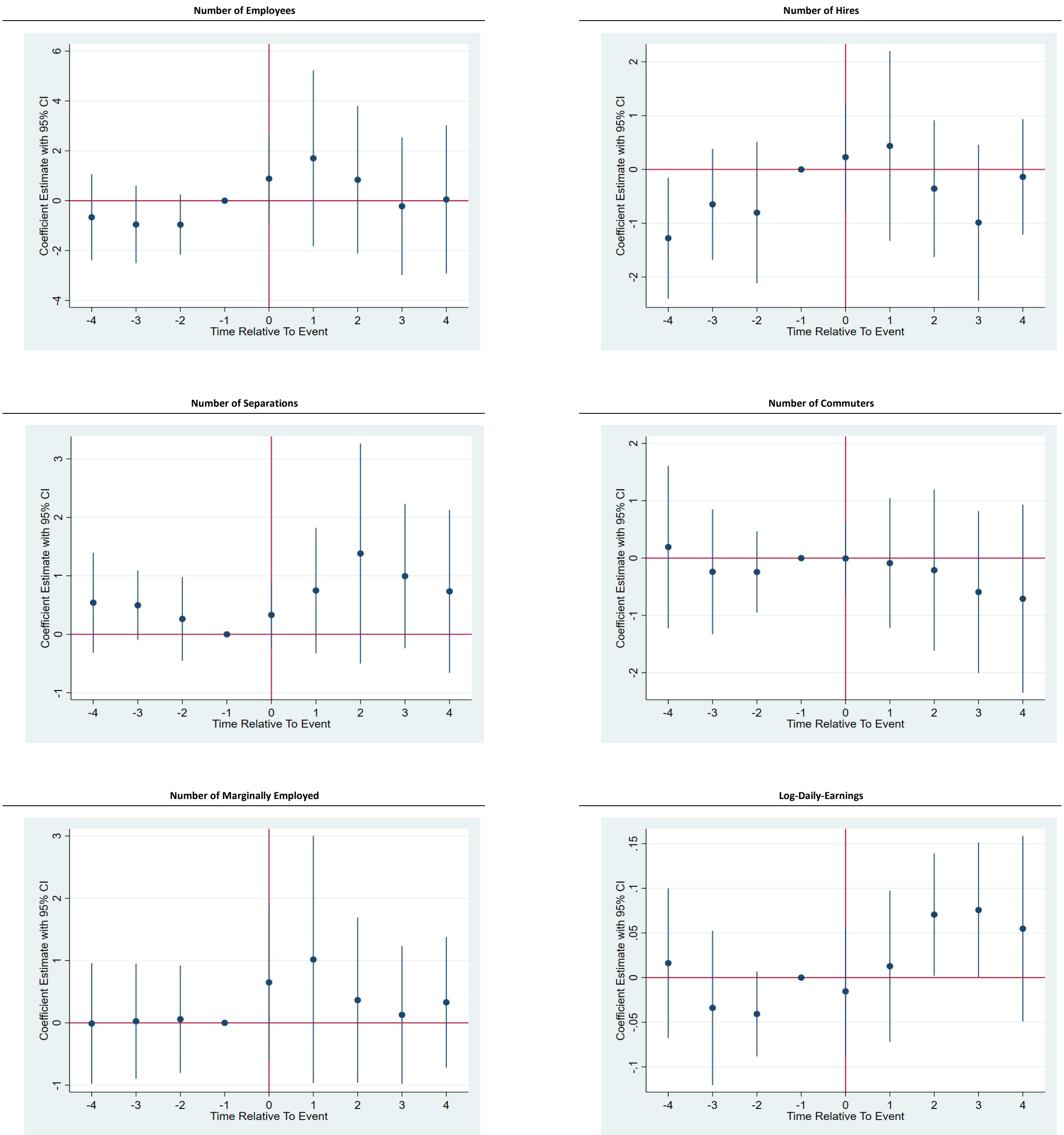


Notes: The figures show point estimates for the same outcomes and the same specification as in figure 5, but up to 10 years after treatment. For details, see notes for figure 5.

FIGURE 7 - EVENT-STUDY PLACEBO REGRESSIONS, FULL MATCH

Number of Observations (after collapse): 3,024

Number of Strata: 168



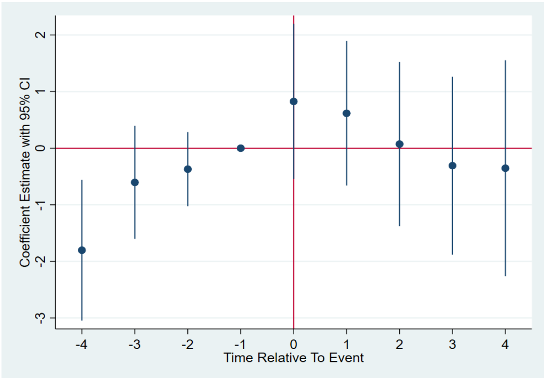
Notes: The figures show point estimates and their 95% confidence intervals for an event study that tracks differences in outcomes between placebo treatment groups (N=2,562 firms) and original control groups over a period of 9 years. Placebo-treated establishments are matched on initial employment, 2-digit industry and municipality to the actually treated firms, the latter of which are excluded from the sample. Point estimates are coefficients on the interaction between event-time dummies and the GRW subsidy rate. Control groups are the same as in the benchmark regressions of figure 2. Point estimates displayed in the figures are differences in outcomes relative to its difference one year prior to the event, scaled by the GRW subsidy rate. This difference is normalized to zero in the baseline period. Increasing (decreasing) point estimates imply that the outcome is growing faster (slower) in the treatment- than in the control group. We show results for our 6 core outcomes. Standard errors are clustered on the strata-level.

FIGURE 8 - EVENT-STUDY PLACEBO REGRESSIONS, MATCH ON 2-DIGIT INDUSTRY ONLY

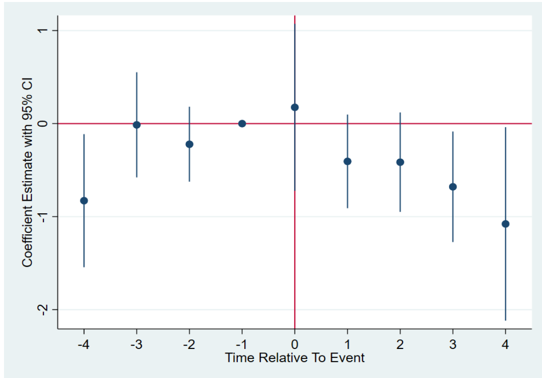
Number of Observations (after collapse): 5,310

Number of Strata: 295

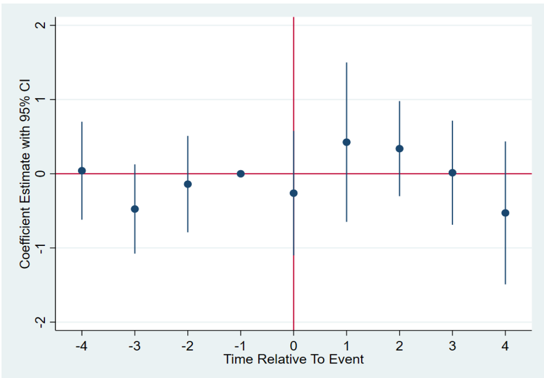
Number of Employees



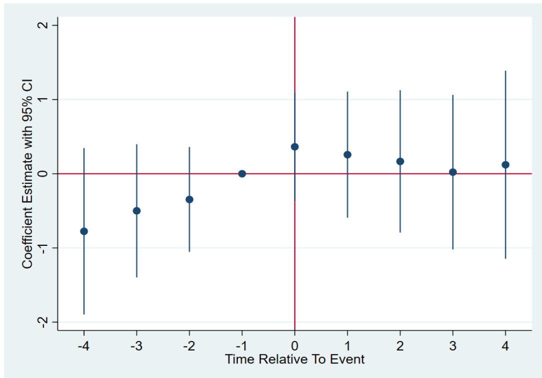
Number of Hires



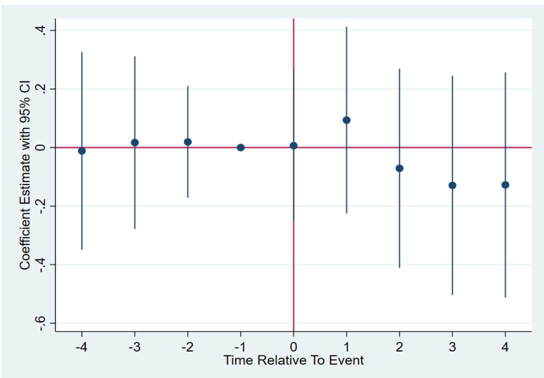
Number of Separations



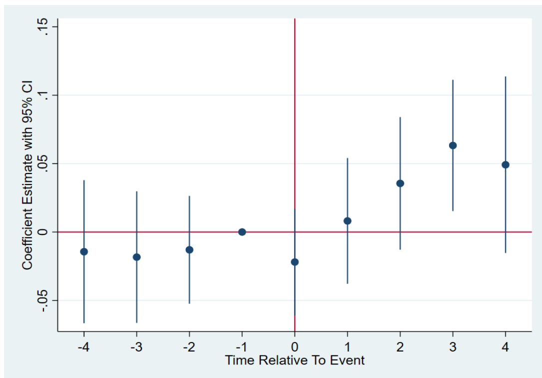
Number of Commuters



Number of Marginally Employed



Log-Daily-Earnings



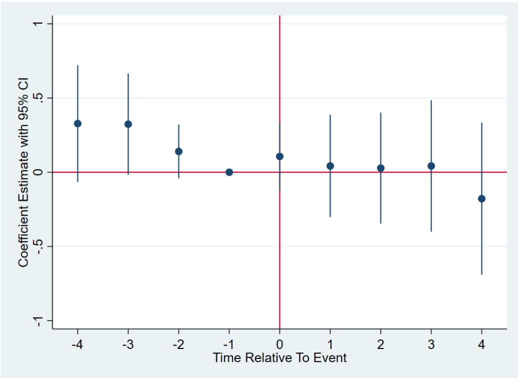
Notes: The figures show point estimates and their 95% confidence intervals for an event study that tracks differences in outcomes between placebo treatment- (N=21,545 firms) and control groups (N=53,881 firms) over a period of 9 years. Placebo-treated establishments are matched on 2-digit industry and municipality to the actually treated firms, the latter of which are excluded from the sample. Control establishments are located in contiguous border municipalities and are also matched on 2-digit industry. Point estimates are coefficients on the interaction between event-time dummies and the GRW subsidy rate. Point estimates displayed in the figures are differences in outcomes relative to its difference one year prior to the event, scaled by the GRW subsidy rate. This difference is normalized to zero in the baseline period. Increasing (decreasing) point estimates imply that the outcome is growing faster (slower) in the treatment- than in the control group. We show results for our 6 core outcomes. Standard errors are clustered on the strata-level.

FIGURE 9 - EVENT-STUDY PLACEBO REGRESSIONS, ALL UNTREATED FIRMS IN CONTIGUOUS BORDER MUNICIPALITIES

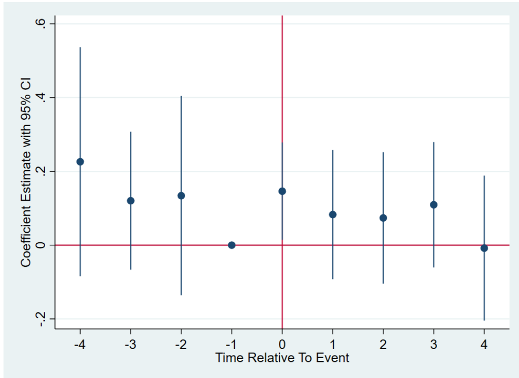
Number of Observations (after collapse): 5,688

Number of Strata: 316

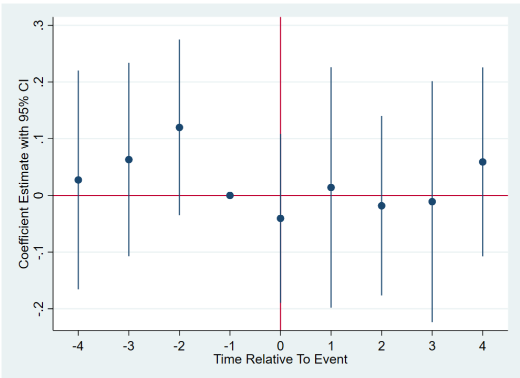
Number of Employees



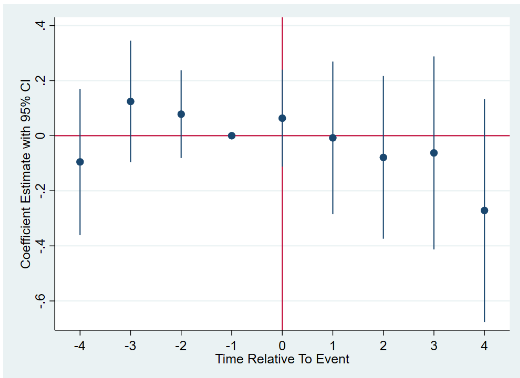
Number of Hires



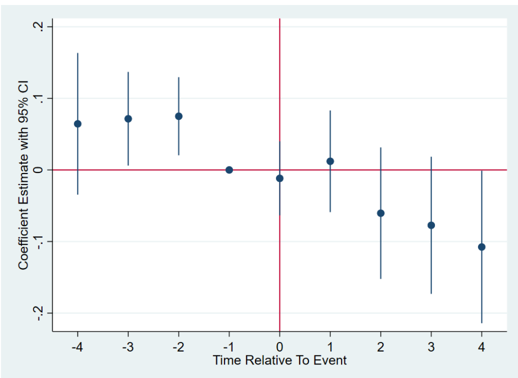
Number of Separations



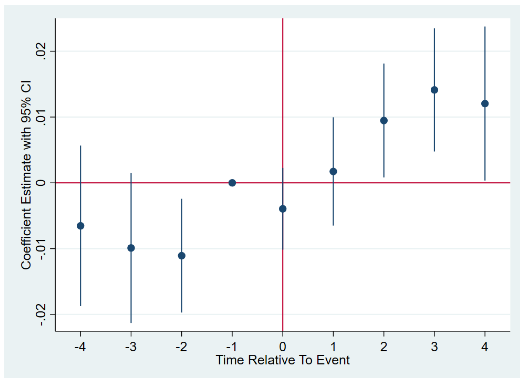
Number of Commuters



Number of Marginally Employed



Log-Daily-Earnings



Notes: The figures show point estimates and their 95% confidence intervals for an event study that tracks differences in outcomes between placebo treatment- (N=177,897 firms) and control groups (N=326,138 firms) over a period of 9 years. Placebo-treated establishments are matched on municipality to the actually treated firms, the latter of which are excluded from the sample. Control establishments are located in contiguous border municipalities. Point estimates are coefficients on the interaction between event-time dummies and the GRW subsidy rate. Point estimates displayed in the figures are differences in outcomes relative to its difference one year prior to the event, scaled by the GRW subsidy rate. This difference is normalized to zero in the baseline period. Increasing (decreasing) point estimates imply that the outcome is growing faster (slower) in the treatment- than in the control group. We show results for our 6 core outcomes. Standard errors are clustered on the strata-level.

APPENDIX TABLE 1 - SHARE OF TOTAL GRW BUDGET, BY STATE

		(1)	(2)	(3)
Macro region	**State**	**EU funding period**		
		2000-2006 ***(RP 29, 33)***	**2007-2013** ***(RP 36, 361, 362)***	**2014-2020** ***(RP 36310, 36311, 36320)***
West Germany	***Schleswig-Holstein***	1.4	2.1	3.7
	Hamburg	0.0	0.0	0.0
	Lower Saxony	3.9	4.4	3.5
	Bremen	0.6	0.3	1.5
	Northrhine-Westphalia	3.8	4.3	6.5
	Hesse	1.0	0.6	1.3
	Rhineland-Palatinate	0.7	0.6	1.1
	Baden-Wuerttemberg	0.0	0.0	0.0
	Bavaria	1.0	1.6	1.6
	Saarland	0.7	0.4	1.4
East Germany	***Berlin***	10.2	10.0	10.9
	Brandenburg	14.3	14.1	12.5
	Mecklenburg-Pommerania	11.3	11.1	10.1
	Saxony	22.3	21.9	20.0
	Saxony-Anhalt	15.4	15.2	13.9
	Thuringia	13.6	13.4	12.2

NOTE: The table shows the share of total GRW funds allocated to each German state for three funding periods of the European Union. Details of the GRW are described in master plans (listed in the table as "RP" for "Rahmenplan"). The benchmark rule for this allocation is the population share of state-specific eligible areas relative to all eligible areas. Deviations from this benchmark rule do occur, as described in the main text.

SOURCES: Rahmenplaene. See Appendix Table 3 for a list of references.

APPENDIX TABLE 2 - COMPOSITION AND WEIGHTS OF THE ELIGIBILITY SCORING RULE

Funding Period (Europ. Union)	Regional coverage	Economic Indicators	Weight (%)
2000 - 2006	***West Germany***	Average unemployment rate 1996-1998	40
		Gross wages and salaries per capita 1997	40
		Quality of infrastructure	10
		Employment projection 1997-2004	10
	East Germany	Average underemployment rate 1996-1998	40
		Gross wages and salaries per capita 1997	40
		Quality of infrastructure	10
		Employment projection 1997-2004	10
2007 - 2013	***Germany***	Average unemployment rate 2002-2005	50
		Gross wages and salaries per capita 2003	40
		Quality of infrastructure	5
		Employment projection 2004-2011	5
2014 - 2020	***Germany***	Average unemployment rate 2009-2012	45
		Gross wages and salaries per employee (subject to so	40
		Quality of infrastructure	7.5
		Employment projection 2011-2018	7.5

NOTES: The table shows the variables and their weights entering the administrative scoring rule for determining the eligibility status of Labor Market Regions for the GRW, separately for the EU funding period. For the first funding period in the table, the rule used Unemployment for West Germany and Underemployment for East Germany.

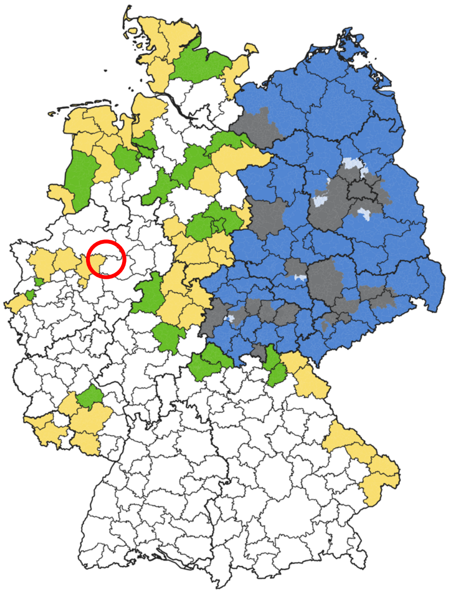
SOURCES: Schwengler and Binder (2006), Alm and Fisch (2014).

APPENDIX TABLE 3 - POLICY REFERENCES, BY RAHMENPLAN (FUNDING PLAN)

Funding Period (Europ. Union)	Rahmenplan (Funding Plan)	Rahmenplan reference	Notification documents	Guidelines on National Regional aid (A-C)
2000 - 2006	***29 (Jan 2000 - Jan 2004)***	BT-Drucksache 14/3250	2001/272/EC, notified under document number C(2000) 809, Official Journal of the European Communities, L 97/27, April 06, 2001	98/C 74/06, Official Journal of the European Communities, C 74/9, March 10, 1998
	33 (Feb 2004 - Dec 2006)	BT-Drucksache 15/2961	State aid N 641/2002, notified under the document number C (2003) 904, April 02, 2003	
2007 - 2013	***36 (Jan 2007 - Sep 2008)***	BT-Drucksache 16/5215	State aid N 459/2006, notified under the document number C (2006) 4958, November 11, 2006	2006/C 54/08, Official Journal of the European Union, C 54/13, March 04, 2006
	361 (Oct 2008 - Jan 2011)	BT-Drucksache 16/13950		
	362 (Feb 2011 - Jun 2014)	Bundesanzeiger Amtlicher Teil 20.01.2011		
2014 - 2020	***36310 (Jul 2014 - Dec 2016)***	Bundesanzeiger Amtlicher Teil 01.07.2015 B1	State aid No. SA.37423 (2013/N), notified under the document number C (2014) 1293, March 11, 2014	2013/C 209/01, Official Journal of the European Union, C 209/1, July 23, 2013
	36311 (Jan 2017 - Dec 2017)	Bundesanzeiger Amtlicher Teil 05.10.2017 B1	State aid No. SA.46343 (2016/N), notified under the document number C(2016) 6915, November 03, 2016	
	36320 (Jan 2018 - Dec 2020)	Bundesanzeiger Amtlicher Teil 05.10.2018 B2		

APPENDIX FIGURE 1 - AN EXAMPLE OF STRATA CONSTRUCTION: "HAMM" AND "AHLEN"

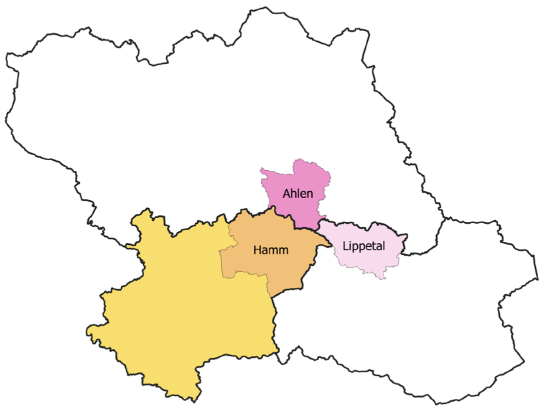
PANEL A: MAP OF GERMANY, ITS LABOR MARKET REGIONS, AND THEIR ELIGIBILITY STATUS (2000-2004)



PANEL B: RED-CIRCLED AREA FROM PANEL A WITH ITS THREE LABOR MARKET REGIONS

PANEL C: MUNICIPALITY "HAMM" (ELIGIBLE) AND ITS CONTIGUOUS NEIGHBORS

PANEL D: MUNICIPALITY "AHLEN" (NON-ELIGIBLE) AND ITS CONTIGUOUS NEIGHBORS



APPENDICES AND SUPPLEMENTARY MATERIAL: NOT FOR PUBLICATION

APPENDIX 1: DATA

1. Outcome Variables from the Social Security Establishment Data

The main outcome variables are defined as follows:

- ***Employment:*** The total number of employees who are subject to social insurance contributions (including part-time workers, workers in marginal jobs, etc).
- ***Share of Commuters:*** Share of an establishment's employees whose place of residence differs from their place of work, both measured on the municipality level.
- ***Share of "marginal jobs":*** Share of an establishment's marginal employees, which are defined as those with monthly earnings not exceeding EUR 450 per month.
- ***Worker inflows:*** The number of workers starting formal employment at an establishment in the current period.
- ***Worker outflows:*** The number of workers who terminated formal employment at an establishment in the previous period.

2. Record Linkage

Here, we describe how we carry out the linkage of the IAB data to the GRW funding data. The IAB data are on the establishment-year level, and the GRW funding data are on the project level. Starting in 2004, the GRW data systematically report the administrative *establishment* identifiers of the German Federal Employment Agency. For cases in which the GRW data provide no such id or in which the id is invalid, a particularly relevant issue for the years prior to 2004, we match both on an establishment's name and the location of its branch. Both the GRW data and the administrative employment records provide city, street, and house numbers.

A common reason for incomplete matches is missing information on establishment name, establishment id, or branch address in either of the two data sets. Another less common reason is the GRW-funded creation of new branches of large establishments that never materialized and that are thus reported in the GRW data but not in the administrative employment records. In either of these two cases, we cannot complete a match. For the cases where neither establishment id nor name and location helped produce a successful linkage of a project to the IAB data, we proceed as follows. If deviations from an exact match are minimal, typically due to typos in street or company names, we accept it as a successful match. For larger deviations from an exact match, we rely on probabilistic matching. Specifically, for high matching scores, we perform the linkage manually by comparing the addresses and establishment names in both samples. Only if we are sufficiently confident that we have found a valid match do we keep it in the data. All remaining projects are dropped from our final data.

3. Regional Concordance Matrices

We use historical municipality codes for merging the digitized policy data to our IAB establishment sample. This is possible because the IAB has retained regional classification variables from all past years in which data were collected and published. However, the econometric models require time-consistent regional identifiers, hereafter referred to as "baseline regional codes," so that geographic fixed effects are defined for exactly the same geographic units in each sampling year. We, therefore, carry out a transcoding of the historical codes to our baseline regional codes. To this end, we use regional concordance matrices developed at the Research Data Centre of the Halle Institute for Economic Research (IWH) (see Kauffmann 2015).[1] These matrices are year-specific and contain as elements shares $T_{t_{ij}}$ of the population in municipality *i* in year *t* that is "reassigned" to municipality *j* in either year (t-1) in case of backward transcoding or year (t+1) in the case of forward transcoding. Using these matrices iteratively allows transcoding regional codes for any year to the baseline regional codes.

We use municipal identifiers for 2017 as our baseline regional codes for two reasons. First, 2017 is the last year covered by our sample in which the municipality codes were updated.[2] Second, transcoding forward is attractive in our context because, apart from very few exceptions, territorial reforms that took place over our sample period involved mergers rather than splits of municipalities. This avoids random reassignment of newly created establishments to the baseline regional codes. To see this, consider an example in which two municipalities, say A and B, are merged in 2010 and called "municipality C" thereafter. For any establishment in these municipalities, no matter the year, forward transcoding is

[1] For a detailed description, see Kaufmann (2015) and https://www.iwh-halle.de/en/research/data-and-analysis/research-data-centre/transformation-tables-for-administrative-borders-in-germany/.

[2] It is important to keep in mind that even though our establishment panel data is constructed from the universe of establishments that were active sometime between 2000 and 2016, our event study sample covers a time period starting in 1996 and ending in 2020 because we track treated establishments and their controls for four years before- and after a funding event.

straightforward and deterministic: Simply recode “A’s” and “B’s” to a “C” before 2010 and do not change codes at all after 2010. The concordance matrices will thus contain only zeros and ones. In contrast, for establishments that were not active before 2010, backward transcoding would involve randomly allocating them a code of “A” or “B”, using past population shares of these municipalities relative to municipality “C”. After all, for any establishment entering the data after 2010 one never observes whether it was located in municipality “A” or “B”. If municipality “A” was twice as large as municipality “B” at the time of the merger, one could only extrapolate by assigning two-thirds of such establishments to the former and one-third to the latter. Because of the nature of the territorial reforms over our sample period, such probabilistic transcoding of municipal codes can be avoided when using the forward mode.

An important implication of municipality mergers is that they can generate cross-sectional variation in eligibility and subsidy rates within a baseline municipality code. In our hypothetical example above, this will be the case if municipalities A and B were located in different LMRs before the merge in 2010 and if eligibility status varied between them. Since we merge our policy data to establishment-level panel data, municipality C will have establishments with differing subsidy rates before 2010. The implications for our two research designs are as follows. First, for the municipality-level IV model, year-specific subsidy rates for municipalities that are eventually merged will be a weighted average over all component municipalities that are part of the same baseline municipality code. Weights are constructed from the number of employees. Second, for the establishment-level event study, the implication is that we are using the historically correct subsidy rate for the treated establishment since we match policy parameters based on historical rather than baseline municipality codes.

APPENDIX 2: MATCHING IN THE EVENT-STUDY DESIGN

1. Matching on Levels

If the reception of subsidies is quasi-randomized, variation in establishment-level funding within strata is independent of the level and the growth of any outcome prior to the funding event. This is our justification for constructing control establishments via matching on initial employment levels and industry. More specifically, in addition to matching on geographic borders, our matching procedure keeps only those establishments in control municipalities of a stratum that

(a) operate in the same 2-digit industry and

(b) have the same average number of employees in years 3 and 4 prior to the event

as the treated establishment. We match on industry to allow implicitly for strata-level time trends that are specific to an industry. For a more precise description of step (b), define $E^1_{s,\tau}$ as the number of employees in period τ of the treated establishment in strata s. Let $E^0_{is,\tau}$ be the corresponding number for any establishment *i* that is located in the control municipality and satisfies condition (a). Also define their respective 2-period averages in periods $\tau = -4$ and $\tau = -3$ by $E^1_{s,-4} = \frac{E^1_{s,-4}+E^1_{s,-3}}{2}$ and $E^0_{i.s,-4} = \frac{E^1_{is,-4}+E^1_{is,-3}}{2}$. A precise statement of condition (b) is $E^1_{s,-4} = E^0_{i.s,-4}$ for any control establishment *i* in stratum *s*. Notice that we use an average over two years rather than, say, employment in year $\tau = -4$ to avoid matching on transitory fluctuations in hiring- and separation rates. By imposing (a) and (b), our event-study design compares the evolution of outcome $y^D_{s,\tau}$, one of which is establishment size, between the treated establishment and establishments in control municipalities that start from the same level of employment and have the same 2-digit industry code. This approach is attractive because it matches on only one employment statistic such that, mechanically, $\left(\frac{\hat{\beta}_{-4}+\hat{\beta}_{-3}}{2}\right) = 0$. This leaves as free parameters three of the four pre-event treatment coefficients $\beta_{\tau<0}$, which we use for testing for differential pre-trends.

The implementation faces two main challenges, however. First, it is data intensive. After all, there may not be many pairs of contiguous municipalities along borders of LMRs with different eligibilities left after conditions (a) and (b) are imposed. For this reason, we match on 2-digit rather than 3-digit industry codes. It is also for this reason that our approach needs to rely on the universe of matched employer-employee data rather than random subsamples of them. Still, sample size remains an issue, and we thus soften requirement (b) by matching on intervals around the variable $E^1_{s,-4}$ rather than on its exact levels, with the exception of cases in which the treated establishment enters the first two years of a stratum with an average of one employee. In particular:

- If $E^1_{s,-4} = 1$, then we perform an exact match.
- If $E^1_{s,-4} \in [2,5]$, then we match any establishments for which $\left|E^1_{s,-4} - E^0_{i.s,-4}\right| = 1$.
- If $E_{s,-4} \geq 6$, then we match any establishments for which $\left|ln\left(E^1_{s,-4}\right) - ln\left(E^0_{i.s,-4}\right)\right| = .2$.

Our general preference is to select control establishments whose establishment size $E^0_{i.s,-4}$ is contained within a percentage interval around $E^1_{s,-4}$. The third of these three conditions states that we allow for a

20% deviation in the number of employees on each side of $E^1_{s,-4}$ for larger establishments. We chose this number because we found that it yielded a sufficient increase in sample size without generating too large size differences between treated units and their controls. However, for small establishments, this does not work because either the interval will contain establishments with no employees four years prior to the event or because the relative difference in establishment size between treated establishments and their controls is too large. We, therefore, match exactly when a treated establishment enters the strata with an average of one employee in the first two periods, and we allow for a size difference of one employee for establishments that are slightly larger initially.[3]

The second challenge comes from the heavy skew of the establishment-size distribution. It is well-known that the distribution of employees across establishments or establishments can be well-approximated by distributions that satisfy "power laws". This is indeed the case for Germany and, more specifically, for our data. As a consequence, any matching algorithms that rely on symmetric interval differences or categorical groups in the number of employees between treated establishments and their controls will not achieve balance mechanically. This problem is less severe for larger establishments since a log transformation mostly eliminates this skew when performed on the right tail of the distribution of $E^0_{i.s,-4}$. For smaller establishments, we need to rely on level differences, as described above. Balance can be achieved by randomly dropping "excess small establishments" or through reweighting. Due to efficiency considerations in light of small samples we choose the latter. As a consequence, the matching criterion $\left(\frac{\hat{\beta}_{-4}+\hat{\beta}_{-3}}{2}\right) = 0$ is met exactly. This is also convenient for the graphical representation and the interpretation of our coefficient estimates.

2. Matching on Pre-Trends

The approach described in the previous section is motivated by the assumption that treated establishments and their controls are ex-ante identical. This assumption is satisfied if the spatial discontinuity in program parameters is quasi-random and there is no systematic selection into treatment. While we find strong support for the first of these assumptions, we document overwhelming evidence for the failure of the second. In particular, treated establishments grow substantially faster in the four years prior to receipt of funding, even with perfect balance in their 2-digit industry and average establishment

[3] We switch to relative size differences starting with treated establishments for which $E_{s,-4} \geq 6$ since for smaller firms a one-worker difference is more than 20%.

size in years 3 and 4 before treatment. This is unlikely due to receiving any subsidies before the year in which we observe the "event" for at least two reasons. First, we define the year of the event based on the year the funded project is initialized. Our administrative GRW data indicate that it is an extremely rare occurrence that establishments receive subsidies beforehand.[4] Second, we focus on events that represent the first time an establishment receives any GRW funds. Hence, it is more likely that the pre-trends we find in the number of employees indicate that establishments that plan to expand persistently apply for and receive GRW funds.

We address this issue using matching on pre-trends. Let $E^0_{i.s,-2} = \frac{E^1_{is,-2}+E^1_{is,-1}}{2}$ be the average establishment size in the two periods preceding the funding event, calculated for each control establishment that is left in the sample after imposing conditions (a) and (b). Define establishment-level employment growth over the four pre-event periods by $\Delta ln\left(E^D_{i.s}\right) = ln\left(E^D_{i.s,-2}\right) - ln\left(E^D_{i.s,-4}\right)$. As before, we use within-establishment time-series averages to avoid matching on transitory employment fluctuations. Also, define the *q-th* strata-level quantile of the variable $\Delta ln\left(E^0_{i.s}\right)$ by $Q^0_{s,q}(\Delta)$, where we use the subscript to highlight that the quantile is computed over control establishments only. We then impose a third matching condition:

(c) Among all strata for which at least two control establishments are left after the first two matching stages, we only keep control establishments for which $\Delta ln\left(E^0_{i.s}\right) \geq Q^0_{s,.5}(\Delta)$, that is, establishments with employment growth in the pre-event period above the strata-specific median. We drop strata with one control establishment.

This matching criterion has the advantage that it is simple, transparent, does not involve any tuning parameters, and does not involve any direct matching on characteristics of the treated establishment other than those used in earlier stages of the matching algorithm, namely industry, initial employment levels, and location. It is also conservative: We find that treated establishments grow slightly less than their controls that are left after imposing condition (c). If one accepts the assumption that these controls provide an upper bound on the counterfactual employment evolution for treated establishments, then our estimates of the impact of funding should be interpreted as lower bounds. This is because we compare the evolution of treated establishments with controls that start from the same level of employment and that grow, on average, slightly faster during the pre-event period.

[4] On the other hand, there are several cases in which subsidies are paid out after initialization of the project.

Selecting control establishments based on their pre-event employment growth has the central shortcoming that it takes up all degrees of freedom in the employment data. Furthermore, balance on pre-trends is achieved on data that have already been drawn and is, therefore, an in-sample "algebraic exercise": As long as there are establishments that grow faster and establishments that grow slower prior to the funding event, so that the median used in criterion (c) is calculated over a non-degenerate distribution of employment growth, balance can be achieved mechanically even if the assumption of identical pre-trends in unobserved variables is violated. Testing externally for pre-trends in employment is thus not possible anymore.

We address this issue by validating our approach in three different ways. First, our spatial discontinuity design compares establishments in municipalities with identical *aggregate* pre-trends. As a consequence, we find differential employment growth because it is particularly quickly growing establishments that select into treatment, not because treated establishments are located in areas that are on a trajectory of higher economic growth than control municipalities. It is establishment heterogeneity, not differential aggregate trends, driving differential pre-trends in employment. Second, while we have no degrees of freedom left for testing for pre-trends in employment, we do not use any of our other outcome variables in the matching procedure. Since outcomes, such as the share of commuters, wages, or hiring- and separation rates, are not deterministically linked to the stock of employment, this provides ample data for external validation. Third, as a further validation that our coefficient estimates are not spurious, we carry out several back-of-the-envelope calculations in section 7 that investigate if our event-study estimates are consistent with our IV estimates from the municipality-level analysis. The logic of this approach is that our IV estimates are not affected by selection into treatment and, therefore, have strong internal validity. Furthermore, since the municipality-level analysis also includes openings of establishments that are subsidized by the GRW, with potentially particularly large effects, while our event study only focuses on projects in establishments that are already in operation, we expect that our IV estimates provide an upper bound on the causal effect of the GRW on labor market outcomes. If we found that our event-study micro-estimates aggregate to larger labor market effects, this would be concerning. On the other hand, if they are not, this would be further confirmation that they are not severely biased upwards.

APPENDIX 3: DESCRIPTIVE ANALYSIS OF PRE-EVENT EMPLOYMENT GROWTH

Our results from placebo regressions suggest that it is unlikely that the differential pre-trends in employment between treated establishments and their controls documented in Figure 3 are driven by differential *aggregate* trends. Given that we compare establishments in contiguous municipalities that do not have any influence on the policy parameters of the GRW, this finding is not surprising. In fact, it is precisely why exploiting border discontinuities between municipalities is an attractive approach in the context of the GRW place-based policy. A more plausible explanation for differential pre-trends is, therefore, selection into treatment, whereby establishments that grow particularly quickly in the pre-treatment period apply for and are accepted into the program. We, therefore, carry out a simple descriptive analysis of employment growth at the establishment level. The key questions of this analysis are how large idiosyncratic variation of pre-event employment growth is among control establishments, and where in the distribution of this variable employment growth of treated establishments is located. Key results from this analysis are shown in Figure 2. Panel A lists in tabular form employment growth over both, the four years preceding and following the funding event, together with the coefficient from regressing the latter on the former. To smooth out transitory fluctuations in employment, we calculate the employment growth variable as the growth rate of average employment taken over the first- and the last two years of a four-year period. We document these statistics separately for the treated and the control establishments and separately for all strata used in the baseline regressions of Figure 3 and for strata with at least four control establishments. This last restriction is important because, in panel B of the figure, we plot the evolution of employment by quartiles of the pre-event employment growth distribution.

As shown in the table, the restriction on strata with at least four control establishments leads to a substantial decrease in the number of strata, from 316 in the benchmark sample to 237. At the same time, the number of control establishments decreases relatively little, from 12,729 to 12,572. Thus, the number of control establishments per strata has a very skewed distribution. Its overall average is 12,729/316 = 40.28 establishments, but the number of establishments per dropped strata is only (12,729-12,572)/(316-237) = 1.99. Yet, dropping these strata has virtually no effect on the descriptive statistics displayed in the figure. In either case, employment growth of treated establishments is slightly above 30% in the pre-event period and about 14% in the post-event period. The corresponding numbers for control establishments are 10% and 3%. All of these statistics have substantial sampling variability, so there is quite a lot of dispersion in employment growth both between treated establishments and among their controls. We also find that there is very little serial correlation between establishment-level employment growth before and after the funding event, at least among control establishments. Indeed, in both samples the regression coefficient of post-event- on pre-event employment growth is -.002 with a standard error of .003. Notice that in either case we restrict the sample to establishments that are matched to treated establishments on initial employment and on 2-digit industry. Furthermore, the sample is a balanced panel, with 9 observations per establishment. The results in the table thus suggest that establishments belonging to the same industry and tracked from a point where they have near-identical sizes do experience a wide range of growth trajectories thereafter. At the same time, employment ***growth*** tends to revert to a common average over time.

It is important to note that the negligible impact of restricting the sample to strata with at least four control establishments on the descriptive statistics of treated establishments is an important result. After all, for each stratum that is dropped from the sample, one loses exactly one treated establishment. Given that we are starting with a moderate number of events, if heterogeneity in observed- and unobserved characteristics of establishments were large and correlated with their economic performance, one would expect that a further reduction in sample size would have a substantial impact on the statistics reported in the table. However, this is not the case, an early indication of what we find below when estimating our event-study models: There is little evidence for large treatment heterogeneity, and our point estimates tend to be precise, even with conservative clustering of standard errors.

Panel B of Figure 2 is a graphical depiction of the evolution of employment over the 9 sample years, split by quartiles of the employment growth distribution. This figure is computed as follows. First, we restrict the sample to strata with at least four control establishments so that quartiles on the strata level are distinct. Second, for each stratum we calculate the quartiles of its employment growth distribution among control establishments. Third, we estimate a panel regression of employment on the 9 dummy variables I_τ, where τ is the time period relative to the funding event. We allow the coefficients on these dummy variables to vary freely by quartile of the strata-level employment growth distribution and for treated establishments. To control for common strata-specific factors, one of which is, by construction, initial employment, a shared LMR border, and 2-digit industry, we include strata fixed effects. Fourth, we plot the coefficients on the “time” variable in the figure for each of the five groups. The intercept of the figure is group-level averages of initial employment.

Three main results come out of this exercise. First, as indicated by the simple descriptive statistics in panel A of the figure, there is quite a large variation in employment dynamics within each stratum. The first quartile of employment growth is negative, indicating that low-growth establishments become smaller over the sample period. This decline is large: On average, these establishments start with 18 employees and end up with 15 employees 8 years later. In contrast, establishments in the two middle quartiles experience very little changes in their sizes. Starting from approximately the same number of employees, the 3rd quartile ends up with approximately 2 employees more than establishments in the 2nd quartile. The fastest-growing establishments, however, grow substantially. They start on average with 18 employees and exit the sample with 27 employees.

Second, there is reversion in employment growth, but not in employment levels, a result that is reflected by the precise zero regression coefficient of past-event- on pre-event employment growth documented in panel A. Some establishments in our sample become smaller quite rapidly, others grow quickly, but eventually, their sizes stabilize.

Third, treatment establishments behave systematically differently in the post-event period than their controls. On the other hand, for the four years before they receive funding, they are very clearly not an outlier. During that period, their size trajectory is located slightly above control establishments in the third quartile of employment growth but noticeably below those in the fourth quartile. In sharp contrast, the evolution of their size diverges strongly afterward. In fact, funded establishments are the only group that experiences continued growth in the post-event period. This result already summarizes what we find in our more systematic causal inference below. GRW funding is not an exogenous shock that allows a stagnant establishment to grow. Rather, it seems that it provides the funding to allow its above-median employment growth in the pre-event period to persist afterward. In contrast, establishments with similar growth in the pre-event period stagnate eventually.

APPENDIX 4: ADDITIONAL DISCUSSION OF ESTABLISHMENT-LEVEL DID ESTIMATES

1. Matching on Counties and Matching on Growth Only

Even though the number of events in our baseline specification compares favorably to the literature on place-based policies and, more generally, to other studies estimating the causal labor market impact of establishment-level shocks, such as worker displacement shocks or patent allowances, one may still be concerned that it is too low to generalize to the broader impact of the GRW. We, therefore, experiment with two approaches to increasing sample sizes, measured in terms of the number of strata/events. The first defines contiguous regions by their county rather than municipality. Because counties are substantially larger than municipalities, this can be expected to have a major effect on sample size. Indeed, as shown at the bottom of column (3) in Table 6, the number of strata/events increases from 286 to 1,816.

Our second approach to increasing sample size consists of matching on pre-event employment growth but not on initial establishment size. The motivation for this approach is that matching on initial levels is not a necessary condition for the validity of our research design. Rather, it is identical pre-trends that is the key assumption, as discussed in section 3 of the paper. To maximize the likelihood that this assumption is satisfied we return to performing the analysis on the municipality level.[5] Since municipalities are, on average, rather small and thus unlikely to contain many establishments of similar size, we expect dropping the match on levels to increase sample size substantially. This is true as well: The number of events rises to 744.

Results for the border-county specification are shown in column (3) of Table 6. We find that point estimates generally decrease, in many cases substantially so. For example, the coefficient for the employment outcome drops from 13.78 to 7.81, and the former is not within the 95%-confidence interval of the latter. Similarly, for hires the corresponding estimate falls from 5.37 to 2.68, for the number of commuters from 7.02 to 4.58, and for the number of low-skill workers from 1.41 to .69. Interestingly, the coefficient for log wages remains nearly unchanged, and we also find a lesser impact for the average age- and tenure of employees. All of these estimates remain statistically significant. The exception is separations, where we now find a null-effect. Qualitatively, the conclusions from the decomposition exercises remain unchanged. The only substantial difference is that now the share of commuters living in the same LMR increases even further, from 49 percent of the total effect in column 2 to almost 70 percent in column 3.

What explains the drop in coefficient magnitudes when estimating the event study on the county rather than the municipality level? As we will show in the section on coefficient heterogeneity, to a large extent, the decrease comes from an underrepresentation of East-German projects in the municipality sample and an overrepresentation in the county sample due to much lower population density in that region of Germany. This has a major impact on our findings because estimated treatment effects are much smaller in East than in West Germany. We will explore this issue further below. At this point, it is worth highlighting that even our smallest estimates of the labor market impact of the GRW, those in column (3)

[5] In both approaches we continue to match on 2-digit industry.

of Table 6, are still economically- and statistically significant. Furthermore, our major conclusions remain unchanged qualitatively.

As for the specification that removes that matching on initial firm size, we find no substantial impact on coefficient estimates, but an increase in precision. For example, the coefficient for employment decreases slightly from 13.78 to 12.1 and for hires from 5.37 to 4.51, and it increases for separations from 1.03 to 1.24. Only for the number of commuters do we find a near doubling of the coefficient, though here standard errors are large as well. More generally, estimates in columns (2) and (4) tend to remain in each other's confidence intervals. The main takeaway from this set of results is that the causal impact of GRW funding does not seem to depend in a quantitatively important way on the initial size of establishments. Otherwise, point estimates in columns (2) and (4) should differ more strongly. Another reasonable conclusion is that even with a relatively limited number of events, we obtain remarkably precise estimates. Taken together, the asymptotic prediction that our estimates should not depend on whether one matches on initial levels of employment or not seems to be confirmed by our estimates obtained from moderately sized samples.

2. Pairwise Matching

Control establishments in all of our event-study models are those with above-median employment growth during the pre-event period, where the median is strata-specific. The advantage of this approach is that it has compelling visual evidence, as documented in Figure 2, and that there are no tuning parameters for the matching procedure. Furthermore, it allows for more than 1 control establishment per treated establishment, thereby increasing precision. However, it is a non-standard approach to constructing a control group. In this section, we use a more common approach that relies on pair-wise matching. In particular, within each stratum we choose the "nearest neighbor" of the treated unit in terms of pre-event growth as potential control establishment. We discard all strata in which the absolute value of the growth difference between treated- and control establishments is more than 5 percentage points. To keep the number of strata reasonably high, we do not match on initial employment in this specification. This leaves us with 468 events/strata, each of which has exactly one treated and one control unit. Results are shown in the last column of Table 6. Coefficient estimates are 13.27 for employment, 3.68 for hires, 2.14 for separations, 6.73 for commuters, 1.4 for marginal employment, and .15 for log wages. Hence, for our 6 core outcomes, the estimates from this approach to matching are, in almost all cases, statistically and economically very similar to those from our benchmark specification in column 2. The same applies to the results from the decomposition exercises and to the estimates for age and tenure. Standard errors tend to be quite a bit smaller than in column (2) and somewhat larger than in column (4). Hence, it seems to be the number of strata, which is lower in the benchmark specification and higher in the matching-on-growth specification in column (4), rather than the average number of controls per stratum, which is larger in both of these specifications, which matter for the precision of our causal estimates. For our main empirical conclusions, this does not matter much. When constructing strata from border municipalities, our estimates are remarkably robust to whether we match on initial employment and whether we use a standard pairwise match instead of our approach to achieve balance on pre-event employment growth.

APPENDIX 5: EFFECT HETEROGENEITY

A general conclusion from Table 6 is that our results are rather robust across various specifications distinguished by how comparison groups were constructed, even though the number of events, and thus treatments, differed greatly among them. Because each stratum corresponds to one treatment, this suggests that treatment heterogeneity may not be great. Only when using border counties- rather than municipalities have we documented some substantially lower, albeit still highly significant, estimates than in our benchmark specification. In this section we explore more systematically to which extent our results depend on the sample. We also find a clear answer as to why it matters in our context whether one matches on border counties or border municipalities.

Results are shown in table 7. The list of subsamples, varying across columns of the table, together with our reasoning for why we choose these particular selections, is given in the following:

- ***Contiguous border pairs located along the "inner-German" border (col 2):*** Before the German reunification, West Germany provided subsidies to establishments that remained active in the economically disadvantaged regions along this former border, also referred to as "Zonenrandgebiet". Ehrlich and Seidel (2018) estimate the economic effect of this pre-unification place-based policy and find that they are persistent. Because there is a discontinuity in policy parameters along this border for our sample period, one may be concerned that the shadow of the Zonenrandgebiet policy confounds the effect of the GRW. We therefore exclude this region from our sample in column 2.

- ***East Germany (col. 3) versus West Germany (col. 4):*** For a substantial part of our sample period, there is very little variation in policy parameters among LMRs in East Germany. In particular, because of the persistently poor economic performance of East Germany, almost all of its LMRs were eligible for the highest subsidy rates. We therefore explore to which extent our estimates are driven by West Germany.
- ***Contiguous border pairs that do not include the state of Berlin (col. 5):*** Berlin is by far and large the biggest municipality in Germany. It is located in East Germany, where in later funding periods it was one of the only municipalities not eligible for the highest funding rate, thereby becoming an important "donor" of control establishments. Another issue is that with Berlin being the capital of Germany, it does receive other types of subsidies, which may be viewed as "place-based." We, therefore, explore the impact on coefficient estimates after removing Berlin from the sample.
- ***Non-service sector establishments (col. 6) versus service sector establishments (col. 7):*** Germany has an unusually large manufacturing sector among rich countries. This is particularly true for East Germany, where the lack of growth in the service sector raises concerns. From a policy perspective, exploring the heterogenous impact of the GRW policies across these two sectors is important. It also serves as a point of comparison to Criscuolo et al. (2019) who evaluate a place-based policy in the UK which is similar to the GRW but focuses on the manufacturing sector.
- ***Small establishments (col. 8) versus large establishments (col. 9):*** A recent literature in macroeconomics focuses on the importance of the firm-size distribution on economic growth. Bachmann et al. (2022), for example, argue that the lack of large firms in East Germany can explain to a large extent its underperformance in terms of productivity. Estimating coefficient heterogeneity by firm size speaks to this literature.
- ***Strata that correspond to funding events that took place no earlier than 2004 (col. 10):*** Since the distinction between place-of-birth and place-of-residence is introduced in the IAB data in 1999, 2004 is the first year in which we can calculate full pre-trends for all variables relying on this distinction. In particular, for funding events prior to 2004 it is not possible to estimate pre-trends for the entire pre-event period for the commuter share. We, therefore, explore if this has an impact on our estimates.
- ***Strata that correspond to funding events that took place no earlier than 2006 (col. 11):*** In 2006, the EU-scoring model did not allow the parameters entering its scoring rule to vary between East- and West Germany anymore. This may be viewed as a sufficiently large change to the scoring model to explore if it had an effect on our estimates.

For comparison, the first column of the table reproduces the baseline estimates from Table 6.

Since our findings from this table paint a rather clear picture, we will only focus on the main qualitative patterns rather than a more detailed comparison of individual parameter estimates. Generally, we find estimates that are remarkably robust for the flow variables, that is, for the number of hires and separations. At the same time, small differences in flow rates can have a substantial impact on stocks, and we do indeed find more variability in estimates for the number of employed and for the number of commuters. However, they are qualitatively consistent and tend to be located within each other's confidence intervals. Overall, there is a limited amount of coefficient heterogeneity. Importantly, there is no evidence that keeping all years and all candidate municipalities in our sample has any substantial impact on our estimates. Also noteworthy are our findings that GRW funding had a larger policy impact among service sector establishments and among large establishments. However, in the case of splitting the sample based on establishment size, one needs to keep in mind that the dependent variable is the raw number of workers. Thus, a larger coefficient for bigger establishments is to be expected if such establishments have a general tendency to hire more workers. Furthermore, it is important to note that by design of the GRW even below-median size establishments are relatively large.

There is one exception to the general robustness of coefficient magnitudes: Splitting the sample into East and West Germany does indeed affect our estimates substantially. Generally, GRW-funding seems to be less effective in the Eastern part of the country than for the Western part and for the pooled sample. Particularly interesting is that separations, commuters and low-skill workers play a much smaller role in the employment impact of the policy in East Germany. These results need to be interpreted with care, however. From a purely statistical perspective, identification of the policy impact in East Germany is difficult because, as shown in Figure 1, there is very little cross-sectional- and time-series variability in program generosity. In particular, with average wages and aggregate productivity still lagging behind West Germany – current estimates place them at less than 80% of the West German values – for most of the funding periods, the largest part of East Germany is eligible for the highest funding rates. Strongly discontinuous changes at the borders of LMRs, a common case in West Germany, are rare in East Germany. On the other hand, East Germany reacting differently to policy interventions than the economically stronger West should not be particularly surprising in light of its generally weak economic performance over the sample period.[6] First, compared to West Germany, the five Eastern states have, on average, smaller establishments and a relatively inflated manufacturing sector. Both mechanically yield a lower policy impact because they are exactly the groups for which we find smaller coefficient estimates in a geographically pooled sample, as discussed above and shown in Table 6. Second, East Germany is less population dense than West Germany, and it has substantially higher un- and non-employment rates. Both may tend to suppress the reliance on commuters and low-skill workers. More generally, labor market opportunities may be less favorable than in the West, even conditioning on eligibility, explaining the negligible impact on separations. A more detailed analysis of the relationship between aggregate conditions and the effectiveness of place-based policies is, due to the lack of policy variation in Eastern Germany, infeasible, at least with our research design.

One issue with the estimates in panel A of the table is that, in some cases, the number of strata becomes quite low. For example, there are only 85 strata in East Germany and 68 strata with small establishments. As before, we increase sample size by re-estimating all models on a sample that uses border counties rather than border municipalities for constructing strata. Overall, our conclusions from Table 6 and Panel A of Table 7 remain unaltered: County-level estimates tend to be smaller for most outcomes, and there is a limited amount of coefficient heterogeneity. The exception again is the split of the sample into East- and West Germany.

The latter result has another important implication: Comparing estimates in columns (3) and (4) between the two panels indicates that once one conditions on all strata being located in either East- or in West Germany, coefficient magnitudes do not drop substantially if one moves from the municipality-level- to the county-level geographical matching. How then can the large drop between columns (2) and (3) in Table 6, where we perform the same exercise but for the entire area of Germany, be explained? The answer comes from the number of strata included in each of these empirical exercises. First, moving from the municipality- to the county-level geographical matching adds no less than 1,530 strata/events to the sample. Among these additional strata, East Germany is overrepresented compared to the baseline specification. In particular, of the 286 strata in Panel A, 30% are located in East Germany. In Panel B on the other hand, this share increases to 55% as 1,009 of the 1,816 county-level strata are located in that part of Germany. We thus find that the GRW policy has been much more effective in improving local labor

[6] Descriptive statistics for East German worker- and establishment performances are provided for example in Hoffmann and Lemieux (2015) for industrial composition, Bachmann et al. (2022) for establishment size and productivity and Heise and Porzio (2023) for worker mobility.

market conditions in West Germany. To some extent, this is because the service sector is relatively bigger and because it attracts more large firms.

A final question we address is whether the program has more of an impact if its funds go to establishments in capital-intensive industries. After all, the GRW amount per project is determined by the volume of investment in physical capital. To keep sufficiently many strata per industry we use a fairly coarse industrial classification and only report results for the county-level analysis. Estimates are shown in panel C of Table 7. We find the largest estimates for “Trade and Transportation” and “Other Services and Public Administration”. On the other end of coefficient magnitudes is the industry group of “Communications, Finance, Insurance, Real Estate” where all estimates are insignificant and small. At the same time, this sector does not seem to attract many funded projects: Of the 1,816 funding events, only 32 take place here. Other sectors with relatively small, but still economically and statistically meaningful, effects are “Manufacturing” and “Hospitality.” Overall, these findings are suggestive that the relationship between program impact and capital intensity is of minor importance.

APPENDIX 6: PLACEBO ANALYSIS

The central identifying assumption for the parameters β_τ in equation (5) is that treated establishments and their controls face identical aggregate economic trends. It is exactly due to the centrality of this assumption that our identification strategy relies on border discontinuities in policy parameters. Yet, the results presented above are strong evidence against this assumption. In light of these findings, how should one proceed to obtain credible estimates of the policy impact of GRW subsidies? One may be tempted to modify our empirical approach by constructing control establishments that are matched on employment growth in the pre-event period to the treated establishment. Ultimately this is indeed what we do. However, matching on pre-trends requires justification because a naïve implementation cannot convincingly address the violation of the common pre-trend assumption. After all, this identification assumption is a population-level assumption about unobserved conditional moments, while matching on pre-trends is an algebraic exercise of fitting control establishments to trends of treated establishments, using data that have already been drawn. As long as there is some idiosyncratic variation in employment growth and as long as there is at least one establishment in the sample of potential controls that has a larger pre-event growth rate than the treated establishment, it will always be possible to construct an average synthetic control establishment that "looks like" the treated establishment. Importantly, this is true even if the common pre-trend assumption is violated. Being able to fit synthetic controls to the pre-trends of treated establishments does not imply that the common pre-trend assumption is satisfied.

We, therefore, do not immediately implement the specification we refer to as "Matching on Pre-Trends" in section 3. Rather, we first explore whether the discrepancy in employment growth rates in the pre-event period between treated establishments and their controls are indeed driven by differential aggregate economic trends on each side of the border of an LMR. To this end, we implement "placebo tests" in which we replace treated- with untreated establishments located in the same municipalities while using exactly the same control establishments as before. A central issue with this empirical test is that the rising hiring activity of the treated establishment documented above is likely to affect other establishments in the same municipality as well. In the extreme case where this establishment hires all its additional workers from other establishments in the same municipality, pre-trends in employment between treated establishments and their placebos would be exactly inversely related. This will generate what looks like a violation of the common pre-trend assumption among placebo establishments. What makes placebo tests meaningful and informative in our context anyway is the fact that the common pre-trend assumption is about aggregate trends on the municipality level rather than individual-level trends on the establishment level. Common unobserved economic trends predict that the employment of all establishments in the same municipality should move in the same direction. The "spill-over" mechanism, on the other hand, makes exactly the opposite prediction on employment.

Results from the placebo tests are shown in Figure 7, using the same outcomes as in Figure 3 and displayed in the same order. Compared to the regressions underlying Figure 3, we keep the same control establishments but replace the treated establishment with establishments located in the same municipality, belonging to the same 2-digit industry, and starting from the same initial level of employment. Placebos are, therefore, matched to the treated establishments in the same way as control establishments, with the difference that the former are located in the municipality containing the treated establishment. Treated establishments, on the other hand, are dropped from the sample. A practical issue is that since the average municipality in the sample is relatively small, we do not find a placebo-treated establishment for every actually treated establishment. This is the case for 148 of the strata, leaving 168 strata for the placebo regressions.

As the first panel of Figure 7 shows, there is no evidence of pre-trends in employment. Not only are the point estimates smaller by orders of magnitudes as in the corresponding panel of Figure 3, but they also have wide confidence intervals, all of which include an effect of zero. This lack of any evidence of pre-trends in employment is inconsistent with the presence of differential unobserved aggregate trends. Interestingly, the lack of any significant differences in employment between placebo-treated establishments and control establishments persists throughout the 9 years over which we track them. Furthermore, the same conclusions can be drawn for the outcomes "hires" and "marginally employed". In fact, for all 6 core outcomes, we find no evidence for any differential pre-trends between placebo-treated- and control establishments. For the post-event period, we find no significant effects of the GRW either, with the exception of separation rates and wages. If firm-level treatment has spill-over effects to other establishments in the same municipality, they are rather small.

A slight outlier is the commuter variable. Even though each of the eight coefficients is insignificant, visual inspection suggests that placebo-treated establishments are on the inverse trend to actually treated establishments in the sense that their number of commuters is decreasing steadily over the sample period. There are at least two reasons why this is not a cause for concern. First, if the differential pre-trends documented in Figure 3 were driven by differential aggregate economic trends, then one would expect that the number of commuters to placebo-treated firms and actually treated firms would move in the same- not the opposite direction. As argued above, the evidence is more consistent with a local spill-over effect, whereby treated establishments hire some workers from their local competitors, and these workers are commuters. Second, the point estimates are, in absolute value, only one-hundredth of the corresponding estimates in Figure 3. Economically, this finding is insignificant. We, therefore, conclude from Figure 7 that there is no evidence for differential aggregate trends between municipalities on each side of the border that generate differential aggregate employment growth. At the same time, there is some, albeit marginal, evidence of spill-over effect in the post-treatment period.

One concern with the exercise documented in Figure 7 is that it is based on a subsample of strata used in Figure 3 because we cannot find a placebo-treated establishment for every actually treated establishment. Furthermore, aggregate trends should affect other establishments that are not matched on either initial employment or 2-digit industry as well. Indeed, it is neither plausible that differential pre-trends operate only for establishments that are identical with respect to initial size and 2-digit industry, nor that treated establishments hire only from these establishments. Not including them may, therefore, mask differential pre-trends between placebo- and actual controls. To address these concerns we gradually relax the matching criteria. In Figure 8, we show coefficient estimates when dropping the requirement that either control establishments or placebos are matched on initial employment to the treated establishment, and in Figure 9, we additionally drop the requirement that they are part of the same 2-digit industry. This has the effect that the entire population of establishments on each side of an LMR is included in the sample, with the exception of the treated establishment. Because no municipality in our sample contains only a single establishment, this also adds back all municipalities that we lost in Figures 7 and 8 so that the municipalities from which establishments are drawn are identical to those in Figure 3. Econometrically, with all non-treated establishments included in the regressions, our placebo regressions are akin to estimating event-study DiD models on the municipality level, albeit controlling for strata-specific time trends. This restricts across-group comparisons to contiguous border municipalities. Because of this, figure 9 is also a test of the identifying assumption of the aggregate IV models estimated in Section 5.

The results from these additional placebo regressions are broadly consistent with those documented in Figure 7. If anything, relaxing the matching criteria and thereby gradually increasing sample size further lowers the precision of our estimates relative to coefficient magnitudes, suggesting that there is little evidence for systematic differences between contiguous border municipalities. For example, event-study estimates for the employment variable in Figure 9 are essentially a sequence of zeros in the pre-event period. With the exception of the commuter variable, there is also no evidence for differential pre-trends in the other outcome variables. In the case of the commuter variable, we find the same pattern of a negative- rather than a positive pre-trend, as in Figure 7.

It is worth emphasizing that the results in Figures 8 and 9 are particularly reassuring for two reasons. First, in the pre-event period, our estimates are distributed noisily around zero even though underlying sample sizes are large. For example, in Figure 9, we have 177,897 placebo-treated firms and 326,138 control firms in only 316 strata. If there were any underlying differentials in aggregate pre-trends, one would expect that these formidable sample sizes are sufficient to uncover them with some precision. Second, coefficient sizes are very small, about two orders of magnitudes below those in Figure 3.

On the other hand, it is worth highlighting that for three of the six outcomes we do find some evidence for positive spill-over effects in the post-treatment period, namely for employment, the number of hires and log-daily wages. As for the pre-event coefficients, the magnitudes are small, but this should be expected in light of the underlying sample sizes, and given that, at least in the case of employment and hires, we use raw counts as outcomes.

Halle Institute for Economic Research –
Member of the Leibniz Association

Kleine Maerkerstrasse 8
D-06108 Halle (Saale), Germany

Postal Adress: P.O. Box 11 03 61
D-06017 Halle (Saale), Germany

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

www.iwh-halle.de

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