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ABSTRACT

We study the granular wage and employment effects of a German place-based policy using a research design that leverages EU-wide rules governing program parameters at the regional level. The program subsidizes investments to create jobs with a subsidy rate that varies across labor market regions. We use 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. Spatial spillovers of the program linked to changing commuting patterns can be assessed using information on place of work and place of residence, a unique feature of the data. 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 level are slightly larger than establishment-level estimates, suggesting limited economic spillover effect. Spatial spillovers are large as over half of the employment increase comes from commuters. 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.

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1. Introduction

In many countries, rich and poor, governments operate so-called place-based policies that use subsidies or tax advantages to financially support private firms in selected geographical areas. The typical motivation behind these policies is to create jobs in economically disadvantaged regions to help narrow spatial socio-economic inequalities. Whether they are cost-effective remains 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 place-based policies. For example, Bartik (2020) estimates that the United States spends $60 billion annually on such programs. Germany, the country we study in this paper, has spent over a trillion euros supporting firms, individuals, and local governments in Eastern Germany alone since reunification in 1990. Recent findings in Juhasz et al. (2023) suggest that industrial policies, which often have place-based characteristics, have become even more prevalent over the last decade, with the US Inflation Reduction Act of the Biden Administration being a particularly noteworthy example.[2] More generally, place-based policies seem to be experiencing a revival, and economists such as Austin, Glaeser, and Summers (2018), traditionally highly skeptical of these programs, have started to formulate principles for "effective place-based policies in the 21st century".

Empirical evidence evaluating a plethora of place-based policies around the world suggests that they often have sizeable positive employment effects in targeted areas.[3] Most of these studies focus on the aggregate-level impact of these policies at the regional level. Since financial aid is typically directed at specific firms, these aggregate effects are best interpreted as intent-to-treat (ITT) effects on employment rates and related outcomes. However, aggregate effects are of limited use for evaluating the welfare implications of place-based policies for several reasons. First, aggregate employment measured as a stock variable cannot be used to study whether place-based policies increase hiring rates or decrease job separation. This is an important limitation because a key principle of welfare-enhancing place-based policy must be that in targeted geographic areas, jobs can be created relatively "cheaply", that is, along highly elastic labor supply curves of workers at the margin of employment. Similarly, employment rates mask potential regional spillovers whereby subsidized firms hire workers from non-targeted areas, thereby diluting any positive effects of place-based policies.[4]

[1] See Glaeser and Gottlieb (2008) for a review of the arguments in favor and against place-based policies.
[2] Juhasz et al. (2023) find textural evidence of a resurgence of industrial policies in the post-Great-Recession era. Place-based policies are only one type of industrial policies, but in many cases the latter have place-based characteristics due to political or legal considerations. For example, EU pro-competition laws tend to generate industrial policies that are de-facto, but not necessarily de-jure, place-based.
[3] See the reviews by Kline and Moretti (2014b) and Neumark and Simpson (2015).
[4] These points are discussed in depth in Austin, Glaeser, and Summers (2018) and Duranton (2018).

Second, aggregate-level analyses do not shed light on which groups of firms and workers benefit from place-based policies. Well-designed policies should arguably target firms with a good potential for long-term growth, but little is known about the characteristics of the funded firms. Furthermore, it remains unknown how place-based policies translate into firm-level labor-market outcomes, such as the extent to which subsidized firms hire from other firms or the pool of non-employed individuals and whether their hiring practices change the local skill structure of employment. Yet, all of this is crucial for understanding how modern place-based policies should be designed.

In this paper, we fill these important gaps 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) on establishment-level labor market outcomes.[5] [6] There are several reasons why the GRW provides an ideal context for estimating these effects. First, the GRW is large, with an average expenditure per establishment-year of about EUR 300,000 over the sample period (2000-2016). Furthermore, unlike similar programs in other countries, the GRW does not restrict eligibility to the manufacturing sector. This makes the GRW policy substantially more generous and broad than, for example, the British Regional Selective Assistance Program evaluated by Criscuolo et al. (2019). [7]

A second advantage of studying the GRW is that pro-competition laws of the European Union constrain the local disbursement of subsidies. Local eligibility for subsidies and the subsidy rate on investments are determined by a scoring model that generates spatial discontinuities in policy parameters, and local governments cannot manipulate how the subsidy borders are drawn. This sharply contrasts with the situation in other countries like the United States, where different levels of government have much fewer constraints on how establishments in different local areas can be subsidized. Our research design, a matched establishment-level event study, crucially relies on this spatial variation in program parameters.

Finally, Germany operates a data infrastructure that enables us to merge the universe of German administrative matched employer-employee data with the universe of administrative funding data of the GRW program and its policy parameters. This yields the first data set that combines detailed labor market information 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. A particularly useful feature of these data is that

[5] GRW is the acronym for "**G**emeinschaftsaufgabe Verbesserung der **R**egionalen **W**irtschaftsstruktur".
[6] We refer to "establishments" rather than "firms" because this is the level of observation in the data we will be using. GRW subsidies are paid to establishment-level projects. See Sections 2 and 3 for more details.
[7] Criscuolo et al. (2019) report average totals per establishment-year between £56,000 (EUR 92,000) in the late 1990s and £36,000 (EUR 59,000) in the 2000s. The UK program in their study is also restricted to the manufacturing sector. Many other place-based policies, such as those in Japan, also target a specific industry. In sharp contrast, only the natural resource sector is excluded from the GRW.

they report both place-of-work and place-of-residence of employees so that we can directly measure local spillovers.

We use these unique data to answer several questions: Which establishment baseline characteristics predict participation in the program? Based on the dynamic effect of subsidies on hiring- and separation rates, is there evidence that establishments treat subsidies as a "windfall" to co-fund investments they would have undertaken anyway? Are there program spillovers linked to workers' flows into funded establishments coming from non-employment, other establishments in the same local area, and establishments in other local areas? Does the pattern of heterogeneous effects indicate that the program reaches the target population defined by educational attainment, tenure, and labor market status? What is the effect of the policy on wages, which is linked to the labor supply elasticity at the firm level?

We address these questions using an establishment-level event-study design that compares funded to control establishments for 9 years – 4 years before and after the year the subsidy is paid to the treated establishment.[8] Finding appropriate control establishments is essential to our research design for two reasons. The first is that, by design, the GRW targets economically underperforming regions where establishments likely grow slower than in other regions. We tackle this issue by leveraging exogenous regional discontinuities in program parameters on the level of labor market regions (LMR) that the GRW's scoring model generates. Since LMRs are geographically connected groups of municipalities (Gemeinden), these spatial discontinuities are observed between contiguous municipalities that likely share similar aggregate trends. We utilize them by selecting treatment and control establishments located in contiguous municipalities with different program parameters.

The second challenge is that since establishments self-select into the program, they may not be a random draw from the distribution of establishments. We address this issue by matching treated and control establishments based on a rich set of characteristics, including the level and growth in establishment size in the pre-treatment period. One disadvantage of matching on pre-trends is that we can no longer test the validity of the research design by comparing pre-trends for treated and control establishments, an issue that we address in three ways. First, we match on pre-trends for a single outcome variable, the number of employees. This leaves 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, for which we can test for pre-trends. Second, we implement placebo tests on various levels of aggregation, where we replace the treated establishment with a placebo with the same

[8] Observing establishment characteristics prior to treatment is required for finding comparable control establishments that did not receive funding. This limits our analysis to the impact of GRW funding on establishment expansion rather than creation. ITT estimates at the regional level combine these two effects of the policy.

matched characteristics, including its location, to estimating the event study on a sample of all untreated establishments in municipalities on both sides of the border. Third, we estimate "pass-through" regressions – the effect of a euro of GRW funding on employment outcomes – at the establishment level and compare them to their aggregate counterpart using a more conventional research design at the municipality level.

Our main findings are as follows. First, we find that fast-growing as opposed to declining establishments select into treatment, contradicting the commonly-held expectation that the opposite is the case.[9] At the same time, funded establishments are not outliers. Rather, their pre-treatment employment growth lies, on average, slightly above the median employment growth of establishments in "donor" municipalities. After matching to control for these small differences in pre-treatment employment growth, we find no evidence of pre-trends in all other outcomes we consider. The placebo analysis also bolsters the validity of our research design.

Second, the estimated effect of subsidies on treated establishments is large and highly persistent. Starting from an average of about 20 employees in our matched sample, treated establishments funded at the average subsidy rate add about seven more jobs over the post-event period relative to control establishments. This effect is driven by increased hiring rather than reduced separation rates. Separations eventually catch up, but only to the extent that establishment size stabilizes at a new and higher level. This remains true even 10 years after treatment. There is thus little evidence that funded establishments treat subsidies merely as a windfall.

Third, more than half 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. Interestingly, while we do not find significant positive wage effects for this group, incumbent workers experience faster wage growth than controls.

Fourth, our estimates are remarkably robust, both qualitatively and quantitatively, across a wide variety of specifications. There is also limited effect heterogeneity across establishment characteristics, such as establishment size, location, and industries. Importantly, the policy is as effective in capital intensive as in capital non-intensive industries. This is a novel result because most existing empirical research comes from place-based policies that target the manufacturing sector.

[9] This view is articulated for example in Neumark and Simpson (2015).

Fifth, the municipality-level pass-through estimates are only slightly larger than their establishment-level counterpart, which supports the validity of the establishment-level research design. The estimates suggest that the GRW investment subsidy amounts required to create one additional job are approximately EUR 20,000 at the municipality level and EUR 25,000 at the establishment level. One interpretation of the relatively small difference between these estimates is that there are no major spillover effects to non-treated establishments in the same municipality. This is, in fact, consistent with our placebo regressions, where we find that non-treated establishments on each side of the border have similar growth rates in outcomes both in the pre- and post-treatment period.

We conclude that the GRW policy partially adheres to the principles of place-based policies for the 21st century, as formulated in Austin, Glaeser, and Summers (2018). It creates jobs in the medium to long run at a relatively small per-worker cost because a substantial proportion of new hires come from the target population, namely workers at the margin of employment with high labor supply elasticities. However, the policy also has at least two likely distortionary effects: It creates wage gains for incumbent workers with strong labor force attachment, and it has major geographic spillovers. Our paper is the first to directly quantify the latter by explicitly using commuter information. The finding that the GRW has a substantial impact on commuter flows suggests that the potential issue of regional spillovers emphasized in recent theoretical work on optimal place-based policies, such as Gaubert et al. (2025), is of first-order importance.

Our paper contributes to several important literatures. An extensive literature on the effect of place-based policies on local labor market outcomes focuses on the estimation of aggregate ITT effects at the regional level. Neumark and Simpson (2015) and What Works Centre (2016 a, b) provide an extensive discussion of these programs.[10] Regardless of the level of geographic targeting, a significant limitation of these studies is that they apply, by design, to depressed areas. This issue is particularly severe in the US, where different levels of government have a lot of freedom in picking subsidized firms.

Recent studies have, therefore, focused on programs that feature institutional constraints that lend themselves to more credible research designs. In the context of the GRW, Brachert, Dettmann, and Titze (2019) use a

[10] 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 and Simpson (2007)) for the RSA program in the UK, Bronzini and de Blasio (2006), Bernini and Pellegrini (2011), de Castris and Pellegrini (2012), and Cerqua and Pellegrini (2014) for the Law 488/1992 in Italy.

regression discontinuity design to estimate the effect of eligibility on various county-level outcomes, and Siegloch, Wehrhöfer, and Etzel (2025) estimate the dynamic causal effect of recent drops in subsidy rates in East Germany on county-level employment. Arguably closest to our paper are the studies by Criscuolo et al. (2019) for the Regional Selective Assistance Program in the UK and LaPoint and Sakabe (2024) for the Technopolis Policy and the Intelligent Location Policy programs in Japan, both of which combine plausibly exogenous changes in policy rules with rich firm-level data to estimate intent-to-treat effects of the policies. However, for all of these studies, treatment variation is on the level of geography or geography-industry, so firm-level data are implicitly or explicitly aggregated to the geographic level of policy variation. In light of this limitation, our main contribution is to estimate the effect of subsidies ***actually received*** at the establishment level rather than the effect of the subsidy rate a county(-industry) is eligible for on ***detailed labor flows*** within- and across areas at an exceptionally detailed geographic level.[11]

Establishment-level treatment is also observed in Greenstone, Hornbeck, and Moretti (2010)'s "million dollar plants" study. Relative to that study, we focus on the funded establishment itself as opposed to spillover effects on other establishments in the same geographic area. Furthermore, the "million dollar plants" tend to open in areas with particularly favorable socio-economic characteristics and are thus not part of the types of place-based policies considered here. Even though their focus is on a very different causal effect than ours, it is nevertheless worth noting that some of our placebo regressions are very close to their main empirical models. Interestingly we do not find evidence for major spillover effects in the post-event period. While we interpret this as supporting our identification assumption of identical aggregate trends in contiguous municipalities, this finding also suggests that agglomeration effects from the expansion of small- to medium-sized establishments in socio-economically disadvantaged regions typically targeted by the GRW are of limited magnitude.

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. (2023) look at what happens to winning and losing construction firms that are bidding in 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

[11] There are a number of empirical studies that, like us, were able to match funding amounts to treated establishments. The studies we are aware of are Bernini and Pelligrini (2011), Cerqua and Pellegrini (2014), Pellegrini and Muccigrosso (2017), Decramer and Vanormelingen (2016), Brachert, Dettmann and Titze (2018), and Dettmann, Weyh and Titze (2024). None of these studies estimate the causal effects of establishment-funding on detailed labor market outcomes and commuter patterns, which is a primary contribution of our paper.

case, the GRW subsidy is the source of the shock, and our wage estimates suggest that funded establishments have some market power. The magnitude of the distortionary effects of place-based policies depends crucially on the slope of labor supply curves, and our estimates help quantify this parameter.

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 popular redistribution policies, such as income assistance for poor households or earned income tax credits. They also inform the recent theoretical characterizations of optimal place-based policies of Gaubert et al. (2025) and Fajgelbaum and Gaubert (2025). Using spatial equilibrium models with regional spillovers, they 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. Thanks to the richness of our data, our paper helps quantify this argument by providing evidence on the magnitude of regional labor market spillovers and on who precisely benefits from place-based policies in economically depressed areas.[12]

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.[13] 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.[14] The program's generosity is determined by subsidy rates that vary across three establishment-size categories and multiple eligibility groups. Only establishments with supra-regional sales are eligible for the program. In contrast to many place-based policies, such as those in Japan or the UK, the GRW is not restricted to the manufacturing sector – only very few industries, most importantly the natural resource sector, are ineligible.

[12] 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.

[13] 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.

[14] Strictly speaking, municipality-level subsidies can also be used for non-investment-related activities, as long as they help boost an area's competitiveness.

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.[15]

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.[16] 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 varies at the level of 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.[17] 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

[15] Berkowitz, Storper, and Herbertson (2025) provide a detailed overview of the legislative framework for place-based policies and its history in the EU.

[16] 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").

[17] In 2017, the year we use for normalizing the geographic classification in our data, there were 258 LMRs and 11,053 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.

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 for the early years of our sample (January 2000 to January 2004) and the last year funded projects start in our sample (year 2016). 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.[18] Second, in both periods, many LMRs in West Germany are eligible but are surrounded by non-eligible LMRs. Municipalities located along each side of their borders are the primary "donors" for our treatment and control establishments. Third, for 2016, 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 (including Berlin) 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 after 2014. 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.[19] 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

[18] Siegloch, Wehrhöfer, and Etzel (2025) study the county-level intent-to-treat impact of the GRW in East Germany using time variation in subsidy rates.

[19] Key legislative documents are listed in Appendix Table 7.

the highest subsidy rate.[20] 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.[21]

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 used this option by adding a first set of "C" regions with a lower subsidy rate meeting the EU-wide population share rule mentioned above.[22] Germany also added a 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).[23]

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.[24] 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.

[20] These EU policies are described in the "Guidelines on National Regional Aid". For references, see the last column of Appendix Table 7.

[21] Weights by funding period are listed in Appendix Table 2.

[22] 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 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%.

[23] 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 7.

[24] 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%.

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 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: approximately 0.7 percent of all projects in the administrative data had to pay back the subsidies.

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 six funding plans in total, two for the first EU funding period (2000 to 2006), three for the next (2007 to 2013), and one for the last three years during which projects in our data are initiated (2014 to 2016).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.[25] This is an increase in generosity even when accounting for inflation. In contrast, we do not detect any noticeable trend in average funding per establishment, which is shown in the last column of the table. This number ranges from approximately EUR 424,000 per establishment for the funding plan beginning in July 2014 to EUR 673,000 per establishment for the funding plan starting in February 2011.

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

[25] 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).

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- 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.[26]

3. 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 about the data construction are provided in Appendix 3.1.

[26] The same applies to the EFRE/ESF-programs, i.e. those programs funded by EU Cohesion policy.

3.1 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 et al. (2022).

The establishment data are processed as follows. We start with the universe of establishments that reported a positive number of employees at least once between 2000 and 2016. 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 so that inflows- and outflows of commuters can only be computed starting in 2000. It stops in 2016 because we need 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.[27] 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 income ("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

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

outcomes by various subgroups (e.g., by education, commuters from other LMRs, etc.). Further details on these variables are provided in Appendix 3.1.

Panel A of Table 2 reports sample sizes for the administrative employment data from which our establishment panel is built. We start with the full-population data, which has 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. Of the 402 LMR's and 11,053 municipalities in Germany, 247 LMR's containing 7,468 municipalities are left after conditions (a) and (b) are imposed on the data.

3.2 Administrative GRW Project Data

Information on GRW funding comes from the Federal Office for Economic Affairs and Export Control (BAFA).[28] 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.[29] An establishment may apply for multiple projects in the same funding period.[30] 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.[31] In cases where an establishment receives subsidies for multiple projects with non-overlapping project dates, we follow Kline et al. (2019) and only keep first-time projects within our sample period (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

[28] The BAFA has the status of a federal agency and is subordinated to the Federal Ministry for Economic Affairs and Climate Action.

[29] We computed funds as eligible cost times the subsidy rate (both in actual numbers, as opposed to the target numbers).

[30] 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.

[31] 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. Money is paid out by banks at project initiation however, and this is why we define its date as event time.

German Federal Employment Agency.[32] 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. More details on the linkage are provided in Appendix 3.2. As shown in the top portion of Table 2, Panel B, we managed to match over 80% of the projects to the corresponding establishment. This rate increases after establishment identifiers were introduced in the GRW data in 2004.[33] For the 2000 to 2016 period, the GRW funded 40,790 projects in 28,603 establishments over. There are more projects than establishments since some establishments received funding for multiple GRW-funded projects.

3.3 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 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 3.1 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

[32] 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.

[33] 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.

[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.

[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.

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. Establishment-Level Analysis

In this section, we describe our establishment-level event-study research design, its implementation, and our main empirical results. Various validation tests that are conducted on a higher level of aggregation of the data will be discussed in section 5 of the paper.

4.1 Econometric Model

Our firm-level event study relies on a comparison between funded establishments in a treated municipality and unfunded establishments in contiguous border municipalities where the GRW program is less generous.[36] The main outcome variables are establishment size, hiring- and separation rates, the number of commuters among employees, the number of marginal employees, and the wage structure, some of which we decompose further by worker background characteristics. Comparing pre-trends of treated- and untreated establishments for a sufficiently long period is possible only for establishments that have existed for some time before the funding event. We, therefore, limit the event studies to establishments operating for at least nine consecutive years around treatment, including four years before the funding event.[37] This restriction implies that we do not consider firm openings subsidized by the GRW.

To describe the empirical model formally, let *i* index establishments and define D_i as a dummy variable indicating whether an establishment received funding. Define a stratum by a group consisting of one treated establishment and all its controls, and let *s* be the index for strata. 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. Our preferred specification is the following dynamic regression model:

[36] Examples of recent empirical research in labor economics with similar research designs are Kline et al. (2019) and Jäger and Heining (2022), who estimate respectively the effect of patent allowance or worker deaths on various firm-level outcomes. None of these studies exploit regional discontinuities in their matching.

[37] For expositional clarity we assume that funding is received in the year in which the project starts.

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

This is a highly flexible, near satiated, event-study model, which includes firm fixed effects α_i and strata-specific time trends μ_{st}. 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 treated establishments received funding.[38] Given the inclusion of separate time trends for each strata, these coefficients measure the differential evolution of treated establishments compared to their control group within a stratum over the 9 years relative to the baseline period. The assumption of identical pre-trends between the two groups is equivalent to the hypothesis that the $\{\beta_\tau\}$ are jointly zero for $\tau < 0$. We will start our empirical analysis by plotting all eight of these coefficients together with their confidence intervals against the running variable τ for six core outcomes. To keep the number of figures manageable, we follow the convention in empirical studies using event studies and subsequently report in tabular forms the parameter estimates from a difference-in-difference (DiD) version of equation (1). In this DiD-version, the $\{\beta_\tau\}$ are assumed to be zero in the pre-treatment period ($\tau < 0$) and constant in the post-treatment period ($\tau \geq 0$), reducing the number of treatment parameters from eight to just one.

If the parallel trends assumption within each stratum is satisfied, then equation (1) recovers the dynamics of the average treatment effect of ***receiving*** subsidies at rate $R_{i,t^e(s)}$ on various outcomes, averaged over the population of establishments receiving these funds. Another parameter of interest, especially from a public-policy perspective, is the "pass-through" of a euro of subsidies to firm-level outcomes. Since funding amounts received by an establishment are endogenous, we estimate the pass-through consistently using an instrumental variables approach where subsidies per employee are instrumented with the subsidy rate. The first stage of the model is an event-study regression like equation (1) with subsidies received as the dependent variable. We implement this complementary but distinct approach at the end of this section.

4.2 Matched Control Establishments

The parallel trends assumption may be violated for two reasons. First, within strata, control establishments may be subject to different ***aggregate*** trends than treated establishments; and second, treated establishments may have ***idiosyncratically*** different growth potential in labor market outcomes than their controls because of self-selection into the program. We address the first concern by matching treated establishments to control

[38] As discussed in section 2.6, we also include a dummy variable indicating whether the municipality where an establishment is located received public business-related infrastructure subsidies. This does not have any substantial impact on our estimates.

establishments located in contiguous border municipalities that are subject to very similar aggregate trends. Section 5 presents evidence that supports the validity of this research design.

This leaves self-selection into the program based on establishment-level growth potential as the main threat to identification. We confront this challenge by matching 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.[39] Specifically, our matching procedure selects control establishments in two steps. First, for each treated establishment, we select municipalities on the other side of the LMR border with a lower subsidy rate R_{mt}.[40] Second, within geographically matched municipalities, we only consider establishments that are matched on two-digit industry, employment in the baseline period, and employment growth in the pre-period. Here, baseline employment is the average establishment employment 3 and 4 years before the funding event, and employment growth is the difference between average employment 1 and 2 years before the funding event and baseline employment.[41] We sometimes refer to the establishments remaining in the sample after this step as "donors".

We match on pre-treatment employment growth using two complementary approaches. First, based on the evidence that will be presented below that the average growth rate of funded establishments is similar to the above-median growth rate among donor establishments, we drop donors whose pre-trend growth is below this median. 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 "average" control establishment observed over nine years in each stratum.

The primary disadvantage of matching on pre-trends 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 concern in

[39] 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.

[40] 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 substantially reduces the number of observations, and limits the analysis to West Germany since all municipalities in East Germany are eligible for the program.

[41] We use these two-year employment averages to reduce the role of transitory year-to-year employment variation in the matching. 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. More details about the matching procedure, including how we modify the 20 percent matching criteria for very small establishment, are provided in Appendix 3.2.

three ways. First, we match only on pre-trends in ***employment*** but no other of our outcome variables. Because these other outcomes, such as wages and hiring- and separation rates, are not mechanically related to the employment stock variable, we can test for pre-trends in these outcome variables. Second, 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 a violation of the parallel trends assumption. Third, we estimate the causal effect of the pass-through of an additional euro of funding using municipality-level DiD methods with rank controls. As explained in Section 5, the internal validity of this model is strong. In particular, by construction of the GRW, controlling appropriately for regional scores eliminates any concerns about municipality-level selection into the program. The true causal municipality-level pass-through is expected to be larger than the true establishment-level pass-through because (a) the former includes establishment openings, which are likely to have particularly large effects, and (b) it also reflects possible municipality-level spillovers from increasing economic activity. Hence, if one finds the opposite result, this would be strong indication that our establishment-level estimates are biased upward because of selection into treatment. On the other hand, if establishment-level estimates are indeed smaller, then such selection is unlikely to be a major source of biases in our research design.

4.3 Matching: Descriptive Results

In Appendix Figure 1, we visualize our spatial matching procedure using the municipality "Hamm" as an example. This municipality 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, the only municipalities in Germany contributing donor establishments for subsidized establishments in Hamm. The spatial dimension of any stratum that includes a treated establishment from Hamm is shown in panel C, with Hamm colored in red and the six contiguous donor municipalities colored in pink.

Geographic matching is the first step in constructing our strata. As discussed above, its primary rationale is to rule out that our results are driven by differential aggregate trends in economic conditions that treated- and control establishments are subjected to. We next address the issue of selection into treatment by narrowing down control establishments further, using matching on 2-digit industry, average establishment size in years 3 and 4 before treatment, and pre-event employment growth. However, it is not ex-ante clear how strong, and in which direction, pre-event employment growth is correlated with eventually receiving GRW subsidies. We explore this issue in Figure 2, using a sample that is constructed exactly as explained in the previous section, but without matching on

pre-event employment growth yet. We show how employment trends in treated establishments compare to those in control establishments by dividing the latter in four quartiles based on employment growth in the pre-treatment period (measured as the difference between average employment 1-2 years and 3-4 years before treatment). The sample used in Figure 2 is limited to strata with at least four control establishments, the minimum required to compute four quartiles of the within-strata employment growth distribution.[42]

Figure 2 shows substantial heterogeneity and mean reversion in employment growth among control establishments. Employment grows by about 40 percent in the pre-period for control establishments in the top quartile (Q4), but declines for establishment in the lowest quartile (Q1). Consistent with mean reversion, employment of top quartile establishments only grows slightly faster than others in the post-period. The figure also 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. Thus, treated establishments are not outliers in the pre-event employment growth distribution. In contrast, treated establishment grow much faster than all quartiles of control establishments once they start receiving funding at event time 0. The evidence suggests that far from being on the verge of bankruptcy, establishments that self-select into the program have good growth potential.

The evidence reported in Figure 2 suggests that a simple and effective way of controlling for pre-trends is to simply keep matched control establishments with employment growth above the median in the pre-period. To have a well-defined median for each stratum, we drop strata with just one donor establishment. The rationale for our baseline matching 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. In addition to this visually transparent approach to matching, we will also document below estimates from our event studies when using a conventional "pairwise matching" approach, as briefly described earlier.[43]

Sample sizes after sequentially imposing our matching criteria are shown in rows (a) to (g) of panel B and in panel C of Table 2. As shown in row (a) of panel B, we start our matching algorithm on a sample of approx. 4.3 million establishments located in border counties, 2.8 of which are contained in border municipalities (row b). The discrepancy between this initial number and the 4.8 million establishments listed in panel A of the same table

[42] This restriction slightly reduces the number of treated and control establishments from 316 and 12,729 to 237 and 12,572, respectively.

[43] A potential downside of pairwise matching is the possibility of "matching on noise" due to the large 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.

comes from dropping establishments with unusual time-series behavior.[44] The numbers in parentheses are the corresponding counts for treated establishments, of which 21 thousand and 13 thousand are in border counties and municipalities, respectively. For our research design, only border pairs of counties with a gradient in subsidy rates are relevant. Row (c) shows that this additional restriction lowers the number of treated establishments to 7,879. 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 were not observed in the pre-funding period are dropped at that stage. 1,816 treated establishments (row f) are left in our most restrictive matched sample where we match on both baseline employment and pre-event employment growth. Further restrictions to border municipalities leads to a final sample of 286 treated establishments (row g). These numbers appear small but compare favorably to the literature. For example, the number of treated units in the seminal papers on US place-based policies by Greenstone, Hornbeck, and Moretti (2010) and Busso, Gregory, and Kline (2013) is 47 and 234 respectively. Still, we will consider the impact of relaxing some of our matching criteria on estimates in our empirical analysis.

Panel C compares the evolution of pre-treatment average employment for the final event-study samples. The first two columns display these numbers for the sample from panel B, row (g), for which we have 286 treatments and 6,807 controls. 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. As we discuss in the next section, our event studies control for firm fixed effects. For this reason, we also estimate our models when we match on pre-event employment growth but not baseline employment. A prime advantage of this specification is a substantial increase in sample size. Indeed, the number of treated establishments increases to 744, and the number of controls to 39,792. The third and fourth rows of Panel C show the evolution of employment in the pre-period for this sample. 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.

[44] 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.

4.4 Event-Study Results

Figure 3 shows the event-study estimates together with their 95% confidence intervals from estimating equation (1) for our six key outcomes. Apart from a dummy variable indicating whether the municipality in which an establishment is located received public business-related infrastructure subsidies, we do not include any additional controls. The subsidy rate is normalized on a scale of 0 to 1. To obtain the impact of the average subsidy rate, which is .37 in this sample, the coefficients need to be divided by about 3. As noted earlier, we aggregate all control establishments within a stratum into an "average control establishment" so that the level of observation in our data is stratum-treatment-year. With 286 treated pairs observed over nine years that satisfy the matching requirements, the estimation sample consists of $5{,}148 = 286 * 2 * 9$ observations. The standard errors are clustered at the strata (treatment-pair) level.

Panel A of Figure 3 plots the evolution of $\hat{\beta}_\tau$ for total employment (headcounts). As expected from our approach to matching, we detect no significant pre-trends. On the other hand, the estimated effects grow steadily after reception of GRW funds. The coefficient of approximately 20 for the fourth post-treatment year means that about seven jobs are created when the subsidy rate increases from 0 to its average of 0.37. This is a large effect relative to a baseline employment of about 20 one year before treatment.

The positive employment effects relative to control establishments can be generated by a larger hiring rate or a lower separation rate of funded establishments. While the explicit goal of the GRW is to create jobs, establishments may receive subsidies even if they merely commit to maintaining their current establishment size. In that case, the impact of subsidization will be generated by a relative decrease in separations rather than an increase in hires. The next two panels, which show estimates of equation (1) for labor market flows rather than stocks, suggest that this is, on average, not the case. Rather, 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 due to the well-documented empirical regularity that separation rates are generally higher at lower levels of tenure. Overall, these findings suggest that employment growth in the post-treatment period is not just a windfall effect. Another important conclusion from the figures is that there are no significant pre-trends in either of the two variables. Given that we do not match on these outcomes, and that they are not mechanically linked to the employment stock variable, this finding is strong external validation of the parallel trends assumption.

Panel D presents the event-study estimates for the number of commuters. This is a particularly important variable for assessing the effectiveness of placed-based policies since geographic spillovers into non-treated areas may dilute any positive economic effects of the place-based policy. We can calculate this variable from the data because, starting in 1999, they record both place-of-work and place-of-residence of employees. We find a strikingly large effect on the number of commuters: With a coefficient of more than 10 four years after treatment, slightly more than one half of the additional workers in the treated establishments are commuters. The GRW thus has substantial geographic spillover effects and may dilute its positive employment effect on residents of targeted areas. Whether this is indeed the case depends on where commuters come from, a question we will turn to below.

Our final two outcomes are the number of workers in marginal jobs (Panel E) and log daily earnings (Panel F). The first of these outcomes is one measure of the skill content of jobs, while the second of these outcomes serves to quantify any potential productivity effects and how they are passed through to workers. Panel E shows that there is excess creation of marginal jobs in subsidized firms. Furthermore, as for all other outcomes considered so far, we do not find any pre-trends. In the case of log daily earnings, we limit our analysis to workers with at least five years of tenure. The rationale for doing so is that the composition of employment in growing establishment subsidized by the GRW may be changing over time compared to control establishments. Indeed, Table 3 below shows that newly-hired workers are younger and have, by definition, less tenure, two factors associated with lower earnings. The results show no evidence of pre-trends. The estimated treatment effects revolve around 0.1 so that the average subsidy rate of .37 translates into a wage increase for incumbent workers by about four percentage points. In contrast, the event-study results for all workers reported in Appendix Figure 2 shows evidence of pre-trends linked to composition effects. These wage- and employment effects are consistent with a labor supply elasticity of 6 to 7 percent, which is similar to recent estimates in the monopsony literature (e.g., Lamadon, Mogstad, and Setzler 2022).

4.5 Difference-in-Differences Estimates

The graphical depiction of our event study estimates in the previous section has established that (a) GRW subsidies have a substantial positive impact on employment in stocks and flows and, for incumbent workers, on daily earnings, and (b) there is no evidence for differential pre-trends for the outcomes we did not target in our matching procedure. We now move on to estimating these effects for a broader set of outcomes, exploring robustness with respect to specification choice, and investigating heterogeneity in treatment impacts. To reduce the dimensionality of estimates and to keep presentation manageable, we report Difference-in-Difference (DiD)

estimates for the remainder of the paper. This aggregates the event-study estimates into a one-point estimate, the average of the dynamic treatment effects in the post-event period.

Results for employment outcomes, commuting patterns, and log daily earnings are presented in Tables 3, 4, and 5, respectively. Each of these tables is structured identically. Column 1 displays sample averages of the outcomes in our benchmark sample, columns 2 to 5 show DiD estimates from four different specifications, and column 6 expresses the benchmark estimates from column 2 in percentage terms. Columns 2 and 3 correspond to the DiD-version of equation (1) estimated for contiguous border municipalities and counties, respectively. Expanding the analysis to the county level increases the number of events substantially, from 286 to 1,816. However, it comes at the cost of comparing establishments that are geographically further apart and thus less likely to have identical aggregate trends. Another way of increasing sample size is matching on employment growth, but not on initial levels, which we do in column 4. This increases the sample size to 744. In column 5, we show estimates when we match treated establishments to their controls using a conventional pairwise matching approach, as described in section 4.3, which gives us 468 strata.

Table 3 focuses on outcomes related to employment and its structure. The primary goal is to analyze which types of workers benefit from the program in terms of employment and where excess hires due to the funding come from. We concentrate our discussion on our benchmark results in column 2, which are translated into percentages of baseline averages in column 6, and briefly discuss the robustness of the estimates below. As expected, the estimated effect of the subsidy rate on total employment of 13.11 (first row of Table 3) is approximately equal to the average of the corresponding five post-treatment coefficients plotted in Figure 3. 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, an important finding coming out of Table 3 is that in terms of employment more educated workers benefit relatively less from the GRW program than their less-educated co-workers.

To quantify geographical spillovers, we next decompose total hiring flows into three different origin states: hires from establishments in the same municipality, from establishments in a different municipality, and from non-

employment.[45] Here we are leveraging a prime advantage of our data, namely that we can distinguish between place of residence and place of work. Notice that, since the overall effect of the subsidy rate on separations is small and not statistically significant, as shown in the next row, we do not conduct any further decomposition for this outcome. As for hires, the results indicate that treated firms increase their recruitment activity uniformly by about 40 percent for each of these three origin states (see column 6). If anything, the hiring effect is a bit lower for non-employed workers, indicating that the GRW is not disproportionately benefitting out-of-employment individuals. On the other hand, in light of the substantial share of hires coming from other municipalities – some of the non-employed may indeed commute as well, so that the share of new recruits who are commuters is likely higher than 40 percent – the GRW does have large geographic spillovers. Generally, such spillovers will dilute the positive employment effects of place-based policies, but, as we show below, this is less of a concern in the context of the GRW because the majority of commuters commute within the same – economically disadvantaged – LMR.

As argued in Austin, Glaeser, and Summers (2018), place-based policies will be particularly beneficial if they improve labor market outcomes of workers with high labor supply elasticities. These are likely workers relatively close to the margin of employment. The final three rows of Table 3 show results for outcomes that are well-suited for testing if this is the case for the GRW, specifically the number of additional marginal jobs and the average age and tenure of employees. We use the latter two outcomes because younger individuals and individuals with little tenure are more likely to have weaker labor force attachment. As the estimates show, we do indeed find that, relative to their controls, funded establishments generate an excess amount of marginal jobs, are more likely to hire younger workers and experience a reduction in workers' average tenure. Besides confirming that subsidies received by establishments have an impact on the composition of workers that is likely beneficial in an economic welfare sense, they also provide additional evidence on the validity of the research design. If treated firms were on a steeper growth path regardless of funding, there would be no particular reason why tenure would decline after the funding is received.

A general takeaway from our robustness analysis carried out in columns 3, 4, and 5 is that our estimates are remarkably robust to the choice of sample and the matching procedure. Qualitatively, our conclusions from the baseline estimates are unaltered as almost all estimates retain their sign and significance level. Quantitatively,

[45] 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.

whether we drop matching on initial employment, as in column 4, or whether we adopt a pairwise matching approach, as in column 5, does not matter substantially: The estimates tend to remain in each other's confidence intervals. On the other hand, estimates for the employment outcomes are smaller when using the broader sample of contiguous border counties- instead of municipalities, as in column 3. One reason for this decrease may be that when defining strata by counties, we allow comparisons of establishments that are potentially quite far away from the border. This may lead to a violation of the parallel trend assumption, resulting in lower estimates because treated establishments are, by construction, in areas with a lower socio-economic trajectory.[46]

Table 4 investigates whether place-based policies have geographic spillovers, one of the fundamental questions that remains open in the literature. We can answer this question convincingly because we directly measure commuter flows in our data. While we present results for the same specifications as in Table 3, the findings are robust and we concentrate our discussion on baseline estimates in column 2.

Comparing the first rows of Tables 3 and 4 indicates that over half of the additional employees added to the establishment due to GRW funding are commuters. The effect of the GRW for all commuters, estimated at 22 percent when evaluated at the average subsidy rate, is more or less proportional to the effect for all workers (21 percent). Whether these large effects on the commuter share among additional employees dilute or not dilute the overall employment effect of the GRW depends on where they come from. After all, subsidy rates are set at the level of LMRs, not municipalities. The next three rows show that commuters living in the same LMR indeed disproportionately benefit from the program (31 percent effect) relative to commuters from other LMRs. This evidence indicates the GRW is relatively well-targeted, possibly because LMRs are defined by commuting patterns in the first place. Furthermore, 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 in terms of employment. Commuter flows grow the strongest (34 percent effect) for workers without a secondary degree. Hence, the effect of GRW funding on establishment-level employment policies does not seem to be affected in a substantial way by its effect on commuters. Both commuters and non-commuters are somewhat less likely to be highly educated than the overall population.

Table 5 presents a more in-depth analysis of the impact of GRW funding on wages. Consistent with the evidence from the event study, the coefficient estimates in the first row of the table indicate that the GRW subsidy rate has a positive and significant impact of about 4 percent (coefficient of 0.10 multiplied with the average subsidy rate

[46] As will be discussed in the heterogeneity section below, we can pinpoint a second source of this difference: We find that estimated effects are substantially smaller for establishments in East Germany, and the latter starts to be over-represented in the border-county relative to the border-municipality design because of its lower population density.

of 0.37) on daily wages. Although the effect is mainly driven by offsetting pre-trends, as discussed earlier in the context of Figure 3 and Appendix Figure 2, 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 contiguous 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.

Viewed through the lens of the Baily-Chetty-type formula (Baily, 1978; Chetty, 2006) for optimal place-based policies presented in Austin, Glaeser, and Summers (2018), these findings have the implication that the GRW partially satisfies the properties of an optimal policy because it induces subsidized firms to hire workers at the margin of employment with high labor supply elasticities. However, the policy is also beneficial to a subgroup of workers – those with high educational attainment and strong labor force attachment – in terms of wages. Because of this group, the marginal cost of the program will tend to be too high to be fully efficient. This shows that constructing a welfare-optimal place-based policy is difficult because it will be hard to target it sufficiently precisely to satisfy the optimality conditions of public finance.

4.6 Additional Evidence: Long-Term Effects and Heterogeneity

For a subset of 164 out of the 286 treated establishments that can be matched to suitable controls from a neighboring municipality, we are able to follow establishments for up to 10 years after the funding event. The event-study estimates for this subsample are reported in Figure 4.[47] These long-term estimates are informative because regulations of the GRW about the requirement to create- or at least maintain the number of workers run out five years after the project ends (subsidies are typically paid over 2-3 years). Hence, if funded establishments

[47] The reason for this large decrease in the number of treatments is that, with our sample tracking establishments until 2020, only establishments that receive funding up to 2010 can be followed for ten years afterwards. This is also the reason for why we do not present these estimates as our benchmark results.

treated subsidies as a windfall, one would expect that they start increasing separations and lowering hires after this period. However, neither the estimates for total employment reported in the first panel nor the estimates for hires and separations in the following two panels of the figure support such an interpretation. Instead, employment grows steadily until about five years after the funding event and stabilizes thereafter. There is also no evidence of mean reversion after establishments stop receiving financial support a few years after the initial funding event. Instead, hiring sharply increases following the funding event but eventually stabilizes and decreases slightly until a new steady state is reached where hires and separations are in balance. The impact of GRW funds on establishment size is, therefore, long lasting.[48]

The next two panels in Figure 4 show a similar pattern for the two remaining employment-related outcomes, the number of commuters and the number of marginally employed workers. More specifically, they grow steadily until about five years after treatment and remain relatively stable after that. As in Table 4, 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. On the other hand, 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 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.

We also conduct an extensive heterogeneity analysis with respect to geography and establishment characteristics. Results for our benchmark specification are shown in Appendix Table 3 and discussed in some detail in Appendix 3.3. Here we only briefly summarize the main findings. First, our estimates are robust across subsamples in a qualitative sense, whether we exclude a number of regions that may drive our results, such as Berlin or the "inner-German border", or whether we slice the sample by establishment size or by industry. Second, the GRW subsidy rate has a considerably lower impact on establishments in the East than in the West. 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 areas

[48] This is consistent with the county-level intent-to-treat estimates of Siegloch, Wehrhöfer, and Etzel (2025), who show that counties in East Germany that experienced a decrease in subsidy rates had a negative permanent impact on employment.

do not have a neighboring area with lower funding rates. 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. As we explain in Appendix 3.3, two leading 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, and that recruiting commuters may be more challenging due to the East's lower population density.

Third, we do not find any evidence that GRW-funded investment projects are more effective in creating jobs in the relatively capital-intensive manufacturing sector. In fact, our estimates suggest that the opposite is the case, with below-average estimated effects in manufacturing, at-average estimates in service industries and construction, and above-average estimates in "Trade and Transportation." There is, therefore, little empirical support for targeting place-based policies at any particular sector.

4.7 The Cost of Job Creation: IV-Estimates

One key parameter in the Baily-Chetty formula for optimal subsidies (Baily, 1978; Chetty, 2006) applied to place-based policies is the marginal cost of creating one job. Estimating this parameter is challenging because not only are the amounts spent on establishment-level investment projects endogenous, but employment as a stock variable is not well-suited for quantification because of compounding. We address these challenges as follows: First, we instrument subsidy amounts actually received by an establishment, annualized over the five years of the post-treatment period, using the subsidy rate.[49] This means that we use equation (1) as the first stage, with the subsidy amounts as an outcome. Second, we use the number of hires, a flow variable, as outcome. To the best of our knowledge, our study is the first to use this approach for quantification in the place-based policy literature. Yet, using hires as outcome is the most direct way of measuring the cost per job created. We also normalize each establishment's employment outcomes and the total amount of subsidies relative to baseline employment. This is important to strengthen the first stage because the subsidy rate may not be a strong predictor of subsidy

[49] 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.

amounts if, for example, large establishments receive large funding amounts and if large establishments are more likely to be located in relatively economically strong regions with lower subsidy rates.[50]

First- and second-stage estimates, together with a number of additional statistics, are presented in Table 6. We estimate the model for all specifications considered in Tables 3, 4, and 5. The first-stage estimates, presented at the top of the table, are large and statistically significant, and they pass conventional tests for strong instruments. 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 second-stage 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 we find at the municipality level (see below). This estimate means that the cost of job creation at the establishment level is approximately EUR 1,000 / 0.040 = EUR 25,000.[51]

5. Validation: Placebo Regressions and Aggregate Models

The validity of our establishment-level research design crucially relies on the parallel trends assumption holding for treated establishments and their matched controls. As discussed earlier, we present three sets of evidence supporting the assumption. First, we test for pre-trends in outcomes not used in the matching process; second, we carry out placebo tests; and third, we compare IV-estimates of the cost of job creation with corresponding estimates on the municipality level. We have already documented and discussed the first validation exercise in the context of Figure 3, where we found no evidence for differential pre-trends among the outcomes not matched on. This section presents results from the two other validation exercises.

[50] This normalization is particularly important for the municipality-level IV model estimated in section 5. We use the analogues normalization here for comparability. Another approach is to use logarithms of funding amounts. This is unattractive in our context because control units have zero funding.

[51] These amounts are consistent with recent international evidence, such as LaPoint and Sakabe (2024), who evaluate an indirect place-based subsidy on capital expenditures in Japan.

5.1 Placebo-Analysis

The main advantage of the contiguous border design is that municipalities containing treated establishments likely follow the same aggregate trends as control municipalities. We formally test for this using placebo regressions in which treated establishments are replaced with comparable 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 in the pre-period. However, placebo treatment effects in the post-period could be statistically significant due to spillover effects linked to the expansion activities of the subsidized 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. The placebo event studies are, therefore, a test of differential pre-trends and agglomeration effects in the post-treatment period.

We report placebo estimates for the same outcomes as in Figure 3 for three different sets of placebo establishments. In the first placebo analysis reported in Appendix Figure 3, we keep the same control establishments as in Figure 3 but for each stratum replace the treated establishment with non-funded 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 find placebo establishments for 160 strata. In the second placebo analysis reported in Appendix Figure 4, we drop the requirement that either control establishments or placebos are matched on initial employment to the treated establishment. In the main placebo analysis reported in Figure 5, we drop any matching requirements other than that placebo-treated establishments and their controls are located in contiguous municipalities. This is our main specification, where all 111,998 non-funded establishments in the 286 strata are compared to all 272,582 establishments in control municipalities. The advantage of this last specification is that it captures the potential spillover effect on all non-funded establishments and is directly connected to municipality-level estimates presented below (total effect for all funded and non-funded establishments).[52]

Figure 5 shows no indication of differential pre-trends despite the small standard errors linked to the large number of placebo establishments (the scale in Figure 5 is about 10 times smaller than in Figure 3). We also find little evidence of placebo effects in the post-treatment period for employment-related outcomes. A possible

[52] It is also comparable to Greenstone, Hornbeck, and Moretti (2010), with the crucial difference that the subsidized establishments are small- to median sized and located in socio-economically disadvantaged areas rather than "million dollar plant" openings in areas with favorable economic fundamentals.

explanation for lack of spillover effects in employment is that a substantial part of excess hires come from other municipalities (Table 4). We reach similar conclusions for the two other placebo analyses reported in Appendix Figures 3 and 4.

By contrast, we find that wages increase by about 1 percent after the funding event. Although the wage effect is much smaller than the direct 10 percent effect on treated establishments (Figure 3), it is statistically significant in most post-treatment years. Interestingly, the wage effect is larger (about 5 percent) for the sample of placebo firms in the same 2-digit industry reported in Appendix Figures 3 and 4. These positive spillover effects are consistent with employers in the same 2-digit industry having to compete more aggressively with funded establishments than employers in other industries.

5.2 Municipality-Level Instrumental Variable Estimates

Our last validation check is based on a comparison of establishment- and municipality-level "pass-through" estimates of the impact of GRW funding on labor market outcomes. This comparison is informative because whereas ***establishments*** can and do choose to apply for GRW subsidies, whether ***municipalities*** are eligible, and which subsidy rate applies to them, is conditionally exogenous due to the institutional features of the GRW program described in Section 2. To see this latter point, let R_{mt} be the subsidy rate in municipality *m* in year *t.* As described in Section 2, this is a deterministic function of the EU agreed-upon scoring rule. To make the score comparable over time, we normalize it using the inverse CDF of the score's distribution over municipalities in year t. Let $rank_{mt}$ be this inverse. If the scoring rule remained constant over time, then conditional on this rank variable there would be no variation in subsidy rates. Municipalities may change rank over time because of fluctuations in their socio-economic well-being, but municipalities that are assigned the same rank will always have the same subsidy rate. However, the scoring rule does change over time, and this provides conditional exogenous variation for our IV-model. In particular, as discussed in Section 2 the GRW has become less generous over the sample period due to the EU expansion. This has been operationalized in practice by adjusting the scoring rule. As a consequence, conditional on $rank_{mt}$, subsidy rates R_{mt} can in fact change over time, but this is solely due to exogenous modifications of the underlying scoring rule.[53]

[53] The underlying idea of "extracting" exogenous changes in subsidy rates over time for identification is similar to the approach of Criscuolo et al. (2019) in their study of a prominent UK place-based policy. A central difference compared to our approach is that they do not observe actual scores, so they can only estimate the reduced-form of the model.

This suggests estimating a municipality-level "pass-through" regression where R_{mt} is used as an instrumental variable for funding amounts F_{mt}, and $rank_{mt}$ is flexibly controlled for using a function $g(rank_{mt})$. As in the establishment-level IV model described in section 4.7, the outcome variable is the number of hires, y_{mt}. Both y_{mt} and F_{mt} are normalized on a per capita basis using baseline municipality population.[54] We estimate the municipality-level IV DiD model

$$y_{mt} = \gamma * F_{mt} + g(rank_{mt}) + \alpha_m + \mu_t + \varepsilon_{mt}, \qquad (2)$$

where α_m is a municipality effect and μ_t is a time effect. [55] The function $g(rank_{mt})$ is parametrized using a quartic in $rank_{mt}$ and a set of decile rank dummies. As discussed above, R_{mt} is used as IV for funding amounts F_{mt}. Similar to the establishment-level analysis, we restrict our sample to contiguous border municipalities or counties.

The municipality-level pass-through estimates are reported in Table 7.[56] Unlike the establishment-level estimates that focus on ongoing establishment, the municipality-level estimates also incorporate the impact of the GRW on new establishment creation and spillover effects. As such, they should provide an upper bound for the establishment-level estimates.

The first-stage estimates are reported in the top panel of Table 7, both for the amount of subsidies paid to establishments, with and without the amount of subsidies for public business-related infrastructure projects. In addition to the baseline municipality-level estimates, we also report results at the LMR level in columns 2 and 4. Our preferred estimate in column 1 suggests that an increase of the subsidy rate from 0 to 34 percent (the observed average in the municipality sample) 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.

[54] As discussed in section 4.7, this normalization is important for strengthening the first stage. This is because larger municipalities are more likely to receive large funding amounts simply because they have more establishments, but larger municipalities are also more likely to be located in economically relatively stronger LMR's with lower scores. This weakens the correlation between funding amounts and subsidy rates. We do not use logarithms to address this issue because there are many municipalities with zero funding. We normalize by population rather than employment because the latter is endogenous.

[55] Siegloch, Wehrhöfer, and Etzel (2025) estimate DiD-models that are akin to the reduced-forms of our model on the county-level for East Germany. They neither control for rank, nor do they estimate the pass-through, which is our second-stage.

[56] For completeness, we show estimates from the reduced form, first-stage and the second stage for all of our core outcomes and various specifications in Appendix Tables 4 to 6. Appendix 3.4 provides a detailed discussion of the results.

Comparing coefficient estimates from the different specifications indicates that the data aggregation level has little impact on first-stage coefficients for subsidies to establishments. In contrast, the estimates at the LMR level (columns 2 and 4) 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 IV (second stage) estimates reported in the lower panel of the table 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. The estimated coefficient in column 1 indicates that increasing per capita subsidies by EUR 1,000 increases the hiring rate by about 0.05 (per capita), implying that it takes EUR 20,000 of subsidies to create an additional job. The estimates from the other specifications are similar, ranging from 0.043 to 0.070. These numbers are also in the same range as those of Criscuolo et al. (2019) and Siegloch, Wehrhöfer, and Etzel (2025), who use a rather different approach than us to translate reduced-form impacts of changes of policy parameters into subsidies per job.

Most importantly, we do indeed find that it takes fewer euros to create one additional job at the municipality level (EUR 20,000) than at the establishment level (EUR 25,000). A different way of expressing this finding is to calculate the average second-stage estimates in Tables 6 and 7, which are 0.04 and 0.053, respectively, and compute the indirect effect of the funding. This indirect effect is given by ((0.053-0.040)/0.040 = 0.33) and represents the additional jobs created in non-funded establishments, a type of local spillover effect. However, as discussed earlier, this estimate needs to be interpreted with caution for several reasons. First, our establishment-level analysis only looks at intensive margin effects for existing 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. In any case, what these estimates suggest is that our establishment-level results are not likely to be severely biased upwards due to selection into treatment. As indicated by our external validation exercises, once one compares establishments in contiguous municipalities, matching on pre-event employment growth is a sufficient control for this selection.

6. Concluding Comments: Who Benefits from Place-Based Policies?

We end by returning to the main questions asked at the beginning of the paper: Which establishments take advantage of place-based policies? Does the policy reach its main target, workers with relatively weak labor market attachment and high labor supply elasticities living in the eligible areas, or does it favor workers with strong labor market outcomes who are commuters from non-eligible regions? Our results give a clear answer to the first question. In contrast to commonly held expectations, establishments that apply- and receive funds tend to be those with positive growth potential rather than establishments at the margin of operating.

The answer to the second question must be more nuanced. The group of workers that gain over-proportionally in terms of employment outcomes from the GRW indeed consists of workers with lower levels of commonly used skill proxies, in particular age, tenure, and education. Because these are also the groups for which we find high labor supply elasticities, the GRW partially satisfies the condition for an "effective place-based policy for the 21st century", as formulated in Austin, Glaeser, and Summers (2018) and Duranton (2018). However, the GRW also has effects that are likely to be distortionary. There are substantial geographic spillovers as over half of the excess jobs from funding are commuters, and highly skilled individuals with lower estimated labor supply elasticities experience significant wage gains. In the case of the GRW, the efficiency loss from the former is likely to be small because we find that most commuters come from the same LMR and, thus, still from the targeted area. However, this will not necessarily apply to other place-based policies because commuters may come from non-targeted areas. One interpretation of our finding is that using commuting zones for determining eligibility may be a good idea because it will contain any local spillovers from subsidized establishment expansions within the eligible region.

A more general takeaway is that minimizing sources of distortions from place-based policies will be difficult because some groups of non-targeted workers will experience gains as well. On the other hand, and more positively, our findings suggest that place-based policies can achieve desirable distributional goals at a relatively low cost, in the case of the GRW of no more than EUR 25,000 per new job.

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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 per establishment (€)**
				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	2,056	16,710	519,110
	33 (Feb 2004 - Dec 2006)	2003	A - E; B1 - C1	15 - 50	7.5 - 50	0 - 35	90	5,523.4	7,428	1,186	17,380	601,478
2007 - 2013	36 (Jan 2007 - Sep 2008)	2006	A - D; C1 - C5	20 - 50	10 - 40	0 - 30	90	2,682.5	4,564	487	17,369	452,072
	361 (Oct 2008 - Jan 2011)	2008	A - D; C1 - C5	20 - 50	10 - 40	0 - 30	60	4,067.0	6,549	802	21,006	473,259
	362 (Feb 2011 - Jun 2014)	2010	A - D; A1; C1 - C5	20 - 50	10 - 40	0 - 30	60	4,717.0	6,410	768	22,142	672,767
2014 - 2016	36310 (Jul 2014 - Dec 2016)	2013	C1p; C2p; Cnp; D	20 - 40	10 - 30	0 - 20	60	1,269.5	2,861	241	19,376	423,919

NOTES: This table shows key descriptive features of the GRW program, split by funding period ("Rahmenplan"). Column (3) shows the historical administrative municipality codes. 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 after 2014. "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 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 funds per establishment. 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

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 - MAIN RESULTS (DID): EMPLOYMENT OUTCOMES

	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)***
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)***
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*	*286*	*1,816*	*744*	*468*	*286*
Nr of Cells	*5,148*	*5,148*	*32,688*	*13,392*	*8,424*	*5,148*

Notes: This table reports difference-in-differences estimates of the effect of the subsidy rate on treated establishments relative 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. Column 6 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, using coefficient estimates for the benchmark specification reported in column 2. Standard errors are clustered at the strata-level.

TABLE 4 - MAIN RESULTS (DID): COMMUTING OUTCOMES

	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)***
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)***
Nr of Strata	*286*	*286*	*1,816*	*744*	*468*	*286*
Nr of Cells	*5,148*	*5,148*	*32,688*	*13,392*	*8,424*	*5,148*

Notes: This table reports difference-in-differences estimates of the effect of the subsidy rate on treated establishments relative 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. Column 6 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, using coefficient estimates for the benchmark specification reported in column 2. Standard errors are clustered at the strata-level.

TABLE 5 - MAIN RESULTS (DID): EARNINGS OUTCOMES

	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)***
Nr of Strata	*286*	*286*	*1,816*	*744*	*468*	*286*
Nr of Cells	*5,148*	*5,148*	*32,688*	*13,392*	*8,424*	*5,148*

Notes: This table reports difference-in-differences estimates of the effect of the subsidy rate on treated establishments relative 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. Column 6 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, using coefficient estimates for the benchmark specification reported in column 2. Standard errors are clustered at the strata-level.

TABLE 6 - IV-ESTIMATES OF FUNDING AMOUNTS (1000s of EUR) ON THE NUMBER OF HIRES

	(1)	(2)	(3)	(4)
	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. 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 Tables 3 to 5 otherwise apply.

TABLE 7 - MUNICIPALITY-LEVEL IV ESTIMATES: CAUSAL IMPACTS OF 1,000 EUR OF ADDITIONAL GRW FUNDS

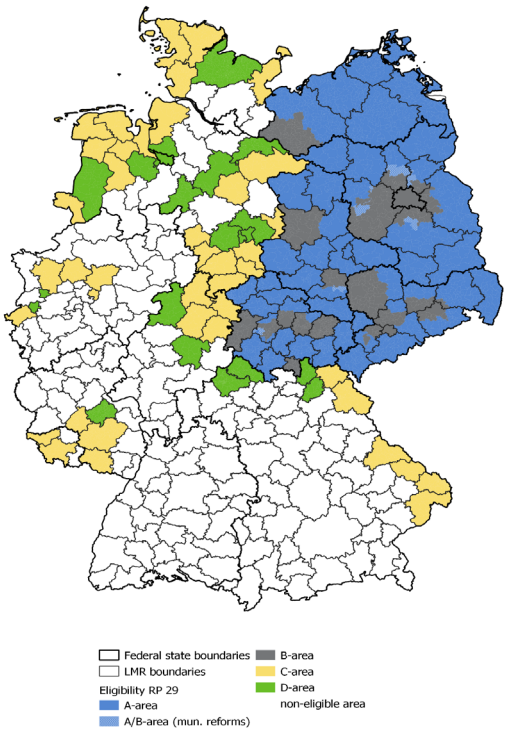
	Instrumented: Total Subsidies to Establishments		Instrumented: All GRW Subsidies, incl. Infrastructure	
	(1)	(2)	(3)	(4)
	Municipality Level	LMR Level	Municipality Level	LMR Level
First Stage Coefficient				
	0.177	0.218	0.197	0.354
	(0.020)	*(0.032)*	*(0.022)*	*(0.069)*
Second Stage Coefficient				
	0.052	0.070	0.047	0.043
	(0.020)	*(0.021)*	*(0.018)*	*(0.014)*
Summary Statistics: Treated Establishments				
Hires per Capita	0.044	0.063	0.044	0.063
Subsidy rate (conditional)	0.34	0.35	0.34	0.35
Observations	48,024	3,200	48,024	3,200

Notes: This table shows first-stage and second-stage coefficients when estimating municipality- or LMR-level DiD models where the subsidy rate is used as IV for the actual amount of subsidies. All variables are normalized relative to year-2000 population. All regressions include fixed effects for municipalities and years together with rank decile fixed effects and a quartic polynomial 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. Only estimates for the hires outcome are shown. For the complete municipality-level IV estimates, including reduced form, see appendix tables 4 to 6.

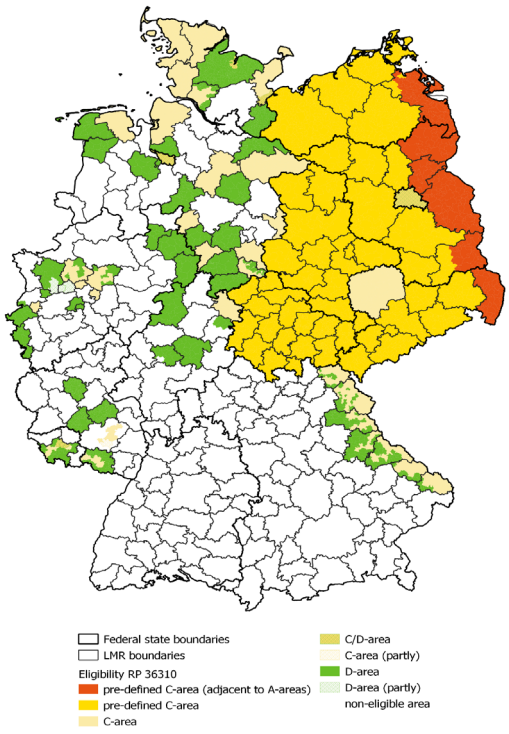
FIGURE 1 - MAPS OF ELIGIBILITY STATUS AND SUBSIDY RATES OF THE GRW PROGRAM: EXAMPLES FROM TWO FUNDING PLANS

PANEL A - ELIGIBILITY STATUS FOR FUNDING PLANS 29 (JAN 2000 to JAN 2004) AND 36310 (JUL 2014 to DEC 2016), BY LABOR MARKET REGION (LMR)

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

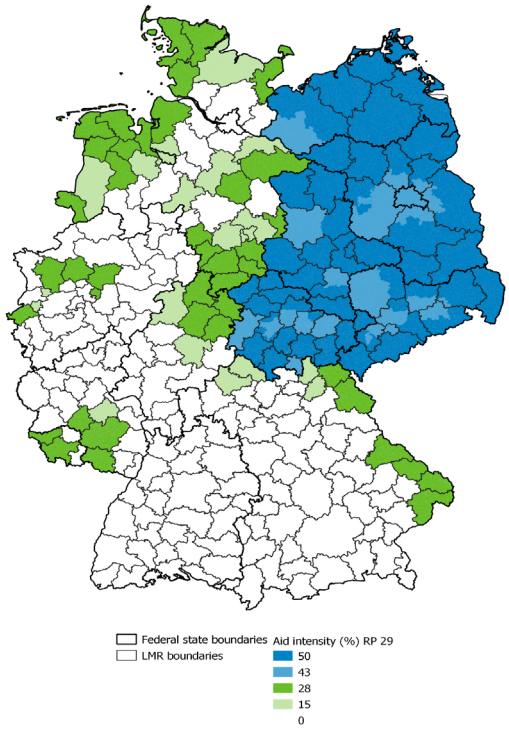


(b) Funding Plan 36310 (Jul 2014 to Dec 2016)



PANEL B - SUBSIDY RATES FOR FUNDING PLANS 29 (JAN 2000 to JAN 2004) AND 36310 (JUL 2014 to DEC 2016), BY LABOR MARKET REGION (LMR)

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



(b) Funding Plan 36310 (Jul 2014 to Dec 2016)

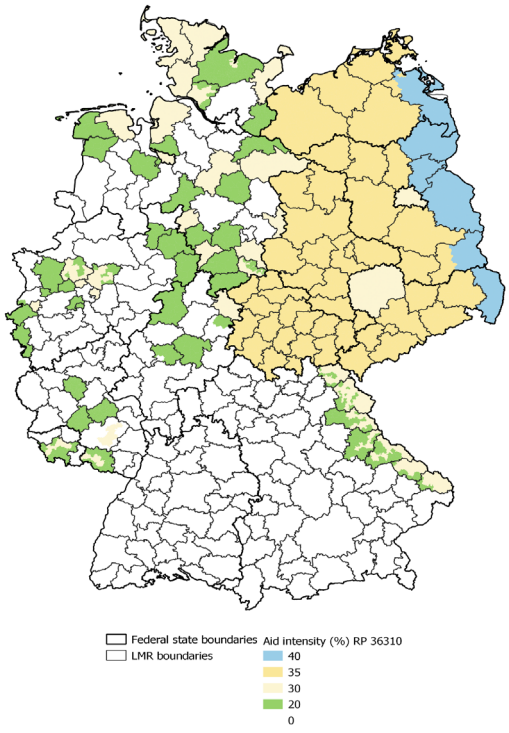
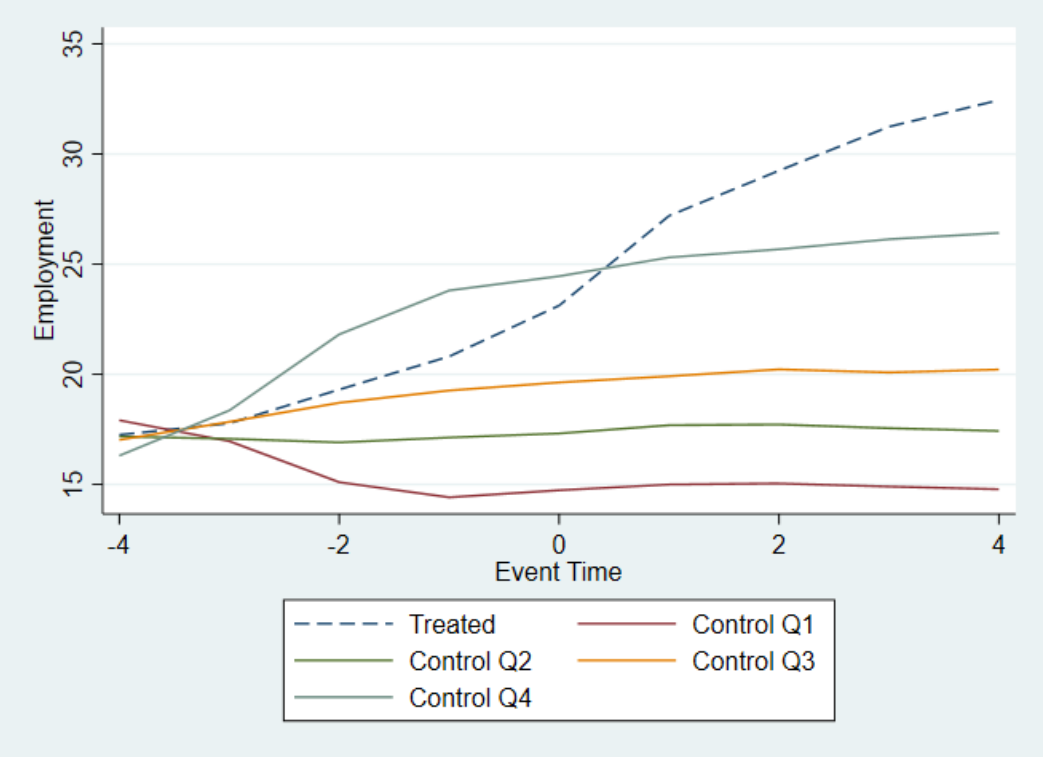


FIGURE 2 - EMPLOYMENT TRENDS, TREATED VS. CONTROL ESTABLISHMENTS



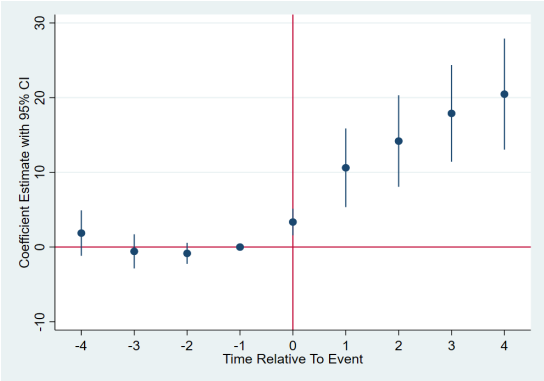
Notes: This figure shows the evolution of average employment over the 9 years used in the event studies, separately for treated firms and their controls in a sample that does not match on pre-event employment growth. The latter are split into 4 groups defined by their standing in the distribution of within-strata pre-event 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 LEVEL AND PRE-EVENT GROWTH OF EMPLOYMENT

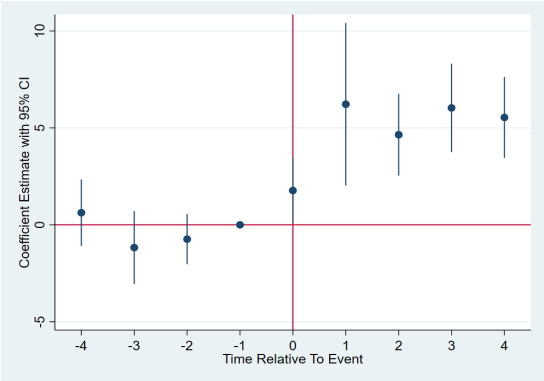
Number of Observations: 5,148

Number of Strata: 286

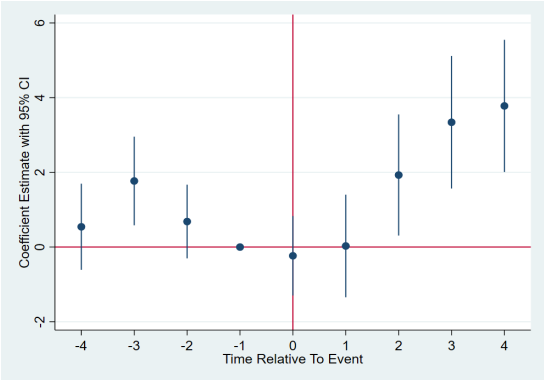
A. Number of Employees



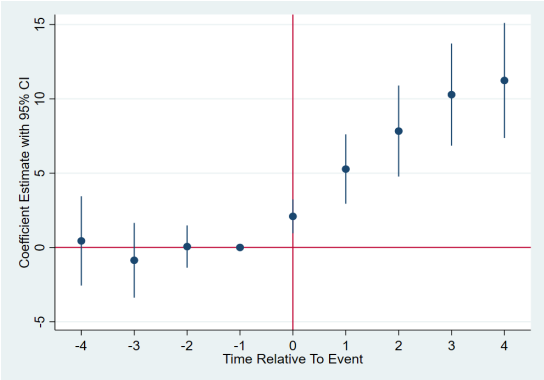
B. Number of Hires



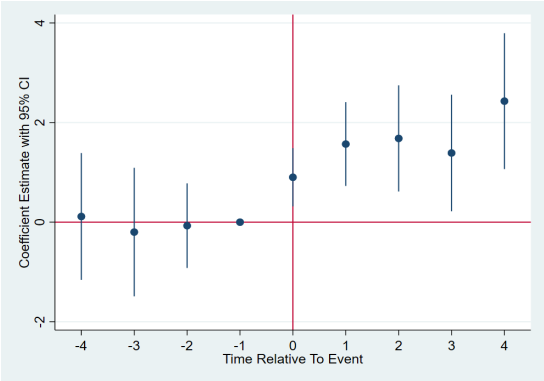
C. Number of Separations



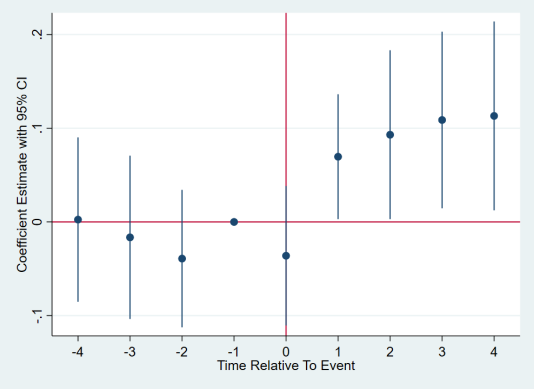
D. Number of Commuters



E. Number of Marginally Employed



F. Log Daily Earnings (tenured workers)



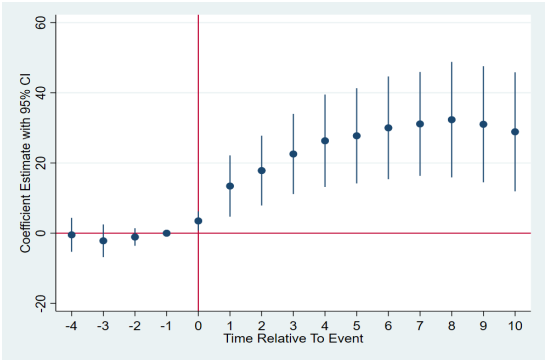
Notes: The figures show point estimates and their 95% confidence intervals for an event study that tracks differences in outcomes between treatment- and control groups over a period of 9 years. Point estimates are coefficients on the interaction between event-time dummies and the GRW subsidy rate. Within 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 4 - EVENT-STUDY RESULTS FOR CORE OUTCOMES, LONG-RUN EFFECTS (10 YEARS POST TREATMENT)

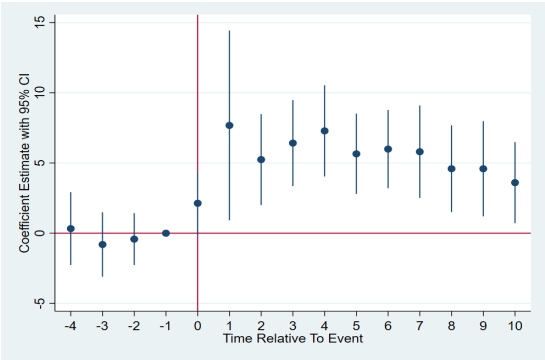
Number of Observations: 4,920

Number of Strata: 164

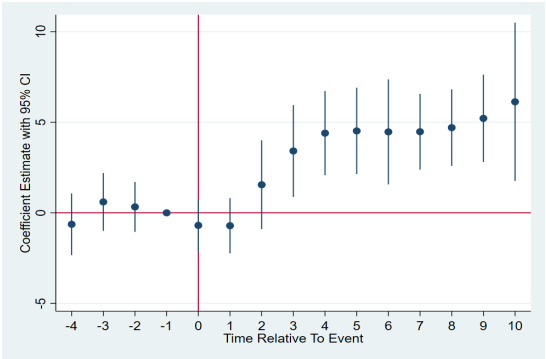
A. Number of Employees



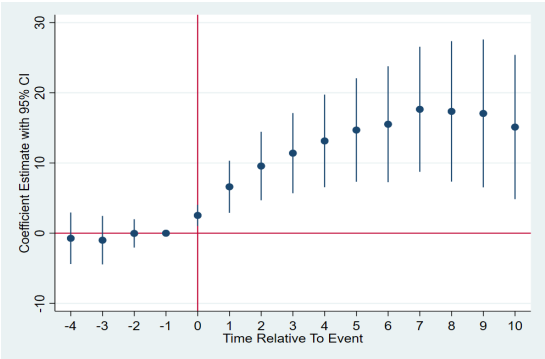
B. Number of Hires



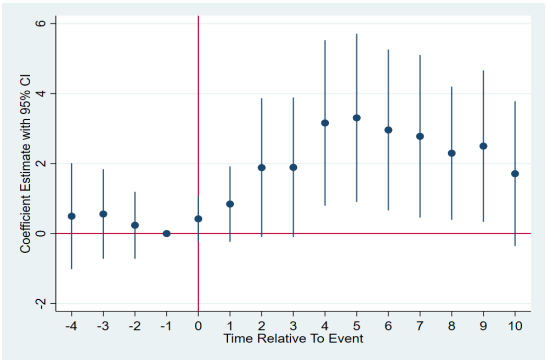
C. Number of Separations



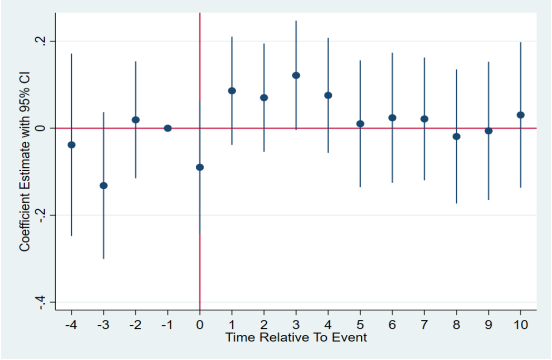
D. Number of Commuters



E. Number of Marginally Employed



F. Log Daily Earnings (tenured workers)



Notes: The figures show point estimates for the same outcomes and the same specification as in figure 4, but up to 10 years after treatment. Because we observe less firms for 10 years- than 4 years after funding, the number of strata in this specification is smaller. For further details, see notes for figure 3.

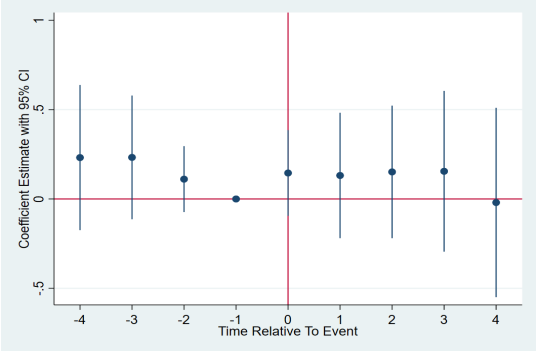
FIGURE 5 - EVENT-STUDY PLACEBO REGRESSIONS, ALL UNTREATED FIRMS IN CONTIGUOUS BORDER MUNICIPALITIES

Number of Observations (after collapse): 5,148

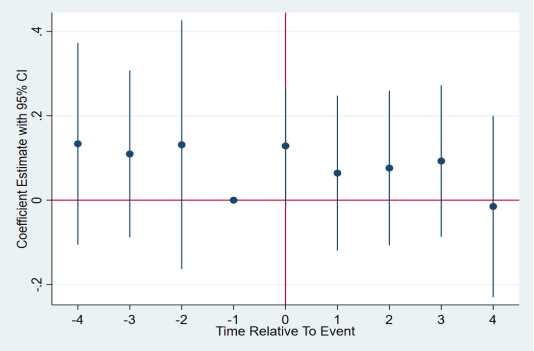
Number of Strata: 286

Number of Placebo-Treated (-and Control-) Establishments: 111,998 (272,582)

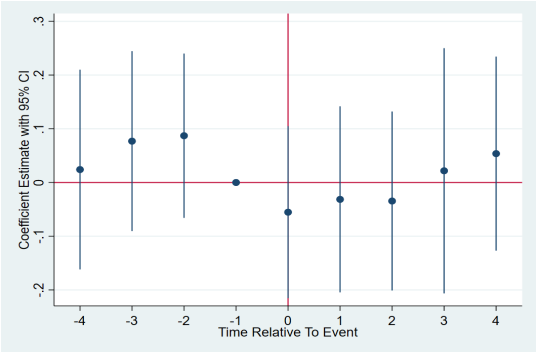
A. Number of Employees



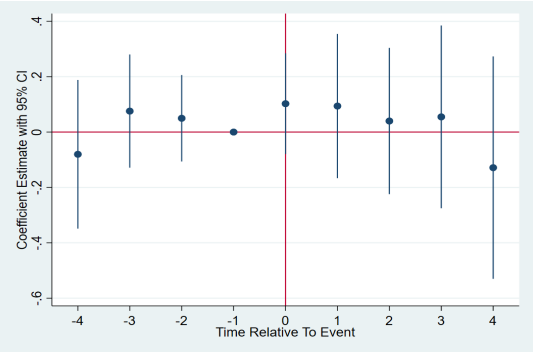
B. Number of Hires



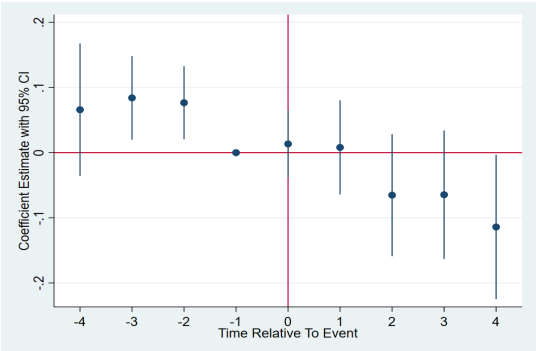
C. Number of Separations



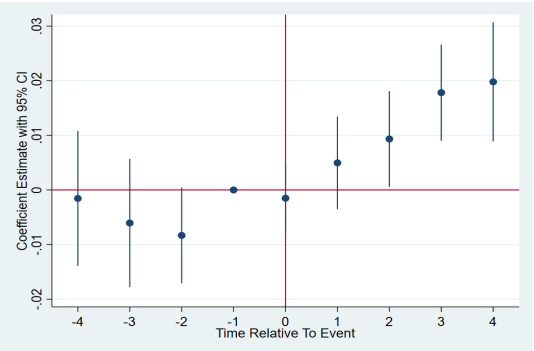
D. Number of Commuters



E. Number of Marginally Employed



F. 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- and control groups 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.

APPENDICES AND SUPPLEMENTARY MATERIAL

(For Online Publication Only)

Appendix 1: Appendix Tables

Appendix 2: Appendix Figures

Appendix 3: Additional Information

3.1 Data

3.2 Matching in the Event-Study Design

3.3 Effect Heterogeneity

3.4 Municipality-Level IV Estimates

APPENDIX 1: APPENDIX TABLES

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 7 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 - 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 tables 3-5, 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

APPENDIX TABLE 4 - 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 avg (using col. 6)**
	(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*	
Rank-Percentile FE		*No*	*No*	*No*	*Yes*	*Yes*	
FE for Mun and Year		*No*	*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 avg (using col. 6)
	(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*	
Rank-Percentile FE		*No*	*No*	*No*	*Yes*	*Yes*	
FE for LMR and Year		*No*	*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. 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).

APPENDIX TABLE 5 - 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.

APPENDIX TABLE 6 - 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 appendix tables 4 and 5. They include fixed effects for municipalities and years together with rank decile fixed effects and a quartic polynomial 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.

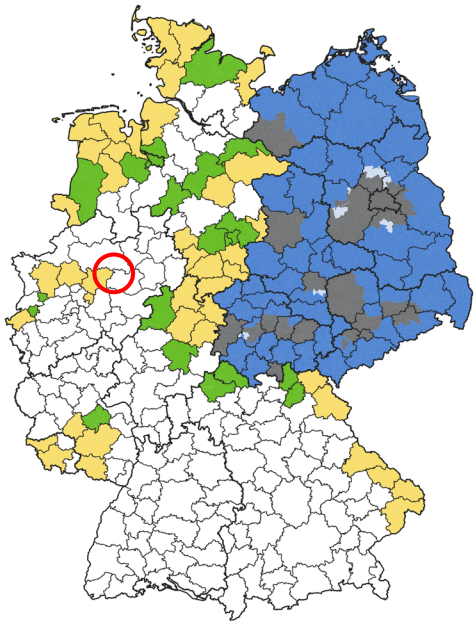
APPENDIX TABLE 7 - POLICY REFERENCES, BY RAHMENPLAN (FUNDING PLAN)

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 2: APPENDIX FIGURES

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)

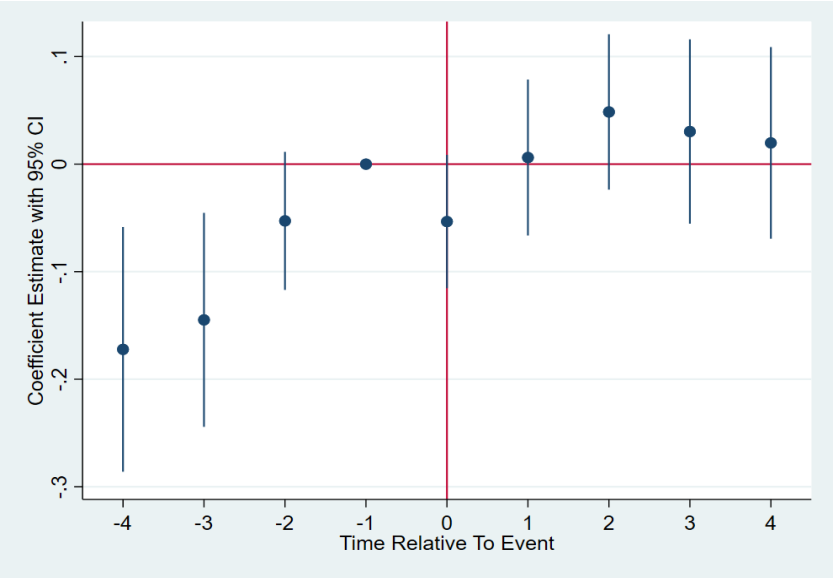


PANEL B: RED-CIRCLED AREA FROM PANEL A WITH ITS THREE LABOR MARKET REGIONS

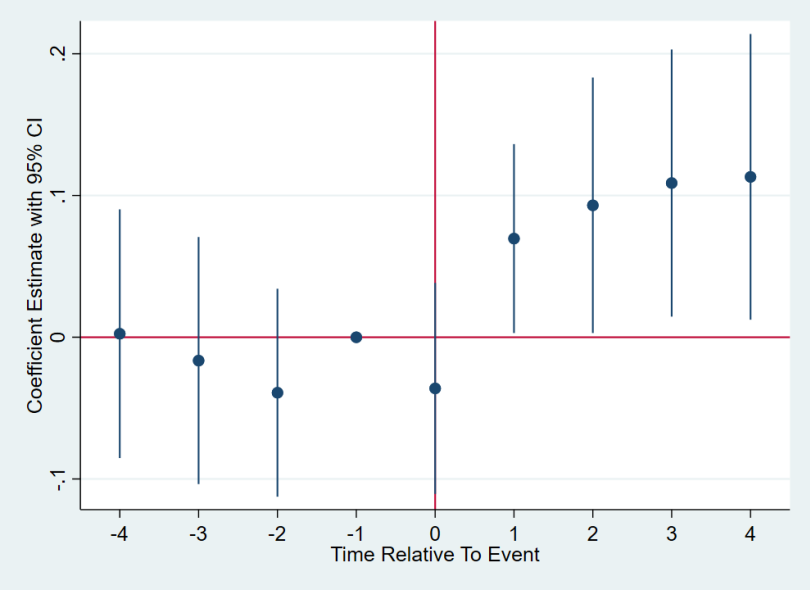
PANEL C: MUNICIPALITY "HAMM" (ELIGIBLE) AND ITS CONTIGUOUS NEIGHBORS

APPENDIX FIGURE 2 - EVENT-STUDY RESULTS FOR LOG-DAILY EARNINGS, WITH OR WITHOUT FIVE YEARS OF TENURE

A. ALL WORKERS



B. AT LEAST 5 YEARS OF TENURE



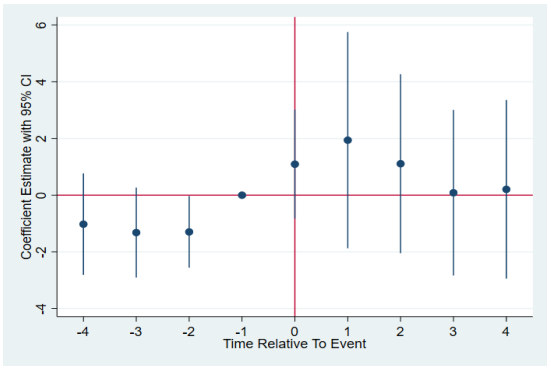
Notes: The figures show point estimates and their 95% confidence intervals for an event study for the log daily earnings of all employees (Panel A) and those with at least 5 years of tenure (Panel B). Point estimates are coefficients on the interaction between event-time dummies and the GRW subsidy rate. 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. Standard errors are clustered on the strata-level.

APPENDIX FIGURE 3 - EVENT-STUDY PLACEBO REGRESSIONS, FULL MATCH

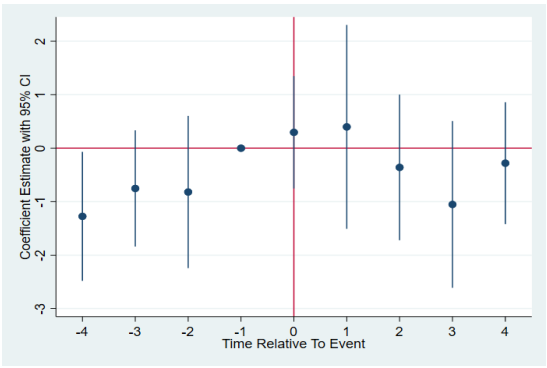
Number of Observations (after collapse): 2,880

Number of Strata: 160

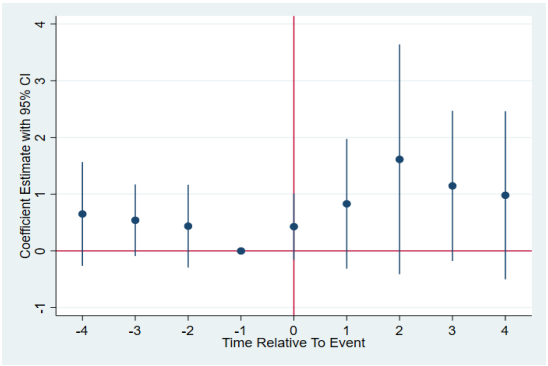
A. Number of Employees



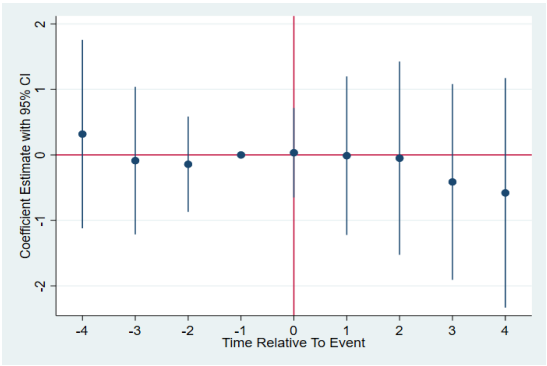
B. Number of Hires



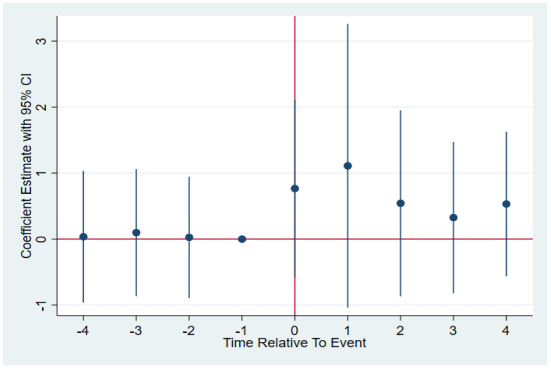
C. Number of Separations



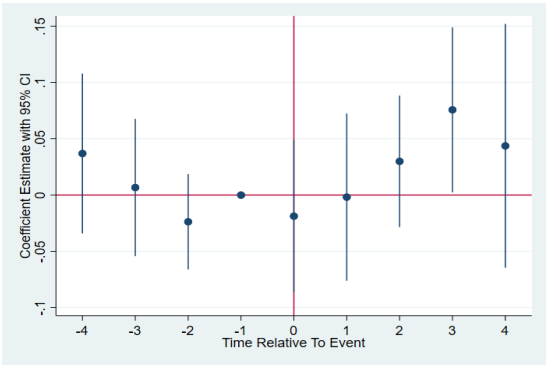
D. Number of Commuters



E. Number of Marginally Employed



F. Log-Daily-Earnings



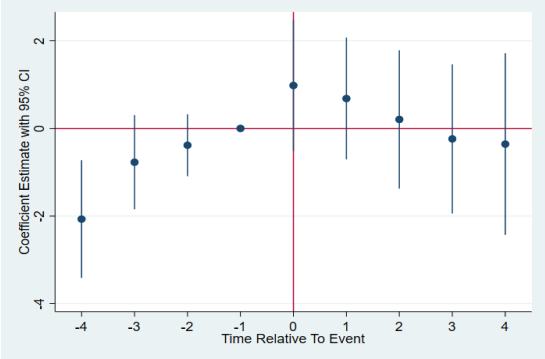
Notes: The figures show point estimates and their 95% confidence intervals for an event study that tracks differences in outcomes between placebo treatment groups (N=2,562 firms) and original control groups over a period of 9 years. Placebo-treated establishments are matched on initial employment, 2-digit 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 3. 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 FIGURE 4 - EVENT-STUDY PLACEBO REGRESSIONS, MATCH ON 2-DIGIT INDUSTRY ONLY

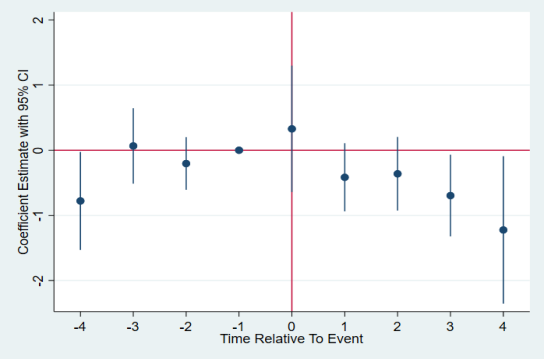
Number of Observations (after collapse): 4.878

Number of Strata: 271

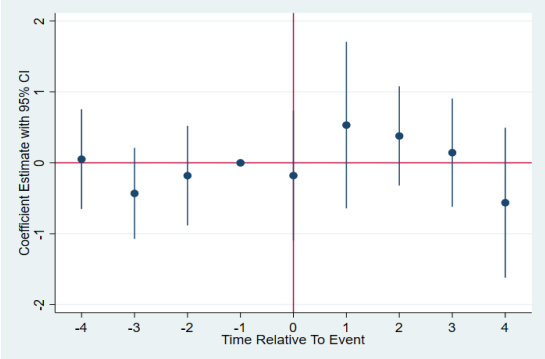
A. Number of Employees



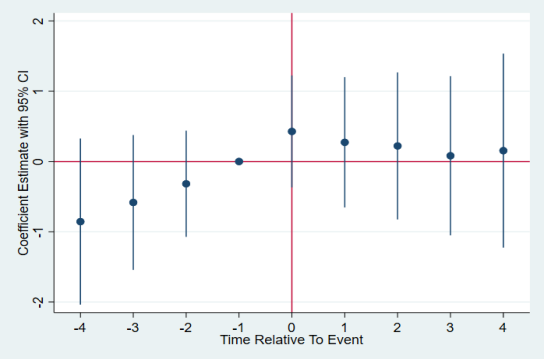
B. Number of Hires



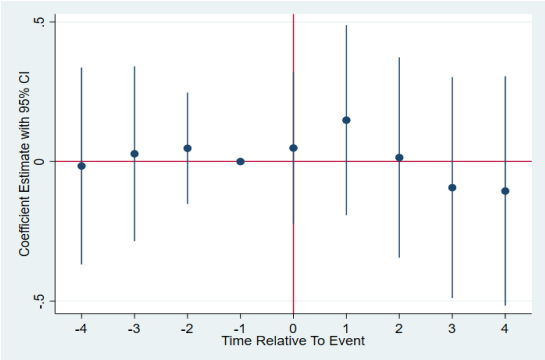
C. Number of Separations



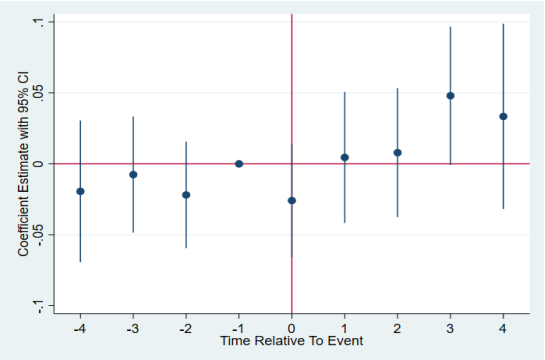
D. Number of Commuters



E. Number of Marginally Employed



F. 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.

APPENDIX 3: ADDITIONAL INFORMATION

APPENDIX 3.1: DATA

In this appendix we provide additional information about the definition of our six core outcome variables and about sample construction.

1. Outcome Variables

The main outcome variables, which we also refer to as six core outcomes, are defined as follows:

- ***Employment:*** The total number of employees who are subject to social insurance contributions, including part-time workers and workers in marginal jobs.
- ***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.
- ***Hires:*** The number of workers starting formal employment at an establishment in the current period.
- ***Separations:*** The number of workers who terminated formal employment at an establishment in the previous period.

2. Record Linkage

We perform record linkage on the IAB data on the one hand, which are aggregated to the establishment-year level, and the GRW funding data on the other hand, whose observations are recorded on the level of GRW-funded projects. Exact linkage is rather straightforward for the period starting in 2004, when the GRW started to systematically report the administrative *establishment* identifiers of the German Federal Employment Agency. Even in these years however we do not observe these identifiers for all projects. For cases in which we do not

observe identifiers in the GRW data, which are more frequent in the period prior to 2004, we start with matching exactly (rather than probabilistically) both on an establishment's name and the location of its branch. Both the GRW data and the administrative employment provide city, street, and house numbers.

A common reason for incomplete matches is missing information on establishment name, establishment id, or branch address in one of the two data sets. Another less common reason is that the GRW data list projects involving the creation of new branches of a firm that were approved for funding, but that never materialized. In either of these cases, crucial information for a successful linkage is missing, and we therefore cannot complete a match. On the other hand, for cases in which the information is filled, yet we do not find an ***exact*** match, 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 need to transcode 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.

[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/.

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 during 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 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 municipal 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 establishment-level event study, 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. Second, 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.

[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.

APPENDIX 3.2: MATCHING IN THE EVENT-STUDY DESIGN

This appendix describes in some detail how we implement the matching procedure for the event-study design. As in our computer programs, we start with finding matches on initial establishment sizes, as defined by their number of employees four years prior to the event ("matching on levels"). The second part of the appendix turns to finding matches on pre-event establishment growth ("matching on pre-trends").

1. Matching on Levels

Any matching we perform for the event-study starts from a sample of establishments located in contiguous border municipalities that are situated in separate LMR's. At the time of the funding event, the LMR's must have different subsidy rates. We construct strata in such a way that a treated establishment is contained within the municipality with a higher subsidy rate, while control establishments are located on the other side of the border. Starting from this sample, 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). We introduce the index *i* for control establishments only because there is always exactly one treated establishment per strata. Also define 2-period employment 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, $\left(\frac{\widehat{\beta}_{-4}+\widehat{\beta}_{-3}}{2}\right) = 0$. This leaves as free parameters three of the four pre-event treatment coefficients $\beta_{\tau<0}$.

The implementation faces two main challenges, however. First, it is data intensive because 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 $E^1_{s,-4}$. This is reflected in the third of these three conditions, which 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. Hence, we 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 can be well-approximated by distributions that satisfy "power laws". This is indeed the case for Germany and, more specifically, in 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

[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%.

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 $\left(\frac{\widehat{\beta}_{-4}+\widehat{\beta}_{-3}}{2}\right) = 0$ is met exactly. This is also convenient for the graphical representation and the interpretation of our coefficient estimates.

2. Matching on Pre-Trends

As shown and discussed in sections 4.2 and 4.3 of our paper, treated establishments grow substantially faster in the four years prior to receipt of funding than establishments in contiguous border municipalities that are matched on 2-digit industry and average establishment 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.

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

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.

APPENDIX 3.3: EFFECT HETEROGENEITY

This appendix discusses effect heterogeneity across various samples, using our six core outcomes and the benchmark specification that matches on employment levels and pre-event growth rates. This is a substantial extension of the description of these results in the main text.

A general conclusion from Tables 3 to 5 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 between them. Because each stratum corresponds to one treatment, this suggests that treatment heterogeneity may not be large. Only when using border counties- rather than municipalities have we documented some substantially lower, albeit still highly significant, estimates. In this section we explore more systematically to which extent our results depend on the sample. Results are shown in Appendix Table 3. The list of subsamples, varying across columns of the table, together with our reasoning for why we choose these particular selections, are 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 effects 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 "long 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 receives 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 heterogeneous 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.

For comparison, the first column of the table reproduces the baseline estimates for the core outcomes listed in Tables 3 to 5.

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 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.[5] 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. 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 small. For example, there are only 85 strata in East Germany and 149 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. Estimates are shown in panel B of the table. Overall, our conclusions from Tables 3 to 5 and from panel A of Appendix Table 3 are 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

[5] Descriptive statistics for East German worker- and establishment performances are provided for example in Hoffmann and Lemieux (2016) for industrial composition, Bachmann et al. (2022) for establishment size and productivity and Heise and Porzio (2023) for worker mobility.

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 Tables 3 to 5, 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. Moving from the municipality- to the county-level 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 1,007 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 market conditions in West Germany. To some extent, this is because the service sector is relatively bigger and because it attracts more firms that are large.

A final question we address is whether the program has a larger impact if it subsidizes establishments in capital-intensive industries. This is a reasonable hypothesis because 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 Appendix Table 3. 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. There is thus little empirical justification for targeting place-based policies at particular industries.

APPENDIX 3.4: MUNICIPALITY-LEVEL IV ESTIMATES

In this appendix we provide a full analysis of municipality-level IV estimates. To this end we estimate municipality-level reduced-form models and the corresponding first- and second stages for core aggregate outcomes. Results are shown in Appendix Tables 4 to 6. This analysis goes substantially beyond the discussion in the main text, where we focus on estimates from first- and second stages of the IV model for the hiring outcome.

1. Reduced Form Estimates

The reduced form of the municipality-level IV model is a regression of outcomes on the municipality-year level on the subsidy rate, flexible controls for the municipality's rank in the distribution of time-varying scores, and municipality- and year fixed effects. Formally, this is equivalent to equation (2) in the text, but with (endogenous) funding amounts replaced by the (conditionally exogenous) subsidy rates. We estimate this model on the population of contiguous border municipalities that have a gradient in subsidy rates at least once over the sample period. The subsidy rate is normalized on a scale of 0 to 1 to simplify the interpretation of the results.

Estimates of the reduced form effects of the subsidy rate are shown in Appendix Table 4, with municipality-level results reported in Panel A and LMR-level results reported in Panel B. To demonstrate the importance of properly controlling for our rank variable, we consider progressively richer model specifications across columns. Column (2) lists the coefficient when we do not include any controls in the regression model. As expected from the design of the GRW that targets areas with poorer labor market outcomes, the results show that the raw correlation between the subsidy rate and labor market outcomes are, in most cases, negative and statistically significant. Once we add fixed effects for the level of geography – municipalities in Panel A and LMR's in Panel B – and for time, the estimated effects move closer towards zero and become statistically insignificant. Hence, a conventional DiD-model is not sufficient to control for the endogeneity of the subsidy rate. Only when adding controls for the funding rank (columns 4-6) do the estimates flip sign and become highly significant. A conclusion from this table is 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.[6] 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 establishments, a finding that is consistent with our establishment-level analysis.

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 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, Gregory, and Kline, 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

[6] 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. A reason is that the administrative demarcation of eligibility is done with the help of the population share rather than the share 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.

(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. Again, this is supported by our findings from the establishment-level analysis.

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.

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 Appendix Table 5, both for the amount of subsidies paid to establishments with- and without the amount of subsidies for public business-related infrastructure projects. As in Appendix Table 4, 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 Appendix Table 6 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.

Most of the other results reported in the other rows of the table 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.[7]

[7] Since daily earnings are averaged over calendar days, the average daily earnings 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.

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