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**Spillover, Efficiency and Equity Effects of
Regional Firm Subsidies**

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Abstract

Abstract. We analyze the effects of a large place-based policy, subsidizing up to 50% of the investment costs of manufacturing firms in East Germany. We show that a one-percentage-point decrease in the subsidy rate leads to a 1% decrease in manufacturing employment. We document important local spillovers for untreated construction and retail sectors, counties connected via trade, and local tax rates. There is no evidence for regional reallocation or changes in commuting and residential decisions. The cost per job amount to at most \$23000. Last, we show that local subsidies are substantially more effective in curbing regional inequality than place-blind policies.

Keywords: place-based policies, employment, spillovers, administrative microdata

JEL codes: H24, J21, J23

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1 Introduction

Place-based policies are a widely used policy instrument to support regions that are economically lagging behind. From 2014 to 2020, the European Union spent more than €350 billion – about a third of its budget – on regional policies during the funding period (Ehrlich and Overman, 2020). The United States currently devote about \$60 billion to place-based policies – mostly through business tax incentives (Bartik, 2020, Slattery and Zidar, 2020). Policymakers often justify the use of place-based policies with a redistributive argument, stressing it as an important instrument to curb spatial inequality – a pervasive problem in many Western countries. Critics of these policies are concerned with potential inefficiencies as government transfers flow to relatively unproductive places, while productive regions implicitly or explicitly have to finance these expenses (Glaeser and Gottlieb, 2008).

While there has been a recent wave of papers studying the effects of place-based policies, there are still some open questions centering around the efficiency-equity debate.¹ First, how (in)efficient are place-based policies relative to other redistributive policies? The literature has shown that place-based policies typically unfold positive direct effects on treated regions. However, regional subsidies might lead to substantial distortions, such as population reallocation or an increase in the local cost of living, that could fully offset the direct effect (Glaeser, 2008). Whether the benefits outweigh the costs can also depend on the context and the policy under study. Second and related, it is well-known that the efficiency of place-based policies also depends on indirect spillover effects affecting untreated workers, firms, and regions (Kline and Moretti, 2014a, Austin et al., 2018). The literature, which is discussed in more detail below, has discussed that place-based policies can trigger agglomeration effects, sectoral relocation, trade and human capital spillovers or externalities on other (local) policy instruments. However, existing studies typically focus only on one spillover at a time. In order to assess the overall efficiency of the policy, all relevant spillover effects need to be quantified and taken into account. Third, for given efficiency, there is little evidence on the effectiveness of place-based policies to curb regional inequality. To find the right policy instrument to reduce spatial inequality, it is crucial to know whether local subsidies are an effective tool to reduce regional inequality relative to place-blind redistributive policies.

In this paper, we intend to provide answers to these questions by studying the (i) direct and (ii) spillover effects of place-based policies, (iii) assessing their overall efficiency, and (iv) testing their effectiveness to reduce spatial inequality. We investigate a prominent place-based policy that subsidized up to 50% of firms' investments in East Germany after reunification. First, we provide cleanly identified reduced-form estimates of the direct effects and arguably all relevant spillovers. In particular, we investigate a host of potential spillovers on other neighboring and far-away regions, untreated sectors, local housing markets, and local policy instruments. Second, we conduct a cost-benefit analysis and calculate the cost per job to assess the efficiency of the GRW and compare it to other (place-based) policies. Third, we shed novel insights on the potential of place-based policies to curb regional inequality by comparing its effects on regional inequality to revenue-neutral place-blind redistributive policies.

We study the case of the most prominent German place-based policy called GRW.² The GRW con-

¹ See Kline and Moretti (2014b), Neumark and Simpson (2015), and Duranton and Venables (2018) for overviews of the literature.

² The German name of the policy is *Gemeinschaftsaufgabe Verbesserung der regionalen Wirtschaftsstruktur* – throughout the

stitutes Germany's main regional policy scheme for underdeveloped regions (Deutscher Bundestag, 1997). While not exclusively targeted at East Germany, the overwhelming share of the subsidies went to the formerly socialist part of the country after reunification. In fact, the GRW was the main regional subsidy to revitalize the East German economy after the fall of the Iron Curtain. The GRW's key instrument is an investment subsidy for manufacturing firms in eligible regions. These subsidies can be, for example, used for purchasing new machines or building new production sites. The explicit goal of the policy is to boost investment, which is in turn supposed to create new jobs and stimulate regional growth.

Our identifying variation comes from multiple reforms of the maximum subsidy rate of investment cost between 1997 and 2014. These reforms changed subsidy rates differentially across East German counties based on a predetermined economic performance indicator that combines past unemployment rates, average wages, and infrastructure amenities. For each new policy regime, the measure of economic development is based on different weights for the sub-components, which are determined at a higher regional level. Hence, the measure is difficult to manipulate for counties and we provide evidence that selection into treatment does not seem to be a concern. Explicitly, we compare counties that are below the threshold yielding a higher subsidy rate to counties that are above. In other words, we zoom in on counties that are relatively similar in terms of wages, unemployment rate, and infrastructure amenities prior to treatment. Eligibility thresholds change across budgeting periods and these changes are partly triggered by reunification dynamics at the federal level or EU legislation, which are exogenous to regional economic development.

We combine official data on the universe of subsidy cases with administrative social security data on firms and workers to estimate the reduced-form effects of the policy, differentiating between the direct policy effects and various spillover effects across regions, sectors, and to other local policies. We use the Establishment History Panel, an administrative plant-level data set, provided by the Institute for Employment Research (IAB) of the German Federal Employment Agency. For the years 1996-2017, we have access to a fifty percent random sample of plants in East Germany. The data cover the annual number of employees at the plant level as well as the county in which it is located. In addition, we rely on administrative data on individual wages included in the Sample of Integrated Labour Market Biographies (SIAB) – a representative two percent sample of German employees subject to social security contributions from 1996 to 2014. Official subsidy data from 1996 to 2016 is provided by the Federal Ministry for Economic Affairs. We have obtained the universe of GRW subsidy cases, including the county the plant is located in, investment volume, and amount of subsidies paid. In addition, we gathered regional data to replicate the indicators determining treatment status across all budgeting periods.

Econometrically, we estimate event study designs to pin down the policy effects. The main outcome of interest in our study is the effect of GRW subsidies on regional employment. However, we do not restrict our analysis to the direct policy effect but also capture arguably all relevant spillovers. We investigate spillovers within treated regions, in the neighborhood of treated regions, and to distant regions that have an economic nexus with treated regions. Moreover, we consider spillovers to local policy instruments.

We use our direct and indirect reduced-form results to infer the efficiency of the policy using a

paper, we will refer to it using the official German acronym GRW.

simple cost-benefit analysis. We follow standard practice and calculate the cost per job of the GRW and compare it to estimates of other related policies. Last, we simulate the policy's capacity to affect regional inequality and compare it to revenue-equivalent place-blind transfers.

In terms of direct effects, we derive the following three results. A one-percentage-point decrease in the subsidy rate in the treated manufacturing sector decreases subsidized investment by 14.6% and total (i.e. subsidized plus unsubsidized) investment by 7.4%. Second, manufacturing employment decreases by 1% in the long run. We do not find asymmetric effects of subsidy cuts and increases. Effects for small plants are twice as large as effects for large plants. Third, wages are largely unaffected.

In terms of spillover effects, we derive the following results. First, a one-percentage-point decrease in the subsidy rate for the manufacturing sector leads to a 0.26% and 0.47% employment reduction in the untreated retail and construction sector, respectively. This implies that for every manufacturing job lost, 0.47 additional jobs in retail and construction are lost. Second, there is no evidence of positive or negative spillovers of a county-level shock within the local labor market. Third, we find evidence for negative manufacturing employment responses of counties that have a higher trade exposure to treated counties. Fourth, we demonstrate important negative policy spillovers. A decrease in the subsidy leads to a long-run increase in local business and property tax rates, which can be rationalized with a fixed expenditure requirement of municipalities and a decreasing tax base. The increase in the local business tax rate can explain about 10% of the overall employment effect.

In terms of efficiency and equity, we derive the following results. First, the cost per job of the GRW is at most \$23000 and could be lower depending on the assumption about the deadweight loss of taxation. Importantly, we show that the cost per job is substantially less favorable if one does not account for spillovers. Second, we show in a simulation exercise that place-based policies are more effective in reducing regional inequality than place-blind cash transfers.

Overall, our results imply that the German GRW stabilizes employment at comparatively low efficiency costs. Busso et al. (2013) show in a general framework that the deadweight loss of place-based policies depends crucially on the mobility of individuals in terms of their residential choices and commuting behavior as well as on the effect on local wages and the number of jobs created. While local wages are not affected, there is a substantial employment effect. Importantly, the increase in employment is not driven by locational distortions – we find no effects on population numbers, commuting behavior, or regional substitution of workers. Instead, the lion's share of the employment effect is coming from unemployment.

We contribute to the existing and recently growing literature on place-based policies in several ways. First, we provide novel evidence on the direct, reduced-form effects of an important place-based policy. We go beyond many existing studies by showing how treatment effects evolve over time in dynamic event study models. Moreover, we rely on firm-level investment and employment data, while previous research often looked at aggregate-level employment effects. Our findings reinforce recent findings that place-based policies are effective – in the sense that they have a positive and long-lasting effect on the local economy. Kline and Moretti (2014a) show that the Tennessee Valley Authority, the most prominent regional subsidy program in U.S. history had a positive effect on manufacturing employment that lasted even after the program ended due to agglomeration forces.

Looking at Chinese cities, Alder et al. (2016) show that special employment zones have a strong

positive effect on GDP mainly driven by an increase in capital accumulation. A series of papers investigating the effects of the EU Structural Funds (ESF), a regional subsidy paid by the European Union, show that ESF increases GDP in the subsidized regions, but had no clear effect on employment (Becker et al., 2010, 2012, 2013). Criscuolo et al. (2019) analyze an industrial policy in the UK, which is similar to the GRW, and find employment effects that are quite comparable to our effects qualitatively and quantitatively. For Germany, Ehrlich and Seidel (2018) investigate a different place-based subsidy paid to West German regions close to the Iron Curtain from the 1970s until reunification and find positive treatment effects.

Only a few studies have analyzed the impact of the GRW so far. Our results are in contrast to the recent findings by Brachert et al. (2019), who find no significant employment effects. However, their paper differs from ours in various dimensions. They focus on West Germany instead of East Germany, the latter being the major recipient of the GRW funding after reunification. Moreover, they use aggregate data at the regional level and a different identification strategy. Our findings of a positive (negative) employment effect of a subsidy increase (decrease) are in line with descriptive and more policy-oriented German papers (Bade and Alm, 2010, Bade, 2012).

Analyzing the direct effect of local subsidies is naturally related to studies investigating the local labor market effects of regional tax policy on workers and firms (see, e.g. Suárez Serrato and Zidar, 2016, Fuest et al., 2018, Fajgelbaum et al., 2019). In a recent study, Ohrn (2019) finds that state-level accelerated depreciation policies in the US had a strong effect on investment. Garrett et al. (2020) exploit county-level variation in the exposure to federal accelerated depreciation policies showing a significant effect of the policy on local employment. Our work is also related to the large empirical literature studying the effects of place-blind industrial policies targeting specific sectors (see, e.g. Aghion et al., 2015, Liu, 2019, Lane, 2020, Manelici and Pantea, 2021).

Second, we systematically investigate the indirect spillover effects of place-based policies by providing cleanly identified reduced-form estimates. While various empirical studies have looked at single spillover effects, this is – to the best of our knowledge – the first comprehensive analysis providing reduced-form evidence on all relevant spillovers discussed in the literature. In line with Criscuolo et al. (2019), we find no evidence of positive or negative regional spillovers in neighboring counties. In other words, we find no evidence of relocation from unsubsidized to subsidized places (Kline and Moretti, 2014a). However, we demonstrate important local employment multiplier effects as the untreated retail sector and construction sector are negatively affected by decreases in regional subsidies to manufacturing firms. Our estimated local employment multiplier of 0.47 is well within the range of estimates in the literature ranging from 0.4 in Gathmann et al. (2020) using German data to 1.6 in Moretti (2010) for US cities. We also point to trade spillovers that have not been investigated in the literature before in a reduced-form way, complementing evidence from structural models (Blouri and Ehrlich, 2020). We do not find evidence that local subsidies are capitalized into housing prices (Busso et al., 2013), and neither do we find that human capital spillovers are relevant in our setting (Glaeser and Gottlieb, 2008, Diamond, 2016). Last, we find important policy spillovers that have not received much attention so far. We show that local tax rates increase as a result of decreasing subsidies, which adds an additional burden on local firms. This finding is in line with a result by Ehrlich and Seidel (2018) who look at a different German place-based policy and show that the regional subsidy leads to higher local public investment levels. Our results suggest that a decrease in

the GRW erodes firm profits and thus the local tax base, yielding higher local tax rates to finance the largely pre-committed local expenditures.

Third, we add to the current debate on the efficiency and equity effects of (place-based) policies. Our approach is an alternative to important structural approaches that have seen a recent surge in the literature (Gaubert, 2018, Rossi-Hansberg et al., 2019, Fajgelbaum and Gaubert, 2020, Gaubert et al., 2021). Clearly and as established in other contexts, both approaches have their advantages and disadvantages. While the structural approach allows estimating policy counterfactuals and is capable to capture general equilibrium effects of non-marginal policy changes, we opt for a partial equilibrium approach and a simple cost-benefit analysis that relies on our reduced form estimates. Looking specifically at the efficiency and equity effects of a place-based policy, our study can be seen as the reduced-form counterpart to recent structural work by Gaubert et al. (2021).

The remainder of this paper is organized as follows. We explain the institutional setting in Section 2, followed by Section 3 on the data. Section 4 presents our baseline empirical strategy and results for the direct policy effects. In Section 5, we describe our approach to estimating the spillovers of the policy and present the results. In Section 6, we discuss the efficiency and equity implications of the policy. Section 7 concludes.

2 The GRW Policy

In this paper, we study the main German regional economic policy, called “Gemeinschaftsaufgabe Verbesserung der regionalen Wirtschaftsstruktur” (GRW). The GRW is jointly coordinated and financed by the federal government and the individual states. The explicit goal of the policy is to equalize standards of living across German regions by stimulating local business activity. Equalizing living standards across space is an important principle and policy goal in Germany, which is explicitly mentioned in the constitution. The GRW is the main federal program to achieve this goal.

The policy was implemented in 1969 and subsidized West German underdeveloped regions throughout the 1970s and 1980s. In this study, we focus on the post-reunification effect of the GRW until 2017. After reunification, the majority of GRW funds were directed to East German regions, which were considerably less industrialized than their West German counterparts. As such, the GRW was seen as one of the main instruments aiming at re-industrializing East Germany and bringing it to Western levels.³

While the GRW consists of a bundle of different instruments, we focus on investment subsidies paid out to plants – the central instrument accounting for 74% of the total GRW budget in our sample period.⁴ These subsidies covered up to 50% of the costs of a specific investment project filed by a plant. The subsidy rate varied across counties depending on the regional economic development, making the GRW a place-based policy (see Section 2.2 for more details). From 1991 to 2016, on average €1.8 billion of subsidies (in 2010 €) were paid out annually to East German firms.⁵ Over our

³ Other policy measures targeted at plants in Eastern Germany include the investment tax credit program (*Investitionszulagengesetz*), the special depreciation allowance (*Fördergebietsgesetz*) as well as the EU structural funds. We describe in Appendix B.3 how we account for these policies. Another class of programs directed funds to municipalities rather than to plants. We check that the reforms exploited for identification did not affect funds paid to municipalities.

⁴ The other important instruments are infrastructure subsidies to municipalities, which were granted independently of the investment subsidies. Importantly, the maximum infrastructure subsidy rates do not exhibit variation across space.

⁵ These numbers include co-payments by the European Union via the European Regional Development Fund (ERDF).

sample period, the GRW accounts for about 0.3% of total investment in Germany making it unlikely to induce meaningful general equilibrium effects.

2.1 Eligibility

To receive the subsidy, plants need to file an application for approval with their respective state government. In the application, they need to clearly define the investment project to be subsidized. Typical projects comprise the acquisition of machinery, the construction or modernization of buildings, but also licenses and patents. Labor costs can only be subsidized if employees can be directly linked to the corresponding investment project.

The eligibility of a project is determined by three criteria. First, the project has to be relatively large. Either annual investment costs have to exceed the average amount of the plant's capital consumption (economic depreciation) in the preceding three years by at least 50% (criterion 1a), or the project has to increase the number of regular employees by at least 15% (criterion 1b). New plant openings qualify under criterion 1b. Second, the project has to be limited in time. The maximum duration of the project is three years (criterion 2). Third, the subsidies are intended for exporting firms. At least half of the plants' revenues have to be made outside of the county (criterion 3). The rationale behind criterion 3 as revealed by the policy discussion in the 1960s is that export-oriented firms are supposed to generate additional income within a county, which, in turn, is supposed to stimulate local demand. Due to criterion 3, about 77% of the GRW funds went to manufacturing firms (Bade and Alm, 2010). In Appendix Table B.6, we show an official list of sectors that automatically qualified for the subsidy according to Criterion 3 without the need to provide further evidence. Notice that certain industries were de jure excluded from the subsidies. These include the construction and retail sectors which we will investigate for potential spillover effects.

States have an annual budget for projects to be subsidized under the GRW program. In more than 90% of cases, states did not exhaust their annual budgets, which suggests that there was usually no rationing of the funds and no rivalry between projects. Nevertheless, not all projects were granted. While official data on rejected projects are unfortunately not available, survey data for the state of Thuringia from 2011 to 2016 suggests that roughly 39% of applications were denied (IWH, 2018). However, these rejections were almost entirely due to formal reasons. The two main reasons for rejection, accounting for 96% of rejections, were (i) missing documents and (ii) not meeting the eligibility criteria. Hence, there is no reason to believe that the selection of projects was based on their assessed quality.

2.2 Subsidy Rates

Upon successful application, plants receive subsidies to cover a certain share of the investment cost stated in the application.⁶ There is a binding maximum subsidy rate imposed by federal law, which varies by plant type, year, and – importantly – plant location, the latter source of variation making

Whether subsidies were paid for by the ERDF or GRW is irrelevant for our analysis since in Germany, ERDF funds simply increase states' subsidy budgets. Restrictions on subsidy usage, such as sectoral restrictions and maximum assistance rates are thus identical for ERDF and GRW funds.

⁶ It takes on average about 8 months for an application to be approved (IWH, 2018).

the GRW a place-based policy.⁷

Below, we exploit the variation in maximum subsidy rates to estimate the causal effects of the policy. In the following, we describe this variation in detail. As a general principle, the policy accounted for differences in the economic performance *within* East Germany and assigned higher subsidy rates to relatively less productive counties. Importantly, differentiation was conducted on the *national* level by the Federal government based on *past* economic performance – both the national decision and the past economic behavior being important features for our identification strategy. More precisely, local productivity was measured by a performance indicator at the level of the commuting zone (*Arbeitsmarktregion*) with counties being nested in commuting zones. There were 76 counties in East Germany and 53 commuting zones in the boundaries of 2014.⁸

In the following, we give an example of the performance indicator and how it affected subsidy rates for the year 1997. The performance indicator for commuting zone r is the weighted geometric mean of three sub-indicators and is described by the following formula:

$$indicator_r^{1997} = \left(\frac{wage_r^{1995}}{wage_{East}^{1995}} \right)^{0.40} \times \left(2 - \frac{unemp_r^{1995}}{unemp_{East}^{1995}} \right)^{0.50} \times \left(\frac{infr_r^{1995}}{infr_{East}^{1995}} \right)^{0.10},$$

where *infr* measures the quality of a county's infrastructure in the reference year 1995, *wage_r* represents per-capita earnings in 1995, and *unemp_r* measures the unemployment rate in 1995, entering negatively, all relative to their respective East German average. All counties are ranked according to this indicator as depicted in Appendix Figure A.1. Counties with an index value below 100 were classified as high-funding-priority and counties with a value above the threshold as low-funding-priority. Counties with a high funding priority receive a higher subsidy rate.⁹

Importantly, indicators, reference years, cut-off values, and subsidy rates are valid for specific regimes that last between 3 and 7 years. At the end of a regime, indicator function, priority statuses, and subsidy rates change, which leads to substantial variation in maximum subsidy rates from the perspective of the individual county. In the last part of the subsection, we document the evolution of regimes and the resulting policy variation.

Table 1 gives an overview of the policy variation. In the early 1990s, all East German counties were treated equally, with the maximum subsidy rate for small and medium-sized plants being 50% and 35% for large plants. As of 1997, policymakers started to differentiate funding priorities spatially. Based on the performance indicator described above, 27 out of 76 counties were assigned

⁷ Unfortunately, we do not observe the actual subsidy rates at the plant level. Therefore, we have to rely on the reduced form relationship between the maximum subsidy rate and our outcomes. However, we can infer from the negative effect of the maximum subsidy rate on the amount of subsidies paid that the maximum subsidy rate has to be binding (see Figure 2).

⁸ Over the years, some counties in East Germany merged. In a robustness check, we make sure that mergers do not affect our results by excluding all counties that were partially treated. We exclude the county of Berlin from all of our analyses because of its status as a federal state.

⁹ The rule is almost perfectly deterministic such that all counties above the threshold receive lower funding probability. However, there is some noise in the assignment as revealed by Appendix Table B.3. We see that a few counties below the cut-off were assigned low priority. This is mainly due to county mergers that occurred after the reform, i.e. a county above the threshold was merged with a county that was below the threshold. As mentioned above, we exclude partially treated counties in a robustness check. In addition, the Federal government (jointly with state governments) reserves the right to deviate from the ranking in rare exceptions (two counties in 1997). This is mostly due to one county biasing the commuting zone average upwards. For example, the relatively poorer county of Gifhorn is located in the same commuting zone as the county of Wolfsburg, which contains the headquarters of Volkswagen. Therefore, policymakers decided to assign Gifhorn to a higher priority even though the commuting zone index was too high (Brachert et al., 2019).

Table 1: Subsidy regimes for East German counties since 1990

	Regime 1 1990-1996		Regime 2 1997-1999		Regime 3 2000-2006		Regime 4 2007-2010		Regime 5 2011-2013		Regime 6 2014-2017		Regime 7 2018-		
priority	high	low	high	medium	low	high	low								
small plants	50%	n/a	50%	43%	50%	43%	50%	n/a	50%	40%	40%	35%	30%	40%	30%
medium plants	50%	n/a	50%	43%	50%	43%	40%	n/a	40%	30%	30%	25%	20%	30%	20%
large plants	35%	n/a	35%	28%	35%	28%	30%	n/a	30%	20%	20%	15%	10%	20%	10%
# counties	76	n/a	49	27	41	35	76	n/a	58	18	9	64	3	9	67

Sources: Deutscher Bundestag (1996), Deutscher Bundestag (1997), Deutscher Bundestag (2000), Deutscher Bundestag (2007), Deutscher Bundestag (2016) Notes: Plant size is defined by the number of employees. Small plants have less than 51 employees, medium-sized plants 51 to 250, and large ones above 250.

to low funding priority and consequently experienced a cut in the maximum subsidy rates by 7 percentage points across all three plant size groups (see Table 1, regimes 1 vs. 2). In 2000, a new ranking of the counties was generated based on updated measures of past economic performances and slight changes in the indicator function (see Appendix B.2 for a detailed description of the indicator function). As a consequence, additional counties switched from high to low priority status.

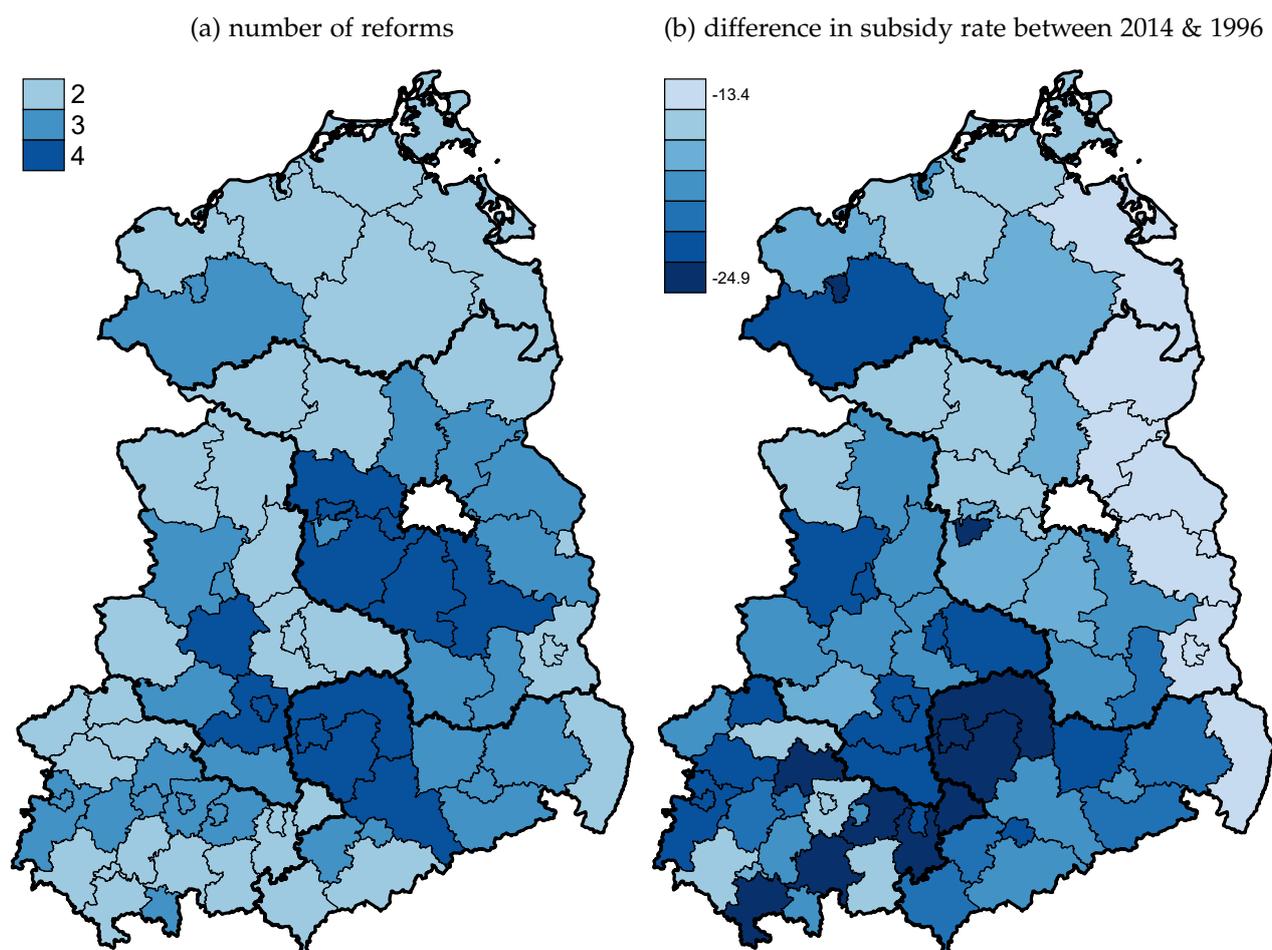
In 2007, the ranking of counties was renewed. This time, all German counties (East and West) were jointly assessed and ranked – in contrast to previous years, where East German regions were assessed separately. As West German regions were still richer than their East German counterparts, all East German counties received high priority status. As a consequence, 35 counties saw an increase in their (employment-weighted) subsidy rate.¹⁰ This particular reform is interesting for various reasons. First, the re-ranking was completely exogenous to the economic performance of East German counties. Second, the reform enables us to test whether the effects are symmetric.

The next reassessment occurred in 2011 when 18 counties were downgraded in their priority status. The reason for this change was the EU's enlargement from 15 to 25 member states resulting in a decline in the average regional GDP per capita in the EU. According to EU regulations, regions above the 75th percentile of GDP per capita lose eligibility for the highest maximum rates. In 2014, Germany was required by the EU to again lower their maximum subsidy rates in two steps. Until 2018, no German county was allowed to have subsidy rates above 30% for small plants. Hence, three well-performing counties were directly downgraded and 64 counties experienced an intermediate decrease to a maximum rate of 35% for small plants (25% and 15% for medium and large plants). An exception was made for counties that were located directly at the border with Poland since the difference in the subsidy rate between them and the Polish regions would be higher than EU regulations allow. In 2018, the 64 counties with medium priority saw the second cut of 5 percentage points to bring them to the required 30% rate. The 9 counties bordering Poland kept their special status. Note that, even though we do not exploit the 2018 reform directly since our data ends in 2017, we still account for these future reforms in our event study setup.

Overall, the various reforms generate substantial variation in maximum subsidy rates across East German counties, which we exploit in our empirical research design presented in Section 4.1. Figure 1a illustrates that all counties experience at least two changes in the subsidy rate, while more than 50% experience three or four changes. The change in the (employment-weighted) maximum subsidy rate varies from a reduction of 13.4 to 24.9 percentage points (see Figure 1b). The right panel

¹⁰ The subsidy rate for small and large plants, which account for two-thirds of manufacturing employment on average, rose by 7 and 2 percentage points, respectively, while the rate for medium plants decreased by 3 percentage points.

Figure 1: Map of reforms from 1996 to 2014



Sources: Deutscher Bundestag (1996), Deutscher Bundestag (1997), Deutscher Bundestag (2000), Deutscher Bundestag (2007), Deutscher Bundestag (2016) Notes: Berlin is excluded from the analysis.

also shows some interesting regional clustering, e.g. the counties bordering Poland experienced the smallest cuts in rates, while the area around Leipzig, saw the largest. Note that our identification strategy only exploits changes within federal states – indicated by the thicker line – for identification.

3 Data

In this section, we present the data that we use in our analysis. Detailed information on variable definitions and sources can be found in Appendix Table B.1 and summary statistics are presented in Appendix Table B.2.

3.1 Subsidy Data

We use administrative subsidy data provided by the Federal Ministry for Economic Affairs. For the years 1996-2016, we obtained the universe of GRW subsidy cases in East Germany including investment volume, subsidy amount, and the receiving plant's county. Matching these data to plants is prohibited due to data protection laws, hence we are unable to identify which plants did receive subsidies and which did not. We follow standard practice and estimate the intent-to-treat

effect, investigating the employment response of plants in a treated area (Criscuolo et al., 2019). As mentioned above, 74% of all subsidies were paid to manufacturing firms. Appendix Table B.2 shows that the average yearly subsidy payments received by a county amount to €18 million, supporting investment projects worth €82 million.

3.2 Employment and Wage Data

We measure employment using the Establishment History Panel (BHP), which is based on social security records and provided by the Institute of Employment Research in Nuremberg (Schmucker et al., 2018). We have access to a 50% random sample of plants in Germany for the period of 1996-2017. The dataset includes the annual number of employees by skill at a plant as well as the county in which it is located and its industry classification.

To measure wages, we additionally use the IAB's Sample of Integrated Labour Market Biographies (SIAB) from 1996 to 2014 (Antoni et al., 2016). The dataset is a 2% sample of individual earnings biographies and includes individual characteristics as well as employer information from the BHP. Earnings histories are in general recorded for persons who have appeared at least once in the social security system, either as an employee or as being unemployed, since 1975. We drop all apprentices, social service workers, working students, and interns and convert wages to 2010 €. Then, we calculate the median wage at the county level both for manufacturing workers and all workers. As one can see in Appendix Table B.2, workers in the manufacturing sector have a higher median wage than workers in other sectors. We also calculate wages by education level within the manufacturing sector. As expected, high-skill workers earn substantially higher wages than their low-skilled peers.

3.3 Investment Data

Moreover, we obtain investment data at the plant level from the AFiD Establishment-Panel provided by the Federal Statistical Office of Germany. The data cover the universe of German manufacturing and mining plants with 20 or more employees for the period from 1996 to 2016. Importantly, we can observe total investment on the plant level which we deflate to 2010 €. Additionally, the AFiD data provide industry codes and information on the plant's location at the municipal level. We use that information to restrict our sample to manufacturing plants and locate plants within the current county borders.

3.4 Trade Flow Data

We use trade flow data from the Federal Ministry of Transport and Digital Infrastructure to calculate the trade exposure of German counties as described in Section 5.2. The data include a complete matrix of trade flows between all German counties as well as foreign countries for the year 2004.¹¹ Trade flows are measured in tons per year and we can observe the direction of trade, i.e. we can differentiate between imports and exports between two counties.

¹¹ Unfortunately, there no earlier data are available. Therefore, one has to keep in my mind that trade flows might be endogenous to the policy we are investigating. We also have the same measure for the year 2010, which yields qualitatively similar results.

3.5 Other Regional Variables

Last, we use further regional variables either as outcomes or as control variables. We obtain administrative data on the local business cycle (GDP per capita and local unemployment) as well as labor force and population numbers, provided by the statistical offices of the German states. To assess policy spillovers, we additionally obtain data on the municipal local business and property tax rate. While the tax base of these taxes is set at the national level, municipalities can freely set their own tax rates (see Fuest et al., 2018, Löffler and Siegloch, 2021, for a detailed description). Furthermore, we obtain tax revenues from business and property taxation and use them to calculate the respective tax base.

In addition, we gather data on municipality-level grants and subsidies to make sure that other transfers are not confounding our GRW effect. To keep the level of analysis consistent, we aggregate the municipal-level data to the county level using pre-reform population shares as weights if necessary. Moreover, we collect data on the net commuting flows normalized by the number of employees from the Federal Office for Building and Regional Planning.

Last, we add on housing prices and rent per square meter, to assess whether the GRW subsidies are capitalized into housing prices. To populate our long panel starting in the 1990s, we use data from the German real estate association IVD. These data cover the largest city within a county and describe the house price as well as the rent per square meter.¹² Both house prices and rents are computed for an object of average quality.

4 Direct Policy Effects

In Section 4.1, we present our empirical model for the direct policy effects of the place-based policy. Section 4.2 focuses on the direct policy effect of a subsidy cut for manufacturing plants in the treated counties. In Section 4.4, we address various identification challenges and demonstrate that our main effects are robust.

4.1 Empirical Model

We estimate the causal effect of the subsidy implementing different variants of event study designs. Given that the policy variation described in Section 2 is quite complex, we develop our preferred empirical model step-by-step.

Simple Event Study. As described in Section 2, the vast majority of subsidy rate changes were decreases. In the simplest form, the event study model regresses an outcome y (such as employment or investment) of plant i in county c and year t , $y_{i,t}$ on dummy variables indicating a subsidy cut in county c at time t as follows.

¹² For some county-year pairs, no data is available. We interpolate occasionally missing data points linearly. More comprehensive microdata, e.g. from the online platform ImmobilienScout24 (the German Zillow), only started much later in the mid-2000s.

$$\ln y_{i,t} = \sum_{k=-4}^{10} \beta^k D_{c,t}^k + \zeta X_{c,t} + \delta_i + \gamma_c + \psi_{s,t} + \varepsilon_{i,t}. \quad (1)$$

where $D_{c,t}^k$ is the mentioned set of event indicators indicating whether a change in the maximum subsidy rate occurred for the county $k \in [-4, \dots, 10]$ periods ago. We refer to $D_{c,t}^k$ as *binned* event indicators as the indicators at the endpoints of the effect windows, $k = -4$ and $k = 10$, take into account all observable past (future) events going beyond the effect window (McCrary, 2007, Schmidheiny and Siegloch, 2020). Let $d_{c,t-k} = 1$ if county c experienced a subsidy cut in year $t - k$, $d_{c,t-k} = -1$ in case of a subsidy increase and $d_{c,t-k} = 0$ otherwise, then the binned event indicators $D_{c,t}^k$ are formally defined as

$$D_{c,t}^k = \begin{cases} \sum_{s=-\infty}^{-4} d_{c,t-s} & \text{if } k = -4 \\ d_{c,t-j} & \text{if } -4 < k < 10 \\ \sum_{s=10}^{\infty} d_{c,t-s} & \text{if } k = 10. \end{cases} \quad (2)$$

The event study design enables us to test for flat pre-trends ($k \leq -1$) and informs about the adjustment paths of the post-treatment effect ($k \geq 0$). All other estimates are to be interpreted relative to the pre-treatment period $k = -1$, whose coefficient is normalized to zero. In some specifications, we additionally include time-varying control variables at the county-level $X_{c,t}$. Our specifications include plant and county fixed effects γ_c and δ_i as well as state-by-year fixed effects $\psi_{s,t}$ to absorb state-specific shocks.¹³ This is important because state governments play a role in granting the subsidy and we see regional clustering of the intensity of subsidy rate cuts (see Section 2). Standard errors are clustered at the county level in our baseline specification.

Accounting for Treatment Intensity. Table 1 showed that there is variation in the subsidy rate cuts over time and across counties and plant types as the reforms differentially affected maximum subsidy rates for different plant sizes. To exploit this variation, we define treatment intensity $I_{c,t}^k$ of county c , year t and lead/lag $k \in [-4, 10]$ as

$$I_{c,t}^k = \Delta s_{c,t-k}^{small} \omega_c^{small} + \Delta s_{c,t-k}^{med} \omega_c^{med} + \Delta s_{c,t-k}^{large} \omega_c^{large}. \quad (3)$$

The intensity measure is a weighted average of the (absolute) change in maximum subsidy rate $\Delta s_{c,t-k}^p = |s_{c,t-k}^p - s_{c,t-k-1}^p|$ across plant types, $p \in [small, med, large]$. Respective weights are denoted by ω_c^p and defined as the manufacturing employment share of plants of size p in county c

$$\omega_c^p = \frac{E_{c,1995}^p}{E_{c,1995}^{small} + E_{c,1995}^{med} + E_{c,1995}^{large}} \quad \forall f \in [small, med, large].$$

where $E_{c,t}^f$ denotes the number of workers in manufacturing plants of size f in county c at time t . Weights ω_c^p are time-invariant and calculated in the data year 1995, hence before the first reform.

¹³ The county fixed effects are mostly absorbed by the plant fixed effects. We show in a robustness check that excluding them does not change our results.

Ideally, one would like to use investment weights instead of employment weights since the policy is an investment subsidy. However, the most comprehensive micro data set including investments, the AFiD, only covers establishments with 20 or more employees. Therefore, the weight given to small establishments (those with 50 or less employees) would be systematically downward biased since the majority of establishments has less than 20 employees. To make sure that employment weights are a good substitute, we use the AFiD data to calculate the employment and investment shares as well as their correlation for establishments with between 20 and 50 employees (correlation of 0.82), between 51 and 250 employees (correlation of 0.88) and more than 250 employees (correlation of 0.88). We think it is a reasonable assumption that these very high correlations also extend to establishments with less than 20 employees for which we cannot observe investment.¹⁴

Based on these definitions, the generalized event study design that accounts for the different treatment intensities is given by:

$$\ln y_{i,t} = \sum_{k=-4}^{10} \beta^k \left[D_{c,t}^k \cdot I_{c,t}^k \right] + \xi X_{c,t} + \delta_i + \gamma_c + \psi_{st} + \varepsilon_{i,t} \quad (4)$$

Compared to the basic model given in equation (1), this variant of the event study replaces the dummy treatment indicator with an indicator that is specific to the event. As shown in Schmidheiny and Siegloch (2020), event studies – just as the numerically equivalent distributed lag models – can be easily generalized to account for multiple changes of different intensities if treatment effects are homogeneous over time.

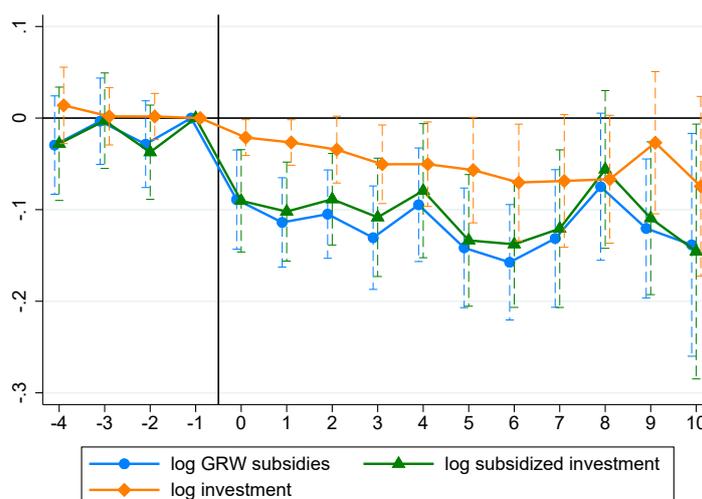
Improving Comparability. Given our institutional setup, we can further improve the comparability of the treatment and control group. Using the cut-off between high- and low-priority counties and the resulting discontinuity in subsidy rates, we can restrict the sample to counties close to the cut-off. Denote $\mathbb{T}^{M,R}$ ($\mathbb{C}^{M,R}$) the set of the M counties closest to the performance cut-off from below (above) following the indicator for regime R . Let $\mathbb{S}^{M,R} = \mathbb{T}^{M,R} \cup \mathbb{C}^{M,R}$ be the set of $2M$ counties around the cut-off during regime R . As we look at multiple regimes and counties might move toward and away from the regime-specific thresholds, we define the set \mathbb{S}^M that includes all counties that are at least once within the set of counties close to the threshold: $\mathbb{S}^M = \bigcup_R \mathbb{S}^{M,R}$. We can then refine our empirical model in equation (4) by restricting the underlying estimation sample to counties in \mathbb{S}^M :

$$\ln y_{i,t} |_{\mathbb{S}^M} = \sum_{k=-4}^{10} \beta^k \left[D_{c,t}^k \cdot I_{c,t}^k \right] + \xi X_{c,t} + \delta_i + \gamma_c + \psi_{s,t} + \varepsilon_{i,t}. \quad (5)$$

In our preferred baseline model, we choose $M = 30$. We also vary M by reducing it or increasing to capture the full sample and find that results (pre and post-treatment effects) do not change in a meaningful way lending credibility to our identification strategy.

¹⁴ Additionally, we use the investment weights in a robustness check even though they underestimate the share of small establishments.

Figure 2: Event study estimates: subsidies and investment



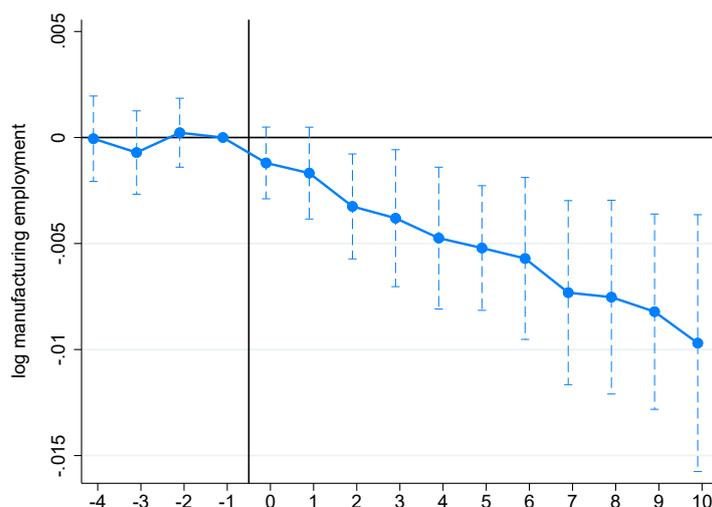
Source: Federal Ministry for Economic Affairs, AFiD Notes: This figure plots coefficients along with 95% confidence intervals of a regression of changes in log subsidies paid to counties, log subsidized investment and log investment on leads and lags of a change in the maximum assistance rate as in equation (5). The sample includes the 55 counties closest to cutoffs ($M=30$). Standard errors are clustered at the county level. See Appendix Tables C.1 and C.2 for the point estimates.

4.2 Results: Direct Policy Effects

Investment Effects. First, we assess whether cuts of the maximum subsidy rates affect the subsidies paid out, that is, we test our first stage. Figure 2 shows the effect of a one-percentage-point decrease in the maximum subsidy rate on GRW subsidies at the county level. We find that a subsidy rate cut in treated counties decreases subsidy amounts by 13.8% after ten years which corresponds to a decrease of €2.5 million for the average county. As a consequence, the volume of investment projects that are subsidized decreases in a very similar manner. Reassuringly, treatment and control groups exhibit a very similar development before a reform for both variables as revealed by the pre-treatment trends. Last and most importantly, we are interested in the effects of subsidy rate cuts on total investments by plants. Using the AFiD data, we show that overall investment decreases by roughly 7.4% after ten years. The investment response is almost exclusively driven by investment in equipment, which makes up about 85% of all investment (see Appendix Figure C.1). Note that it is difficult to make a statement about possible crowding-out of private investment because of two reasons. First, there might be positive or negative spillovers on untreated manufacturing firms, which are reflected in the AFiD estimates, but not in the effect on subsidized investments. Second, the AFiD data only contain plants with 20 or more employees, which might react more strongly to subsidy cuts. This might be due to smaller firms being more financially constrained, which amplifies the cost reduction of the subsidies (Criscuolo et al., 2019). We will test for heterogeneous effects by firm size in our employment data and find support for the hypothesis that smaller plants react more strongly.

Employment Effects. We now move to our main outcome, the employment effect of the GRW policy. For this purpose, we employ the BHP data set, which is a 50% sample of all East German plants. Consistent with the finding of a decrease in investments, Figure 3 shows that cuts in the subsidy rate significantly reduce plant-level manufacturing employment. While pre-trends are flat, our estimates imply that a one-percentage-point decrease in the maximum subsidy rate leads to a

Figure 3: Event study estimates: plant-level manufacturing employment



Source: BHP. Notes: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate as in equation (5). The sample includes the 55 counties closest to cutoffs ($M=30$). Standard errors are clustered at the county level. See Appendix Table C.3 for the point estimates.

decrease in manufacturing employment of 1% after ten years for our baseline sample.¹⁵ To further investigate how our investment results from the AFiD data, which only include plants with 20 or more employees, relate to the employment effect in the BHP, where we observe all plants, we estimate heterogeneous effects by plant size. We define small plants as plants with less than 20 employees in 1996 and large plants as plants with 20 or more employees in 1996.¹⁶ Appendix Figure A.2 shows both the results of our baseline model on the restricted data set, which are very similar to our baseline results, and the results of a fully interacted model.¹⁷ We find that the employment effect is about twice as large for small plants as for large plants and can reject equality in a joint test of the post-reform effects (p -value = 0.037). These results are both consistent with the quantitatively small investment effect we find for large plants in the AFiD data. Furthermore, we find that the decrease in employment is mostly driven by medium-skilled workers, which make up 80% of all manufacturing workers, whereas low- and high-skill employment decreases to a lesser extent (see Appendix Figure A.3). Thus, these results do not speak in favor of human capital spillovers playing a major role in the context of the GRW subsidy, which targets mainly German manufacturing firms (Glaeser and Gottlieb, 2008, Diamond, 2016).

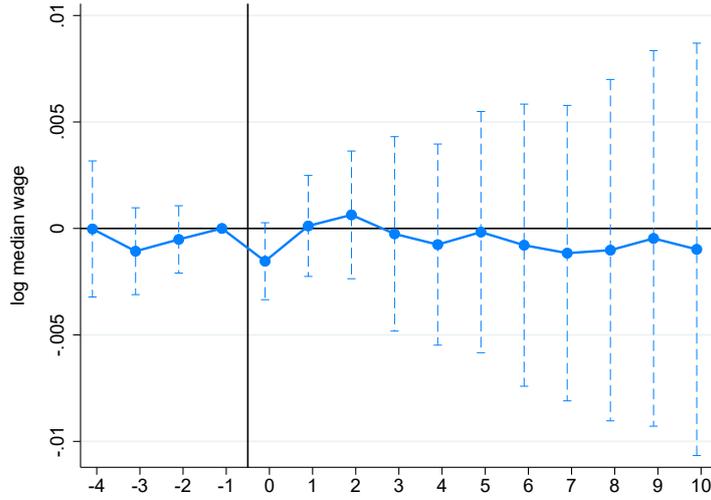
Since the negative effect on manufacturing employment at the plant level only reflects adjustments at the intensive margin, we also look at the number of manufacturing plants on the county level. Appendix Figure A.4 shows that there is little evidence for any effects on the extensive margin. Accordingly, the negative effect on total manufacturing employment at the county level in Figure A.4 is quantitatively very similar to the plant-level effect in Figure 3.

¹⁵ For each regime, we pick the 30 counties which are closest to the cut-off from below and the 30 counties that are closest from above. Aggregating over regimes, we end up with 55 counties that are at least once close to the cut-off. In some years, less than 30 counties are above the threshold, which is why the number of counties is below 60.

¹⁶ We condition on firm size in 1996 since employment is endogenous to the subsidy cuts. Doing so reduces our sample by about one-third.

¹⁷ We adjust for the differential subsidy rates by plant size and include plant size \times state \times year fixed effects.

Figure 4: Event study estimates: median manufacturing wages



Source: SIAB. Notes: This figure plots coefficients along with 95% confidence intervals of a regression of changes in log median manufacturing wages on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs ($M=30$). Standard errors are clustered at the county level. See Appendix Table C.9 for the point estimates.

Wage Effects. Last, the decrease in labor demand could lead to decreasing wages in the manufacturing sector. Using the SIAB data, we calculate the median wage of workers in the manufacturing sector at the county level. We use the median wage as around 13% of wages are top-coded in the SIAB data. As Figure 4 shows, wages are virtually unaffected by subsidy cuts. Also, when differentiating by skill, wages for all skill groups are largely unaffected (see Appendix Figure C.2). Overall wages do not respond significantly to subsidy cuts either (see Appendix Figure C.3a). Results are similar when using average wages instead of median wages (see Appendix Figure C.3b).

4.3 Quantifying the results

In this subsection, we compare our baseline estimate of the investment and direct employment effect with other estimates from the literature. In a later section, we also calculate the costs per job created after taking into account the spillover effects of the GRW (see Section 6.1).

First, we compute the investment elasticity with respect to the user cost of capital. Following Yagan (2015), the elasticity of investment with regard to the user cost of capital is defined as the quotient between ϵ_s^{Inv} , the elasticity of investment with respect to the subsidy s , and, ϵ_s^{UCC} , the elasticity of the user cost of capital with respect to the subsidy.

We calculate the elasticity only for large establishments, i.e. those with 20 more employees, since we do not observe investment for smaller establishments. In the German context, the user cost of capital is defined as follows: $UCC = p \left[\frac{(1-s)(1-z\tau)}{1-\tau} \right] + (1-p) \left[\frac{(1-z\tau)}{1-\tau} \right]$, where p is the probability that the subsidy is granted, τ is the corporate tax rate and z is the present value of one euro of investment deductions.¹⁸ Next, we derive the elasticity of the user cost of capital with respect to the subsidy as $\epsilon_s^{UCC} = \frac{dUCC}{ds} \frac{s}{UCC} = -\frac{ps}{1-ps}$. Using data on the share of large establishments receiving the subsidy ($p = 0.16$) from the IAB establishment survey and the average subsidy level for large establishments

¹⁸ We focus on the relevant policy component of the user cost of capital just as Zwick and Mahon (2017) or Chen et al. (2022) do, omitting the interest rate and a measure of economic depreciation. Including those would not change the elasticity.

($s = 0.39$), we calculate ϵ_s^{UCC} to be -0.067 . We derive the long-run elasticity of the investment effect with respect to the subsidy rate by multiplying the semi-elasticity from Section 4 with the average subsidy level for large establishments, which yields $\epsilon_s^{Inv} = \frac{dInv}{ds} \frac{1}{Inv} \cdot s = 0.074 \cdot 0.39 = 0.029$. Therefore, the investment elasticity with respect to the user cost of capital is given by

$$\epsilon_{UCC}^{Inv} = \frac{\epsilon_s^{Inv}}{\epsilon_s^{UCC}} = \frac{0.029}{-0.067} = -0.433. \quad (6)$$

Our estimate of the investment elasticity with respect to the user cost of capital is at the lower end of the range suggested by Hassett and Hubbard (2002). More recent studies have shown that elasticities depend strongly on the institutional context and which agents are affected. Yagan (2015) shows a zero response of investments to the U.S. capital gains tax reform of 2003. In contrast, Moon (2022) finds an elasticity of -1.84 when studying capital gains taxes in Korea. Zwick and Mahon (2017) study the effect of accelerated depreciation during recessions and reports an investment elasticity with respect to the tax component of the user cost of capital of -1.60 . Ohrn (2019) finds similar user cost of capital elasticities studying accelerated deduction policies as well. Overall, our elasticity estimate is rather low. A likely reason is that we had to exclude small plants when estimating the investment responses due to data availability. We show above that these smaller firms show much larger employment responses. This is in line with the results by Zwick and Mahon (2017) who demonstrate that larger firms show significantly smaller investment responses with elasticities well below -1 . An additional reason for the low response might be that the subsidized firms are located in regions with relatively low productivity, hence investment responses might be lower in general. Last, the GRW policy imposes restrictions on the investment to qualify for the subsidy. It seems possible that these restrictions force firms to conduct not the most efficient investment project.

We also benchmark the labor market effects against other policies studied in the literature. Our estimate shows that a one-percentage-point decrease in the regional subsidy leads to a 1% decrease in manufacturing employment after ten years. This estimate is quantitatively very similar to the main finding of Criscuolo et al. (2019) who study a similar policy in the UK. In terms of wage effects, they also find no significant impact. We can also compare the effect of the place-based policy to results from studies that investigate the labor market effects of local business taxes. In the German context, Fuest et al. (2018) report significantly negative wage effects following local business tax increases, in particular for low-skilled workers. Siegloch (2018) shows that local business taxes also lead to decreases in employment – in particular for those labor market groups that have not seen a decrease in wages such as high-skilled workers. It seems that the GRW policy design, which forced firms to adjust through the employment margin, leads to negligible wage effects.

4.4 Identification and Sensitivity Checks

In the following, we present various tests demonstrating the robustness of our main results.

Improving Comparability. First, our baseline specification improves the comparability of treatment and control group counties by focusing on the jurisdictions that are close to the eligibility cut-off that determines treatment status. Our preferred specification uses 55 counties around the cut-off per regime. This is clearly an arbitrary choice trading off comparability and statistical power. Appendix

Figure C.4a presents results for different cut-off samples including the full sample. The magnitude of the employment effect is hardly affected as we vary the number of counties around the cutoff.

Controlling for Observables. Next, we add control variables that pick up local business cycle fluctuations (and consequently affected treatment status via the eligibility indicator). We control for log GDP per capita and the unemployment rate lagged by one year. This specification tries to account for remaining differences in past economic performance and thereby purifies our estimates from potential bias. Reassuringly, estimates are hardly affected and as expected, if anything, slightly more negative, as demonstrated in Appendix Figure C.4b. Importantly, we do not find significant pre-trends when using log GDP per capita or unemployment as an outcome (see Appendix Figures A.5 and A.6). We also add 3-digit industry times year fixed effects to test whether our results are driven by differential industry trends. As Appendix Figure C.5 shows, this hardly changes our results.

Other Subsidies. As discussed in Section 2, we test whether changes in the GRW subsidy rate have triggered changes in other regional subsidy programs, which could in turn bias our estimates. We test for this possibility by looking at the effect of GRW subsidy cuts on the sum of other subsidies received. Appendix Figure C.6 shows that reforms did not have a significant effect on other subsidies received by municipalities. Moreover, we check whether other prominent firm-level or regional subsidies targeted at East Germany confound our results. In particular, there was the investment tax credit program (*Investitionszulagengesetz*) and the special depreciation allowance (*Fördergebietgesetz*) as well as the EU structural funds. In Appendix B.3, we describe these policies in more detail and show that these other subsidy programs do not confound our effects.

Symmetry. The majority of subsidy rate changes are decreases. However, the reform in 2007 in which all East German counties were assigned high-priority status, led to an increase in subsidy rates for roughly half of the East German counties (see Section 2.2). Therefore, we can estimate a model that allows for different effects of subsidy increases and decreases. Appendix Figure A.7 shows a symmetric pattern. We can not reject the joint null hypothesis that the post-reform effects are asymmetric (p-value = 0.279).

Heterogeneous Treatment Effects. When treatment effects are homogeneous across cohorts, applying an event study with multiple treatments of different intensities produces unbiased estimates of the treatment effect (Schmidheiny and Siegloch, 2020). However, there has been a recent important literature emphasizing that (static and dynamic) difference-in-difference designs with differential treatment timing estimated with a two-way fixed effect model can be severely biased in the presence of heterogeneous treatment effects (de Chaisemartin and D'Haultfoeuille, 2020, 2022, Callaway and Sant'Anna, 2021, Sun and Abraham, 2021). Several new estimators have been proposed to get unbiased estimates when treatment effects are not homogeneous. However, these estimators are not valid for environments with multiple events for the same unit. To test for potential biases due to heterogeneous treatment effects, we cut our sample in 2006 to have a set-up with a maximum of one treatment per county and retain a group of never-treated units. (see Table 1). We apply the

estimators developed in de Chaisemartin and D’Haultfoeuille (2022) and Sun and Abraham (2021) to our basic dummy variable specification described in equation (1).¹⁹ Notice that the two estimators use different control groups since Sun and Abraham (2021) only allow comparisons to never-treated units, whereas de Chaisemartin and D’Haultfoeuille (2022) are also using not-yet treated units as controls. We find that our estimates are unlikely to be driven by heterogeneous treatment effects. To ensure comparability across specifications, we also estimate equation (1) as a standard event study on the same sample. We plot the resulting estimates and their standard errors in Appendix Figure A.8. The effects are very close both in size and pattern to our baseline event study estimates. We conclude that heterogeneous treatment effects are unlikely to drive our results.

Sensitivity to Modeling Choices. Last, we provide a set of checks that assess the sensitivity of our findings to the modeling choices we make in our baseline specification. First, we test whether implementing a standard event study design using a discrete treatment indicator following equation (1) yields similar results. As Appendix Figure C.7a shows, results are very similar when comparing our baseline model and the dummy-variable specification scaled by the average cut. This implies that the effect we measure has a linear relationship to the subsidy rate. Second, we use investment weights instead of employment weights as discussed in Section 4.1. This yields very similar, but slightly smaller estimates (see Appendix Figure C.7b) which is to be expected since the investment data does not cover establishments with less than 20 employees, which are driving our results. Third, recall that due to changes in county border definitions, in some counties only a subset of municipalities receives a decrease in the maximum rate, effectively reducing treatment intensity. Dropping these few partially treated counties yields larger effects, suggesting that our baseline estimate is conservative (see Appendix Figure C.8). Fourth, we vary the number of lags of our event window between nine and eleven years. As Appendix Figure C.9a shows, the effects tend to level off after ten years. Also, when increasing the number of leads included in the model up to eight, the pre-trend remains flat (see Appendix Figure C.9b). Even when pre-trends are insignificant, the post-treatment estimates still might be biased (Roth, 2022). We linearly extrapolate our pre-trends and show that these can not explain our results (see Appendix Figure C.10). Fifth, our results are also robust when estimated in first differences (see Appendix Figure C.11a) or when dropping the county fixed effects (see Appendix Figure C.11b). Last, we conduct several robustness checks regarding inference. Clustering additionally at the plant level or the higher local labor market level as well as allowing for spatial correlation hardly changes standard errors (see Appendix Table C.26).

5 Spillover Effects

5.1 Conceptual Deliberations

A special focus of the paper is to estimate spillover effects of place-based policies in a comprehensive way. Our analysis aims to capture all relevant spillovers that have measurable first-order effects. As we want to provide a comprehensive empirical analysis, we abstain from setting up a (structural) model. Such a model would become too complicated given the large number of mechanisms and

¹⁹ Note that in our setup without covariates and with never-treated units, the estimators from Sun and Abraham (2021) and Callaway and Sant’Anna (2021) coincide.

spillovers we investigate. We strive to identify all relevant spillovers, find a way to estimate these spillovers in a reduced-form way, and translate the estimates into a simple efficiency measure (see Section 6 for more details).

In the case of a place-based policy, spillovers can occur either in treated or untreated regions. In treated regions, workers in treated industries are affected directly in terms of employment and wages – these are the direct effect we estimate in Section 4. Spillovers in treated regions occur if workers in untreated industries are affected in terms of employment and/or wages. Spillovers might be positive, e.g. due to local demand effects, or negative, because of sectoral employment shifts between treated and untreated sectors.

All these effects imply that the (real) income of workers will change, which subsequently affects local tax revenues. This could cause local governments to respond to the central policy by changing local policies, such as local tax rates. The effect is theoretically ambiguous. If local politicians want (or are forced) to balance their budget, they might need to increase local tax rates to counteract the loss of tax revenue. On the other hand, local politicians being aware of tax competition might try to compensate firms for the decrease in subsidies by lowering tax rates. In any case, spillover effects on local policy measures might have a feedback effect on the incomes of local agents. Note that we capture those kinds of feedback effects as we look at reduced-form effects on employment, wages, and profits.

In untreated regions, we differentiate between two groups: neighboring and distant regions. Neighboring regions within the same local labor market could be affected positively by the place-based policy through agglomeration effects or negatively, in case employment is shifted across county borders. We capture this by estimating employment effects at the commuting zone level and comparing it to the direct effect in treated counties within commuting zones. We also analyze commuting behavior and changes in county-level population. Even employment in distant regions could be affected by the policy. To induce non-negligible spillovers over longer distances, there needs to be a systematic economic link between treated regions and distant non-treated regions. One important nexus considered in the literature are trade connections (Blouri and Ehrlich, 2020).²⁰ Hence, we test whether distant regions with systematic trade connections with treated regions are affected by the policy.

In terms of general equilibrium effects, there are two potential price effects. First, direct policy effects and spillover effects in treated and neighboring untreated regions could affect the local price level. We test for this channel by investigating whether the overall policy effects are capitalized into housing prices. Second, the price of capital could theoretically be affected. However, given that the GRW only accounts for 0.3% of total investment in Germany, we do not expect the rate of return to change in any meaningful way. We test all the above-discussed spillovers empirically and will present our approach to measuring them.

²⁰ Other possible links over long distances include internal firm networks (Giroud and Mueller, 2019) or banking relationships (Huber, 2018). Since we can only observe employment at the plant level and do not observe banking relationships, we leave these links to further research.

5.2 Testing for Spillovers

Depending on the context, we have to adjust our baseline model, given in equation (5) to assess the role of the spillover.

Testing for Sectoral Spillovers. We test for sectoral spillovers by simply estimating equation (5) using employment in sectors other than manufacturing as an outcome. More specifically, we look at the retail and construction sector which were de jure excluded from receiving GRW subsidies. Hence, we compare the outcomes of de jure excluded sectors in treated counties to the outcomes of firms in the same sectors in non-treated counties, allowing us to pinpoint the spillover effects (see Appendix Table B.6). Among the de jure excluded sectors, we focus on construction and trade, which are the ex-ante more likely candidates for such sectoral spillover. Investments undertaken in the manufacturing sector are likely to lead to more economic activity in the construction sector since a sizable part of the investment is used to build or expand production facilities. The retail sector can plausibly benefit from the increase in employment and therefore overall income which leads to an increase in local demand.

Testing for Regional Spillovers. A cut in subsidies might have spillover effects that go beyond county borders and affect neighboring counties. Theoretically, these spillovers can be positive in the case of agglomeration effects radiating beyond county lines. They may also be negative if economic activities are relocated from control to treatment counties. We test for those kinds of spillovers by moving the analysis to a higher level of aggregation. Explicitly, we follow Criscuolo et al. (2019) and aggregate equation (5) to the level of the local labor market.²¹ The difference between the estimate at the county level and the estimate at the local labor market level indicates regional spillovers.²² In addition, we run a regression, where we include the subsidy rate changes of other counties in the same local labor market in our baseline specification. In the presence of regional spillovers, we would expect changes in the subsidy of other nearby counties to have a significant impact.

To test for spillover of counties outside of the local labor market, we follow two complementary strategies. First, we follow the approach taken by Bruhn (2018), who replaces the time fixed effects with a time polynomial, and therefore, we can include the sum of all subsidy changes outside of the county into our baseline model. To test, whether the time polynomials are sufficient to control for aggregate trends, we re-run our baseline model with time polynomials instead of fixed effects. As Appendix Figure C.13 shows, using a fourth-order polynomial we get estimates very close to our baseline. Second, run models in which we include the subsidy rate changes of all counties within a radius of 100 or 200 kilometers. In these regressions, we assume that regional spillover effects are not present outside of the particular radius and use the counties outside of the radius as a control

²¹ There are 53 local labor markets in East Germany all of which are nested within states. Therefore, using state-by-year fixed effects does not absorb any of the variation that we are interested in this exercise. We also run a robustness check in which we use year fixed effects instead of state-by-year fixed effects (see Appendix Figure C.12).

²² Note that there is variation in subsidy rates across counties within local labor markets. First, counties have different plant size distributions. Second, there were county-level mergers beyond commuting local labor market borders. Third, there were some exceptions in the assignment rules discussed in Section 2, for instance, the special treatment of counties bordering Poland in the late 2010s or due to extreme outlier counties in terms of economic performance within local labor markets. This is important since the aggregation can, by construction, only yield different results if there is sufficient within-local labor-market variation. In our baseline sample, 14% of our variation in the subsidy rate reforms occurs within local labor markets.

group to estimate the spillover effects. Last, we also use population and net commuting numbers as outcomes in our baseline model.

Testing for Trade Spillovers. Given that manufacturing firms in East German counties are part of a larger value chain, we also test for trade spillover. In particular, we test whether manufacturing plants in German counties, including West German counties, that have significant trade exposure to treatment counties also respond to the subsidy cuts. First, we divide the imports (measured in tons per year) that county c imported from treated East German county g by the total imports of c . Equivalently, we calculate the share of exports of county c going to the treated East German county g . We define trade exposure as the sum of the import and export share. For each German county c , we assign the trade exposure-weighted sum of subsidy cuts of all treated East German counties g .

$$\text{trade exposure to subsidy cuts}_{c,t}^l = \sum_{g \neq c} \left[\frac{\text{imports}_{cg}}{\text{total imports}_c} + \frac{\text{exports}_{cg}}{\text{total exports}_c} \right] \left[D_{g,t}^l \cdot I_{g,t}^l \right] \quad (7)$$

where $D_{g,t}^l$ and $I_{g,t}^l$ are defined as above. To test for trade spillovers, we include the trade exposure to subsidy cuts in our baseline model, which we run on the sample of all German counties in our baseline specification.

$$\ln y_{i,t} = \sum_{k=-4}^{10} \beta^k \left[D_{c,t}^k \cdot I_{c,t}^k \right] + \sum_{l=-4}^{10} \beta_{trade}^l \text{trade exposure to subsidy cuts}_{c,t}^l + \delta_i + \gamma_c + \psi_{s,t} + \varepsilon_{i,t} \quad (8)$$

where β_{trade}^l represents the effect on plants with a 1% trade exposure to a one-percentage-point subsidy cut l years ago. Therefore, we compare counties that have a high trade exposure to the treated East German counties to other counties that have a low trade exposure to the treated East German counties. Importantly, we also control for the changes in the treatment status of the East and West German counties themselves. In robustness checks, we differentiate between import and export exposure and restrict the model to only West German counties.

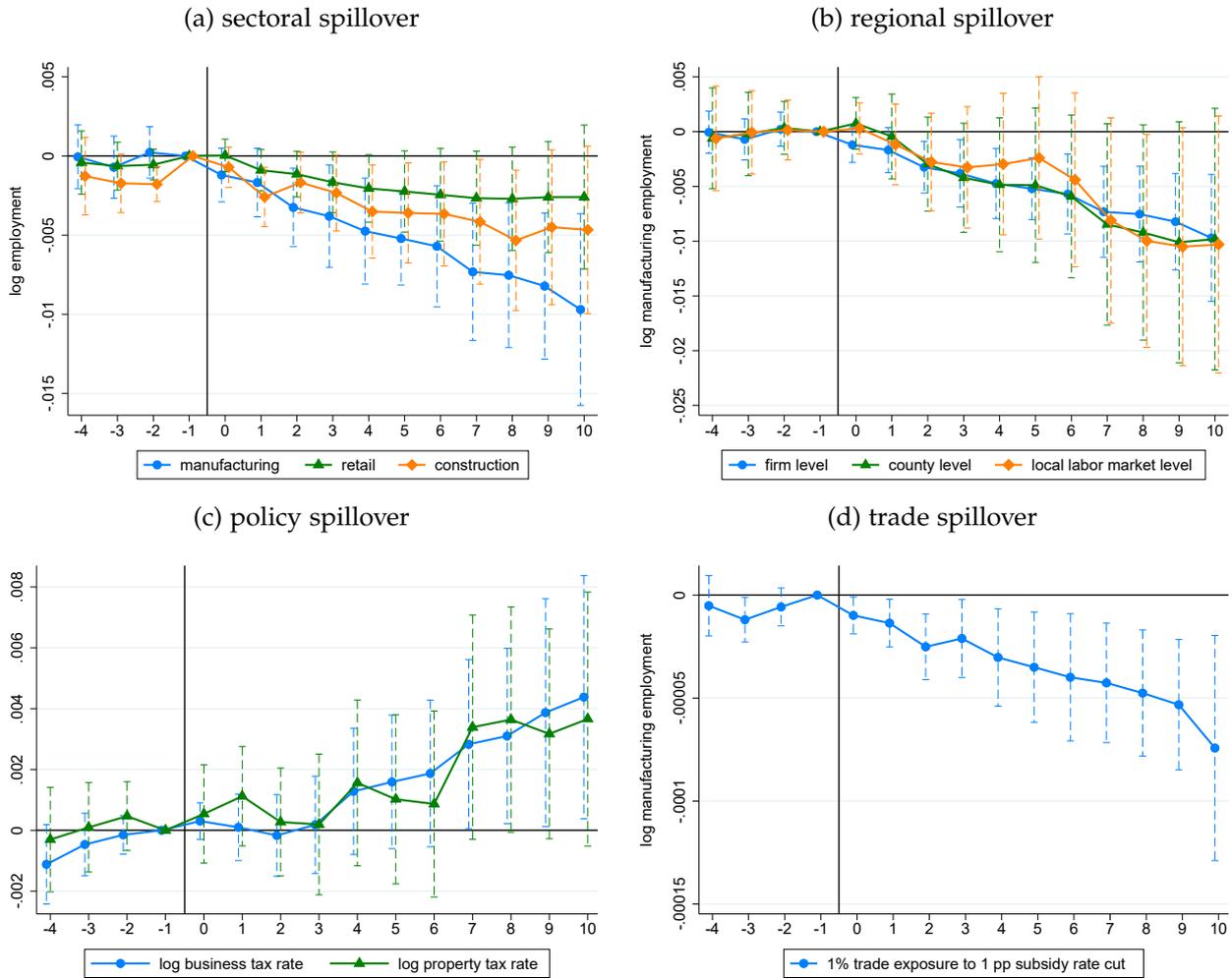
Testing for Policy Spillovers. We test for policy spillovers by using the business and property tax rates as outcomes in the empirical model specified in equation (5). In addition, we use the respective tax revenues and tax bases as outcomes.

5.3 Results: Spillover Effects

Intra-county Spillovers. First, we check whether the place-based policy had an effect on untreated industries in treated counties. Figure 5a shows (positive) spillover effects for the untreated retail and construction sector.²³ A cut in the subsidy rate leads to an immediate decrease in employment in the construction sector, which seems intuitive as we have seen that subsidy cuts trigger an immediate decrease in investment projects like building new or extending production facilities. Likewise, we detect a (smaller) negative effect on retail employment, which could be explained by a decrease in local demand. In total, one job lost in the manufacturing sector leads to 0.47 additional jobs lost in the retail and construction sectors. This estimate is well within the range of estimates in the literature

²³ We define a positive spillover as going in the same direction as the direct policy effect.

Figure 5: Event study estimates: spillover effects



Source: BHP, Statistical Offices of German States Notes: Panel (a) plots coefficients along with 95% confidence intervals of a regression of log industry employment at the plant level on leads and lags of a change in the maximum assistance rate as in equation (5). The sample includes the 55 counties closest to cutoffs ($M=30$). Standard errors are clustered at the county level. See Appendix Table C.27 for the point estimates. Panel (b) plots coefficients along with 95% confidence intervals of a regression of changes in log manufacturing employment on leads and lags of a change in the maximum assistance rate at the county and local labor market level. When aggregating to the local labor market level, treatment intensities of counties are weighted by the number of manufacturing employees. The sample includes the counties or local labor markets that contain the 55 counties closest to cutoffs ($M = 30$). Clustering of standard errors is at the county or local labor market level. See Table C.29 for the point estimates. Panel (c) plots coefficients along with 95% confidence intervals of a regression of changes in the log local business and property tax rates on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs ($M=30$). Standard errors are clustered at the county level. See Appendix Tables C.39 and C.40 for the point estimates. Panel (d) plots coefficients along with 95% confidence intervals of a regression as in equation (8) using log manufacturing employment at the plant level as the outcome. The sample includes all German counties. Standard errors are clustered at the county level. See Appendix Table C.36 for the point estimates.

ranging from 0.4 in Gathmann et al. (2020) using German data to 1.6 in Moretti (2010) for US cities. We also test whether subsidy rate changes are capitalized in house prices and rent per square meter. If an increase in the subsidy rate would lead to increased house prices, the distributional impact of the policy would change with (pre-existing) homeowners being the main beneficiaries. As Appendix Figure A.9 shows, we do not find any effect on either house prices or rent per square meter.

Regional Spillovers. Next, we test whether negative manufacturing employment effects in treated counties spread across county borders within the local labor market. We aggregate county-level manufacturing employment to the local labor market level and use the weighted average of counties' treatment intensities to re-estimate equation (4) on the baseline sample. Figure 5b shows that the treatment effect on manufacturing employment at the labor market level is very similar to our baseline at the plant and county levels.²⁴ Alternatively, we include the average subsidy rate change of other counties in the same local labor market in our baseline specification. As Appendix Figure A.10 shows, the baseline effect is robust to this inclusion and there is no effect of the treatment of neighboring counties. This implies that there was little reallocation of workers across counties within local labor markets. This is consistent with the null effects on the net commuting flow per employee and population we find (see Appendix Figure A.11). Last, we also test for crowding out to counties outside of the local labor market. First, we estimate the effect of subsidy changes in other counties using the approach by Bruhn (2018). Subsidy changes in other counties do not significantly affect employment in the county itself and the direct effect is unchanged (see Appendix Figure C.14). Second, we include changes to the subsidy rate in counties within a 100- or 200-kilometer radius of the county itself into our baseline model. As Appendix Figures C.15a and C.15b show, there is no evidence for regional spillover to counties up to 200 kilometers away.

Trade Spillovers. We assess whether cuts in the GRW affected untreated East and West German counties that were connected to treated East German counties via trade flows using the empirical model specified in equation (8). Figure 5d shows that a 1% trade exposure to a one-percentage-point decrease in the subsidy rate reduces manufacturing employment by 0.0074% ten years after the subsidy cut. We find a similar effect when restricting the sample to only West German counties (see Appendix Figure C.16a). These findings are consistent with the effect of the subsidies propagating through the value chain and thereby also affecting untreated counties with higher levels of trade exposure to treated counties. Broken trade relationships do not seem to play an important role since we do not observe any effect at the extensive margin (see Appendix Figure A.4). Next, we show that both import and export exposure play an equally important role and are not statistically different from each other (see Appendix Figure C.16b). Therefore, firms in untreated counties that had a significant trade exposure to treated counties experience both a supply and demand shock. These results imply that the trade substitutability of firms is quite low since there should not be any employment effects if firms could switch to other trade partners without any frictions.

Policy Spillovers. We also test for the possibility of policy spillovers. Since a subsidy cut negatively impacts local employment, municipal finances are also affected. Figure 5c shows that both local

²⁴ The same result holds if the state-by-year fixed effects are replaced by simple year fixed effects (see Appendix Figure C.12).

business and property tax rates are raised in response to a cut in the maximum subsidy rate. This finding is not surprising in the context of German municipalities, which are not very flexible in adjusting their expenditures (Löffler and Siegloch, 2021).

Our results on policy spillovers imply that businesses in treatment counties not only receive a subsidy cut but also face higher business and property tax rates. Local tax revenues from property taxation increase slightly, whereas business tax revenues decrease (see Appendix Figure C.17a). The latter effect implies a shrinking business tax base. As we do not see any effects on the number of plants, the most plausible answer is that firm profits decrease (see Appendix Figure C.17b).

Overall, the observed policy spillovers are one of the factors that contribute to the continuous negative employment effect that we document in Figure 3. Using the employment elasticity of the German local business tax rate from Siegloch (2018), we can attribute about 10% of the overall employment effect to the increase in the local business tax.²⁵ These results are similar to Ehrlich and Seidel (2018), who study a different place-based policy in a different context. While they find that local infrastructure improves, we demonstrate a related, but slightly different channel going through business tax rates, which reinforces the subsidy effect.

5.4 Taking Stock: Overall (Un)employment Effects

As discussed in Section 2, the rationale of the GRW policy was to stimulate the export-oriented manufacturing sector and thereby push the entire local economy. Figure 3 shows that the manufacturing sector, which accounts for 18% of total employment, is responding as intended, with employment decreasing by 1% after ten years. In Figure 5a, we document spillover effects of the subsidy on some untreated sectors. These findings translate into a modest effect on total employment, which decreases by 0.2% (see Appendix Figure A.12). GDP per capita at the county-level declines in a similar vein (see Appendix Figure A.5). A central question for the effectiveness of the policy is what happens with the jobs that were lost due to the cut in the subsidy. Theoretically, people could transition to a new job in or outside of the county, become unemployed or withdraw from the labor force. In line with the effect on employment, we detect that the number of unemployed increases by about 0.5%, however, estimates are imprecise (see Appendix Figure A.6). We do not find an effect on the size of the labor force (see Appendix Figure A.6). Taking the point estimates of the direct employment effect, the local multiplier effect and the unemployment effect at face value, more than 95% of lost jobs translate into higher unemployment. This is consistent with the null effects on both commuting flows and population numbers as well as the lack of regional spillover.

6 Discussion: Efficiency and Inequality Effects

In this section, we provide an efficiency analysis of the GRW policy by assessing its efficiency and redistributive implications.

²⁵ We calculate the share of the employment effect due to the local business tax as the product of the increase in the local business tax and the employment elasticity of the German local business tax rate taken from Siegloch (2018) divided by the overall employment effect: $\frac{0.004 \cdot (-0.263)}{-0.010} = 0.1052$. Similarly, using the wage elasticity of Fuest et al. (2018), the increase in the local business tax rate can explain all of the small and insignificant decrease in the wage we observe in Figure 4.

6.1 Efficiency Assessment

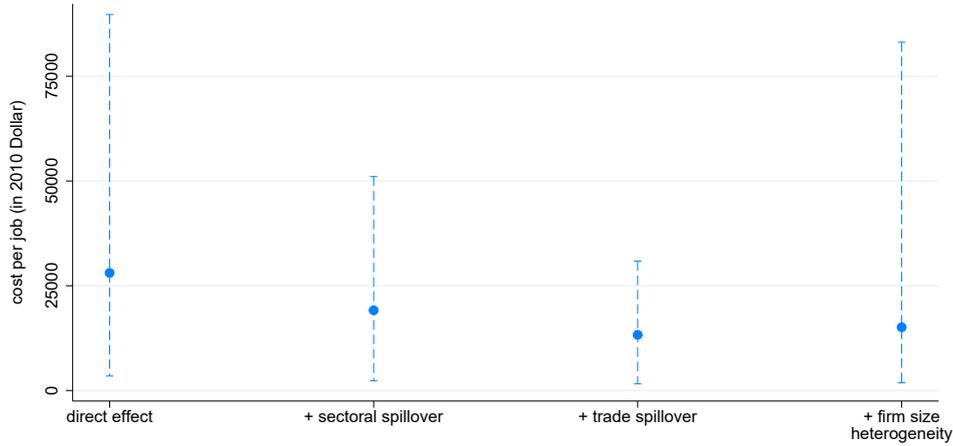
To assess the efficiency of the GRW, we calculate the cost per job created. This measure is easy to interpret and can be used to compare the GRW to other place-based policies evaluated in the literature. We use our empirical estimates to calculate both the number of jobs created and the direct program costs due to a one-percentage-point increase in the subsidy rate for all East German counties. In these calculations, we will always use the last point estimate from the event study regressions, corresponding to 10 years after the reform, in order to measure the long-term impact of the GRW.

The cost of the subsidy change is computed as the product of the last point estimate of the effect on subsidies paid out in Figure 2 and the average level of GRW subsidies paid throughout our sample period. In order to compare our results to the literature, we convert all monetary variables into \$ using the average yearly exchange rate and normalize with the CPI to 2010 \$. This yields a direct cost of €194 million or \$220 million.

The number of jobs created comprises the directly lost manufacturing jobs in the treated counties as well as the jobs lost due to both sectoral and trade spillover. Since we do not find evidence for regional spillover, we use our plant-level estimates to calculate the number of manufacturing jobs created. We multiply the long-term estimate of an about 1% increase with the average number of manufacturing jobs in East Germany in our sample period yielding 7843 new manufacturing jobs. As Figure 6 shows, when only considering the direct effect of the policy, the cost per job is \$28064 (€24681). Next, using the same method, we calculate that 1659 additional retail jobs and 1996 additional construction jobs are created via sectoral spillover. Adding these jobs, the cost per job is reduced to \$19142 (€16835). In contrast to the sectoral spillover, the trade spillover effect reflects jobs not only in treated East German counties but in the all German counties that have trade exposure to the treated counties. Therefore, we first multiply the trade exposure of the average German county by the treated East German counties with the long-term point estimate in Figure 5d. Next, we multiply with the average number of manufacturing employment outside the county itself in Germany as a whole which gives us 5050 jobs created via trade spillover. Taking these jobs into account drives the cost per job further down to \$13301 (€11698). Furthermore, we account for the heterogeneous effect by plant size by calculating the number of manufacturing jobs for small and large plants separately. Since our effect is mainly driven by small plants, this slightly reduces the number of overall manufacturing jobs created and increases the cost per job to \$15112 (€13290). In a last step, we also take the deadweight cost of taxation into account. We follow Criscuolo et al. (2019) and assume a 50% deadweight cost of taxation, which pushes up the cost per job to \$22667 (€19935).

All in all, this puts the GRW with a cost per job between \$15000 and \$23000 at the lower end of the cost per job estimated in the literature for other (place-based) policies. It is comparable to estimates for the depreciation schemes in Japan, where LaPoint and Sakabe (2022) find a cost per job of \$16000 to \$22222, and in the US, where Garrett et al. (2020) estimate a cost per job of about \$20000. Looking at other place-based policies that are closer to the GRW such as the UK Regional Selective Assistance, which is also a regionally targeted capital subsidy, Criscuolo et al. (2019) find a somewhat higher cost per job at \$26572. This result is more comparable to our estimate without including any spillover, which highlights the importance of accounting for those spillovers. Other place-based policies find even higher cost per job estimates such as the US New Markets Tax Credits at about \$50000 (Freedman, 2012) and the Italian Law 488 between \$40000 and \$70000 (Cerqua and

Figure 6: Cost per job



Source: BHP, Federal Ministry for Economic Affairs, FRED Notes: Confidence intervals are based on 9999 bootstrap draws.

Pellegrini, 2014, Pellegrini and Muccigrosso, 2017).

The relatively low cost per job estimate of the GRW goes hand-in-hand with our set of reduced form results that point to a low deadweight loss of the specific subsidy. In their important work, Busso et al. (2013) show that the deadweight loss of place-based policies depends on potential location distortions of individuals. We show above that the GRW neither affected county-level population nor commuting behavior. In addition, we did not find that sectoral spillovers suggest a substitution pattern – quite the opposite – or that there is regional relocation. Hence, it seems that many individuals are inframarginal in their mobility. This finding is in line with evidence on migration patterns in East Germany after reunification. First, unemployment in East Germany was not a push factor driving individuals into the West (Hunt, 2006). Second, in the late 1990s, when our observation window starts, individuals moving to the West were mostly highly educated, which is a group that is less likely to hold jobs directly or indirectly affected by the GRW (Fuchs-Schündeln and Schündeln, 2009).

6.2 Implications for Regional Inequality

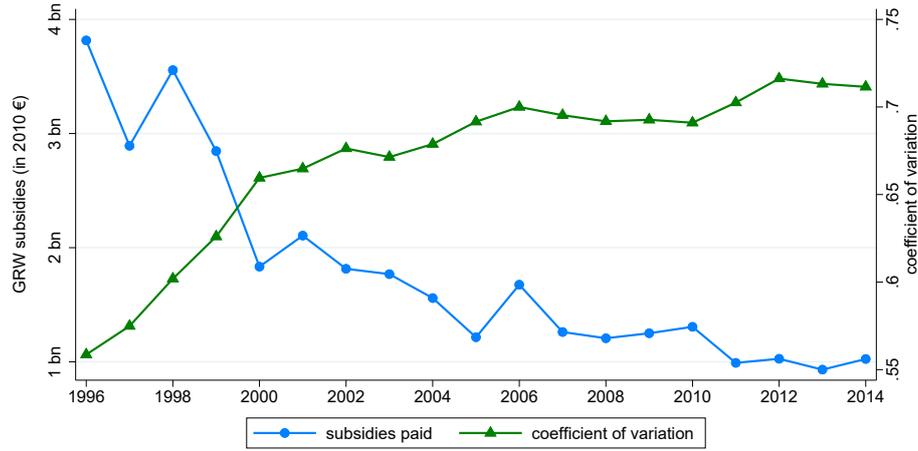
In many countries, place-based policies are also implemented to curb spatial inequality (Neumark and Simpson, 2015). In the context of the GRW, the ultimate goal of equalizing living standards across space is explicitly mentioned in the law. Yet, there is little systematic evidence on the efficacy of place-based policies to actually reduce regional inequality. In this subsection, we investigate the effectiveness of the GRW to achieve its politically stated goal.

Our measure of spatial inequality is the coefficient of variation of county-level post-transfer income as suggested by Ehrlich and Overman (2020). We define post-transfer income per capita as follows:

$$\text{post-transfer income per capita}_{ct} = \frac{1}{N_{ct}} \left[\sum_{s \in S} \text{jobs}_{cst} \cdot \text{wage}_{cst} + \text{unemp}_{ct} \cdot \text{benefit}_t \right] \quad (9)$$

where N_{ct} denotes population in county c and year t . jobs_{cst} and wage_{cst} are the number of

Figure 7: GRW Subsidies and regional inequality over time in Germany



Source: SIAB, BHP, Federal Ministry for Economic Affairs Notes: The coefficient of variation is computed with respect to the post-transfer income per capita as calculated in equation (9).

jobs and the average wage in sector s , with $S \in \{\text{manufacturing, retail, construction, other}\}$.²⁶ unemp_{ct} represents the number of unemployed in county c and year t and benefit_t is the average unemployment benefit paid in year t .

Figure 7 shows that regional inequality has increased from the mid-1990s until recent years. At the same time, the generosity of the GRW as measured by annual spending has been decreasing over time. Clearly, we cannot causally link the decrease in the generosity of the GRW to changes in inequality. Nonetheless, we can use our causal reduced-form estimates derived in Sections 4 and 5 to assess the potential of the GRW to curb the rise in regional inequality.

In order, to do so we simulate the effect of the GRW on regional inequality under various counterfactual scenarios in partial equilibrium. In our baseline, we increase the subsidy rate of the bottom 20% counties (16 counties) in the East German post-transfer income per capita distribution to their 1996 level. This counterfactual reform is affecting a similar number of counties as the reforms we observe in the data. The size of the reform of a 20.8 percentage points increase is somewhat larger than the largest observed reform of 15 percentage points. As we find no evidence for non-linear effects within the observed reforms, this extrapolation seems reasonable.²⁷

In our simulations, we utilize the hat operator to denote counterfactual outcomes computed with our long-run reduced-form estimates and the labor income distribution in 2014. In order to simulate the number of counterfactual jobs created, we compute:

$$\widehat{\text{jobs}}_{cs} = \begin{cases} \text{jobs}_{cs} \cdot (1 + (\hat{\beta}_s \cdot \text{treat}_c \cdot s_c + \hat{\beta}_{\text{trade}} \cdot \text{trade exposure}_c)) & \text{if } s = \text{manufacturing} & (10a) \\ \text{jobs}_{cs} \cdot (1 + (\hat{\beta}_s \cdot \text{treat}_c \cdot s_c)) & \text{if } s = \text{retail, construction} & (10b) \\ \text{jobs}_{cs} & \text{if } s = \text{other} & (10c) \end{cases}$$

where $\hat{\beta}_s$ is the estimate of the long-term reduced-form employment effects in sector s of a one-percentage-point subsidy rate increase. The dummy treat_c equals one if county c is in the bottom 20%

²⁶ We impute wages above the social security ceiling using the procedure of Dauth and Eppelsheimer (2020).

²⁷ In Appendix Figure C.19b, we show the effect of an eight-percentage-points-increase, which corresponds to the average reform in our sample. The result of a 0.0044 reduction in the coefficient of variation is almost linearly scaled down from our baseline: $\frac{8}{20.8} * -0.0112 = -0.0043$.

of the post-transfer income distribution and is multiplied by the counterfactual increase in the subsidy rate s_c . When calculating the counterfactual manufacturing jobs, we also take the estimated trade spillover into account by taking the product of the long-run reduced-form trade spillover estimate $\hat{\beta}_{trade}$ from Figure 5d and the trade exposure of county c to the subsidy changes of the treated counties, which is defined analogously to equation (7) as $\sum_{g \neq c} \left[\frac{\text{imports}_{cg}}{\text{total imports}_c} + \frac{\text{exports}_{cg}}{\text{total exports}_c} \right] [\text{treat}_g \cdot s_g]$.²⁸ Likewise, we compute the counterfactual number of unemployed individuals as

$$\widehat{\text{unemp}}_c = \text{unemp}_c \cdot (1 + (\hat{\beta}_{unemp} \cdot \text{treat}_c \cdot s_c)) \quad (11)$$

where $\hat{\beta}_{unemp}$ is the estimate of long-term reduced-form unemployment effects of a one-percentage-point subsidy rate increase. Since we do not find a significant effect on population and wages, we assume that the policy does not change N_c and wage_{cs} . Plugging the counterfactual job and unemployment numbers into equation (9) yields a counterfactual distribution and dispersion of post-transfer income per capita.

In order to assess the potential of the GRW to curb regional inequality, we compare the spatial dispersion of post-transfer income in 2014 to the counterfactual dispersion. First, we show the effect ignoring spillovers, i.e. setting $\hat{\beta}_{trade} = \hat{\beta}_{retail} = \hat{\beta}_{construction} = 0$, thus only focusing on the direct effect on manufacturing jobs. The first bar in Figure 8 shows that the coefficient of variation in 2014 is reduced by 0.0087 [0.0032, 0.0139].

Adding sectoral spillovers further reduces regional inequality as it accounts for additional jobs created in poor regions (see the second bar in Figure 8). The effect of trade spillovers is ex-ante ambiguous. On the one hand, if poorer regions generally have a higher trade exposure to other poorer regions, it would further reduce regional inequality. On the other hand, if poorer regions are disproportionately connected to richer regions, trade spillovers would dampen the reduction in regional inequality. We find that for East Germany the first case applies, but the impact of trade spillovers is very small in size. Thus, when accounting for all spillovers, the coefficient of variation is significantly reduced by 0.0112 [0.0055, 0.0166]. Compared to the observed increase in spatial inequality of 0.15 in Figure 7, the potential of the GRW to curb regional inequality is limited, but non-negligible. Especially, since the estimated yearly cost of the counterfactual subsidy increased using our long-term estimate of subsidies paid out amounts to only €475 million.

To put the effect in perspective, we show that the inequality-dampening effect of the place-based policy is strong compared to a revenue-neutral place-blind policy. Instead of saving jobs via the GRW, policymakers could use the money to increase welfare benefit payments to the unemployed. In the case of our counterfactual, each unemployed in East Germany would receive an additional yearly transfer of €738.²⁹ Paying out this amount to each unemployed person in East Germany reduces the coefficient of variation by only 0.0012 [0.0002, 0.0023], which is an order of magnitude less than the effect of the GRW reform (see the fourth bar in Figure 8). The difference in the inequality effect between the place-based and the place-blind policy arises because an increase in welfare benefits also raises the income of recipients in relatively rich regions. We also simulate the same counterfactual for the bottom 15% and 25% of the post-transfer income distribution (see Appendix Figures C.18a and

²⁸ We use the most recent trade flow data from 2010 to calculate trade exposure_c.

²⁹ Note that this calculation is an upper bound since we assume that there are no disincentive effects of the increased benefit amount.

Figure 8: Counterfactual regional inequality



Source: BHP, SIAB, Federal Ministry for Economic Affairs, Statistical Offices of German States Notes: The first bar displays the effect of an increase in the GRW subsidy back to 1996 levels for counties in the bottom 20% of the income distribution on regional inequality within East Germany without accounting for any spillover. The second and third bars add trade and sectoral spillover, respectively. The fourth bar displays the effect of a revenue-neutral policy that pays a fixed cash transfer to every unemployed person in East Germany. The p-value refers to a one-sided test of whether the effect of the GRW policy is larger than the effect of cash transfers. Berlin is excluded from East Germany. Confidence intervals are based on 9999 bootstrap draws.

C.18b) and the resulting patterns are very similar. Furthermore, our results also hold when using the gini coefficient as an alternative measure of inequality (see Appendix Figure C.19a).

So far the counterfactual exercises have only been targeted at East German counties. When we apply the same policy experiment to all German counties, the counterfactual GRW policy would reduce the spatial coefficient of variation by 0.0090 [0.0035, 0.0142]. Interestingly, when looking at Germany as a whole, the trade spillover dampen the reduction of regional inequality (see Appendix Figure C.20). Last, we also compute the effect on the gap in earnings per capita between East and West Germany as it was one of the stated goals of the GRW policy to equalize living conditions between the East and West. According to our simulation, the GRW policy would reduce the gap of 24.80% between East and West Germany by 0.79 [0.41, 1.16] percentage points.

7 Conclusions

In this paper, we investigate the direct and spillover effect of regional firm subsidies and assess their efficiency and equity implications. Investigating the case of investment subsidies predominantly paid to manufacturing firms in East Germany after reunification, we exploit substantial variation in maximum subsidy rates for identification. First, we find that the place-based policy has a strong local effect: a cut in the subsidy rate has a sizeable and robust negative effect on local manufacturing employment. A one-percentage-point decrease in the maximum subsidy rate leads to a decrease in manufacturing employment of 1% ten years after the reform. While wages remain unaffected, local unemployment increases. We provide evidence that policy effects are symmetric, such that subsidy rate increases lead to higher levels of manufacturing employment.

In the second step, we go beyond the effect on treated firms in treated counties and investigate various spillover effects. We find evidence for local multiplier effects in the untreated construction and retail sectors, in which employment also drops as a consequence of the reduction in the subsidy. Our

estimates suggest that one lost manufacturing job implies 0.47 jobs lost in the retail and construction sectors. Counties with a high trade exposure to the treated counties also experience a decline in manufacturing employment. In terms of regional spillovers, we do not find any evidence for reallocation of labor within or outside of the local labor market. Last, we find that local policymakers increase the business and property tax in response to subsidy cuts. In light of little to no spatial distortions, we show that the place-based policy is quite efficient with a cost-per-job estimate of between \$15000 and \$23000. Moreover, we show that the place-based policy is favorable in reducing regional inequality compared to place-blind cash transfers.

In light of the increase in regional inequality observed in many developed countries, place-based firm subsidies could play a role to mitigate regions further drifting apart. In this respect, our study adds to a recent set of papers demonstrating the positive welfare effects of place-based policies. Busso et al. (2013) show that the deadweight loss of place-based policies is low if locational distortions are limited. Austin et al. (2018) argue that place-based policies are more targeted. Fajgelbaum and Gaubert (2020) demonstrate that place-based policies can increase spatial efficiency because sorting off high-skilled workers is inefficient. Finally, Gaubert et al. (2021) show that place-based redistribution is favorable compared to place-blind policies like income taxes when society favors spatial equity. Our paper provides further evidence for this case. For Germany, a country where the goal of spatial equity is referred to in the constitution, we show that targeted place-based policies have important spillovers that go beyond traditional agglomeration forces.

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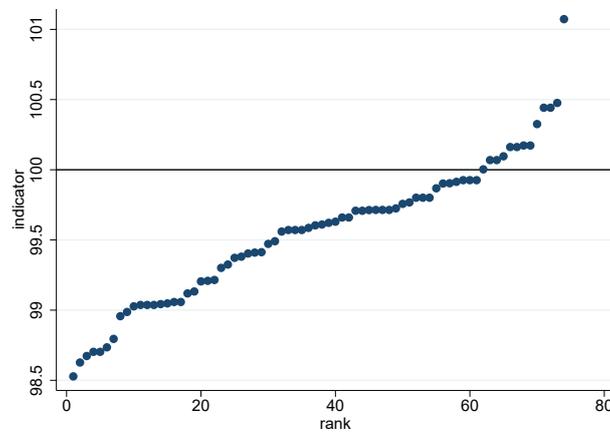
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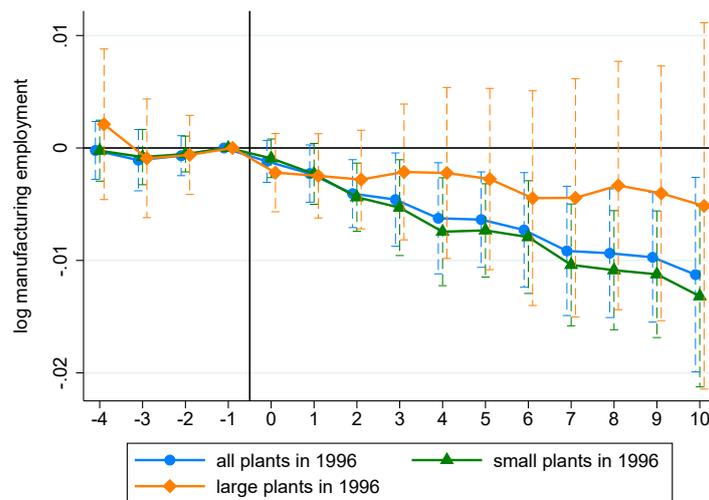
A Additional Results

Figure A.1: Ranking of counties based on the indicator (year 1997)



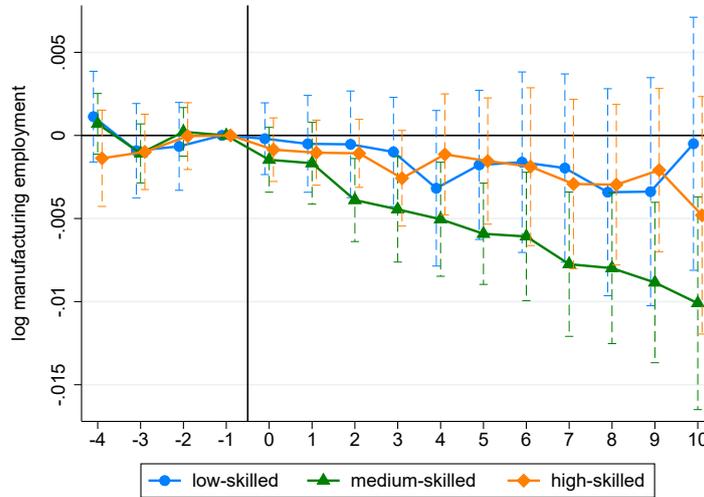
Source: Federal Ministry for Economic Affairs. Notes: This figure plots indicator values and the ranks of counties in the year 1997. The cutoff was formally at indicator value 100.

Figure A.2: Event study estimates: manufacturing employment by firm size



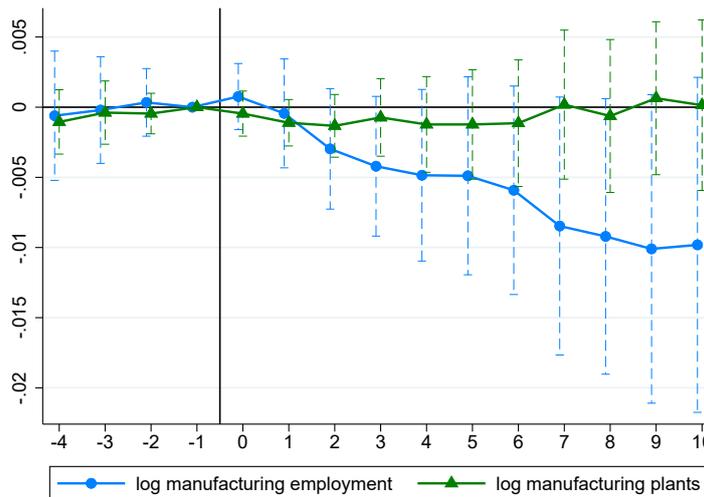
Source: BHP Notes: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment on leads and lags of a (a) change in the maximum assistance rate for the sample of plants which is already present in 1996 (b) change in the maximum assistance rate for small plants interacted with a dummy for small plants in 1996 and of a change in the maximum assistance rate for large plants interacted with a dummy for large plants in 1996. The sample includes the 55 counties closest to cutoffs ($M=30$). Standard errors are clustered at the county level. See Appendix Table C.4 for the point estimates.

Figure A.3: Event study estimates: manufacturing employment by skill



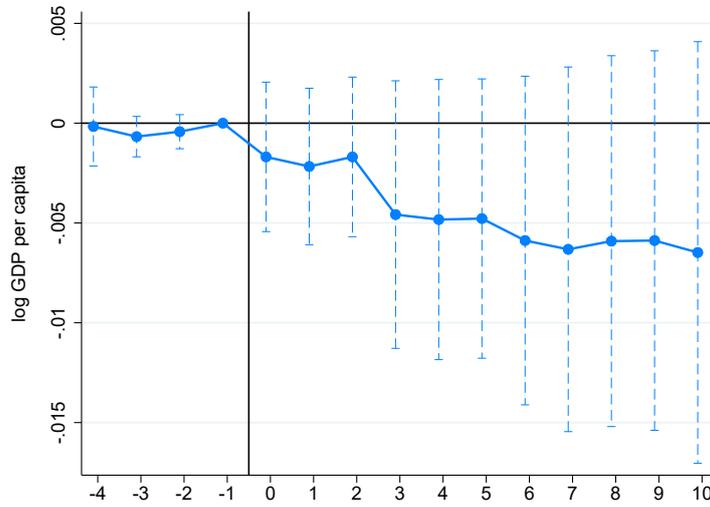
Source: BHP Notes: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment by skill on leads and lags of a change in the maximum assistance rate as in equation (5). The sample includes the 55 counties closest to cutoffs ($M=30$). Standard errors are clustered at the county level. See Appendix Table C.5 for the point estimates.

Figure A.4: Event study estimates: number of manufacturing plants and county-level manufacturing employment



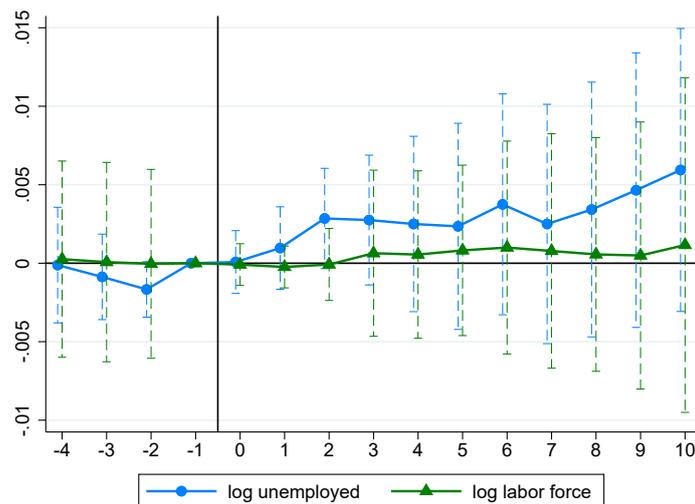
Source: BHP Notes: This figure plots coefficients along with 95% confidence intervals of a regression of changes in the log number of manufacturing plants and log manufacturing employment at the county level on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs ($M=30$). Standard errors are clustered at the county level. See Appendix Table C.6 for the point estimates.

Figure A.5: Event study estimates: GDP per capita



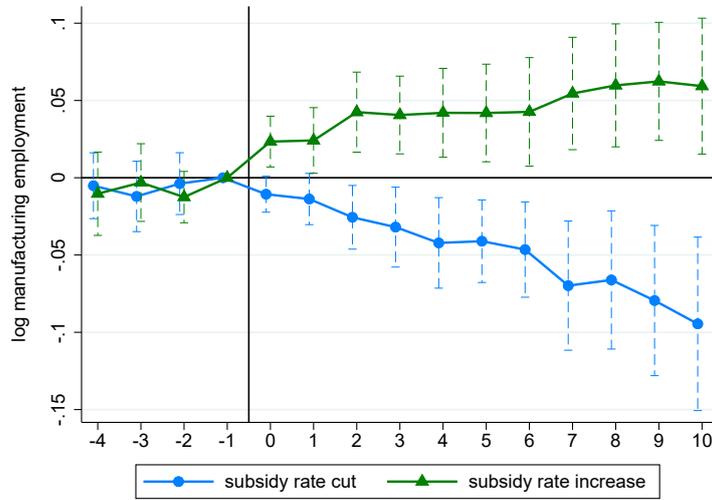
Source: Statistical Offices of German States Notes: This figure plots coefficients along with 95% confidence intervals of a regression of changes in log GDP per capita on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs ($M=30$). Standard errors are clustered at the county level. See Appendix Table C.7 for the point estimates.

Figure A.6: Event study estimates: unemployed and labor force



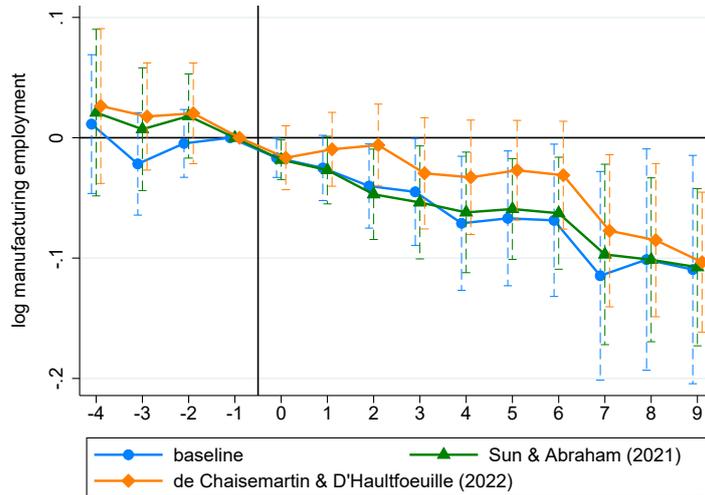
Source: Statistical Offices of German States Notes: This figure plots coefficients along with 95% confidence intervals of a regression of changes in the log unemployed and log labor force on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs ($M=30$). Standard errors are clustered at the county level. See Appendix Table C.8 for the point estimates.

Figure A.7: Event study estimates: manufacturing employment (increases & decreases)



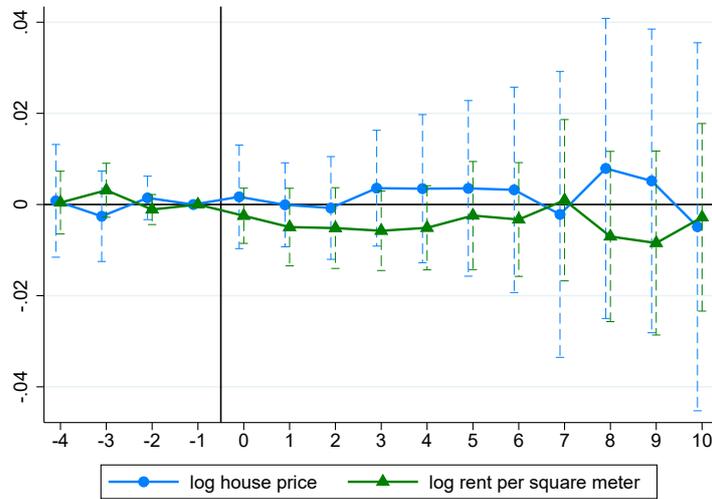
Source: BHP Notes: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate. Treatment is discrete as in equation (1) and we include separate dummies for increases and decreases in the subsidy rate. The sample includes the 55 counties closest to cutoffs ($M=30$). Standard errors are clustered at the county level. See Appendix Table C.17 for the point estimates.

Figure A.8: Event study estimates: heterogeneous treatment effects



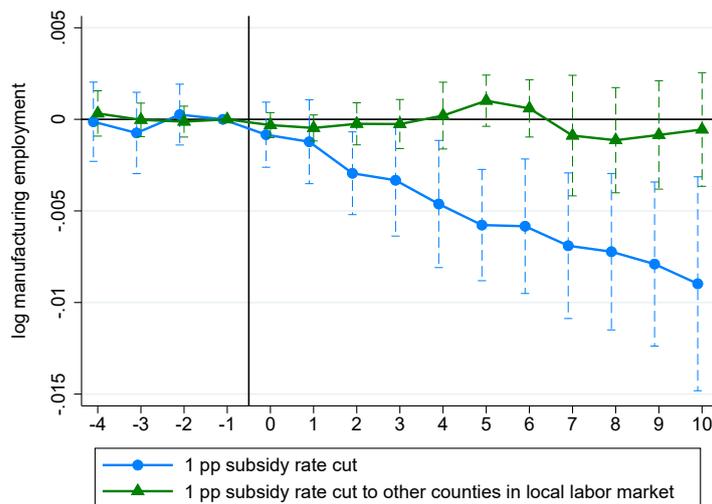
Source: BHP Notes: This figure plots coefficients along with 95% confidence intervals of the methods developed in de Chaisemartin and D'Haultfoeuille (2022) and Sun and Abraham (2021) used on equation (1) with manufacturing employment as the outcome. We limit the sample to the years 1995 to 2006 and drop the Salzlandkreis since it was treated both in 1997 and 2000 for all estimations since in that case we only have one treatment per unit and retain never-treated units. We implement the estimator from Sun and Abraham (2021) using the Stata command `eventstudyinteract`. The estimator from de Chaisemartin and D'Haultfoeuille (2022) is implemented using the Stata command `did_multipleGT` and we obtain standard errors through 999 bootstrap iterations. The sample includes the 55 counties closest to cutoffs ($M=30$). Standard errors are clustered at the county level. See Appendix Table C.18 for the point estimates.

Figure A.9: Event study estimates: house price & rent per square meter



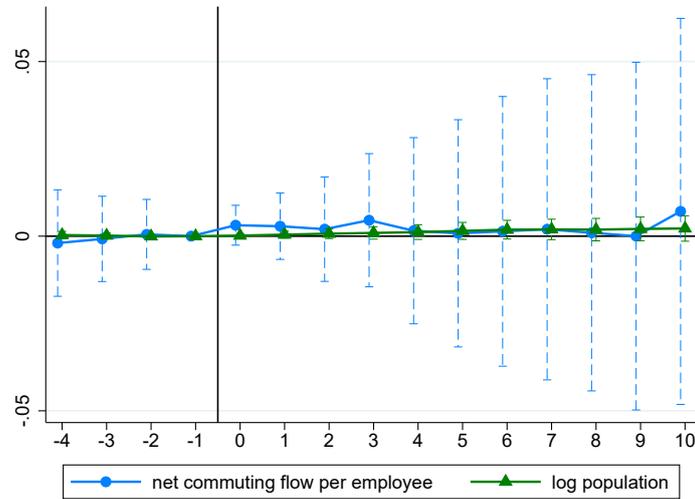
Source: IVD Notes: This figure plots coefficients along with 95% confidence intervals of a regression of changes in the log house price and the log rent per square meter on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs ($M=30$). Standard errors are clustered at the county level. See Appendix Table C.28 for the point estimates.

Figure A.10: Event study estimates: treatment of other counties in the local labor market



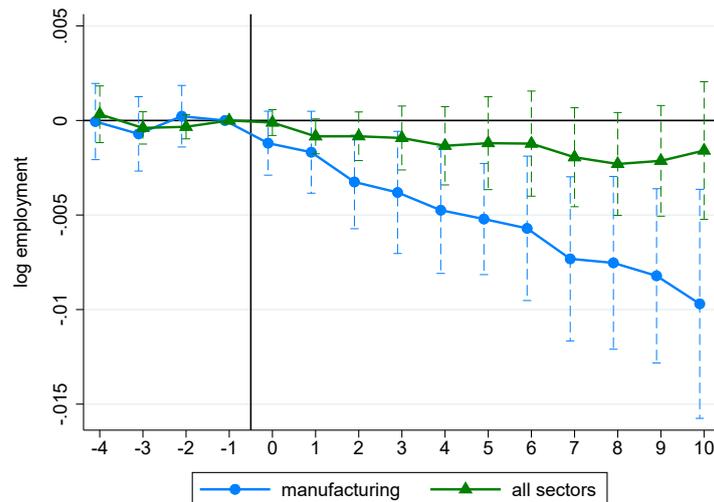
Source: BHP. Notes: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate of the county itself and of the change in the maximum assistance rate of the other counties in the same local labor market. The sample includes the 55 counties closest to cutoffs ($M=30$). Standard errors are clustered at the county level. See Appendix Table C.30 for the point estimates.

Figure A.11: Event study estimates: population and commuting flows



Source: Statistical Offices of German States, Federal Office for Building and Regional Planning Notes: This figure plots coefficients along with 95% confidence intervals of a regression of changes in log population and the inverse hyperbolic sine of the net commuting flow per employee on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs ($M=30$). Standard errors are clustered at the county level. See Appendix Table C.31 for the point estimates.

Figure A.12: Event study estimates: total employment



Source: BHP Notes: This figure plots coefficients along with 95% confidence intervals of a regression of log total employment on leads and lags of a change in the maximum assistance rate as in equation (5). The sample includes the 55 counties closest to cutoffs ($M=30$). Standard errors are clustered at the county level. See Appendix Table C.41 for the point estimates.

B Online Appendix: Data and Institutions

B.1 Data

Table B.1: Definition of variables and data sources

	year	description	source
<i>plant level</i>			
total investment	1996 - 2016	Total investment normalized to 2010 € on the plant-level for manufacturing plants with 20 or more employees.	AFID
equipment investment	1996 - 2016	Equipment investment normalized to 2010 € on the plant-level for manufacturing plants with 20 or more employees.	AFID
employees: manufacturing	1996 - 2017	Total number of manufacturing employees at the plant level.	BHP
employees: low-skill manufacturing	1996 - 2017	Number of manufacturing employees with a lower secondary, intermediate secondary or upper secondary school leaving certificate, but no vocational qualifications at the plant level.	BHP
employees: medium-skill manufacturing	1996 - 2017	Number of manufacturing employees with a lower secondary, intermediate secondary or upper secondary school leaving certificate and a vocational qualification at the plant level.	BHP
employees: high-skill manufacturing	1996 - 2017	Number of manufacturing employees with a degree from a university of applied sciences or a university at the plant level.	BHP
employees: retail	1996 - 2017	Total number of retail employees at the plant level.	BHP
employees: construction	1996 - 2017	Total number of construction employees in at the plant level.	BHP
employees: all	1996 - 2017	Total number of employees at the plant level.	BHP
<i>county level</i>			
employees: manufacturing	1996 - 2017	Total number of manufacturing employees at the county level	BHP
plants: manufacturing	1996 - 2017	Total number of manufacturing plants at the county level.	BHP
GRW subsidies	1996 - 2016	Total subsidies paid out normalized to 2010 € at the county level.	Federal Ministry for Economic Affairs
subsidised investment	1996 - 2016	Total amount of investment that is subsidised by GRW funds normalized to 2010 € at the county level.	Federal Ministry for Economic Affairs
median manufacturing wage	1996 - 2014	Median yearly wage in 2010 € of manufacturing workers at the county level. We weight all observations with the duration of the employment spell within the year and drop all apprentices, social service workers, working students, and interns.	SIAB
mean manufacturing wage	1996 - 2014	Mean yearly wage in 2010 € of manufacturing workers at the county level. We weight all observations with the duration of the employment spell within the year and drop all apprentices, social service workers, working students, and interns.	SIAB
median manufacturing wage: low-skill	1996 - 2014	Median yearly wage in 2010 € of manufacturing workers with a lower secondary, intermediate secondary or upper secondary school leaving certificate, but no vocational qualifications at the county level. We weight all observations with the duration of the employment spell within the year and drop all apprentices, social service workers, working students, and interns.	SIAB
median manufacturing wage: medium-skill	1996 - 2014	Median yearly wage in 2010 € of manufacturing workers with a lower secondary, intermediate secondary or upper secondary school leaving certificate and a vocational qualification. We weight all observations with the duration of the employment spell within the year and drop all apprentices, social service workers, working students, and interns.	SIAB
median manufacturing wage: high-skill	1996 - 2014	Median yearly wage in 2010 € of manufacturing workers with a degree from a university of applied sciences or a university. We weight all observations with the duration of the employment spell within the year and drop all apprentices, social service workers, working students, and interns.	SIAB
median wage	1996 - 2014	Median yearly wage in 2010 € of workers at the county level. We weight all observations with the duration of the employment spell within the year and drop all apprentices, social service workers, working students, and interns.	SIAB
unemployed	1997 - 2014	Number of unemployed at the county level.	Statistical Offices of the German States
population	1997 - 2017	Total population at the county level.	Statistical Offices of the German States
labor force	1997 - 2017	Sum of unemployed and employed at the county level.	Statistical Offices of the German States
GDP per capita	1997 - 2017	GDP per capita normalized to 2010 € at the county level.	Statistical Offices of the German States
other investment subsidies	1996 - 2012	Sum of all other investment subsidies received by municipalities aggregated to the county level.	Statistical Offices of the German States
local business tax: multiplier	1997 - 2017	Average local business tax multiplier weighted with the 1995 population at the county level.	Statistical Offices of the German States
local property tax: multiplier	1997 - 2017	Average local property tax multiplier weighted with the 1995 population at the county level.	Statistical Offices of the German States
local business tax: revenues	1997 - 2017	Local business tax revenues aggregated to the county level and normalized to 2010 €.	Statistical Offices of the German States
local property tax: revenues	1997 - 2017	Local property tax revenues aggregated to the county level and normalized to 2010 €.	Statistical Offices of the German States
local business tax: base	1997 - 2017	Local business tax base normalized to 2010 € and obtained by dividing the local business tax revenues by the product of the local business tax multiplier and the federal business tax rate (<i>Steuermesszahl</i>).	Statistical Offices of the German States

continued

Table B.1 continued

	year	description	source
local property tax: base	1997 - 2017	Local property tax base normalized to 2010 € and obtained by dividing the local property tax revenues by the product of the local property tax multiplier and the federal property tax rate (<i>Steuermesszahl</i>).	Statistical Offices of the German States
net commuting flow per employee	1997 - 2017	Net number of commuters normalized with the number of employees at the county level.	Federal Office for Building and Regional Planning
house price	1996 - 2012	House price index for houses of average quality in the largest city within a county. We linearly impute occasionally missing data points. For some county-year pairs, no data is available.	Immobilienverband Deutschland
rent per square meter	1996 - 2012	Rent per square meter for flats of average quality build after 1949 in the largest city within a county. We linearly impute occasionally missing data points. For some county-year pairs, no data is available.	Immobilienverband Deutschland
trade flows	2004, 2010	Import and export flows between all German counties as well as foreign countries measured in tons per year.	Federal Ministry of Transport and Digital Infrastructure
<i>local labor market level</i>			
employees: manufacturing	1996 - 2017	Total number of manufacturing employees at the local labor market level.	BHP
<i>aggregate level</i>			
unemployment benefits	1996 - 2014	Average unemployment benefits in 2010 € received at the household level in East or West Germany. We linearly extrapolate the data backwards before 2005.	Federal Employment Agency
CPI	1996 - 2017	German and US consumer price index.	Federal Statistical Office, FRED
Euro to Dollar exchange rate	1996 - 2017	Average yearly exchange rate between the Euro and the US Dollar from 2002 to 2017 and between the Deutsche Mark and the US Dollar multiplied by the official exchange rate between the Deutsche Mark and Euro from 1996 to 2001	FRED

Notes: This table provides details on the definition and sources for all variables used.

Table B.2: Descriptive statistics

variable	mean	sd	N	years
<i>plant level</i>				
total investment (in thousand €)	904.00	7088.95	125406	1996 - 2016
equipment investment (in thousand €)	772.23	6554.00	125406	1996 - 2016
employees: manufacturing	21.82	87.53	407694	1996 - 2017
employees: small manufacturing plants	5.63	4.85	326540	1996 - 2017
employees: large manufacturing plants	86.95	181.94	81154	1996 - 2017
employees: low-skill manufacturing	1.52	8.59	407694	1996 - 2017
employees: medium-skill manufacturing	17.42	68.81	407694	1996 - 2017
employees: high-skill manufacturing	2.67	17.65	407694	1996 - 2017
employees: retail	7.82	21.87	897327	1996 - 2017
employees: construction	8.78	23.20	560518	1996 - 2017
employees: all	11.70	60.00	4463572	1996 - 2017
<i>county level</i>				
employees: manufacturing	5319.71	3850.82	1672	1996 - 2017
plants: manufacturing	243.84	159.58	1672	1996 - 2017
GRW subsidies (in million €)	18.39	27.54	1596	1996 - 2016
subsidised investment (in million €)	83.90	140.60	1596	1996 - 2016
median manufacturing wage	22743.42	3623.83	1444	1996 - 2014
median manufacturing wage: low-skill	17750.72	6904.27	1424	1996 - 2014
median manufacturing wage: medium-skill	23117.17	3289.97	1444	1996 - 2014
median manufacturing wage: high-skill	40964.77	7651.66	1444	1996 - 2014
median wage	20453.39	1745.83	1444	1996 - 2014
population	173891.30	96067.54	1672	1996 - 2017
local business tax: multiplier	357.06	45.30	1672	1996 - 2017
local property tax: multiplier	375.26	61.06	1672	1996 - 2017
local business tax: revenues (in million €)	10.80	9.03	1672	1996 - 2017
local property tax: revenues (in million €)	4.29	2.52	1672	1996 - 2017
local business tax: base (in million €)	72.90	63.50	1672	1996 - 2017
local property tax: base (in million €)	32.65	17.31	1672	1996 - 2017
net commuting flow per employee	-0.13	0.21	1596	1997 - 2017
unemployed	13833.10	8588.68	1444	1996 - 2014
labor force	87131.02	52498.05	1672	1996 - 2017
GDP per capita	16901.04	2259.06	1672	1996 - 2017
other investment subsidies (in million €)	63.43	38.68	988	1996 - 2009
house price (in 1000 €)	146.87	42.20	797	1996 - 2012
rent per square meter	4.93	0.85	774	1996 - 2012
<i>local labor market level</i>				
employees: manufacturing	7628.27	5457.74	1166	1996 - 2017

Notes: There are 76 counties in East Germany (excluding Berlin) according to 2014 county definitions. All monetary variables are expressed in 2010 €. For sources and definitions see Table B.1.

B.2 Institutions

Indicator formulas The following formulas describe the indicator used to evaluate the economic performance of commuting zone r across regimes

$$\begin{aligned}
 indicator_r^{1997} &= \left(\frac{wage_r^{1995}}{wage_{East}^{1995}} \right)^{0.40} \times \left(2 - \frac{unemp_r^{1995}}{unemp_{East}^{1995}} \right)^{0.50} \times \left(\frac{infr_r^{1995}}{infr_{East}^{1995}} \right)^{0.10} \\
 indicator_r^{2000} &= \left(\frac{wage_r^{1997}}{wage_{East}^{97}} \right)^{0.40} \times \left(2 - \frac{unemp_r^{1996-1998}}{unemp_{East}^{1996-1998}} \right)^{0.40} \times \left(\frac{infr_r^{1999}}{infr_{East}^{1999}} \right)^{0.10} \times \left(\frac{empforecast_r}{empforecast_{East}} \right)^{0.10} \\
 indicator_r^{2007} &= \left(\frac{wage_r^{2003}}{wage_{Ger}^{03}} \right)^{0.40} \times \left(2 - \frac{unemp_r^{2002-2005}}{unemp_{Ger}^{2002-2005}} \right)^{0.50} \times \left(\frac{infr_r^{2005}}{infr_{Ger}^{2005}} \right)^{0.05} \times \left(\frac{empforecast_r}{empforecast_{Ger}} \right)^{0.05}
 \end{aligned}$$

where $infr_r^t$ measures the quality of a region r 's infrastructure, $wage_r^t$ represents per-capita earnings, $unemp_r^t$ the unemployment rate, and $empforecast_r^t$ is an employment rate projection assessed at time t . The infrastructure sub-indicator is based on measures of accessibility of airports and larger cities by car or train, the traveling time for trucks to the next trans-shipment center, the share of employees in applied research institutes, the share of apprenticeship training position, the share of employees in technical occupations, the share of high school graduates, the capacity of inter-company training centers and population density. For 1997 and 2000, each component is normalized by their respective East German average. Starting in 2007, normalization is with respect to the German average. Note, that the unemployment rate always enters negatively. All components are calculated such that if a region resembles the (East) German average, it gets a value of one.

Construction of cutoff samples Tables B.3, B.4, and B.5 illustrate the indicator rankings and cutoffs for the years 1997, 2000, and 2011, respectively. We do not use the rankings of the 2007 reform since all East German counties were treated. When counties merge, we take the average of the individual counties' indicators.

Table B.3: Counties around the cutoff (year 1997)

county	indicator	priority group
...		
Mittelsachsen	99.725	high
Gotha	99.757	low
Zwickau	99.767	high
Magdeburg	99.801	high
Jerichower Land	99.801	high
Boerde	99.801	high
Ludwigslust-Parchim	99.868	low
Salzlandkreis	99.902	low
Rostock	99.904	high
Chemnitz	99.914	high
Spree-Neiße	99.926	high
KS Cottbus	99.926	high
Dahme-Spreewald	99.926	low
Halle (Saale)	100.003	low
Landkreis Leipzig	100.069	low
Nordsachsen	100.069	low
Schwerin	100.096	low
Weimarer Land	100.162	low
Weimar	100.162	low
Sömmerda	100.173	low
Erfurt	100.173	low
Meissen	100.326	low
Saale-Holzland-Kreis	100.442	low
Jena	100.442	low
Leipzig	100.476	low
Dresden	101.073	low

Source: Federal Ministry for Economic Affairs.

Table B.4: Counties around the cutoff (year 2000)

county	indicator	priority group
...		
Hildburghausen	99.724	high
Suhl	99.724	high
Eichsfeld	99.728	high
Gotha	99.742	low
Vogtlandkreis	99.752	high
Jerichower Land	99.765	high
Cottbus	99.774	high
Spree-Neiße	99.774	high
Dahme-Spreewald	99.774	low
Bautzen	99.813	low
Saale-Orla-Kreis	99.854	high
Teltow-Fläming	99.856	low
Zwickau	99.884	low
Rostock	99.902	high
Nordwestmecklenburg	99.951	high
Chemnitz	100.008	low
Ludwigslust-Parchim	100.034	low
Boerde	100.070	low
Magdeburg	100.070	low
Nordsachsen	100.083	low
Weimar	100.144	low
Weimarer Land	100.144	low
Wartburgkreis	100.151	low
Eisenach	100.151	low
Halle (Saale)	100.169	low
Saechsische Schweiz-Osterzgebirge	100.177	low
Sonneberg	100.181	low
Erfurt	100.246	low
Sömmerda	100.246	low
Jena	100.256	low
Saale-Holzland-Kreis	100.256	low
Landkreis Leipzig	100.377	low
Schwerin	100.388	low
Meissen	100.444	low
Potsdam-Mittelmark	100.496	low
Leipzig	100.563	low
Dresden	101.117	low

Source: Federal Ministry for Economic Affairs.

Table B.5: Counties around the cutoff (year 2011)

county	NUTSII region	priority group	GDP per capita
...			
Magdeburg, Stadt	Magdeburg	high	€20822
Jerichower Land	Magdeburg	high	€20822
Altmarkkreis Salzwedel	Magdeburg	high	€20822
Boerde	Magdeburg	high	€20822
Harz	Magdeburg	high	€20822
Salzlandkreis	Magdeburg	high	€20822
Stendal	Magdeburg	high	€20822
Vogtlandkreis	Chemnitz	high	€20914
Chemnitz, Stadt	Chemnitz	high	€20914
Zwickau	Chemnitz	high	€20914
Mittelsachsen	Chemnitz	high	€20914
Erzgebirgskreis	Chemnitz	high	€20914
Mansfeld-Suedharz	Halle	low	€21228
Burgenlandkreis	Halle	low	€21228
Halle (Saale), Stadt	Halle	low	€21228
Saalekreis	Halle	low	€21228
Elbe-Elster	Brandenburg-Suedwest	low	€22572
Cottbus	Brandenburg-Suedwest	low	€22572
Teltow-Flaeming	Brandenburg-Suedwest	low	€22572
Dahme-Spreewald	Brandenburg-Suedwest	low	€22572
Havelland	Brandenburg-Suedwest	low	€22572
Brandenburg an der Havel, Stadt	Brandenburg-Suedwest	low	€22572
Potsdam-Mittelmark	Brandenburg-Suedwest	low	€22572
Oberspreewald-Lausitz	Brandenburg-Suedwest	low	€22572
Spree-Neisse	Brandenburg-Suedwest	low	€22572
Potsdam	Brandenburg-Suedwest	low	€22572
...			

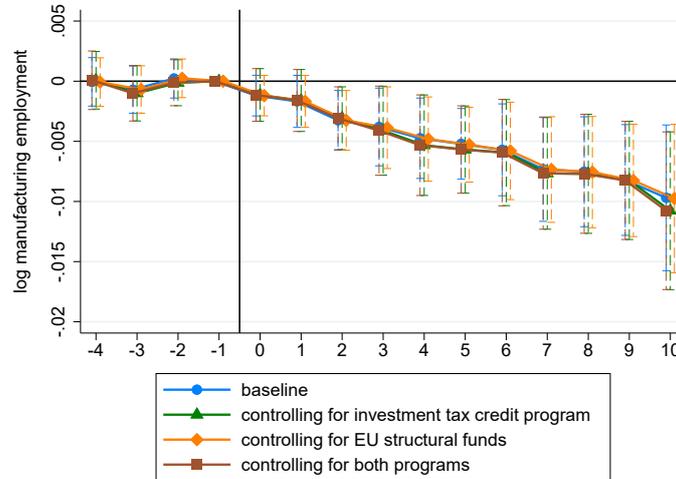
Source: Statistical Offices of German States , Deutscher Bundestag (2007).

Table B.6: Automatically eligible and non-eligible industries for GRW subsidies

Industries that are excluded from GRW subsidies
Agriculture, forestry, and fishing
Mining
Energy and water supply
Construction
Retail except for mail order
Transportation and warehousing
Hospitals
Industries that are automatically eligible for GRW subsidies
Manufacture of chemical products
Manufacture of plastic products
Manufacture of rubber products
Manufacture of ceramic products
Manufacture of concrete products
Manufacture of concrete products
Manufacture of cement products
Manufacture of glass products
Manufacture of signs
Manufacture of iron and steel products
Manufacture of non-ferrous metals
Casting of steel and iron
Casting of non-ferrous metals
Manufacture of machinery and technical devices
Manufacture of office machines and data processing equipment
Manufacture of vehicles
Manufacture of boats
Manufacture of electronics and electric technology
Manufacture of precision-engineered, optical, and surgical products
Manufacture of clocks
Manufacture of sheet metal products
Manufacture of toys, jewelry, musical instruments and sports equipment
Manufacture of timber products
Manufacture of forms, tools and models
Manufacture of pulp, groundwood, paper cardboard
Manufacture of print products
Manufacture of leather products
Manufacture of shoes
Manufacture of textiles
Manufacture of clothing
Manufacture of upholstery
Production of food for sale outside of the county
Production of animal feed
Mail order
Import and export wholesale
Data processing
Administration of industry firms or supra-regional service firms
Organizing congresses
Publishers
Research and experimental development for industry firms
Legal, accounting, book-keeping and auditing activities
Market research and public opinion polling
Business and management consultancy
Laboratory services for industry firms
Logistics
Tourism

B.3 Other Subsidy Programs

Figure B.1: Event study estimates: controlling for other policies



Source: Statistical Offices of German States Notes: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate as in equation (5) including controls for other policies as described in Appendix B.3. Standard errors are clustered at the county level. The sample includes the 55 counties closest to cutoffs ($M=30$). See Appendix Table B.7 for the point estimates.

In this section, we describe the three most important other subsidy programs targeted at East Germany, the investment tax credit program (*Investitionszulagengesetz*), the special depreciation allowance (*Fördergebietsgesetz*), and the Objective 1 EU structural funds.

The special depreciation allowance was introduced in 1991 for all East German firms and was abolished in 1998. It allowed firms to shift the depreciation of investment to earlier periods, thereby deferring the tax burden to future periods (Eichfelder and Schneider, 2014). Since this program never discriminated between regions within East Germany, it is fully captured by our year fixed effects.

The investment tax credit program was in place for East German firms from 1991 to 2013. Before 1999, it granted tax credits of 10% on equipment investment for manufacturing plants with up to 250 employees and 5% for plants with more employees. The tax credits for plants with up to 250 employees were increased to 20% in 1999 and 25% in 2000, whereas tax credits for larger plants were increased to 10% in 1999 and 12.5% in 2000 (Lerche, 2019). There was some minor special differentiation of the policy. First, counties at the Polish and Czech border were granted slightly higher tax credits between 2001 and 2009. These include the counties of Barnim, Bautzen, Chemnitz, Cottbus, Dresden, Erzgebirgskreis, Frankfurt (Oder), Greiz, Görlitz, Meißen, Mittelsachsen, Märkisch-Oderland, Oder-Spree, Saale-Orla-Kreis, Spree-Neiße, Sächsische Schweiz-Osterzgebirge, Uckermark, Vogtlandkreis, Vorpommern-Greifswald, Vorpommern-Rügen and Zwickau. Second, counties in the state of Brandenburg belonging to the local labor market of Berlin received lower rates throughout the whole period. These include the counties of Barnim, Dahme-Spreewald, Havelland, Märkisch-Oderland, Oberhavel, Oder-Spree, Potsdam, Potsdam-Mittelmark and Teltow-Fläming. We test whether the investment tax credit program confounds our effects by modifying equation (5) as follows. First, we include a dummy for plants with up to 250 employees, that were eligible for the increased tax credit, interacted with year dummies to capture the differential treatment of plants over time. Second, we allow these effects to be different in both border regions and the local labor

market of Berlin by fully interacting with the respective dummies.

The Objective 1 EU structural funds aim at fostering regional convergence across the European Union. More specifically, it provides grants to disadvantaged regions, whose eligibility is determined at the NUTS2 level, whereas the variation of the GRW is determined at the county level, which corresponds to the lower NUTS3 level. NUTS2 regions become eligible by having a GDP per capita level below 75% of the EU average (Becker et al., 2010). All NUTS2 regions of East Germany were beneficiaries of EU structural funds between 1994 and 2013. Therefore, our year fixed effects fully capture the program in this period. In 2014, all East German regions except for the NUTS2 region of Leipzig were classified as “transition regions” since their GDP per capita was between 75% and 90% of the EU average. Leipzig was classified as a “more developed region” since its GDP per capita exceeded 90% of the EU average and lost eligibility for EU structural funds. We create a dummy for receiving EU structural funds as a “transition region” and include it as a control variable in our regression. Between 1994 and 2013, all of East Germany was exempted from the Objective 2 program. Starting in 2014, all East German states started to receive funding from Objective 2 funds without any further spatial differentiation. Therefore, our time fixed effects effectively control for this program.

As Appendix Figure B.1 shows, our results do not change when we control for the investment tax credit program, the EU structural fund, or both of them at the same time. These results underline that our baseline results are not driven by other policies enacted over the sample period.

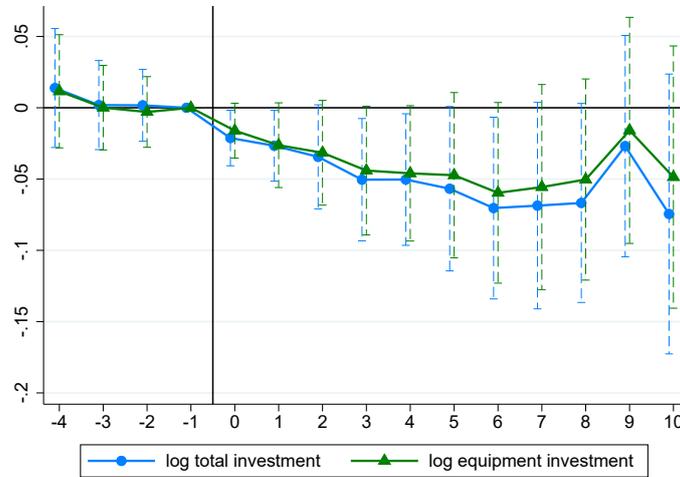
Table B.7: Event study estimates: controlling for other programs

	(1) log manufacturing employment	(2) log manufacturing employment	(3) log manufacturing employment
1 pp subsidy cut: year 4 before reform	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)
1 pp subsidy cut: year 3 before reform	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
1 pp subsidy cut: year 2 before reform	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)
1 pp subsidy cut: year 0 after reform	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
1 pp subsidy cut: year 1 after reform	0.002 (0.001)	0.002 (0.001)	0.002 (0.001)
1 pp subsidy cut: year 2 after reform	0.003** (0.001)	0.003** (0.001)	0.003** (0.001)
1 pp subsidy cut: year 3 after reform	0.004** (0.002)	0.004** (0.002)	0.004** (0.002)
1 pp subsidy cut: year 4 after reform	0.005*** (0.002)	0.005*** (0.002)	0.005*** (0.002)
1 pp subsidy cut: year 5 after reform	0.006*** (0.002)	0.005*** (0.002)	0.006*** (0.002)
1 pp subsidy cut: year 6 after reform	0.006** (0.002)	0.006*** (0.002)	0.006** (0.002)
1 pp subsidy cut: year 7 after reform	0.008*** (0.002)	0.007*** (0.002)	0.008*** (0.002)
1 pp subsidy cut: year 8 after reform	0.008*** (0.003)	0.008*** (0.002)	0.008*** (0.003)
1 pp subsidy cut: year 9 after reform	0.008*** (0.003)	0.008*** (0.002)	0.008*** (0.003)
1 pp subsidy cut: year 10 after reform	0.011*** (0.003)	0.010*** (0.003)	0.011*** (0.003)
investment tax credit program controls	yes		yes
EU structural funds controls		yes	yes
N	312,503	312,503	312,503

Notes: Standard errors in parentheses. See Figure B.1 for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

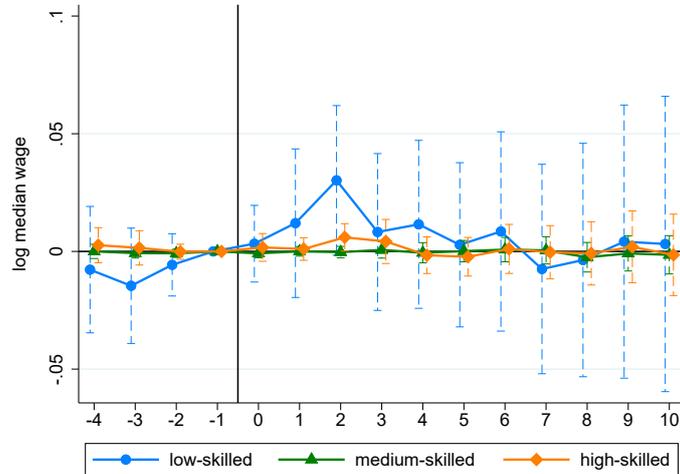
C Online Appendix: Additional Results

Figure C.1: Event study estimates: total & equipment investment



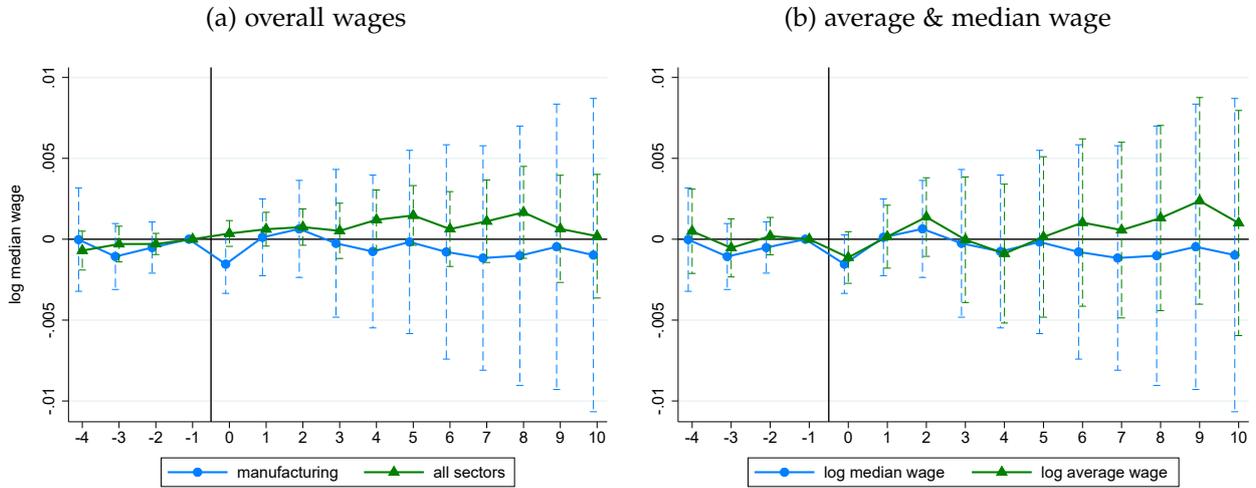
Source: AFiD Notes: This figure plots coefficients along with 95% confidence intervals of a regression of changes in log total and equipment investment on leads and lags of a change in the maximum assistance rate as in equation (5). The sample includes the 55 counties closest to cutoffs ($M=30$). Standard errors are clustered at the county level. See Appendix Table C.2 for the point estimates.

Figure C.2: Event study estimates: wages by skill



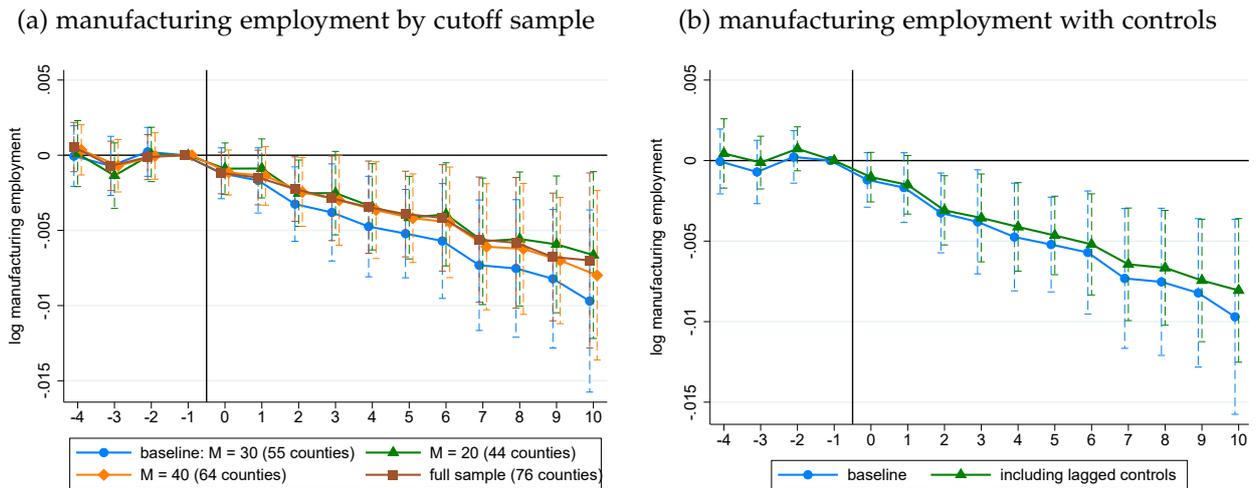
Source: SIAB Notes: This figure plots coefficients along with 95% confidence intervals of a regression of changes in log median wages by skill level on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs ($M=30$). Standard errors are clustered at the county level. See Appendix Table C.10 for the point estimates.

Figure C.3: Event study estimates: wages by sector and average wages



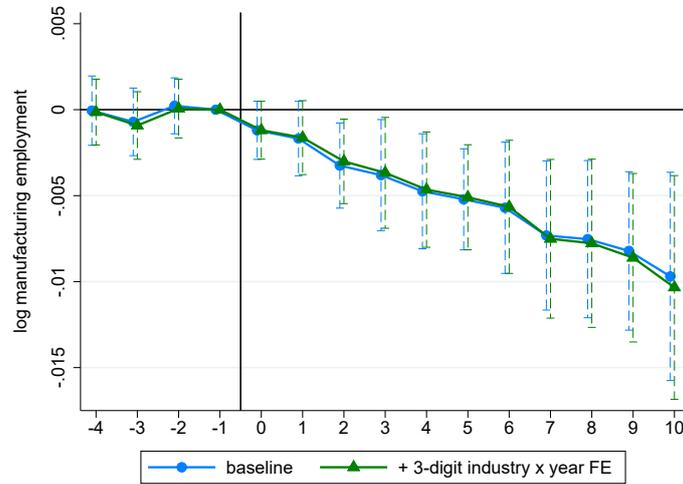
Source: SIAB Notes: This figure plots coefficients along with 95% confidence intervals of a regression of changes in log manufacturing wages by sector (Panel a) and log average wages (Panel b) on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs ($M=30$). Clustering of standard errors is at the county level. See Appendix Tables C.11 and C.12 for the point estimates.

Figure C.4: Event study estimates: manufacturing employment by cutoff sample and with controls



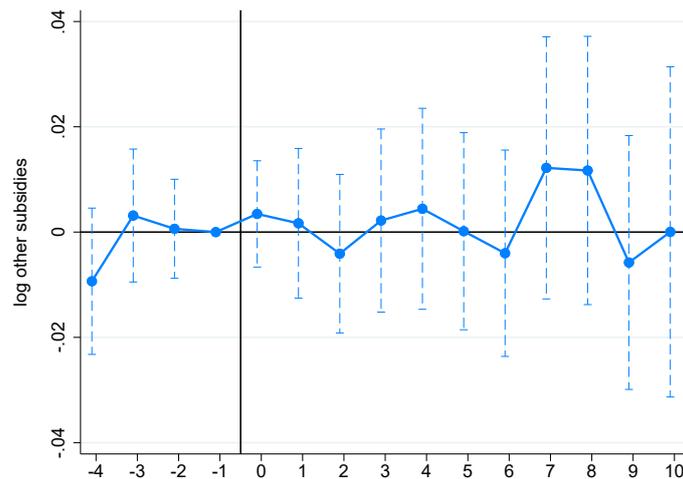
Source: BHP Notes: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate using different samples (Panel a) and including control variables (Panel b) as in equation (5). Standard errors are clustered at the county level. See Appendix Tables C.13 and C.14 for the point estimates.

Figure C.5: Event study estimates: manufacturing employment with 3-digit industry x year fixed effects



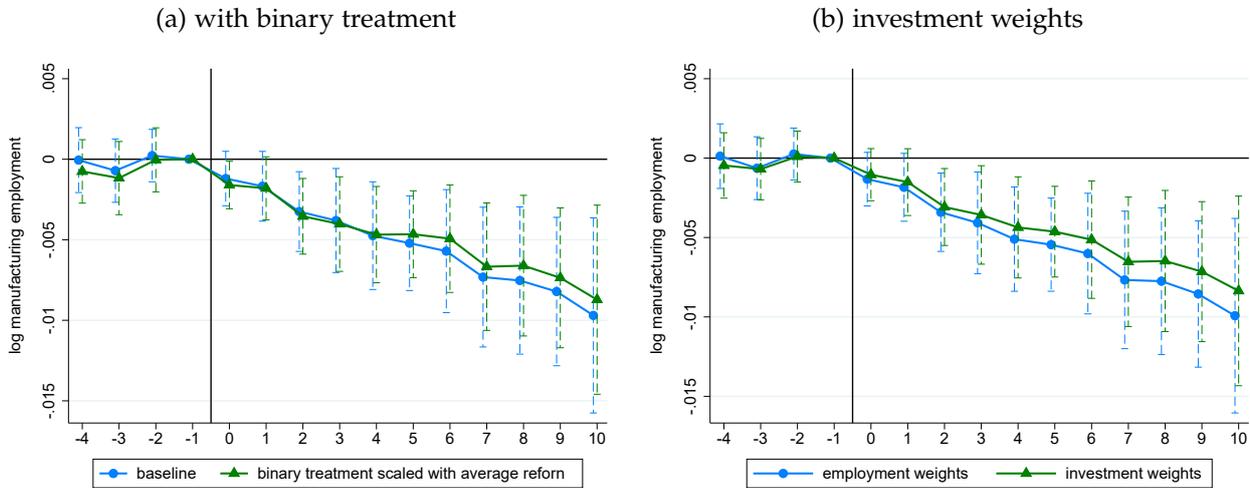
Source: BHP Notes: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate as in equation (5) including 3-digit industry times year fixed effects. Clustering of standard errors is at the county level. The sample includes the 55 counties closest to cutoffs ($M=30$). See Appendix Table C.15 for the point estimates.

Figure C.6: Event study estimates: other subsidies paid to municipalities



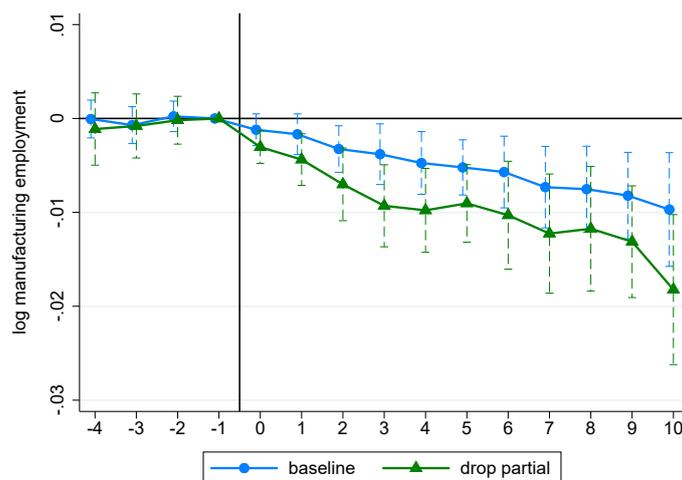
Source: Statistical Offices of German States Notes: This figure plots coefficients along with 95% confidence intervals of a regression of changes in log other subsidies on leads and lags of a change in the maximum assistance rate at the county level. Standard errors are clustered at the county level. The sample includes the 55 counties closest to cutoffs ($M=30$). See Appendix Table C.16 for the point estimates.

Figure C.7: Event study estimates: manufacturing employment with binary treatment and investment weights



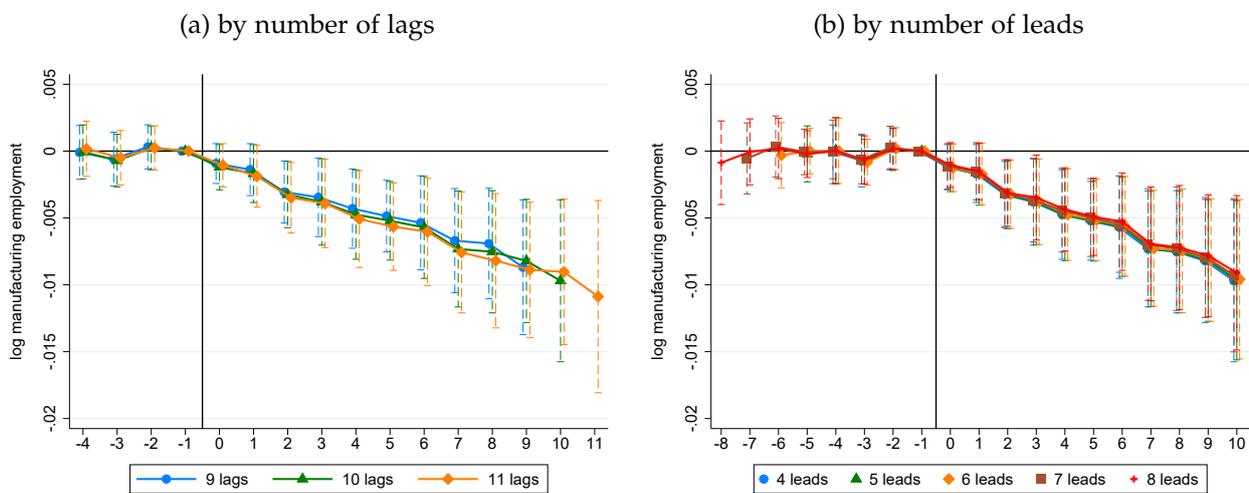
Source: BHP Notes: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate with a binary treatment definition as in equation (1) (Panel a) and with investment weighting (Panel b) as described in Section 4.1. In Panel (b) both the baseline and investment-weighted results are missing one county from the baseline sample for which we were not allowed to export the investment weights due to privacy reasons since there were too few observations in one size cell of the AFiD data. Standard errors are clustered at the county level. See Appendix Tables C.19 and C.20 for the point estimates.

Figure C.8: Event study estimates: manufacturing employment with binary treatment and without partially treated



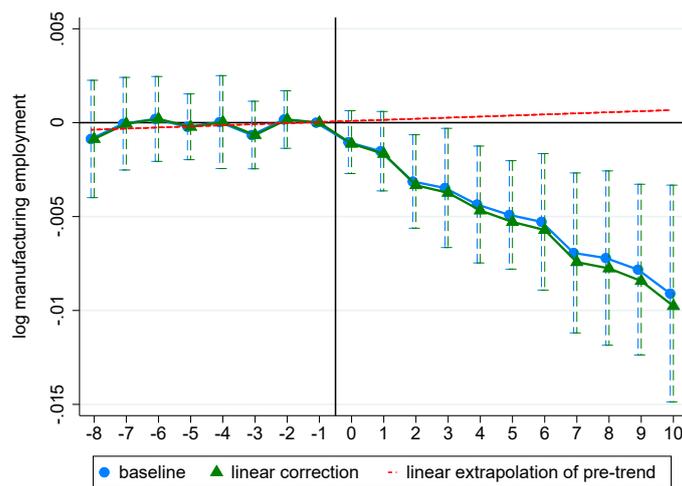
Source: BHP Notes: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate without the partially treated counties. Standard errors are clustered at the county level. See Appendix Table C.21 for the point estimates.

Figure C.9: Event study estimates: manufacturing employment by number of lags and leads



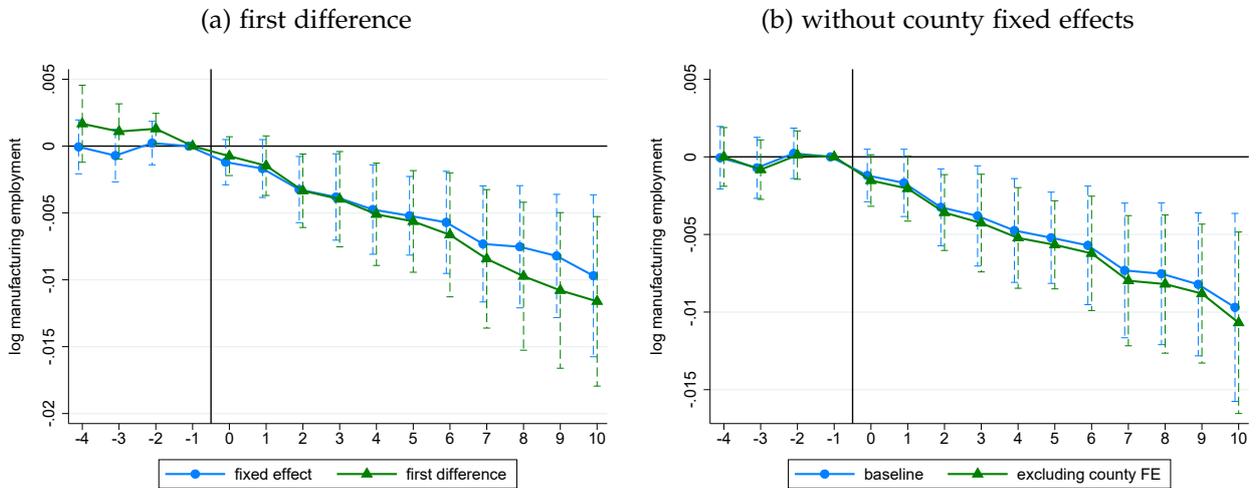
Source: BHP Notes: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate with different lag windows (Panel a) and different lead windows (Panel b) as in equation (5). Standard errors are clustered at the county level. See Appendix Tables C.22 and C.23 for the point estimates.

Figure C.10: Event study estimates: extrapolating pre-trends



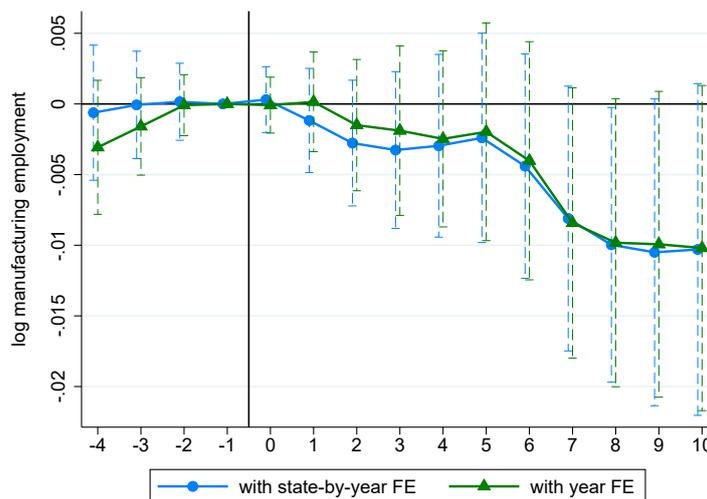
Source: BHP Notes: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate with eight leads. We estimate a linear model on the pre-trend and extrapolate it to the post-treatment time. The linear correction refers to the difference between the estimates and the linear extrapolation. Standard errors are clustered at the county level.

Figure C.11: Event study estimates: manufacturing employment in first differences and without county fixed effects



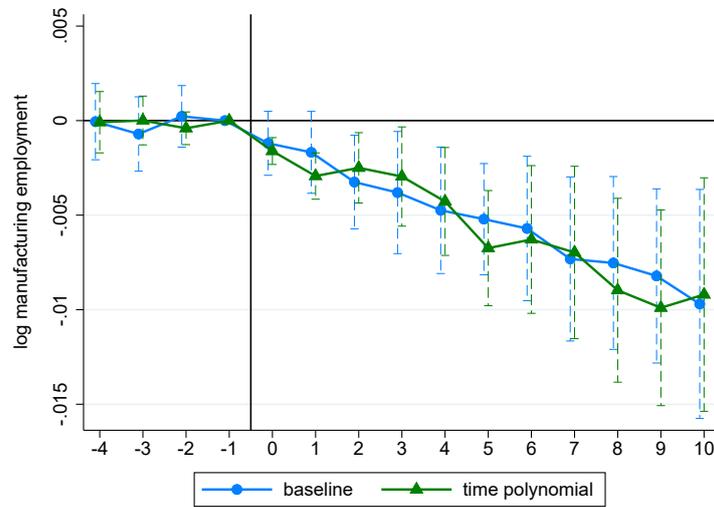
Source: BHP Notes: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate estimated in first differences (Panel a) and without county fixed effects (Panel b) as in equation (5). Standard errors are clustered at the county level. See Appendix Tables C.24 and C.25 for the point estimates.

Figure C.12: Event study estimates: local labor market robustness



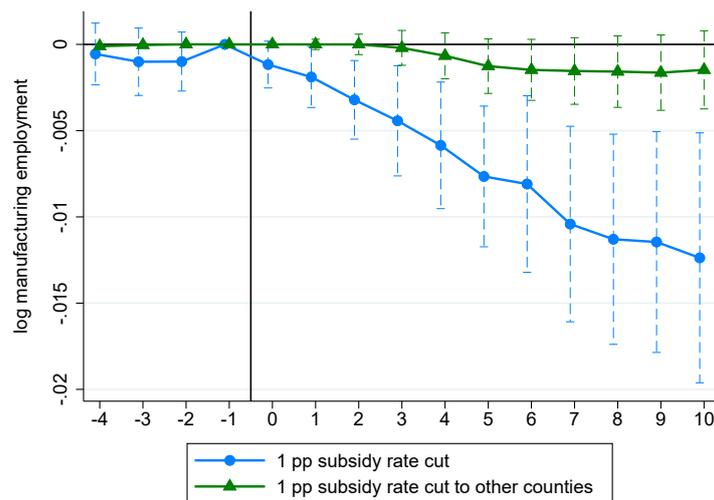
Source: BHP Notes: This figure plots coefficients along with 95% confidence intervals of a regression of changes in log manufacturing employment on leads and lags of a change in the maximum assistance rate at the local labor market level. When aggregating to the local labor market level, treatment intensities of counties are weighted by the number of manufacturing employees. The sample includes the counties or local labor markets that contain the 55 counties closest to cutoffs ($M = 30$). Clustering of standard errors is at the local labor market level. See Appendix Table C.32 for the point estimates.

Figure C.13: Event study estimates: time polynomials instead of time fixed effects



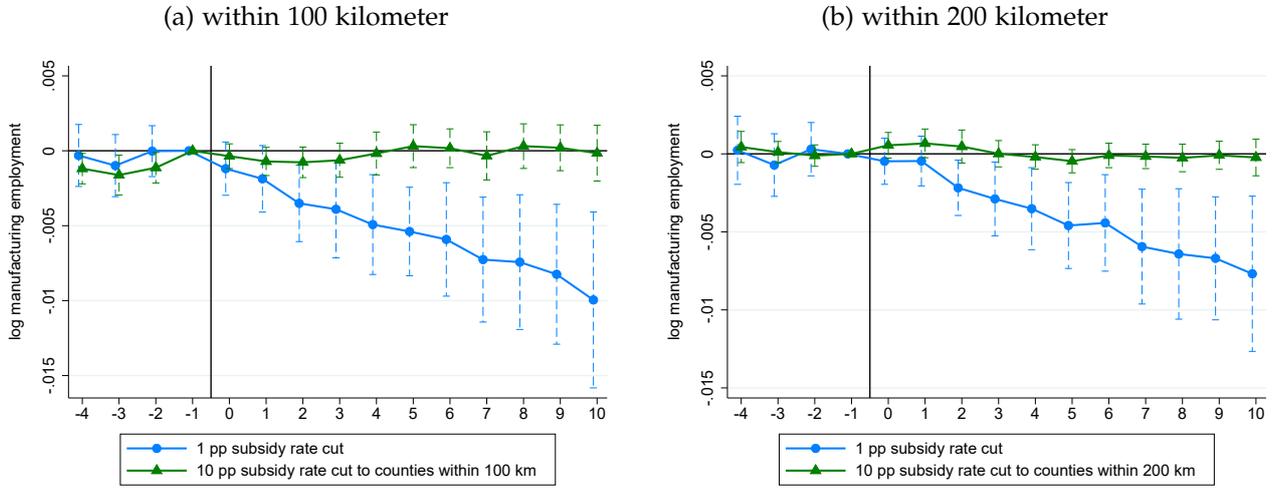
Source: BHP. Notes: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate of the county using a fourth order time polynomial to control for aggregate trends. The sample includes the 55 counties closest to cutoffs ($M=30$). Standard errors are clustered at the county level. See Appendix Table C.33 for the point estimates.

Figure C.14: Event study estimates: Bruhn (2018)



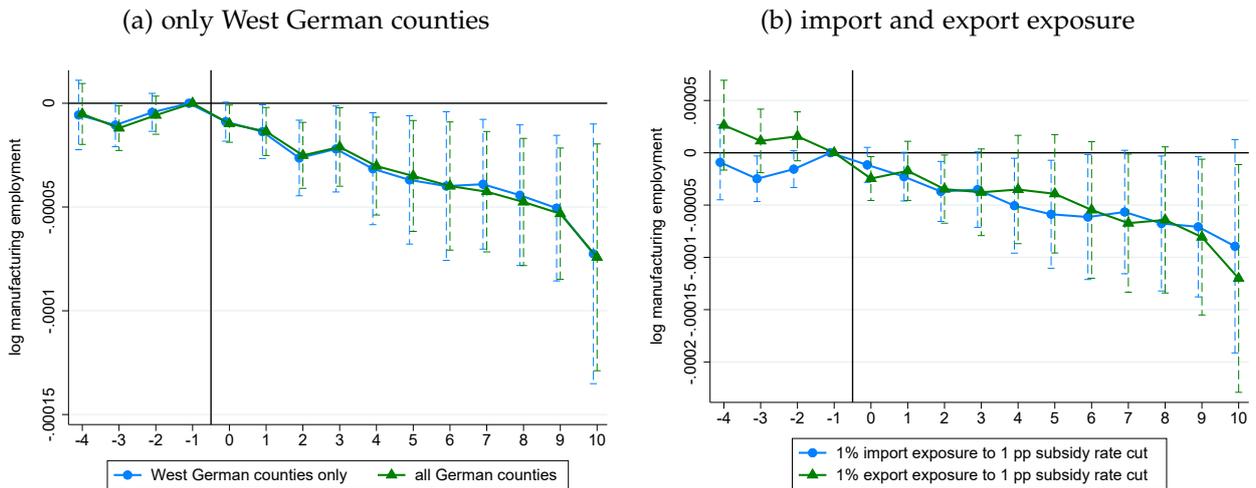
Source: BHP. Notes: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate of the county itself and of the change in the sum of the maximum assistance rate of all other counties using a fourth order time polynomial to control for aggregate trends. The sample includes the 55 counties closest to cutoffs ($M=30$). Standard errors are clustered at the county level. See Appendix Table C.33 for the point estimates.

Figure C.15: Event study estimates: regional spillover (robustness)



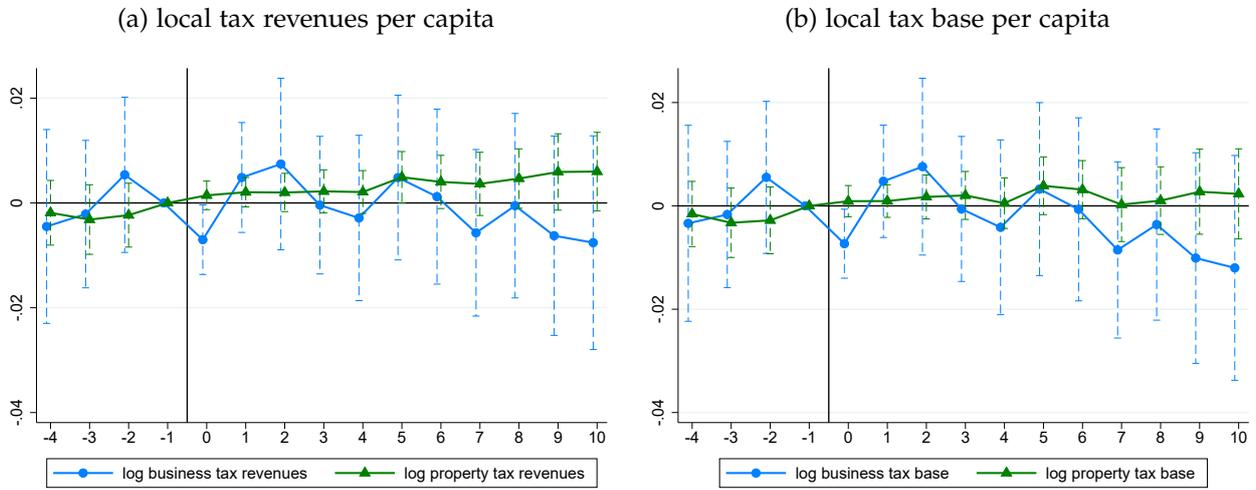
Notes: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate of the county itself and of the change in the sum of the maximum assistance rate of all other counties within 100 (Panel a) or 200 (Panel b) kilometers. The sample includes the 55 counties closest to cutoffs ($M=30$). Standard errors are clustered at the county level. See Appendix Tables C.34 and C.35 for the point estimates.

Figure C.16: Event study estimates: trade spillover (robustness)



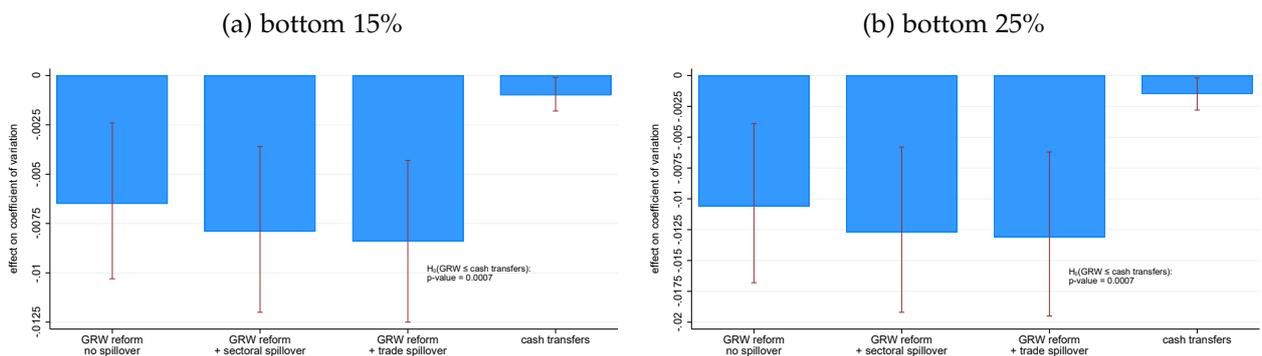
Source: BHP Notes: Panel (a) plots coefficients along with 95% confidence intervals of a regression as in equation (8) using log manufacturing employment at the plant level as the outcome. The sample includes only West German counties. Standard errors are clustered at the county level. See Table C.37 for the point estimates. Panel (b) plots coefficients along with 95% confidence intervals of a regression as in equation (8) including import and export exposure separately using log manufacturing employment at the plant level as the outcome. The sample includes all German counties. Standard errors are clustered at the county level. See Appendix Table C.38 for the point estimates.

Figure C.17: Event study estimates: local tax revenues and tax bases



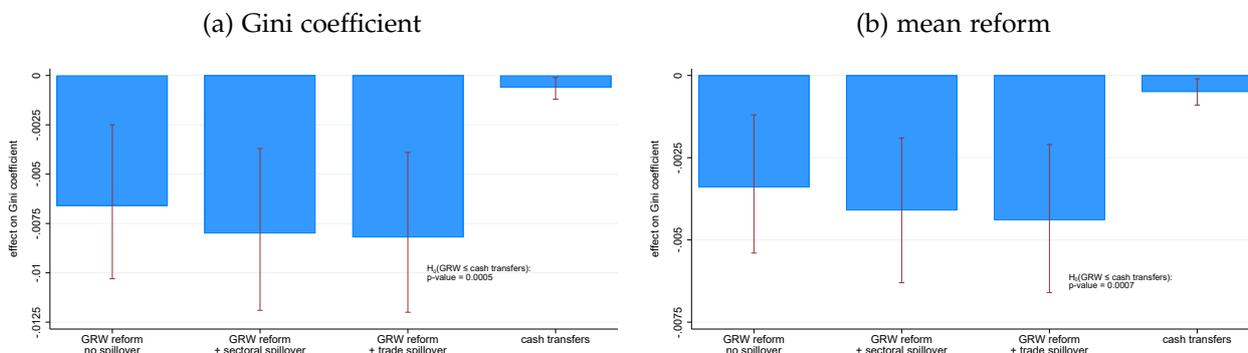
Source: Statistical Offices of German States Notes: This figure plots coefficients along with 95% confidence intervals of a regression of changes in the log local business and property tax revenues (Panel a) and the log local business per capita and property tax base per capita (Panel b) on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs ($M=30$). Standard errors are clustered at the county level. See Appendix Tables C.39 and C.40 for the point estimates.

Figure C.18: Counterfactual regional inequality: bottom 15% and bottom 25%



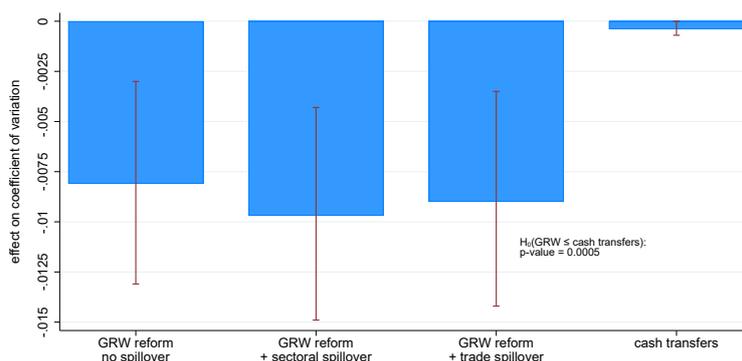
Source: BHP, SIAB, Federal Ministry for Economic Affairs, Statistical Offices of German States Notes: The first bar displays the effect of an increase in the GRW subsidy back to 1996 levels for counties in the bottom 15% (Panel a) or the in the bottom 25% (Panel b) of the labor income distribution on regional inequality within East Germany without accounting for any spillover. The second and third bars add trade and sectoral spillover, respectively. The fourth bar displays the effect of a revenue-neutral policy that pays a fixed cash transfer to every unemployed person in East Germany. The p-value refers to a one-sided test of whether the effect of the GRW policy is larger than the effect of cash transfers. Berlin is excluded from East Germany. Confidence intervals are based on 9999 bootstrap draws.

Figure C.19: Counterfactual regional inequality: Gini coefficient and mean reform



Source: BHP, SIAB, Federal Ministry for Economic Affairs, Statistical Offices of German States Notes: The first bar displays the effect of an increase in the GRW subsidy back to 1996 levels for counties in the bottom 20% of the labor income distribution on regional inequality within (East) Germany without accounting for any spillover. The second and third bars add trade and sectoral spillover, respectively. The fourth bar displays the effect of a revenue-neutral policy that pays a fixed cash transfer to every unemployed person in East Germany. Panel (a) uses the Gini coefficient as an alternative measure of regional inequality and panel (b) shows results for a 9 percentage point increase in the subsidy rate, which corresponds to the mean reform in the data. The p-value refers to a one-sided test of whether the effect of the GRW policy is larger than the effect of cash transfers. Berlin is excluded from East Germany. Confidence intervals are based on 9999 bootstrap draws.

Figure C.20: Counterfactual regional inequality: all German counties



Source: BHP, SIAB, Federal Ministry for Economic Affairs, Statistical Offices of German States Notes: The first bar displays the effect of an increase in the GRW subsidy back to 1996 levels for counties in the bottom 20% of the labor income distribution on regional inequality within Germany without accounting for any spillover. The second and third bars add trade and sectoral spillover, respectively. The fourth bar displays the effect of a revenue-neutral policy that pays a fixed cash transfer to every unemployed person in Germany. The p-value refers to a one-sided test of whether the effect of the GRW policy is larger than the effect of cash transfers. Confidence intervals are based on 9999 bootstrap draws.

Table C.1: Event study estimates: GRW subsidies

	(1) log GRW subsidies	(2) log subsidized investment
1 pp subsidy cut: year 4 before reform	0.029 (0.027)	0.028 (0.032)
1 pp subsidy cut: year 3 before reform	0.003 (0.024)	0.003 (0.027)
1 pp subsidy cut: year 2 before reform	0.028 (0.024)	0.037 (0.026)
1 pp subsidy cut: year 0 after reform	0.089*** (0.028)	0.090*** (0.029)
1 pp subsidy cut: year 1 after reform	0.114*** (0.025)	0.102*** (0.028)
1 pp subsidy cut: year 2 after reform	0.105*** (0.025)	0.089*** (0.026)
1 pp subsidy cut: year 3 after reform	0.131*** (0.029)	0.108*** (0.033)
1 pp subsidy cut: year 4 after reform	0.095*** (0.032)	0.079** (0.037)
1 pp subsidy cut: year 5 after reform	0.142*** (0.033)	0.134*** (0.037)
1 pp subsidy cut: year 6 after reform	0.157*** (0.032)	0.138*** (0.035)
1 pp subsidy cut: year 7 after reform	0.131*** (0.038)	0.121*** (0.044)
1 pp subsidy cut: year 8 after reform	0.075* (0.041)	0.056 (0.044)
1 pp subsidy cut: year 9 after reform	0.120*** (0.039)	0.109** (0.043)
1 pp subsidy cut: year 10 after reform	0.138** (0.062)	0.146** (0.071)
N	1141	1141

Notes: Standard errors in parentheses. See Figure 2 for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.2: Event study estimates: total and equipment investment

	(1)	(2)
	log total investment	log equipment investment
1 pp subsidy cut: year 4 before reform	0.014 (0.021)	0.012 (0.020)
1 pp subsidy cut: year 3 before reform	0.002 (0.016)	0.000 (0.015)
1 pp subsidy cut: year 2 before reform	0.002 (0.013)	0.003 (0.013)
1 pp subsidy cut: year 0 after reform	0.021** (0.010)	0.016 (0.010)
1 pp subsidy cut: year 1 after reform	0.027** (0.013)	0.026* (0.015)
1 pp subsidy cut: year 2 after reform	0.034* (0.019)	0.032* (0.019)
1 pp subsidy cut: year 3 after reform	0.050** (0.022)	0.044* (0.023)
1 pp subsidy cut: year 4 after reform	0.050** (0.024)	0.046* (0.024)
1 pp subsidy cut: year 5 after reform	0.057* (0.029)	0.047 (0.030)
1 pp subsidy cut: year 6 after reform	0.070** (0.033)	0.060* (0.032)
1 pp subsidy cut: year 7 after reform	0.069* (0.037)	0.056 (0.037)
1 pp subsidy cut: year 8 after reform	0.067* (0.036)	0.050 (0.036)
1 pp subsidy cut: year 9 after reform	0.027 (0.040)	0.016 (0.040)
1 pp subsidy cut: year 10 after reform	0.074 (0.050)	0.049 (0.047)
N	87627	87627

Notes: Standard errors in parentheses. See Appendix Figure C.1 for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.3: Event study estimates: plant-level manufacturing employment

	(1)
	log manufacturing employment
1 pp subsidy cut: year 4 before reform	0.000 (0.001)
1 pp subsidy cut: year 3 before reform	0.001 (0.001)
1 pp subsidy cut: year 2 before reform	0.000 (0.001)
1 pp subsidy cut: year 0 after reform	0.001 (0.001)
1 pp subsidy cut: year 1 after reform	0.002 (0.001)
1 pp subsidy cut: year 2 after reform	0.003** (0.001)
1 pp subsidy cut: year 3 after reform	0.004** (0.002)
1 pp subsidy cut: year 4 after reform	0.005*** (0.002)
1 pp subsidy cut: year 5 after reform	0.005*** (0.001)
1 pp subsidy cut: year 6 after reform	0.006*** (0.002)
1 pp subsidy cut: year 7 after reform	0.007*** (0.002)
1 pp subsidy cut: year 8 after reform	0.008*** (0.002)
1 pp subsidy cut: year 9 after reform	0.008*** (0.002)
1 pp subsidy cut: year 10 after reform	0.010*** (0.003)
N	312503

Notes: Standard errors in parentheses. See Figure 3 for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.4: Event study estimates: manufacturing employment by plants size

	(1) log manufacturing employment	(2) log manufacturing employment
1 pp subsidy cut: year 4 before reform	0.000 (0.001)	
1 pp subsidy cut: year 3 before reform	0.001 (0.001)	
1 pp subsidy cut: year 2 before reform	0.001 (0.001)	
1 pp subsidy cut: year 0 after reform	0.001 (0.001)	
1 pp subsidy cut: year 1 after reform	0.002* (0.001)	
1 pp subsidy cut: year 2 after reform	0.004** (0.002)	
1 pp subsidy cut: year 3 after reform	0.005** (0.002)	
1 pp subsidy cut: year 4 after reform	0.006** (0.003)	
1 pp subsidy cut: year 5 after reform	0.006*** (0.002)	
1 pp subsidy cut: year 6 after reform	0.007*** (0.003)	
1 pp subsidy cut: year 7 after reform	0.009*** (0.003)	
1 pp subsidy cut: year 8 after reform	0.009*** (0.003)	
1 pp subsidy cut: year 9 after reform	0.010*** (0.003)	
1 pp subsidy cut: year 10 after reform	0.011** (0.004)	
1 pp subsidy cut for small plants: year 4 before reform		0.000 (0.001)
1 pp subsidy cut for small plants: year 3 before reform		0.001 (0.001)
1 pp subsidy cut for small plants: year 2 before reform		0.001 (0.001)
1 pp subsidy cut for small plants: year 0 after reform		0.001 (0.001)
1 pp subsidy cut for small plants: year 1 after reform		0.002 (0.001)
1 pp subsidy cut for small plants: year 2 after reform		0.004*** (0.002)
1 pp subsidy cut for small plants: year 3 after reform		0.005** (0.002)
1 pp subsidy cut for small plants: year 4 after reform		0.007*** (0.002)
1 pp subsidy cut for small plants: year 5 after reform		0.007*** (0.002)
1 pp subsidy cut for small plants: year 6 after reform		0.008*** (0.003)
1 pp subsidy cut for small plants: year 7 after reform		0.010*** (0.003)
1 pp subsidy cut for small plants: year 8 after reform		0.011*** (0.003)
1 pp subsidy cut for small plants: year 9 after reform		0.011*** (0.003)
1 pp subsidy cut for small plants: year 10 after reform		0.013*** (0.004)
1 pp subsidy cut for large plants: year 4 before reform		0.002 (0.003)
1 pp subsidy cut for large plants: year 3 before reform		0.001 (0.003)
1 pp subsidy cut for large plants: year 2 before reform		0.001 (0.002)
1 pp subsidy cut for large plants: year 0 after reform		0.002 (0.002)
1 pp subsidy cut for large plants: year 1 after reform		0.002 (0.002)
1 pp subsidy cut for large plants: year 2 after reform		0.003 (0.002)
1 pp subsidy cut for large plants: year 3 after reform		0.002 (0.003)
1 pp subsidy cut for large plants: year 4 after reform		0.002 (0.004)
1 pp subsidy cut for large plants: year 5 after reform		0.003 (0.004)
1 pp subsidy cut for large plants: year 6 after reform		0.004 (0.005)
1 pp subsidy cut for large plants: year 7 after reform		0.004 (0.005)
1 pp subsidy cut for large plants: year 8 after reform		0.003 (0.006)
1 pp subsidy cut for large plants: year 9 after reform		0.004 (0.006)
1 pp subsidy cut for large plants: year 10 after reform		0.005 (0.008)
p-value of joint test of equality for all pre-reform effects		0.869
p-value of joint test of equality for all post-reform effects		0.037
N	192063	192063

Notes: Standard errors in parentheses. See Appendix Figure A.2 for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.5: Event study estimates: manufacturing employment by skill

	(1) log manufacturing employment: low-skill	(2) log manufacturing employment: medium-skill	(3) log manufacturing employment: high-skill
1 pp subsidy cut: year 4 before reform	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
1 pp subsidy cut: year 3 before reform	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
1 pp subsidy cut: year 2 before reform	0.001 (0.001)	0.000 (0.001)	0.000 (0.001)
1 pp subsidy cut: year 0 after reform	0.000 (0.001)	0.001 (0.001)	0.001 (0.001)
1 pp subsidy cut: year 1 after reform	0.001 (0.001)	0.002 (0.001)	0.001 (0.001)
1 pp subsidy cut: year 2 after reform	0.001 (0.002)	0.004*** (0.001)	0.001 (0.001)
1 pp subsidy cut: year 3 after reform	0.001 (0.002)	0.004*** (0.002)	0.003* (0.001)
1 pp subsidy cut: year 4 after reform	0.003 (0.002)	0.005*** (0.002)	0.001 (0.002)
1 pp subsidy cut: year 5 after reform	0.002 (0.002)	0.006*** (0.002)	0.002 (0.002)
1 pp subsidy cut: year 6 after reform	0.002 (0.003)	0.006*** (0.002)	0.002 (0.002)
1 pp subsidy cut: year 7 after reform	0.002 (0.003)	0.008*** (0.002)	0.003 (0.003)
1 pp subsidy cut: year 8 after reform	0.003 (0.003)	0.008*** (0.002)	0.003 (0.002)
1 pp subsidy cut: year 9 after reform	0.003 (0.004)	0.009*** (0.002)	0.002 (0.003)
1 pp subsidy cut: year 10 after reform	0.001 (0.004)	0.010*** (0.003)	0.005 (0.004)
N	114771	299229	123354

Notes: Standard errors in parentheses. See Appendix Figure A.3 for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.6: Event study estimates: number of manufacturing plants and county-level manufacturing employment

	(1)	(2)
	log manufacturing employment	log manufacturing plants
1 pp subsidy cut: year 4 before reform	0.001 (0.002)	0.001 (0.001)
1 pp subsidy cut: year 3 before reform	0.000 (0.002)	0.000 (0.001)
1 pp subsidy cut: year 2 before reform	0.000 (0.001)	0.000 (0.001)
1 pp subsidy cut: year 0 after reform	0.001 (0.001)	0.000 (0.001)
1 pp subsidy cut: year 1 after reform	0.000 (0.002)	0.001 (0.001)
1 pp subsidy cut: year 2 after reform	0.003 (0.002)	0.001 (0.001)
1 pp subsidy cut: year 3 after reform	0.004 (0.003)	0.001 (0.001)
1 pp subsidy cut: year 4 after reform	0.005 (0.003)	0.001 (0.002)
1 pp subsidy cut: year 5 after reform	0.005 (0.004)	0.001 (0.002)
1 pp subsidy cut: year 6 after reform	0.006 (0.004)	0.001 (0.002)
1 pp subsidy cut: year 7 after reform	0.008* (0.005)	0.000 (0.003)
1 pp subsidy cut: year 8 after reform	0.009* (0.005)	0.001 (0.003)
1 pp subsidy cut: year 9 after reform	0.010* (0.006)	0.001 (0.003)
1 pp subsidy cut: year 10 after reform	0.010 (0.006)	0.000 (0.003)
N	1210	1210

Notes: Standard errors in parentheses. See Appendix Figure A.4 for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.7: Event study estimates: GDP per capita

	(1) log GDP per capita
1 pp subsidy cut: year 4 before reform	0.000 (0.001)
1 pp subsidy cut: year 3 before reform	0.001 (0.001)
1 pp subsidy cut: year 2 before reform	0.000 (0.000)
1 pp subsidy cut: year 0 after reform	0.002 (0.002)
1 pp subsidy cut: year 1 after reform	0.002 (0.002)
1 pp subsidy cut: year 2 after reform	0.002 (0.002)
1 pp subsidy cut: year 3 after reform	0.005 (0.003)
1 pp subsidy cut: year 4 after reform	0.005 (0.004)
1 pp subsidy cut: year 5 after reform	0.005 (0.004)
1 pp subsidy cut: year 6 after reform	0.006 (0.004)
1 pp subsidy cut: year 7 after reform	0.006 (0.005)
1 pp subsidy cut: year 8 after reform	0.006 (0.005)
1 pp subsidy cut: year 9 after reform	0.006 (0.005)
1 pp subsidy cut: year 10 after reform	0.006 (0.005)
N	1210

Notes: Standard errors in parentheses. See Appendix Figure A.5 for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.8: Event study estimates: unemployed and labor force

	(1) log unemployed	(2) log labor force
1 pp subsidy cut: year 4 before reform	0.000 (0.002)	0.000 (0.003)
1 pp subsidy cut: year 3 before reform	0.001 (0.001)	0.000 (0.003)
1 pp subsidy cut: year 2 before reform	0.002* (0.001)	0.000 (0.003)
1 pp subsidy cut: year 0 after reform	0.000 (0.001)	0.000 (0.001)
1 pp subsidy cut: year 1 after reform	0.001 (0.001)	0.000 (0.001)
1 pp subsidy cut: year 2 after reform	0.003* (0.002)	0.000 (0.001)
1 pp subsidy cut: year 3 after reform	0.003 (0.002)	0.001 (0.003)
1 pp subsidy cut: year 4 after reform	0.002 (0.003)	0.001 (0.003)
1 pp subsidy cut: year 5 after reform	0.002 (0.003)	0.001 (0.003)
1 pp subsidy cut: year 6 after reform	0.004 (0.004)	0.001 (0.003)
1 pp subsidy cut: year 7 after reform	0.003 (0.004)	0.001 (0.004)
1 pp subsidy cut: year 8 after reform	0.003 (0.004)	0.001 (0.004)
1 pp subsidy cut: year 9 after reform	0.005 (0.004)	0.000 (0.004)
1 pp subsidy cut: year 10 after reform	0.006 (0.005)	0.001 (0.005)
N	990	1155

Notes: Standard errors in parentheses. See Appendix Figure A.6 for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.9: Event study estimates: manufacturing wages

	(1)
	log median manufacturing wage
1 pp subsidy cut: year 4 before reform	0.000 (0.002)
1 pp subsidy cut: year 3 before reform	0.001 (0.001)
1 pp subsidy cut: year 2 before reform	0.001 (0.001)
1 pp subsidy cut: year 0 after reform	0.002 (0.001)
1 pp subsidy cut: year 1 after reform	0.000 (0.001)
1 pp subsidy cut: year 2 after reform	0.001 (0.002)
1 pp subsidy cut: year 3 after reform	0.000 (0.002)
1 pp subsidy cut: year 4 after reform	0.001 (0.002)
1 pp subsidy cut: year 5 after reform	0.000 (0.003)
1 pp subsidy cut: year 6 after reform	0.001 (0.003)
1 pp subsidy cut: year 7 after reform	0.001 (0.004)
1 pp subsidy cut: year 8 after reform	0.001 (0.004)
1 pp subsidy cut: year 9 after reform	0.000 (0.005)
1 pp subsidy cut: year 10 after reform	0.001 (0.005)
N	1045

Notes: Standard errors in parentheses. See Figure 4 for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.10: Event study estimates: manufacturing wages by skill

	(1) log median low-skilled manufacturing wage	(2) log median medium-skilled manufacturing wage	(3) log median high-skilled manufacturing wage
1 pp subsidy cut: year 4 before reform	0.008 (0.014)	0.000 (0.002)	0.003 (0.004)
1 pp subsidy cut: year 3 before reform	0.015 (0.012)	0.001 (0.001)	0.001 (0.004)
1 pp subsidy cut: year 2 before reform	0.006 (0.007)	0.001 (0.001)	0.000 (0.002)
1 pp subsidy cut: year 0 after reform	0.003 (0.008)	0.001 (0.001)	0.002 (0.003)
1 pp subsidy cut: year 1 after reform	0.012 (0.016)	0.000 (0.001)	0.001 (0.002)
1 pp subsidy cut: year 2 after reform	0.030* (0.016)	0.000 (0.001)	0.006** (0.003)
1 pp subsidy cut: year 3 after reform	0.008 (0.017)	0.001 (0.002)	0.004 (0.005)
1 pp subsidy cut: year 4 after reform	0.011 (0.018)	0.001 (0.002)	0.002 (0.004)
1 pp subsidy cut: year 5 after reform	0.003 (0.018)	0.000 (0.002)	0.002 (0.004)
1 pp subsidy cut: year 6 after reform	0.008 (0.022)	0.001 (0.003)	0.001 (0.005)
1 pp subsidy cut: year 7 after reform	0.007 (0.023)	0.000 (0.003)	0.000 (0.006)
1 pp subsidy cut: year 8 after reform	0.004 (0.025)	0.002 (0.003)	0.001 (0.007)
1 pp subsidy cut: year 9 after reform	0.004 (0.030)	0.001 (0.004)	0.002 (0.008)
1 pp subsidy cut: year 10 after reform	0.003 (0.032)	0.001 (0.004)	0.001 (0.009)
N	1024	1045	1045

Notes: Standard errors in parentheses. See Appendix Figure C.2 for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.11: Event study estimates: median wage in all sectors

	(1) log median wage
1 pp subsidy cut: year 4 before reform	0.001 (0.001)
1 pp subsidy cut: year 3 before reform	0.000 (0.001)
1 pp subsidy cut: year 2 before reform	0.000 (0.000)
1 pp subsidy cut: year 0 after reform	0.000 (0.000)
1 pp subsidy cut: year 1 after reform	0.001 (0.001)
1 pp subsidy cut: year 2 after reform	0.001 (0.001)
1 pp subsidy cut: year 3 after reform	0.001 (0.001)
1 pp subsidy cut: year 4 after reform	0.001 (0.001)
1 pp subsidy cut: year 5 after reform	0.001 (0.001)
1 pp subsidy cut: year 6 after reform	0.001 (0.001)
1 pp subsidy cut: year 7 after reform	0.001 (0.001)
1 pp subsidy cut: year 8 after reform	0.002 (0.001)
1 pp subsidy cut: year 9 after reform	0.001 (0.002)
1 pp subsidy cut: year 10 after reform	0.000 (0.002)
N	1045

Notes: Standard errors in parentheses. See Appendix Figure C.3a for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.12: Event study estimates: mean manufacturing wages

	(1)	(2)
	log median manufacturing wage	log mean manufacturing wage
1 pp subsidy cut: year 4 before reform	0.000 (0.002)	0.000 (0.001)
1 pp subsidy cut: year 3 before reform	0.001 (0.001)	0.001 (0.001)
1 pp subsidy cut: year 2 before reform	0.001 (0.001)	0.000 (0.001)
1 pp subsidy cut: year 0 after reform	0.002 (0.001)	0.001 (0.001)
1 pp subsidy cut: year 1 after reform	0.000 (0.001)	0.000 (0.001)
1 pp subsidy cut: year 2 after reform	0.001 (0.002)	0.001 (0.001)
1 pp subsidy cut: year 3 after reform	0.000 (0.002)	0.000 (0.002)
1 pp subsidy cut: year 4 after reform	0.001 (0.002)	0.001 (0.002)
1 pp subsidy cut: year 5 after reform	0.000 (0.003)	0.000 (0.003)
1 pp subsidy cut: year 6 after reform	0.001 (0.003)	0.001 (0.003)
1 pp subsidy cut: year 7 after reform	0.001 (0.004)	0.001 (0.003)
1 pp subsidy cut: year 8 after reform	0.001 (0.004)	0.001 (0.003)
1 pp subsidy cut: year 9 after reform	0.000 (0.005)	0.002 (0.003)
1 pp subsidy cut: year 10 after reform	0.001 (0.005)	0.001 (0.004)
N	1045	1045

Notes: Standard errors in parentheses. See Appendix Figure C.3b for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.13: Event study estimates: manufacturing employment by cutoff sample

	(1) log manufacturing employment: full sample	(2) log manufacturing employment: M = 20	(3) log manufacturing employment: M = 30	(4) log manufacturing employment: M = 40
1 pp subsidy cut: year 4 before reform	0.001 (0.001)	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)
1 pp subsidy cut: year 3 before reform	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
1 pp subsidy cut: year 2 before reform	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)
1 pp subsidy cut: year 0 after reform	0.001* (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
1 pp subsidy cut: year 1 after reform	0.001 (0.001)	0.001 (0.001)	0.002 (0.001)	0.001 (0.001)
1 pp subsidy cut: year 2 after reform	0.002** (0.001)	0.003** (0.001)	0.003** (0.001)	0.002** (0.001)
1 pp subsidy cut: year 3 after reform	0.003** (0.001)	0.003* (0.001)	0.004** (0.002)	0.003* (0.002)
1 pp subsidy cut: year 4 after reform	0.003** (0.002)	0.003** (0.001)	0.005*** (0.002)	0.004** (0.002)
1 pp subsidy cut: year 5 after reform	0.004*** (0.001)	0.004*** (0.001)	0.005*** (0.001)	0.004*** (0.002)
1 pp subsidy cut: year 6 after reform	0.004** (0.002)	0.004** (0.002)	0.006*** (0.002)	0.004** (0.002)
1 pp subsidy cut: year 7 after reform	0.006*** (0.002)	0.006** (0.002)	0.007*** (0.002)	0.006*** (0.002)
1 pp subsidy cut: year 8 after reform	0.006** (0.002)	0.006** (0.002)	0.008*** (0.002)	0.006*** (0.002)
1 pp subsidy cut: year 9 after reform	0.007*** (0.002)	0.006** (0.002)	0.008*** (0.002)	0.007*** (0.002)
1 pp subsidy cut: year 10 after reform	0.007** (0.003)	0.007** (0.003)	0.010*** (0.003)	0.008*** (0.003)
N	401290	244169	312503	355601

Notes: Standard errors in parentheses. See Appendix Figure C.4a for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.14: Event study estimates: manufacturing employment (including lagged controls)

	(1) log manufacturing employment
1 pp subsidy cut: year 4 before reform	0.000 (0.001)
1 pp subsidy cut: year 3 before reform	0.000 (0.001)
1 pp subsidy cut: year 2 before reform	0.001 (0.001)
1 pp subsidy cut: year 0 after reform	0.001 (0.001)
1 pp subsidy cut: year 1 after reform	0.002 (0.001)
1 pp subsidy cut: year 2 after reform	0.003*** (0.001)
1 pp subsidy cut: year 3 after reform	0.004** (0.001)
1 pp subsidy cut: year 4 after reform	0.004*** (0.001)
1 pp subsidy cut: year 5 after reform	0.005*** (0.001)
1 pp subsidy cut: year 6 after reform	0.005*** (0.002)
1 pp subsidy cut: year 7 after reform	0.006*** (0.002)
1 pp subsidy cut: year 8 after reform	0.007*** (0.002)
1 pp subsidy cut: year 9 after reform	0.007*** (0.002)
1 pp subsidy cut: year 10 after reform	0.008*** (0.002)
N	312503

Notes: Standard errors in parentheses. See Appendix Figure C.4b for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.15: Event study estimates: manufacturing employment (including 3-digit industry x year FE)

	(1)
	log manufacturing employment
1 pp subsidy cut: year 4 before reform	0.000 (0.001)
1 pp subsidy cut: year 3 before reform	0.001 (0.001)
1 pp subsidy cut: year 2 before reform	0.000 (0.001)
1 pp subsidy cut: year 0 after reform	0.001 (0.001)
1 pp subsidy cut: year 1 after reform	0.002 (0.001)
1 pp subsidy cut: year 2 after reform	0.003** (0.001)
1 pp subsidy cut: year 3 after reform	0.004** (0.002)
1 pp subsidy cut: year 4 after reform	0.005*** (0.002)
1 pp subsidy cut: year 5 after reform	0.005*** (0.002)
1 pp subsidy cut: year 6 after reform	0.006*** (0.002)
1 pp subsidy cut: year 7 after reform	0.008*** (0.002)
1 pp subsidy cut: year 8 after reform	0.008*** (0.002)
1 pp subsidy cut: year 9 after reform	0.009*** (0.002)
1 pp subsidy cut: year 10 after reform	0.010*** (0.003)
N	312470

Notes: Standard errors in parentheses. See Appendix Figure C.5 for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.16: Event study estimates: other subsidies

	(1) log other subsidies
1 pp subsidy cut: year 4 before reform	0.009 (0.007)
1 pp subsidy cut: year 3 before reform	0.003 (0.006)
1 pp subsidy cut: year 2 before reform	0.001 (0.005)
1 pp subsidy cut: year 0 after reform	0.003 (0.005)
1 pp subsidy cut: year 1 after reform	0.002 (0.007)
1 pp subsidy cut: year 2 after reform	0.004 (0.008)
1 pp subsidy cut: year 3 after reform	0.002 (0.009)
1 pp subsidy cut: year 4 after reform	0.004 (0.010)
1 pp subsidy cut: year 5 after reform	0.000 (0.010)
1 pp subsidy cut: year 6 after reform	0.004 (0.010)
1 pp subsidy cut: year 7 after reform	0.012 (0.013)
1 pp subsidy cut: year 8 after reform	0.012 (0.013)
1 pp subsidy cut: year 9 after reform	0.006 (0.012)
1 pp subsidy cut: year 10 after reform	0.000 (0.016)
N	770

Notes: Standard errors in parentheses. See Appendix Figure C.6 for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.17: Event study estimates: manufacturing employment (increases & decreases)

	(1) log manufacturing employment
average subsidy cut: year 4 before reform	0.005 (0.011)
average subsidy cut: year 3 before reform	0.012 (0.012)
average subsidy cut: year 2 before reform	0.004 (0.010)
average subsidy cut: year 0 after reform	0.011* (0.006)
average subsidy cut: year 1 after reform	0.014 (0.008)
average subsidy cut: year 2 after reform	0.026** (0.011)
average subsidy cut: year 3 after reform	0.032** (0.013)
average subsidy cut: year 4 after reform	0.042*** (0.015)
average subsidy cut: year 5 after reform	0.041*** (0.014)
average subsidy cut: year 6 after reform	0.046*** (0.016)
average subsidy cut: year 7 after reform	0.070*** (0.021)
average subsidy cut: year 8 after reform	0.066*** (0.023)
average subsidy cut: year 9 after reform	0.079*** (0.025)
average subsidy cut: year 10 after reform	0.095*** (0.029)
average subsidy increase: year 4 before reform	0.010 (0.014)
average subsidy increase: year 3 before reform	0.003 (0.013)
average subsidy increase: year 2 before reform	0.013 (0.008)
average subsidy increase: year 0 after reform	0.023*** (0.008)
average subsidy increase: year 1 after reform	0.024** (0.011)
average subsidy increase: year 2 after reform	0.042*** (0.013)
average subsidy increase: year 3 after reform	0.041*** (0.013)
average subsidy increase: year 4 after reform	0.042*** (0.015)
average subsidy increase: year 5 after reform	0.042** (0.016)
average subsidy increase: year 6 after reform	0.043** (0.018)
average subsidy increase: year 7 after reform	0.055*** (0.019)
average subsidy increase: year 8 after reform	0.060*** (0.020)
average subsidy increase: year 9 after reform	0.062*** (0.019)
average subsidy increase: year 10 after reform	0.059** (0.022)
p-value of joint test of asymmetry for all pre-reform effects	0.688
p-value of joint test of asymmetry for all post-reform effects	0.279
N	312503

Table C.18: Heterogeneous treatment effects: Sun and Abraham (2021) & de Chaisemartin and D’Haultfoeuille (2022)

	(1) log manufacturing employment: baseline	(2) log manufacturing employment: Sun and Abraham (2021)	(3) log manufacturing employment: de Chaisemartin and D’Haultfoeuille (2022)
average subsidy cut: year 4 before reform	0.011 0.029)	0.021 0.035)	0.026 0.033)
average subsidy cut: year 3 before reform	0.022 0.022)	0.007 0.026)	0.018 0.023)
average subsidy cut: year 2 before reform	0.005 0.014)	0.018 0.018)	0.020 0.021)
average subsidy cut: year 0 after reform	0.017* 0.008)	0.018** 0.008)	0.017 0.014)
average subsidy cut: year 1 after reform	0.025* 0.014)	0.027* 0.014)	0.001 0.016)
average subsidy cut: year 2 after reform	0.040** 0.018)	0.047** 0.019)	0.001 0.017)
average subsidy cut: year 3 after reform	0.045** 0.023)	0.054** 0.024)	0.030 0.024)
average subsidy cut: year 4 after reform	0.071** 0.028)	0.062** 0.026)	0.033 0.024)
average subsidy cut: year 5 after reform	0.067** 0.029)	0.059*** 0.021)	0.027 0.021)
average subsidy cut: year 6 after reform	0.069** 0.032)	0.063** 0.024)	0.031 0.023)
average subsidy cut: year 7 after reform	0.115** 0.044)	0.097** 0.038)	0.077** 0.032)
average subsidy cut: year 8 after reform	0.101** 0.047)	0.101*** 0.035)	0.085*** 0.032)
average subsidy cut: year 9 after reform	0.110** 0.048)	0.108*** 0.033)	0.103*** 0.030)
N	161876	161876	161876

Notes: Standard errors in parentheses. See Appendix Figure A.8 for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.19: Event study estimates: manufacturing employment with binary treatment

	(1)
	log manufacturing employment
average subsidy cut: year 4 before reform	0.006 (0.008)
average subsidy cut: year 3 before reform	0.009 (0.009)
average subsidy cut: year 2 before reform	0.000 (0.008)
average subsidy cut: year 0 after reform	0.012** (0.006)
average subsidy cut: year 1 after reform	0.014* (0.008)
average subsidy cut: year 2 after reform	0.027*** (0.009)
average subsidy cut: year 3 after reform	0.031*** (0.012)
average subsidy cut: year 4 after reform	0.036*** (0.012)
average subsidy cut: year 5 after reform	0.036*** (0.011)
average subsidy cut: year 6 after reform	0.038*** (0.013)
average subsidy cut: year 7 after reform	0.051*** (0.016)
average subsidy cut: year 8 after reform	0.051*** (0.017)
average subsidy cut: year 9 after reform	0.057*** (0.017)
average subsidy cut: year 10 after reform	0.067*** (0.023)
N	312503

Notes: Standard errors in parentheses. See Appendix Figure C.7a for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.20: Event study estimates: plant-level manufacturing employment: investment weights

	(1) log manufacturing employment	(2) log manufacturing employment
1 pp subsidy cut: year 4 before reform	0.000 (0.001)	
1 pp subsidy cut: year 3 before reform	0.001 (0.001)	
1 pp subsidy cut: year 2 before reform	0.000 (0.001)	
1 pp subsidy cut: year 0 after reform	0.001* (0.001)	
1 pp subsidy cut: year 1 after reform	0.002* (0.001)	
1 pp subsidy cut: year 2 after reform	0.003*** (0.001)	
1 pp subsidy cut: year 3 after reform	0.004** (0.002)	
1 pp subsidy cut: year 4 after reform	0.005*** (0.002)	
1 pp subsidy cut: year 5 after reform	0.006*** (0.001)	
1 pp subsidy cut: year 6 after reform	0.006*** (0.002)	
1 pp subsidy cut: year 7 after reform	0.008*** (0.002)	
1 pp subsidy cut: year 8 after reform	0.008*** (0.002)	
1 pp subsidy cut: year 9 after reform	0.009*** (0.002)	
1 pp subsidy cut: year 10 after reform	0.010*** (0.003)	
1 pp subsidy cut (investment-weighted): year 4 before reform		0.001 (0.001)
1 pp subsidy cut (investment-weighted): year 3 before reform		0.001 (0.001)
1 pp subsidy cut (investment-weighted): year 2 before reform		0.000 (0.001)
1 pp subsidy cut (investment-weighted): year 0 after reform		0.001 (0.001)
1 pp subsidy cut (investment-weighted): year 1 after reform		0.002 (0.001)
1 pp subsidy cut (investment-weighted): year 2 after reform		0.003** (0.001)
1 pp subsidy cut (investment-weighted): year 3 after reform		0.004** (0.002)
1 pp subsidy cut (investment-weighted): year 4 after reform		0.004*** (0.002)
1 pp subsidy cut (investment-weighted): year 5 after reform		0.005*** (0.001)
1 pp subsidy cut (investment-weighted): year 6 after reform		0.005*** (0.002)
1 pp subsidy cut (investment-weighted): year 7 after reform		0.007*** (0.002)
1 pp subsidy cut (investment-weighted): year 8 after reform		0.007*** (0.002)
1 pp subsidy cut (investment-weighted): year 9 after reform		0.007*** (0.002)
1 pp subsidy cut (investment-weighted): year 10 after reform		0.008*** (0.003)
N	309288	309288

Notes: Standard errors in parentheses. See Appendix Figure C.7b for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.21: Event study estimates: plant-level manufacturing employment (without partially treated counties)

	(1) log manufacturing employment
1 pp subsidy cut: year 4 before reform	0.001 (0.002)
1 pp subsidy cut: year 3 before reform	0.001 (0.002)
1 pp subsidy cut: year 2 before reform	0.000 (0.001)
1 pp subsidy cut: year 0 after reform	0.003*** (0.001)
1 pp subsidy cut: year 1 after reform	0.004*** (0.001)
1 pp subsidy cut: year 2 after reform	0.007*** (0.002)
1 pp subsidy cut: year 3 after reform	0.009*** (0.002)
1 pp subsidy cut: year 4 after reform	0.010*** (0.002)
1 pp subsidy cut: year 5 after reform	0.009*** (0.002)
1 pp subsidy cut: year 6 after reform	0.010*** (0.003)
1 pp subsidy cut: year 7 after reform	0.012*** (0.003)
1 pp subsidy cut: year 8 after reform	0.012*** (0.003)
1 pp subsidy cut: year 9 after reform	0.013*** (0.003)
1 pp subsidy cut: year 10 after reform	0.018*** (0.004)
N	180020

Notes: Standard errors in parentheses. See Appendix Figure C.8 for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.22: Event study estimates: manufacturing employment by lags

	(1) log manufacturing employment	(2) log manufacturing employment	(3) log manufacturing employment
1 pp subsidy cut: year 4 before reform	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)
1 pp subsidy cut: year 3 before reform	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
1 pp subsidy cut: year 2 before reform	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)
1 pp subsidy cut: year 0 after reform	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
1 pp subsidy cut: year 1 after reform	0.001 (0.001)	0.002 (0.001)	0.002 (0.001)
1 pp subsidy cut: year 2 after reform	0.003** (0.001)	0.003** (0.001)	0.003** (0.001)
1 pp subsidy cut: year 3 after reform	0.003** (0.002)	0.004** (0.002)	0.004** (0.002)
1 pp subsidy cut: year 4 after reform	0.004*** (0.002)	0.005*** (0.002)	0.005*** (0.002)
1 pp subsidy cut: year 5 after reform	0.005*** (0.001)	0.005*** (0.001)	0.006*** (0.002)
1 pp subsidy cut: year 6 after reform	0.005*** (0.002)	0.006*** (0.002)	0.006*** (0.002)
1 pp subsidy cut: year 7 after reform	0.007*** (0.002)	0.007*** (0.002)	0.008*** (0.002)
1 pp subsidy cut: year 8 after reform	0.007*** (0.002)	0.008*** (0.002)	0.008*** (0.003)
1 pp subsidy cut: year 9 after reform	0.009*** (0.003)	0.008*** (0.002)	0.009*** (0.003)
1 pp subsidy cut: year 10 after reform		0.010*** (0.003)	0.009*** (0.003)
1 pp subsidy cut: year 11 after reform			0.011*** (0.004)
N	312503	312503	312503

Notes: Standard errors in parentheses. See Appendix Figure C.9a for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.23: Event study estimates: manufacturing employment by leads

	(1)	(2)	(3)	(4)
	log manufacturing employment	log manufacturing employment	log manufacturing employment	log manufacturing employment
1 pp subsidy cut: year 8 before reform				0.001 (0.002)
1 pp subsidy cut: year 7 before reform			0.001 (0.001)	0.000 (0.001)
1 pp subsidy cut: year 6 before reform		0.000 (0.001)	0.000 (0.001)	0.000 (0.001)
1 pp subsidy cut: year 5 before reform	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)
1 pp subsidy cut: year 4 before reform	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)
1 pp subsidy cut: year 3 before reform	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
1 pp subsidy cut: year 2 before reform	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)	0.000 (0.001)
1 pp subsidy cut: year 0 after reform	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
1 pp subsidy cut: year 1 after reform	0.002 (0.001)	0.002 (0.001)	0.002 (0.001)	0.002 (0.001)
1 pp subsidy cut: year 2 after reform	0.003** (0.001)	0.003** (0.001)	0.003** (0.001)	0.003** (0.001)
1 pp subsidy cut: year 3 after reform	0.004** (0.002)	0.004** (0.002)	0.004** (0.002)	0.003** (0.002)
1 pp subsidy cut: year 4 after reform	0.005*** (0.002)	0.005*** (0.002)	0.004*** (0.002)	0.004*** (0.002)
1 pp subsidy cut: year 5 after reform	0.005*** (0.002)	0.005*** (0.002)	0.005*** (0.001)	0.005*** (0.001)
1 pp subsidy cut: year 6 after reform	0.006*** (0.002)	0.006*** (0.002)	0.005*** (0.002)	0.005*** (0.002)
1 pp subsidy cut: year 7 after reform	0.007*** (0.002)	0.007*** (0.002)	0.007*** (0.002)	0.007*** (0.002)
1 pp subsidy cut: year 8 after reform	0.008*** (0.002)	0.007*** (0.002)	0.007*** (0.002)	0.007*** (0.002)
1 pp subsidy cut: year 9 after reform	0.008*** (0.002)	0.008*** (0.002)	0.008*** (0.002)	0.008*** (0.002)
1 pp subsidy cut: year 10 after reform	0.010*** (0.003)	0.010*** (0.003)	0.009*** (0.003)	0.009*** (0.003)
N	312503	312503	312503	312503

Notes: Standard errors in parentheses. See Appendix Figure C.9b for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.24: Event study estimates: manufacturing employment (first difference)

	(1) log manufacturing employment
1 pp subsidy cut: year 4 before reform	0.002 (0.001)
1 pp subsidy cut: year 3 before reform	0.001 (0.001)
1 pp subsidy cut: year 2 before reform	0.001** (0.001)
1 pp subsidy cut: year 0 after reform	0.001 (0.001)
1 pp subsidy cut: year 1 after reform	0.001 (0.001)
1 pp subsidy cut: year 2 after reform	0.003** (0.001)
1 pp subsidy cut: year 3 after reform	0.004** (0.002)
1 pp subsidy cut: year 4 after reform	0.005** (0.002)
1 pp subsidy cut: year 5 after reform	0.006*** (0.002)
1 pp subsidy cut: year 6 after reform	0.007*** (0.002)
1 pp subsidy cut: year 7 after reform	0.008*** (0.003)
1 pp subsidy cut: year 8 after reform	0.010*** (0.003)
1 pp subsidy cut: year 9 after reform	0.011*** (0.003)
1 pp subsidy cut: year 10 after reform	0.012*** (0.003)
N	293534

Notes: Standard errors in parentheses. See Appendix Figure C.11a for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.25: Event study estimates: manufacturing employment (dropping county fixed effects)

	(1)
	log manufacturing employment
1 pp subsidy cut: year 4 before reform	0.000 (0.001)
1 pp subsidy cut: year 3 before reform	0.001 (0.001)
1 pp subsidy cut: year 2 before reform	0.000 (0.001)
1 pp subsidy cut: year 0 after reform	0.002* (0.001)
1 pp subsidy cut: year 1 after reform	0.002* (0.001)
1 pp subsidy cut: year 2 after reform	0.004*** (0.001)
1 pp subsidy cut: year 3 after reform	0.004** (0.002)
1 pp subsidy cut: year 4 after reform	0.005*** (0.002)
1 pp subsidy cut: year 5 after reform	0.006*** (0.001)
1 pp subsidy cut: year 6 after reform	0.006*** (0.002)
1 pp subsidy cut: year 7 after reform	0.008*** (0.002)
1 pp subsidy cut: year 8 after reform	0.008*** (0.002)
1 pp subsidy cut: year 9 after reform	0.009*** (0.002)
1 pp subsidy cut: year 10 after reform	0.011*** (0.003)
N	312503

Notes: Standard errors in parentheses. See Appendix Figure C.11b for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.26: Event study estimates: manufacturing employment (inference robustness)

	(1) log manufacturing employment	(2) log manufacturing employment:	(3) log manufacturing employment
1 pp subsidy cut: year 4 before reform	0.0001 (0.0010)	0.0001 (0.0010)	0.0001 (0.0012)
1 pp subsidy cut: year 3 before reform	0.0007 (0.0010)	0.0007 (0.0010)	0.0007 (0.0010)
1 pp subsidy cut: year 2 before reform	0.0002 (0.0008)	0.0002 (0.0009)	0.0002 (0.0008)
1 pp subsidy cut: year 0 after reform	0.0012 (0.0008)	0.0012 (0.0008)	0.0012 (0.0011)
1 pp subsidy cut: year 1 after reform	0.0017 (0.0011)	0.0017 (0.0010)	0.0017 (0.0011)
1 pp subsidy cut: year 2 after reform	0.0033*** (0.0012)	0.0033*** (0.0012)	0.0033*** (0.0010)
1 pp subsidy cut: year 3 after reform	0.0038** (0.0016)	0.0038** (0.0015)	0.0038*** (0.0013)
1 pp subsidy cut: year 4 after reform	0.0047*** (0.0016)	0.0047*** (0.0016)	0.0047*** (0.0016)
1 pp subsidy cut: year 5 after reform	0.0052*** (0.0014)	0.0052*** (0.0014)	0.0052*** (0.0016)
1 pp subsidy cut: year 6 after reform	0.0057*** (0.0019)	0.0057*** (0.0017)	0.0057*** (0.0018)
1 pp subsidy cut: year 7 after reform	0.0073*** (0.0021)	0.0073*** (0.0020)	0.0073*** (0.0021)
1 pp subsidy cut: year 8 after reform	0.0075*** (0.0022)	0.0075*** (0.0022)	0.0075*** (0.0022)
1 pp subsidy cut: year 9 after reform	0.0082*** (0.0022)	0.0082*** (0.0021)	0.0082*** (0.0022)
1 pp subsidy cut: year 10 after reform	0.0097*** (0.0030)	0.0097*** (0.0028)	0.0097*** (0.0029)
standard errors	cluster at county and plant level	cluster at local labor market level	spatial correlation adjustment
N	312503	312503	312503

Notes: In column (1), standard errors are clustered at the county and plant level, while in column (2) standard errors are clustered at the local labor market and plant level. In column (3), standard errors are adjusted for spatial dependence as in Conley (1999). Spatial autocorrelation is assumed to linearly decrease up to a cutoff of 100 kilometers. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.27: Event study estimates: employment by finer industries

	(1) log manufacturing employment	(2) log retail employment	(3) log construction employment
1 pp subsidy cut: year 4 before reform	0.000 (0.001)	0.000 (0.001)	0.001 (0.001)
1 pp subsidy cut: year 3 before reform	0.001 (0.001)	0.001 (0.001)	0.002* (0.001)
1 pp subsidy cut: year 2 before reform	0.000 (0.001)	0.001 (0.000)	0.002*** (0.001)
1 pp subsidy cut: year 0 after reform	0.001 (0.001)	0.000 (0.001)	0.001 (0.001)
1 pp subsidy cut: year 1 after reform	0.002 (0.001)	0.001 (0.001)	0.003*** (0.001)
1 pp subsidy cut: year 2 after reform	0.003** (0.001)	0.001 (0.001)	0.002* (0.001)
1 pp subsidy cut: year 3 after reform	0.004** (0.002)	0.002* (0.001)	0.002* (0.001)
1 pp subsidy cut: year 4 after reform	0.005*** (0.002)	0.002* (0.001)	0.004** (0.002)
1 pp subsidy cut: year 5 after reform	0.005*** (0.001)	0.002* (0.001)	0.004** (0.002)
1 pp subsidy cut: year 6 after reform	0.006*** (0.002)	0.002 (0.001)	0.004** (0.002)
1 pp subsidy cut: year 7 after reform	0.007*** (0.002)	0.003* (0.002)	0.004** (0.002)
1 pp subsidy cut: year 8 after reform	0.008*** (0.002)	0.003 (0.002)	0.005** (0.002)
1 pp subsidy cut: year 9 after reform	0.008*** (0.002)	0.003 (0.002)	0.004* (0.002)
1 pp subsidy cut: year 10 after reform	0.010*** (0.003)	0.003 (0.002)	0.005* (0.003)
N	312503	652099	409551

Notes: Standard errors in parentheses. See Appendix Figure 5a for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.28: Event study estimates: house price & rent per square meter

	(1)	(2)
	log rent per square meter	log house price
1 pp subsidy cut: year 4 before reform	0.000 (0.004)	0.001 (0.006)
1 pp subsidy cut: year 3 before reform	0.003 (0.003)	0.003 (0.005)
1 pp subsidy cut: year 2 before reform	0.001 (0.002)	0.002 (0.002)
1 pp subsidy cut: year 0 after reform	0.002 (0.003)	0.002 (0.006)
1 pp subsidy cut: year 1 after reform	0.005 (0.004)	0.000 (0.005)
1 pp subsidy cut: year 2 after reform	0.005 (0.005)	0.001 (0.006)
1 pp subsidy cut: year 3 after reform	0.006 (0.004)	0.004 (0.006)
1 pp subsidy cut: year 4 after reform	0.005 (0.005)	0.003 (0.008)
1 pp subsidy cut: year 5 after reform	0.002 (0.006)	0.004 (0.010)
1 pp subsidy cut: year 6 after reform	0.003 (0.006)	0.003 (0.012)
1 pp subsidy cut: year 7 after reform	0.001 (0.009)	0.002 (0.016)
1 pp subsidy cut: year 8 after reform	0.007 (0.010)	0.008 (0.017)
1 pp subsidy cut: year 9 after reform	0.008 (0.010)	0.005 (0.017)
1 pp subsidy cut: year 10 after reform	0.003 (0.010)	0.005 (0.021)
N	528	550

Notes: Standard errors in parentheses. See Appendix Figure A.9 for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.29: Event study estimates: manufacturing employment at the labor market level

	(1)
	log manufacturing employment
1 pp subsidy cut: year 4 before reform	0.001 (0.002)
1 pp subsidy cut: year 3 before reform	0.000 (0.002)
1 pp subsidy cut: year 2 before reform	0.000 (0.001)
1 pp subsidy cut: year 0 after reform	0.000 (0.001)
1 pp subsidy cut: year 1 after reform	0.001 (0.002)
1 pp subsidy cut: year 2 after reform	0.003 (0.002)
1 pp subsidy cut: year 3 after reform	0.003 (0.003)
1 pp subsidy cut: year 4 after reform	0.003 (0.003)
1 pp subsidy cut: year 5 after reform	0.002 (0.004)
1 pp subsidy cut: year 6 after reform	0.004 (0.004)
1 pp subsidy cut: year 7 after reform	0.008* (0.005)
1 pp subsidy cut: year 8 after reform	0.010* (0.005)
1 pp subsidy cut: year 9 after reform	0.011* (0.006)
1 pp subsidy cut: year 10 after reform	0.010* (0.006)
N	726

Notes: Standard errors in parentheses. See Figure 5b for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.30: Event study estimates: treatment of other counties in the local labor market

	(1) log manufacturing employment
1 pp subsidy cut: year 4 before reform	0.000 (0.001)
1 pp subsidy cut: year 3 before reform	0.001 (0.001)
1 pp subsidy cut: year 2 before reform	0.000 (0.001)
1 pp subsidy cut: year 0 after reform	0.001 (0.001)
1 pp subsidy cut: year 1 after reform	0.001 (0.001)
1 pp subsidy cut: year 2 after reform	0.003** (0.001)
1 pp subsidy cut: year 3 after reform	0.003** (0.002)
1 pp subsidy cut: year 4 after reform	0.005** (0.002)
1 pp subsidy cut: year 5 after reform	0.006*** (0.002)
1 pp subsidy cut: year 6 after reform	0.006*** (0.002)
1 pp subsidy cut: year 7 after reform	0.007*** (0.002)
1 pp subsidy cut: year 8 after reform	0.007*** (0.002)
1 pp subsidy cut: year 9 after reform	0.008*** (0.002)
1 pp subsidy cut: year 10 after reform	0.009*** (0.003)
1 pp subsidy cut to other counties in local labor market: year 4 before reform	0.000 (0.001)
1 pp subsidy cut to other counties in local labor market: year 3 before reform	0.000 (0.000)
1 pp subsidy cut to other counties in local labor market: year 2 before reform	0.000 (0.000)
1 pp subsidy cut to other counties in local labor market: year 0 after reform	0.000 (0.000)
1 pp subsidy cut to other counties in local labor market: year 1 after reform	0.000 (0.000)
1 pp subsidy cut to other counties in local labor market: year 2 after reform	0.000 (0.001)
1 pp subsidy cut to other counties in local labor market: year 3 after reform	0.000 (0.001)
1 pp subsidy cut to other counties in local labor market: year 4 after reform	0.000 (0.001)
1 pp subsidy cut to other counties in local labor market: year 5 after reform	0.001 (0.001)
1 pp subsidy cut to other counties in local labor market: year 6 after reform	0.001 (0.001)
1 pp subsidy cut to other counties in local labor market: year 7 after reform	0.001 (0.002)
1 pp subsidy cut to other counties in local labor market: year 8 after reform	0.001 (0.001)
1 pp subsidy cut to other counties in local labor market: year 9 after reform	0.001 (0.001)
1 pp subsidy cut to other counties in local labor market: year 10 after reform	0.001 (0.002)
N	312503

Table C.31: Event study estimates: net commuting flow per employee and population

	(1) net commuting flow per employee	(2) log population
1 pp subsidy cut: year 4 before reform	0.002 (0.008)	0.000 (0.001)
1 pp subsidy cut: year 3 before reform	0.001 (0.006)	0.000 (0.000)
1 pp subsidy cut: year 2 before reform	0.001 (0.005)	0.000 (0.000)
1 pp subsidy cut: year 0 after reform	0.003 (0.003)	0.000 (0.000)
1 pp subsidy cut: year 1 after reform	0.003 (0.005)	0.000 (0.001)
1 pp subsidy cut: year 2 after reform	0.002 (0.008)	0.001 (0.001)
1 pp subsidy cut: year 3 after reform	0.005 (0.010)	0.001 (0.001)
1 pp subsidy cut: year 4 after reform	0.002 (0.014)	0.001 (0.001)
1 pp subsidy cut: year 5 after reform	0.001 (0.017)	0.002 (0.001)
1 pp subsidy cut: year 6 after reform	0.001 (0.020)	0.002 (0.001)
1 pp subsidy cut: year 7 after reform	0.002 (0.022)	0.002 (0.001)
1 pp subsidy cut: year 8 after reform	0.001 (0.023)	0.002 (0.002)
1 pp subsidy cut: year 9 after reform	0.000 (0.025)	0.002 (0.002)
1 pp subsidy cut: year 10 after reform	0.007 (0.028)	0.002 (0.002)
N	1045	1210

Notes: Standard errors in parentheses. See Appendix Figure A.11 for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.32: Event study estimates: manufacturing employment at the labor market with year fixed effects level

	(1)
	log manufacturing employment
1 pp subsidy cut: year 4 before reform	0.003 (0.002)
1 pp subsidy cut: year 3 before reform	0.002 (0.002)
1 pp subsidy cut: year 2 before reform	0.000 (0.001)
1 pp subsidy cut: year 0 after reform	0.000 (0.001)
1 pp subsidy cut: year 1 after reform	0.000 (0.002)
1 pp subsidy cut: year 2 after reform	0.002 (0.002)
1 pp subsidy cut: year 3 after reform	0.002 (0.003)
1 pp subsidy cut: year 4 after reform	0.002 (0.003)
1 pp subsidy cut: year 5 after reform	0.002 (0.004)
1 pp subsidy cut: year 6 after reform	0.004 (0.004)
1 pp subsidy cut: year 7 after reform	0.008* (0.005)
1 pp subsidy cut: year 8 after reform	0.010* (0.005)
1 pp subsidy cut: year 9 after reform	0.010* (0.006)
1 pp subsidy cut: year 10 after reform	0.010* (0.006)
N	726

Notes: Standard errors in parentheses. See Appendix Figure C.12 for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.33: Event study estimates: Bruhn (2018)

	(1)	(2)
	log manufacturing employment	log manufacturing employment
1 pp subsidy cut: year 4 before reform	0.000 (0.001)	0.001 (0.001)
1 pp subsidy cut: year 3 before reform	0.000 (0.001)	0.001 (0.001)
1 pp subsidy cut: year 2 before reform	0.000 (0.000)	0.001 (0.001)
1 pp subsidy cut: year 0 after reform	0.002*** (0.000)	0.001* (0.001)
1 pp subsidy cut: year 1 after reform	0.003*** (0.001)	0.002** (0.001)
1 pp subsidy cut: year 2 after reform	0.002** (0.001)	0.003*** (0.001)
1 pp subsidy cut: year 3 after reform	0.003** (0.001)	0.004*** (0.002)
1 pp subsidy cut: year 4 after reform	0.004*** (0.001)	0.006*** (0.002)
1 pp subsidy cut: year 5 after reform	0.007*** (0.002)	0.008*** (0.002)
1 pp subsidy cut: year 6 after reform	0.006*** (0.002)	0.008*** (0.003)
1 pp subsidy cut: year 7 after reform	0.007*** (0.002)	0.010*** (0.003)
1 pp subsidy cut: year 8 after reform	0.009*** (0.002)	0.011*** (0.003)
1 pp subsidy cut: year 9 after reform	0.010*** (0.003)	0.011*** (0.003)
1 pp subsidy cut: year 10 after reform	0.009*** (0.003)	0.012*** (0.004)
1 pp subsidy cut to other counties: year 4 before reform		0.000 (0.000)
1 pp subsidy cut to other counties: year 3 before reform		0.000 (0.000)
1 pp subsidy cut to other counties: year 2 before reform		0.000 (0.000)
1 pp subsidy cut to other counties: year 0 after reform		0.000 (0.000)
1 pp subsidy cut to other counties: year 1 after reform		0.000 (0.000)
1 pp subsidy cut to other counties: year 2 after reform		0.000 (0.000)
1 pp subsidy cut to other counties: year 3 after reform		0.000 (0.001)
1 pp subsidy cut to other counties: year 4 after reform		0.001 (0.001)
1 pp subsidy cut to other counties: year 5 after reform		0.001 (0.001)
1 pp subsidy cut to other counties: year 6 after reform		0.001 (0.001)
1 pp subsidy cut to other counties: year 7 after reform		0.002 (0.001)
1 pp subsidy cut to other counties: year 8 after reform		0.002 (0.001)
1 pp subsidy cut to other counties: year 9 after reform		0.002 (0.001)
1 pp subsidy cut to other counties: year 10 after reform		0.001 (0.001)
time polynomial	yes	yes
N	312503	312503

Notes: Standard errors in parentheses. See Appendix Figures C.13 and C.14 for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.34: Event study estimates: treatment of other counties within 100 kilometer

	(1) log manufacturing employment
1 pp subsidy cut: year 4 before reform	0.000 (0.001)
1 pp subsidy cut: year 3 before reform	0.001 (0.001)
1 pp subsidy cut: year 2 before reform	0.000 (0.001)
1 pp subsidy cut: year 0 after reform	0.001 (0.001)
1 pp subsidy cut: year 1 after reform	0.002 (0.001)
1 pp subsidy cut: year 2 after reform	0.004*** (0.001)
1 pp subsidy cut: year 3 after reform	0.004** (0.002)
1 pp subsidy cut: year 4 after reform	0.005*** (0.002)
1 pp subsidy cut: year 5 after reform	0.005*** (0.002)
1 pp subsidy cut: year 6 after reform	0.006*** (0.002)
1 pp subsidy cut: year 7 after reform	0.007*** (0.002)
1 pp subsidy cut: year 8 after reform	0.007*** (0.002)
1 pp subsidy cut: year 9 after reform	0.008*** (0.002)
1 pp subsidy cut: year 10 after reform	0.010*** (0.003)
10 pp subsidy cut to other counties located within 100 km: year 4 before reform	0.001** (0.001)
10 pp subsidy cut to other counties located within 100 km: year 3 before reform	0.001** (0.001)
10 pp subsidy cut to other counties located within 100 km: year 2 before reform	0.001** (0.001)
10 pp subsidy cut to other counties located within 100 km: year 0 after reform	0.000 (0.000)
10 pp subsidy cut to other counties located within 100 km: year 1 after reform	0.001 (0.000)
10 pp subsidy cut to other counties located within 100 km: year 2 after reform	0.001 (0.001)
10 pp subsidy cut to other counties located within 100 km: year 3 after reform	0.001 (0.001)
10 pp subsidy cut to other counties located within 100 km: year 4 after reform	0.000 (0.001)
10 pp subsidy cut to other counties located within 100 km: year 5 after reform	0.000 (0.001)
10 pp subsidy cut to other counties located within 100 km: year 6 after reform	0.000 (0.001)
10 pp subsidy cut to other counties located within 100 km: year 7 after reform	0.000 (0.001)
10 pp subsidy cut to other counties located within 100 km: year 8 after reform	0.000 (0.001)
10 pp subsidy cut to other counties located within 100 km: year 9 after reform	0.000 (0.001)
10 pp subsidy cut to other counties located within 100 km: year 10 after reform	0.000 (0.001)
N	312503

Table C.35: Event study estimates: treatment of other counties within 200 kilometer

	(1) log manufacturing employment
1 pp subsidy cut: year 4 before reform	0.000 (0.001)
1 pp subsidy cut: year 3 before reform	0.001 (0.001)
1 pp subsidy cut: year 2 before reform	0.000 (0.001)
1 pp subsidy cut: year 0 after reform	0.001 (0.001)
1 pp subsidy cut: year 1 after reform	0.001 (0.001)
1 pp subsidy cut: year 2 after reform	0.002** (0.001)
1 pp subsidy cut: year 3 after reform	0.003** (0.001)
1 pp subsidy cut: year 4 after reform	0.004*** (0.001)
1 pp subsidy cut: year 5 after reform	0.005*** (0.001)
1 pp subsidy cut: year 6 after reform	0.005*** (0.002)
1 pp subsidy cut: year 7 after reform	0.006*** (0.002)
1 pp subsidy cut: year 8 after reform	0.007*** (0.002)
1 pp subsidy cut: year 9 after reform	0.007*** (0.002)
1 pp subsidy cut: year 10 after reform	0.008*** (0.003)
10 pp subsidy cut to other counties located within 200 km: year 4 before reform	0.000 (0.001)
10 pp subsidy cut to other counties located within 200 km: year 3 before reform	0.000 (0.000)
10 pp subsidy cut to other counties located within 200 km: year 2 before reform	0.000 (0.000)
10 pp subsidy cut to other counties located within 200 km: year 0 after reform	0.001 (0.000)
10 pp subsidy cut to other counties located within 200 km: year 1 after reform	0.001 (0.000)
10 pp subsidy cut to other counties located within 200 km: year 2 after reform	0.000 (0.001)
10 pp subsidy cut to other counties located within 200 km: year 3 after reform	0.000 (0.000)
10 pp subsidy cut to other counties located within 200 km: year 4 after reform	0.000 (0.000)
10 pp subsidy cut to other counties located within 200 km: year 5 after reform	0.000 (0.000)
10 pp subsidy cut to other counties located within 200 km: year 6 after reform	0.000 (0.000)
10 pp subsidy cut to other counties located within 200 km: year 7 after reform	0.000 (0.000)
10 pp subsidy cut to other counties located within 200 km: year 8 after reform	0.000 (0.000)
10 pp subsidy cut to other counties located within 200 km: year 9 after reform	0.000 (0.000)
10 pp subsidy cut to other counties located within 200 km: year 10 after reform	0.000 (0.001)
N	312503

Table C.36: Event study estimates: trade spillovers to all German counties

	(1) log manufacturing employment
1% trade exposure to 1 pp subsidy cut: year 4 before reform	0.000005 (0.000007)
1% trade exposure to 1 pp subsidy cut: year 3 before reform	0.000012** (0.000006)
1% trade exposure to 1 pp subsidy cut: year 2 before reform	0.000006 (0.000005)
1% trade exposure to 1 pp subsidy cut: year 0 after reform	0.000010** (0.000005)
1% trade exposure to 1 pp subsidy cut: year 1 after reform	0.000014** (0.000006)
1% trade exposure to 1 pp subsidy cut: year 2 after reform	0.000025*** (0.000008)
1% trade exposure to 1 pp subsidy cut: year 3 after reform	0.000021** (0.000010)
1% trade exposure to 1 pp subsidy cut: year 4 after reform	0.000030** (0.000012)
1% trade exposure to 1 pp subsidy cut: year 5 after reform	0.000035** (0.000014)
1% trade exposure to 1 pp subsidy cut: year 6 after reform	0.000040** (0.000016)
1% trade exposure to 1 pp subsidy cut: year 7 after reform	0.000043*** (0.000015)
1% trade exposure to 1 pp subsidy cut: year 8 after reform	0.000048*** (0.000016)
1% trade exposure to 1 pp subsidy cut: year 9 after reform	0.000053*** (0.000016)
1% trade exposure to 1 pp subsidy cut: year 10 after reform	0.000074*** (0.000028)
N	2555361

Notes: Standard errors in parentheses. See Figure 5d for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.37: Event study estimates: trade spillovers only to West Germany

	(1) log manufacturing employment
1% trade exposure to 1 pp subsidy cut: year 4 before reform	0.000006 (0.000009)
1% trade exposure to 1 pp subsidy cut: year 3 before reform	0.000011** (0.000005)
1% trade exposure to 1 pp subsidy cut: year 2 before reform	0.000004 (0.000005)
1% trade exposure to 1 pp subsidy cut: year 0 after reform	0.000009* (0.000005)
1% trade exposure to 1 pp subsidy cut: year 1 after reform	0.000014** (0.000007)
1% trade exposure to 1 pp subsidy cut: year 2 after reform	0.000026*** (0.000009)
1% trade exposure to 1 pp subsidy cut: year 3 after reform	0.000022** (0.000011)
1% trade exposure to 1 pp subsidy cut: year 4 after reform	0.000032** (0.000014)
1% trade exposure to 1 pp subsidy cut: year 5 after reform	0.000037** (0.000016)
1% trade exposure to 1 pp subsidy cut: year 6 after reform	0.000040** (0.000018)
1% trade exposure to 1 pp subsidy cut: year 7 after reform	0.000039** (0.000016)
1% trade exposure to 1 pp subsidy cut: year 8 after reform	0.000044** (0.000017)
1% trade exposure to 1 pp subsidy cut: year 9 after reform	0.000051** (0.000018)
1% trade exposure to 1 pp subsidy cut: year 10 after reform	0.000073** (0.000032)
N	2098734

Notes: Standard errors in parentheses. See Appendix Figure C.16a for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.38: Event study estimates: import and export spillovers

	(1) log manufacturing employment
1% import exposure to 1 pp subsidy cut: year 4 before reform	0.000009 (0.000020)
1% import exposure to 1 pp subsidy cut: year 3 before reform	0.000025** (0.000011)
1% import exposure to 1 pp subsidy cut: year 2 before reform	0.000016* (0.000009)
1% import exposure to 1 pp subsidy cut: year 0 after reform	0.000012 (0.000009)
1% import exposure to 1 pp subsidy cut: year 1 after reform	0.000023* (0.000012)
1% import exposure to 1 pp subsidy cut: year 2 after reform	0.000037** (0.000015)
1% import exposure to 1 pp subsidy cut: year 3 after reform	0.000035* (0.000018)
1% import exposure to 1 pp subsidy cut: year 4 after reform	0.000051** (0.000023)
1% import exposure to 1 pp subsidy cut: year 5 after reform	0.000059** (0.000026)
1% import exposure to 1 pp subsidy cut: year 6 after reform	0.000062** (0.000030)
1% import exposure to 1 pp subsidy cut: year 7 after reform	0.000057* (0.000030)
1% import exposure to 1 pp subsidy cut: year 8 after reform	0.000068** (0.000033)
1% import exposure to 1 pp subsidy cut: year 9 after reform	0.000071** (0.000034)
1% import exposure to 1 pp subsidy cut: year 10 after reform	0.000089* (0.000052)
1% export exposure to 1 pp subsidy cut: year 4 before reform	0.000026 (0.000022)
1% export exposure to 1 pp subsidy cut: year 3 before reform	0.000011 (0.000016)
1% export exposure to 1 pp subsidy cut: year 2 before reform	0.000016 (0.000012)
1% export exposure to 1 pp subsidy cut: year 0 after reform	0.000025** (0.000011)
1% export exposure to 1 pp subsidy cut: year 1 after reform	0.000017 (0.000014)
1% export exposure to 1 pp subsidy cut: year 2 after reform	0.000035** (0.000017)
1% export exposure to 1 pp subsidy cut: year 3 after reform	0.000038* (0.000021)
1% export exposure to 1 pp subsidy cut: year 4 after reform	0.000035 (0.000026)
1% export exposure to 1 pp subsidy cut: year 5 after reform	0.000040 (0.000029)
1% export exposure to 1 pp subsidy cut: year 6 after reform	0.000055 (0.000033)
1% export exposure to 1 pp subsidy cut: year 7 after reform	0.000067** (0.000034)
1% export exposure to 1 pp subsidy cut: year 8 after reform	0.000064* (0.000036)
1% export exposure to 1 pp subsidy cut: year 9 after reform	0.000081** (0.000038)
1% export exposure to 1 pp subsidy cut: year 10 after reform	0.000120** (0.000056)
p-value of joint test of equality for all pre-reform effects	0.303
p-value of joint test of equality for all post-reform effects	0.858
N	2542484

Notes: Standard errors in parentheses. See Appendix Figure C.16b for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.39: Event study estimates: local business tax rate, tax revenues and tax base

	(1) log business tax rate	(2) log business tax revenues per capita	(3) log business tax base per capita
1 pp subsidy cut: year 4 before reform	0.001* (0.001)	0.004 (0.009)	0.003 (0.010)
1 pp subsidy cut: year 3 before reform	0.000 (0.001)	0.002 (0.007)	0.002 (0.007)
1 pp subsidy cut: year 2 before reform	0.000 (0.000)	0.005 (0.008)	0.006 (0.008)
1 pp subsidy cut: year 0 after reform	0.000 (0.000)	0.007** (0.003)	0.007** (0.003)
1 pp subsidy cut: year 1 after reform	0.000 (0.001)	0.005 (0.005)	0.005 (0.006)
1 pp subsidy cut: year 2 after reform	0.000 (0.001)	0.007 (0.008)	0.008 (0.009)
1 pp subsidy cut: year 3 after reform	0.000 (0.001)	0.000 (0.007)	0.001 (0.007)
1 pp subsidy cut: year 4 after reform	0.001 (0.001)	0.003 (0.008)	0.004 (0.009)
1 pp subsidy cut: year 5 after reform	0.002 (0.001)	0.005 (0.008)	0.003 (0.009)
1 pp subsidy cut: year 6 after reform	0.002 (0.001)	0.001 (0.009)	0.001 (0.009)
1 pp subsidy cut: year 7 after reform	0.003* (0.001)	0.006 (0.008)	0.009 (0.009)
1 pp subsidy cut: year 8 after reform	0.003** (0.001)	0.001 (0.009)	0.004 (0.009)
1 pp subsidy cut: year 9 after reform	0.004** (0.002)	0.006 (0.010)	0.010 (0.010)
1 pp subsidy cut: year 10 after reform	0.004** (0.002)	0.008 (0.010)	0.012 (0.011)
N	1210	1210	1210

Notes: Standard errors in parentheses. See Figure 5c and Appendix Figures C.17a and C.17b for detailed information. Statistical significance denoted as:
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.40: Event study estimates: local property tax rate, tax revenues and tax base

	(1) log property tax rate	(2) log property tax revenues per capita	(3) log property tax base per capita
1 pp subsidy cut: year 4 before reform	0.000 (0.001)	0.002 (0.003)	0.002 (0.003)
1 pp subsidy cut: year 3 before reform	0.000 (0.001)	0.003 (0.003)	0.003 (0.003)
1 pp subsidy cut: year 2 before reform	0.000 (0.001)	0.002 (0.003)	0.003 (0.003)
1 pp subsidy cut: year 0 after reform	0.001 (0.001)	0.001 (0.001)	0.001 (0.002)
1 pp subsidy cut: year 1 after reform	0.001 (0.001)	0.002 (0.001)	0.001 (0.002)
1 pp subsidy cut: year 2 after reform	0.000 (0.001)	0.002 (0.002)	0.002 (0.002)
1 pp subsidy cut: year 3 after reform	0.000 (0.001)	0.002 (0.002)	0.002 (0.002)
1 pp subsidy cut: year 4 after reform	0.002 (0.001)	0.002 (0.002)	0.001 (0.002)
1 pp subsidy cut: year 5 after reform	0.001 (0.001)	0.005* (0.002)	0.004 (0.003)
1 pp subsidy cut: year 6 after reform	0.001 (0.002)	0.004 (0.003)	0.003 (0.003)
1 pp subsidy cut: year 7 after reform	0.003* (0.002)	0.004 (0.003)	0.000 (0.004)
1 pp subsidy cut: year 8 after reform	0.004* (0.002)	0.005 (0.003)	0.001 (0.003)
1 pp subsidy cut: year 9 after reform	0.003* (0.002)	0.006 (0.004)	0.003 (0.004)
1 pp subsidy cut: year 10 after reform	0.004* (0.002)	0.006 (0.004)	0.002 (0.004)
N	1210	1210	1210

Notes: Standard errors in parentheses. See Figure 5c and Appendix Figures C.17a and C.17b for detailed information. Statistical significance denoted as:
 * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.41: Event study estimates: total employment

	(1) log total employment
1 pp subsidy cut: year 4 before reform	0.000 (0.001)
1 pp subsidy cut: year 3 before reform	0.000 (0.000)
1 pp subsidy cut: year 2 before reform	0.000 (0.000)
1 pp subsidy cut: year 0 after reform	0.000 (0.000)
1 pp subsidy cut: year 1 after reform	0.001* (0.000)
1 pp subsidy cut: year 2 after reform	0.001 (0.001)
1 pp subsidy cut: year 3 after reform	0.001 (0.001)
1 pp subsidy cut: year 4 after reform	0.001 (0.001)
1 pp subsidy cut: year 5 after reform	0.001 (0.001)
1 pp subsidy cut: year 6 after reform	0.001 (0.001)
1 pp subsidy cut: year 7 after reform	0.002 (0.001)
1 pp subsidy cut: year 8 after reform	0.002 (0.001)
1 pp subsidy cut: year 9 after reform	0.002 (0.001)
1 pp subsidy cut: year 10 after reform	0.002 (0.002)
N	3252514

Notes: Standard errors in parentheses. See Appendix Figure A.12 for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$