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DISCUSSION PAPER SERIES

IZA DP No. 14362

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Sebastian Siegloch
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Sebastian Siegloch
ZEW, University of Mannheim and IZA

Nils Wehrhöfer
University of Mannheim and ZEW

Tobias Etzel
Bundesbank

MAY 2021

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Direct, Spillover and Welfare Effects of Regional Firm Subsidies*

We analyze the effects of a large place-based policy, subsidizing up to 50% of investment costs of manufacturing firms in East Germany after reunification. We show that a 1-percentage-point decrease in the subsidy rate leads to a 1% decrease in manufacturing employment. We document important spillovers for untreated sectors in treated counties, untreated counties connected via trade and local taxes, whereas we do not find spillovers on counties in the same local labor market. We show that the policy is at least as efficient as cash transfers to the unemployed, but is more effective in curbing regional inequality.

JEL Classification: H24, J21, J23

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

Corresponding author:
Sebastian Siegloch
University of Mannheim
Department of Economics
L7, 3-5
68161 Mannheim
Germany

E-mail: siegloch@uni-mannheim.de

* The views expressed in this paper are those of the authors and do not necessarily represent those of the Deutsche Bundesbank or the Eurosystem. Siegloch acknowledges funding from the German Science Foundation (DFG grant #361846460). Wehrhöfer acknowledges support from the Leibniz Gemeinschaft (SAW Project "Regional Inequality in Germany"). We thank Maximilian von Ehrlich, Hans Peter Grüner, Nathan Hendren, Eckhard Janeba, Enrico Moretti, Kurt Schmidheiny, Tobias Seidel for valuable comments. We further thank seminar participants at the universities of Barcelona, Basel, Bern, Lugano, Mannheim, Milan, Nuremberg, Strasbourg, the Research Training Group "Regional Disparities" as well as conference participants at the ZEW Public Finance, Verein für Socialpolitik, SOLE-EALE, IIPF, UEA, NTA, CESifo Public Economics for valuable comments.

1 Introduction

In many countries and federations, place-based policies are a means to support regions that are economically lagging behind (Glaeser and Gottlieb, 2008). The European Union spent more than €350 billion – about a third of its budget – on regional policies during the funding period from 2014 to 2020 (Ehrlich and Overman, 2020). The U.S. currently devotes about $60 billion to place-based policies (Bartik, 2020, Slattery and Zidar, 2020) – mostly through business tax incentives. A recent wave of papers has demonstrated that place-based policies unfold positive economic effects on targeted regions (see Kline and Moretti, 2014b, Neumark and Simpson, 2015, Duranton and Venables, 2018, for further summaries of the literature). However, it is well-known that the overall welfare effects of place-based policies also depend on the indirect policy effects that go beyond direct effects on treated workers and firms in subsidized regions (Austin et al., 2018). We refer to these indirect effects as spillovers throughout the paper.

Spillovers may take various forms and signs. Positive agglomeration effects in subsidized places might induce relocation effects and negative agglomeration in unsubsidized places (Kline and Moretti, 2014a). There might also be relocation of factors between treated and untreated sectors in subsidized places, shaping local multiplier effects (Moretti, 2010). An increasing concentration of educated workers may unfold positive human capital spillovers (Glaeser and Gottlieb, 2008, Diamond, 2016). Local subsidies might be capitalized into housing prices leaving real wages unchanged (Busso et al., 2013, Austin et al., 2018). Local subsidies might also have intra-regional spillovers via trade flows (Blouri and Ehrlich, 2020). Local policymakers might respond to the (foregone) subsidies by adjusting local policy instruments (Ehrlich and Seidel, 2018). Finally, a successful local subsidy should have fiscal externalities on federal-level tax bases and social insurance systems (Austin et al., 2018). Most of the literature has discussed the welfare effects of place-based policies using structural spatial equilibrium models and putting a special emphasis on agglomeration spillovers (Kline, 2010, Kline and Moretti, 2014a, Gaubert, 2018, Fajgelbaum and Gaubert, 2020, Gaubert et al., 2021).

In this paper, we take a different perspective on spillover effects of place-based policies and their welfare implications. We provide cleanly identified reduced-form estimates of the direct and indirect effects of a prominent German place-based policy subsidizing investments of firms in distressed East German regions post reunification. 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. We then use the reduced-form evidence and calculate the efficiency costs of the policy using the recently proposed measure of the marginal value of public funds (MVPF) (Hendren and Sprung-Keyser, 2020, Finkelstein and Hendren, 2020), which provides an intuitive, yet comprehensive way to translate reduced-form behavioral effects into a welfare metric that accounts for additional fiscal spillovers on other sources of government budget such as the personal income tax or unemployment insurance. The MVPF framework benchmarks the welfare effects of a policy by comparing to other policies, which can be taken from the literature, such as place-blind cash transfers, or the same place-based policy ignoring spillovers. In a last step, we simulate the effects of the place-based policy on regional inequality for given efficiency cost and compare the distributional effects to related place-blind policies.

We study the case of the most prominent German place-based policy called GRW.[1] The GRW constitutes 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 and it was the main regional subsidy to revitalize the East German economy after reunification. The GRW's main instrument are investment subsidies for manufacturing firms in eligible regions. These subsidies can be used for purchasing new machines or building new production sites. The explicit goal of the policy – and a criterion to qualify – is to boost investment, and thereby creating new jobs and stimulating regional growth.

We combine official data on the universe of subsidy cases with administrative social security data on firms and workers to estimate the reduce-form effects of the policy, differentiating between the direct policy effects and various spillover effects across regions, sectors and to other local policies. 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 pre-determined economic performance. For each new policy regime, the measure of economic development is based on past performance measures, 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 income, employment dynamics and infrastructure amenities prior to treatment. Eligibility thresholds change across budgeting periods and these changes are partly triggered by EU legislation, which is exogenous to economic developments in East Germany.

We make use of 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, investment volume and amount in subsidies paid. In addition, we gathered regional data to replicate the indicators determining treatment status across all budgeting periods.

The main outcome of interest in our study is the effect of GRW subsidies on regional employment. Econometrically, we make use of event study designs to pin down the policy effects. However, we do not restrict our analysis to the overall effect of the policy on the treated regions, but also study the underlying mechanisms and spillovers. Explicitly, we analyze (i) intra-county sectoral spillovers by looking at non-treated industries in treated regions, (ii) cross-county regional spillovers by studying the effect on untreated counties within the same local labor market, (iii) trade spillover by looking

[1] The German name of the policy is *Gemeinschaftsaufgabe Verbesserung der regionalen Wirtschaftsstruktur* – throughout the paper, we will refer to it using the official German acronym GRW.

at counties with significant trade exposure to the treatment counties, and (iv) policy spillovers by looking at the policy effect on local tax rates. We then use our reduced form estimates to infer the welfare effects of the policy. More specifically, we make use of the novel framework proposed by Hendren and Sprung-Keyser (2020) and Finkelstein and Hendren (2020) to calculate the marginal value of public fund (MVPF) – that is the "bang for the buck" – of the policy. The MVPF explicitly takes into account fiscal spillovers on other tax bases and social insurance programs (Austin et al., 2018). Last, we simulate the policy's capacity to affect regional inequality and compare it to other place-blind transfers for given MVPF.

We derive the following three direct results for a one-percentage-point decrease of the subsidy rate in the treated manufacturing sector. First, subsidized investment decreases by 14.6% and total (i.e. subsidized plus unsubsidized) investment decreases by 6.7%. Second, in the long-run manufacturing employment decreases by 1%. We do not find asymmetric effects of subsidy cuts and increases. Third, wages are largely unaffected.

In terms of spillover effects, we derive the following results. First, a one percentage-point-decrease of 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. Second, there is no evidence for 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 requirements of municipalities and a decreasing tax base.

Last, in terms of welfare implications, we derive the following three results. First, we calculate a marginal value of public funds of 0.96, which is higher than the estimates of the MVPF of unemployment insurance and cash transfers, which target a similar set of beneficiaries (Hendren and Sprung-Keyser, 2020). Second, a simple back-of-the-envelope calculation shows that the cost per job were about 24,000€. Importantly, we show that both the cost per job and the marginal value of public funds are substantially downward-biased if one does not account for spillovers. Third, given the similarity in terms of the MVPF, we show in a simulation exercise that place-based policies are more effective in reducing regional inequality for given efficiency costs compared to cash transfers since place-based policies as more regionally targeted.

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. Our findings reinforce recent findings that place-based policies work – 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 beyond the program end 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 the ESF increase 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 the Iron Curtain from the 1970s to until reunification and find positive treatment effects. In terms of the GRW, Brachert et al. (2019) find no significant treatment effects, looking at West (instead of East) German regions, using different data and a different identification strategy. Our findings of a positive (negative) employment effect of a subsidy increase (decrease) is in line with descriptive, more policy-oriented papers in Germany (Bade and Alm, 2010, Bade, 2012). Analyzing the direct effect of local subsidies is naturally related to work looking at the effect of state and local taxes on workers and firms (see, e.g. Suárez Serrato and Zidar, 2016, Fuest et al., 2018, Fajgenbaum et al., 2019, Slattery and Zidar, 2020). Our work is also related to the large empirical literature studying the effects of place-blind industrial policies (focusing on broader sectors) (see, e.g. Aghion et al., 2015, Liu, 2019, Lane, 2020, Manelicia and Pantea, 2021).

Second, we systematically investigate indirect spillover effects of place-based policies by providing cleanly identified reduced-form estimates. While various empirical studies have looked at single spill-overs, this is – to the best of our knowledge – the first comprehensive analysis looking at various relevant spillovers discussed in the theoretical literature. In line with Criscuolo et al. (2019), we find no evidence of positive or negative regional spillovers on neighboring counties. However, we demonstrate important local demand effects as the untreated retail sector and construction sector are negatively affected by decreases in regional subsidies to manufacturing firms. We also point to trade spillover which have not been investigated in the literature before. In contrast to Moretti (2010), our local multiplier effects are somewhat smaller, which might be explained by the fact that subsidies are not targeted at high-tech, high-skill firms, but rather classic manufacturing firms. 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 welfare effects of (place-based) policies. We make use of the novel framework recently put forward by Hendren and Sprung-Keyser (2020) and Finkelstein and Hendren (2020) that enables us to transform our reduced-from quasi-experimental estimates into a welfare statement. We evaluate one of the first policies targeted at firms within this framework. 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 to estimate policy counterfactuals and is capable to capture general equilibrium effects of non-marginal policy changes, the MVPF framework – similar to the sufficient statistics approach – allows for a more immediate mapping between clean quasi-experimental evidence and its welfare implications (see Chetty, 2009, Kleven, 2021).

The remainder of this paper is organized as follows. We explain the institutional setting in Section 2, followed by Section 3 on the research design. Section 4 presents the data. Our empirical results are presented in Section 5. In Section 6, we discuss the welfare and inequality 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. Equivalent 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.[2]

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.[3] 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 policies (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.[4]

2.1 Eligibility

In order 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 investments 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.

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 opening 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

[2] Other policy measures targeted at plants in Eastern Germany included a capital investment bonus program (Investitionszulage), a non-discretionary capital subsidy targeted at entire Eastern Germany, and loans provided by KfW and the European Recovery Program. Our empirical strategy outlined below makes sure that we isolate the effect of the GRW. 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.

[3] The other important instrument 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.

[4] These numbers include co-payments by the European Union via the European Regional Development Fund (ERDF). Whether subsidies were paid for by the ERDF or GRW is irrelevant for the purpose of 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.

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, 74% of the GRW funds go to manufacturing firms. In Appendix Table B.6, we shows 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 excluded from the subsidies. These include the construction and retail sector 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 is unfortunately not available, survey data for the state of Thueringa 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.[5] 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 the GRW a place-based policy.

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.[6]

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 described by the following formula

$$indicator_r^{1997} = (infr_r^{1995})^{0.1} \times (wage_r^{1995})^{0.4} \times (unemp_r^{1995})^{0.5},$$

where $infr$ measures the quality of a county's infrastructure in 1995, *wage* represents per-capita

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

[6] Over the years, some counties in East German 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.

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	low	high	low	high	low	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.

earnings in 1995 and *unemp* measures the unemployment rate in 1995.[7] All counties were 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, counties with a value above the threshold as low funding priority. Counties with a high funding priority receive a higher subsidy rate.[8]

Importantly, indicators, 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, policy makers started to differentiate funding priorities spatially. Based on the performance indicator described above, 27 out of 76 counties were assigned 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). 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 Germany 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.[9] This particular reform is interesting for

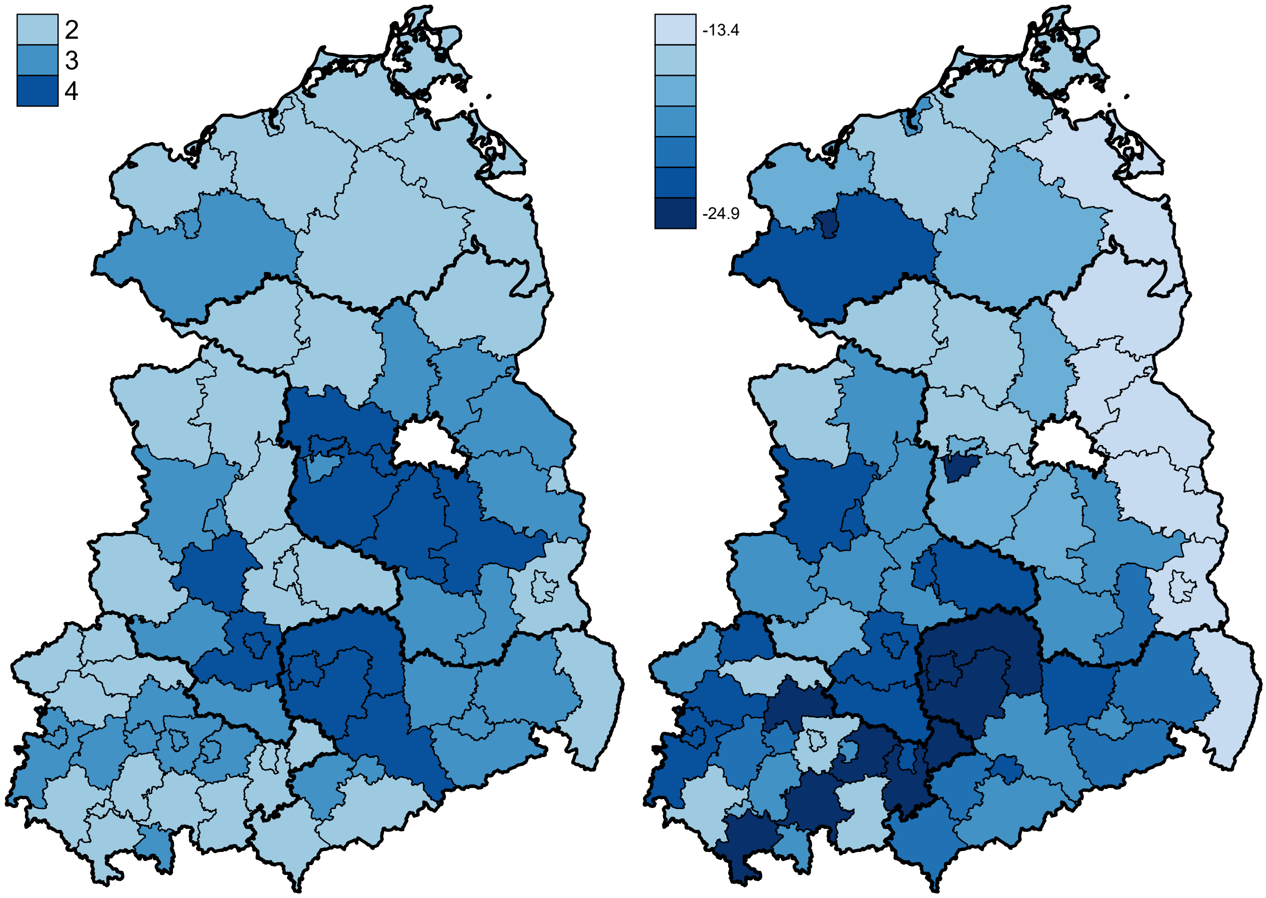
[7] The infrastructure sub-indicator is based on measures of accessibility of airports and larger cities by car or train, of the travelling time for trucks to the next trans-shipment center, the share of employees in applied research institutes, the share of apprenticeship training position, the share of employees in technical occupations, the share of high school graduates, capacity of inter-company training centers and population density.

[8] 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 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 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 head quarters of Volkswagen. Therefore, policy makers decided to assign Gifhorn to a higher priority even though the commuting zone index was too high (Brachert et al., 2019).

[9] The subsidy rate for small and large plants, which account for two thirds of manufacturing employment on average,

Figure 1: Map of reforms from 1996 to 2014

(a) number of reforms (b) difference in subsidy rate between 2014 & 1996



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

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 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 of 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 2017, subsidy rates were lowered a maximum of 35% for small plants (25% and 15% for medium and large plants) and in 2018, there was another cut of 5 percentage points.[10] 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. Therefore, these 9 counties were allowed higher subsidy rates throughout the whole period. 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

rose by 7 and 2 percentage points, respectively, while the rate for medium plants decreased by 3 percentage points.

[10] Three well-performing counties were directly downgraded to the 2018 level.

East German counties, which we exploit in our empirical research design presented in Section 3. 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 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 Research Design

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.

3.1 Empirical Model

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.

$$\ln y_{i,t} = \sum_{k=-4}^{10} \beta^k D^k_{c,t} + \xi X_{c,t} + \delta_i + \gamma_c + \psi_{s,t} + \varepsilon_{i,t}. \tag{1}$$

where $D^k_{c,t}$ is the mentioned set of event indicators indicating whether a change in the maximum subsidy rate occurred for the county $k \in [-4, ..., 10]$ periods ago. We refer to $D^k_{c,t}$ 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 $D^k_{c,t}$ are formally defined as

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

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 always 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. 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 both the county and plant level throughout.

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 $I^k_{c,t}$ of county c, year t and lead/lag $k \in [-4, 10]$ as

$$I^k_{c,t} = \Delta s^{small}_{c,t-k} \omega^{small}_c + \Delta s^{med}_{c,t-k} \omega^{med}_c + \Delta s^{large}_{c,t-k} \omega^{large}_c. \quad (3)$$

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

$$\omega^p_c = \frac{E^p_{c,1995}}{E^{small}_{c,1995} + E^{med}_{c,1995} + E^{large}_{c,1995}} \quad \forall f \in [small, med, large].$$

$E^f_{c,t}$ denotes the number of workers in manufacturing plants of size f in county c at time t. Weights ω^p_c are time-invariant and calculated in the data year 1995, hence prior to the first reform.[11]

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

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

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.

3.2 Identification and Sensitivity

The classical identification check in event study designs is to assess whether pre-treatment effects are statistically different from zero. Nevertheless, even flat pre-trends might not be sufficient to interpret the estimates causally. The key remaining threat to identification is omitted variable biases concurrent with treatment timing. While plant and county fixed effects control for time invariant confounders at the respective levels, state-by-year fixed effects flexibly account for any confounding shock occurring at the state-level. However, if the concurrent and confounding shock is at the county-level, estimates would still be biased.

The prime suspect in our context is local economic performance as subsidy rates are a function of past regional economic performance (see Section 2.2). The better the county performed economically in the past, the higher the probability of a subsidy rate cut. Note that such differences in past economic development should, however, show up in the pre-treatment effects and we would expect pre-treatment effects increasing from below zero. If we expect that a cut in the subsidy rate hurts the

[11] We drop year 1995 from the data after calculating the shares.

local economy, this relationship would bias our estimates towards zero.

Improving comparability. Given our institutional setup, we can further improve the comparability of 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: $\mathbb{S}^{M} = \bigcap_R \mathbb{S}^{M,R}$. We can then refine our empirical model in equation (4) by restricting the underlying estimation sample to counties in $\mathbb{S}^{M}$:

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

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.

Heterogeneous treatment effects. With homogeneous treatment effects, 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, 2020a,b, Callaway and Sant'Anna, 2020, Sun and Abraham, 2020, Borusyak et al., 2021). Several new estimators have been proposed to get unbiased estimates when treatment effects are not homogeneous. However, all these estimators are not valid for environments with multiple events for the same unit. In order to test for potential biases due to heterogeneous treatment effects, we cut our sample in 2006 and focus on the first three regimes since the reform in 2007 treats all counties (see Table 1). This yields a sample where every unit is treated at-most once and we retain a group of never-treated units. We apply the estimators developed in de Chaisemartin and D'Haultfoeuille (2020a) and Sun and Abraham (2020) to our basic dummy variable specification described in equation (1).[12] Notice that the two estimators use different control groups since Sun and Abraham (2020) only allow comparisons to never-treated units, whereas de Chaisemartin and D'Haultfoeuille (2020a) are also using not-yet treated units as controls. We find that our estimates are unlikely to be driven by heterogeneous treatment effects.

Controlling for observables. As another test, we include county-level controls that control for local business cycle effects. 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 β^k estimates from potential bias. Estimates are hardly affected and as expected,

[12] Note that in our setup without covariates and with never-treated units, the estimators from Sun and Abraham (2020) and Callaway and Sant'Anna (2020) coincide.

if anything, slightly more negative. In another check, we include the contemporaneous values of the business cycle covariates – ignoring even more the obvious bad control problem. Effects are again very similar. Last, we also use the business cycle variables as outcomes and test whether we find significant pre-treatment effects pointing to an identification concern. We find flat pre-trends.

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 and find no spillovers.

Symmetry. We estimate a model that explicitly differentiates between subsidy cuts and increases to test for symmetry. Note that we are mostly observing subsidy cuts, but the peculiar reform of 2007 enables us to separately study subsidy increases.

Sensitivity. Apart from these identification tests, we run several sensitivity checks to make sure that modelling choices are not driving our results. First, we implement the basic dummy variable event study specification of equation (1) which ignores the size of the subsidy changes. Second, we drop the few counties that – for various reasons discussed in Section 2 – were only partially treated. Fourth, we vary the event window between nine, ten and eleven lags. Last, we estimate our model in first differences instead of with fixed effects. In none of these checks, results change in a meaningful way.

3.3 Extensions to Test for Spillovers

One contribution of our paper is to systematically look at 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 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 case local demand or agglomeration effects radiate 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. The difference between the estimate at the county level and the estimate at the local labor market level gives an indication of regional spillovers. Note that there is some 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.

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 other counties that have significant trade exposure to the treatment counties also respond to the subsidy cuts. First, we take the imports (measured in tons per year) of county c coming from treatment county g with $c \neq g$ and divide them by the total imports of county c. Equivalently, we calculate the share of exports that are exported from county c to treatment county g. Then, let the trade exposure of county c in year t to a reform that happened l years ago be defined as:

$$\text{trade exposure}^{l}_{c,t} = \sum_{g \neq c} \frac{\text{imports}_{cg}}{\text{total imports}_{c}} \left[D^{l}_{g,t} \cdot I^{l}_{g,t} \right] + \sum_{g \neq c} \frac{\text{exports}_{cg}}{\text{total exports}_{c}} \left[D^{l}_{g,t} \cdot I^{l}_{g,t} \right] \tag{6}$$

where $D^{l}_{g,t}$ and $I^{l}_{g,t}$ are defined as above. To test for trade spillovers, we include the trade exposure measure in our model.

$$\ln y_{i,t} = \sum_{k=-4}^{10} \beta^{k} \left[D^{k}_{c,t} \cdot I^{k}_{c,t} \right] + \sum_{l=-4}^{10} \beta^{l}_{trade} \text{trade exposure}^{l}_{c,t} + \delta_i + \gamma_c + \psi_{s,t} + \varepsilon_{i,t} \tag{7}$$

where β^{l}_{trade} represents the effect on plants with trade exposure to a one-percentage-point subsidy cut l years ago.

4 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.

4.1 Subsidy Data

We make use of 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 in fact 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.

4.2 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 fifty percent 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 make use of the IAB's Sample of Integrated Labour Market Biographies (SIAB) from 1996 to 2014. The dataset is a 2% sample of individual earnings biographies and includes individual characteristics as well as employer information from the BHP.[13] 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 for manufacturing workers, non-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.

4.3 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 location at the municipal level. We use that information to restrict our sample to manufacturing plants and locate plants within in the current county borders.

4.4 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 3.3. The data include a complete matrix of trade flows between all German counties as well as foreign countries for the year 2010. 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.

4.5 Other Regional Variables

Last, we make use of 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. In order 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 it to calculate the respective tax base.

In addition, we gather data on municipality-level grants and subsidies in order 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-form 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.

[13] 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.

Last, we add on housing prices, to assess whether the GRW subsidies are capitalized into housing prices. In order to populate our long panel starting in the 1990s, we use house price data from the German real estate association IVD. These data cover the largest city within a county.[14]

5 Empirical Results

In this section, we present the reduced form effects of the place-based policy. Subsection 5.1 focuses on direct policy effect of a subsidy cut for manufacturing plants in treated counties. In subsection 5.2, we address various identification challenges and demonstrate that our main effects are robust. Subsection 5.3 sheds light on the various spillover effects of the GRW.

5.1 Direct Policy Effects

Investment effects. In a first step, 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. In line with this finding, we also see that log subsidized investment decreases in a very similar manner. The total investment volume subsidized by the GRW decreases by 14.6% ten years after the reform. 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 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 6.7% 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, the AFiD data only contain plants with 20 or more employees.[15] Second, 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. Unfortunately, we cannot disentangle these effects without strong assumptions.

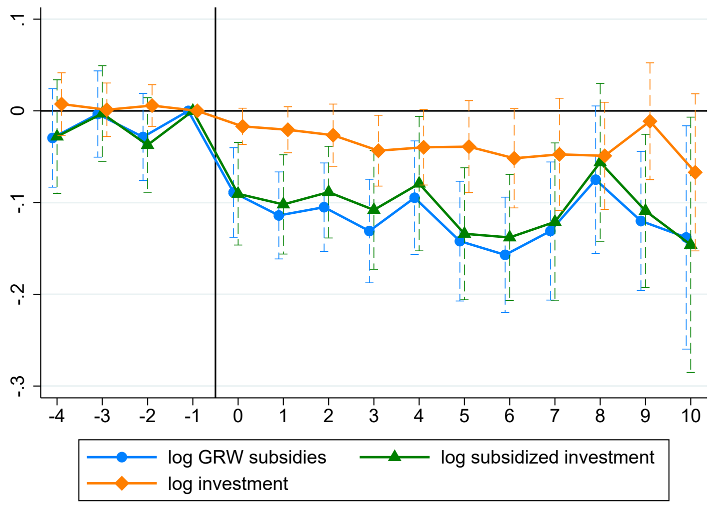
Employment effects. We now move to our main outcome, the employment effect of the GRW policy. Consistent with the finding of a decrease in investments, Figure 3 shows that cuts in the subsidy rate significantly reduce plant-level manufacturing employment. While pre-trends are flat, our estimates imply that a one-percentage-point decrease in the maximum subsidy rate leads to a decrease in manufacturing employment of 1% after ten years for our baseline sample.[16] These

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

[15] The AFiD is the largest and only administrative microdata set on plant-level investment in Germany.

[16] 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.

Figure 2: Event study estimates: subsides 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). Clustering of standard errors is at the county and plant level. See Tables C.1 and C.2 for the point estimates.

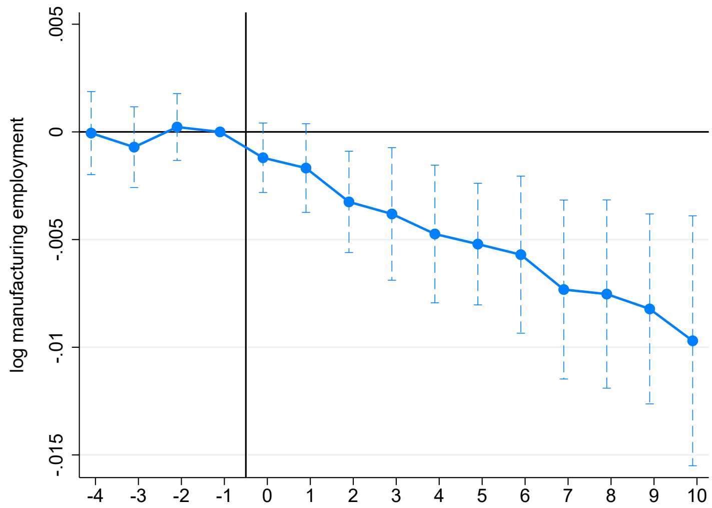
estimates are quantitatively similar to the main finding of Criscuolo et al. (2019). We find that the decrease in employment is mostly driven by medium-skilled workers, which make up 80% of all manufacturing workers, whereas employment of low- and high-skill workers decreases to a lesser extent (see Appendix Figure A.2). Thus, these results do not speak in favor of human capital spillover 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 establishments on the county level. Appendix Figure A.3 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.3 is quantitatively very similar to the plant-level effect in Figure 3.

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. In terms of total employment, however, we find little evidence that the aggregate effects on non-manufacturing plants are particularly strong. As a result, total employment goes down by only 0.2% (Appendix Figure A.4). Nevertheless, the aggregate effect on non-manufacturing employment masks interesting spillovers on certain industries, which we discuss in Section 5.3 below.

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.5). This suggests that the laid-off workers mostly transitioned to unemployment. Consistent with that, there is no effect on the size of the labor force (see Appendix Figure A.5). GDP per capita at the county-level also drops, but the effect is not significant (see Appendix Figure A.6).

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). Clustering of standard errors is at the county and plant level. See Table C.3 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 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). Wages in non-treated sectors and overall wages do not respond significantly to the subsidy cut either (see Appendix Figure C.3a). Results are similar when using average wages instead of median wages (see Appendix Figure C.3b).

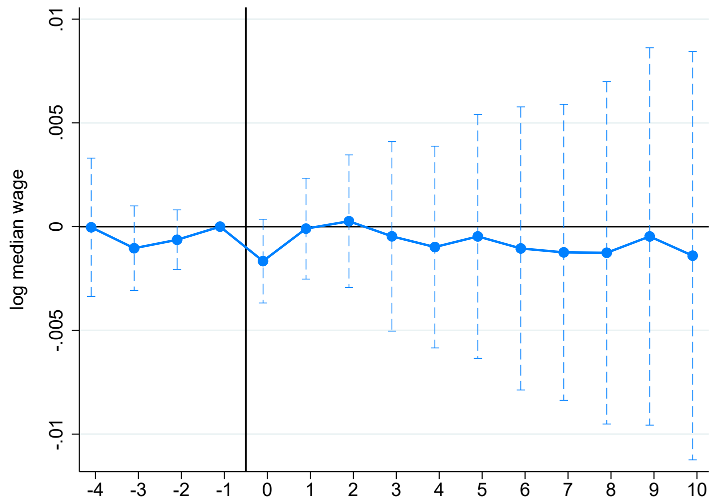
5.2 Identification and Sensitivity Checks

In the following, we present various tests demonstrating the robustness of our main results. The rationale behind the different checks is discussed in Section 3.2.

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). Reassuringly, the inclusion has little effects on the results, 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

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). Clustering of standard errors is at the county level. See Table C.9 for the point estimates.

(see Appendix Figures A.5 and A.6).

Other subsidies. We also test the effect of the GRW reforms on other subsidies received by municipalities. Figure C.5 shows that the reforms did not have a significant effect on other subsidies received by municipalities.

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 (see Section 2.2), led to an increase in subsidy rates for roughly half of the East German counties. 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 neither reject the null hypothesis that any individual post-treatment effect is asymmetric nor the joint test of asymmetry (p-value = 0.213).

Heterogeneous treatment effects. To test whether heterogeneous treatment effects are biasing our results, we apply the estimators by de Chaisemartin and D'Haultfoeuille (2020a) and Sun and Abraham (2020) to our baseline dummy variable model described in equation (1). We stop our sample in the year 2006 to have a set-up with a maximum of one treatment per county and retain a group of never-treated units. To ensure a 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 with regard to modelling choices. Last, we provide a set of checks that assess the sensitivity of our findings with regard to modelling choices we make when setting up our baseline. 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.6a shows, results are very similar when comparing our baseline model and the dummy-variable specification scaled by the average cut.

Second, 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.6b).

Third, we vary the number of lags of our event window between nine and eleven years. As Appendix Figure C.7a shows, the effects tend to level off after ten years. Last, Appendix Figure C.7b shows our baseline results estimated both in a fixed effect and first difference model. Size and pattern are again very similar.

5.3 Spillover Effects

While we have established a clean and robust direct policy effect, we investigate various potential spillover effects in the following subsection.

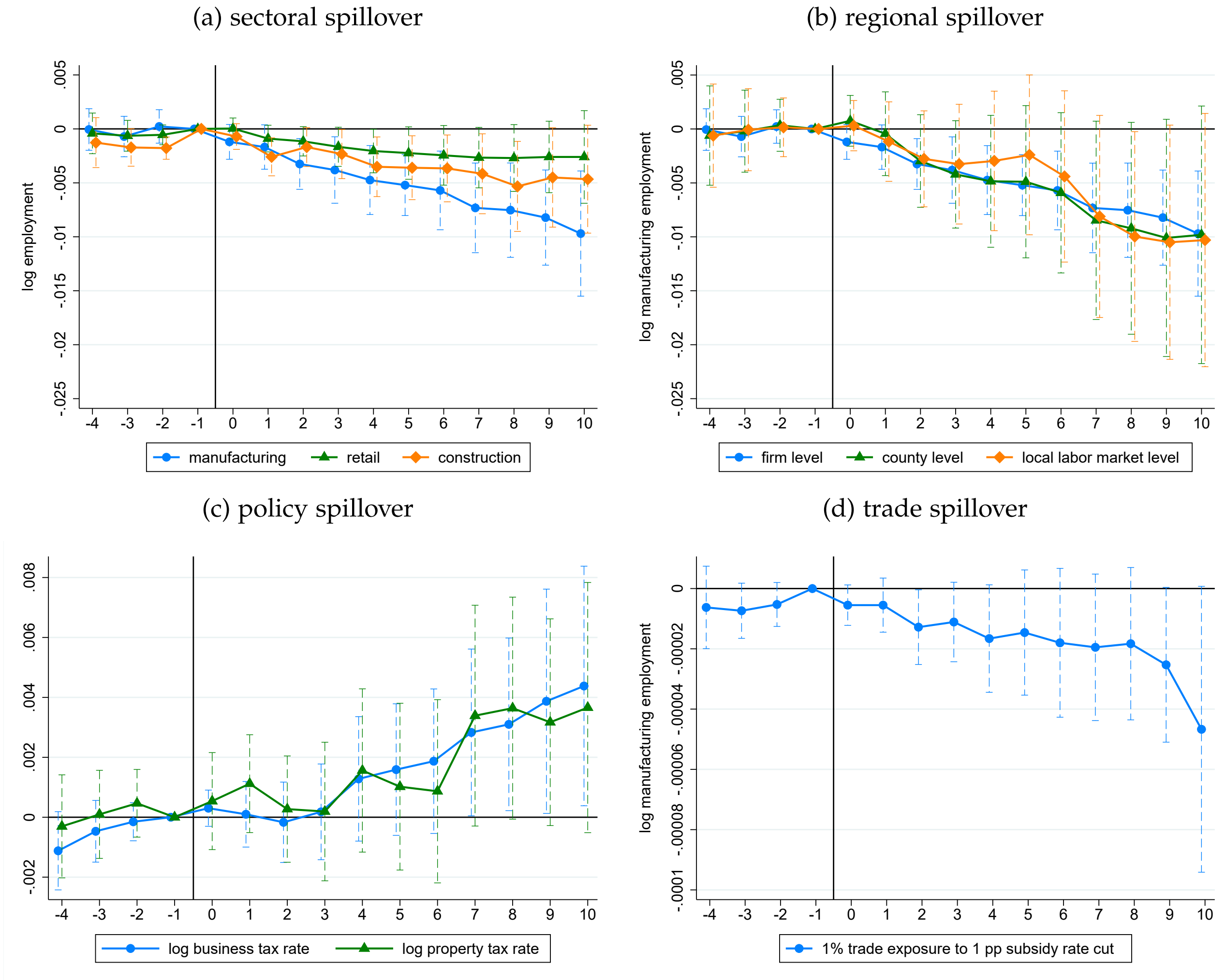
Intra-county spillovers. First, we check whether the place-based policy had an effect on untreated industries in treated counties. Above, we have shown that non-manufacturing employment only responds marginally to the cut in subsidy rates, resulting in a small and imprecise aggregate employment effect. Decomposing non-manufacturing employment into finer industries, however, we do find some evidence of intra-county sectoral spillovers. More specifically, we look at the retail and construction sector which were de jure excluded from receiving GRW subsidies allowing us to pinpoint spillover effects (see Appendix Table B.6). Figure 5a shows (positive) spillover effects for the untreated retail and construction sector.[17] A cut in subsidy rates leads to an immediate decreases 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 0.64 [0.16,1.87] additional jobs lost in retail and construction sectors. This is a somewhat lower estimate of local spillover than Moretti (2010) finds for US cities. A likely reason for this divergence is that the GRW subsidy is paid to traditional manufacturing firms rather than firms that rely heavily on highly-skilled workers.

We also test whether subsidy rate changes are capitalized in house prices. If an increase in the subsidy rate would lead to increased house prices, the distributional impact of the policy would change with (pre-existing) home owners being main beneficiaries. As Appendix Figure A.9 shows, we do not find any effect on house prices.

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

[17] We define a positive spillover as going into 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). Clustering of standard errors is at the county and plant level. See Table C.22 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.24 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). Clustering of standard errors is at the county level. See Tables C.26 and C.27 for the point estimates. Panel (d) plots coefficients along with 95% confidence intervals of a regression as in equation (7) using log manufacturing employment at the plant level as the outcome. The sample includes all German counties. Clustering of standard errors is at the county and plant level. See Table C.28 for the point estimates.

treatment effect on manufacturing employment at the labor market level is very similar to our baseline at the plant and county level implying 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.10).

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

Overall, the results on policy spillovers imply that businesses in treatment counties do 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.8a). 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.8b).

Trade spillovers. Last, we assess whether cuts in the GRW affected untreated counties that were connected to treated counties via trade flows using the empirical model specified in equation (7). We differentiate between import and export exposure to treatment counties. Figure 5d shows that a 1% trade exposure to a 1 percentage-point-decrease in the subsidy rate reduces manufacturing employment by 0.005%. This is 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.

6 Discussion: Efficiency and Inequality Effects

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

6.1 Efficiency Assessment

To asses the efficiency of the GRW, we calculate the marginal value of public funds (Hendren and Sprung-Keyser, 2020). The measure relates marginal benefits of the policy to its marginal costs by taking the ratio of the willingness to pay (WTP) of all beneficiaries of an incremental change in a government policy to the net costs of the policy change:

$$\text{MVPF} = \frac{\text{Willingness to pay}}{\text{Net government costs}} \tag{8}$$

The willingness to pay aggregates the real economic effects of the policy, including the WTP of direct beneficiaries (workers in directly treated plant) as well as additional beneficiaries due to spillovers. Net government costs comprise both the direct program spending and the fiscal externalities caused by the policy, for instance changes in income tax revenues triggered by changes in wages and/or employment.

Since this approach does not rely on assumptions about welfare weights, knowing the MVPF alone is in most cases not informative about the question of whether or not the policy should be implemented.[18] Instead, the MVPF unfolds its potential when comparing across policies that target similar groups of recipients. In this case, the MVPF is informative of which policy can achieve the same goal at a lower cost (Finkelstein and Hendren, 2020). Hence, we compare the MVPF of the GRW with MVPFs for other policies that target a similar group of recipients such as welfare cash transfer or unemployment benefits. Moreover, we can make an internal comparison comparing the the GRW MVPFs with and without accounting for spillover effects.

We consider the policy experiment of increasing the subsidy rate by one percentage point and use our reduced form estimates to calculate the resulting effects. We consider this experiment for two reasons. First, we have a direct mapping between our reduced-form estimates and the MVPF formula. Second, calculating the willingness to pay for marginal policy changes is more straightforward than for large reforms (Finkelstein and Hendren, 2020, Kleven, 2021).

For the willingness to pay, we consider the following effects: (i) the increase in net earnings due to newly created manufacturing jobs, (ii) the increase in net earnings due to positive spillover effects to the retail and construction sector, (iii) the increase in net earnings due to newly created manufacturing jobs via trade spillover, and (iv) the decrease in unemployment benefits payments for newly hired workers.[19]

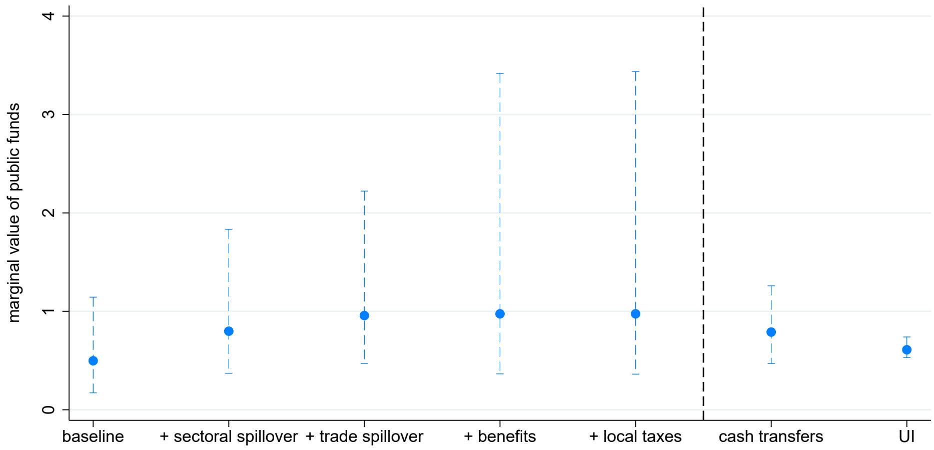
Since we do not find evidence for regional spillover, we use our plant-level estimates to calculate the number of jobs created. We multiply the baseline estimates of a 1% increase with the average number of manufacturing jobs in East Germany in our sample period to get the number of manufacturing jobs created. We then multiply that number with the average manufacturing wage in East Germany obtained from the SIAB in our sample period to get the increase in gross earnings. We iterate this procedure for all estimates from year 0 after the reform to year 10 after the reform assuming a discount rate of 3%.[20] We subtract additional income taxes by applying the average income tax rate to obtain net labor earnings. We conduct this procedure for manufacturing, retail and construction jobs. Next, for the trade spillover, we adjust the coefficients for import exposure by the average import exposure in our sample and compute the number of additional jobs by multiplying the adjusted coefficient with the average number of manufacturing jobs in Germany as a whole. The number of jobs is then multiplied with average manufacturing wage in Germany and income taxes are subtracted. Last, we calculate the number of unemployed using our unemployment estimate and multiply it by the average unemployment benefits in our sample period. This yields an estimate of the unemployment benefits that individuals forgo by being employed.

[18] If net government costs are negative, the policy pays for itself and the policy should always be implemented as long as it the WTP is greater than zero.

[19] The effect on manufacturing wages is very small and noisy. It does not affect the MVPF estimate but inflates standard errors (see Appendix Figure C.9a). Therefore, we do not include it in our baseline.

[20] Our estimates are virtually unchanged when we vary the discount rate (see Appendix Figure C.9b).

Figure 6: Marginal value of public funds



Source: own calculations, Hendren and Sprung-Keyser (2020) *Notes:* Confidence intervals are based on 9,999 bootstrap draws.

For net government costs, we consider the following items: (i) direct program spending, i.e. the costs of the GRW subsidies, (ii) an increase in income taxes paid due to the increase in net earnings, (iii) a decrease in unemployment benefits paid due to the decrease in unemployment, which also reduces the net costs, and (iv) the changes in local business and property tax revenues.

For direct program costs, we make use of our estimate on the subsidies paid out in response to a one-percentage-point change in the subsidy rate multiplied by the average subsidies paid out. We then subtract the increase in income taxes of manufacturing, construction and retail workers as well as the reduced spending on unemployment benefits. We calculate 95% confidence intervals using the bootstrapping algorithm suggested by Hendren and Sprung-Keyser (2020).

We start by focusing on the direct policy effect, i.e. the policy-induced changes in manufacturing jobs. The estimated behavioral responses yield an aggregate willingness to pay of €0.784 billion and net government costs of €1.594 billion. As Figure 6 shows, the resulting marginal value of public funds is 0.50 [0.17,1.14]. Next, we add spillover effects on other sectors, which increases the willingness to pay to €1.214 billion and reduces the net government costs to €1.520 billion. Hence, sectoral spillovers increase the marginal value of public funds to a value of 0.80 [0.37,1.83]. Next, we add the effect of trade spillovers, which increases the willingness to pay to €1.141 billion and reduces the net costs to €1.484 billion. This increases the marginal value of public funds to a value of 0.96 [0.47,2.22]. This shows in turn that disregarding spillover effects of place-based policies can lead to substantially biased welfare conclusions. Last, we add the effect on unemployment benefits and local tax revenues. These additions hardly change the marginal value of public funds (see Figure 6). However, it decreases precision since the unemployment and local tax revenue effects are rather noisy.[21]

After having demonstrated the importance of accounting for spillovers, we compare the MVPF of the place-based policy to the MVPF of other policies targeting a similar group of recipients. The average East German worker in the manufacturing sector is 40 years old. We select unemployment insurance and cash transfers as they target individuals of similar ages (30-40 years old) (Hendren and

[21] As discussed in Section 2, the European Regional Development Fund (ERDF) covers some of the direct policy costs. Adopting a purely national perspective and ignoring these direct costs would increase the total MVPF, including spillovers, to 1.16.

Sprung-Keyser, 2020). Place-based policies aim at saving and creating jobs and thereby stabilizing incomes. Unemployment insurance and cash transfers (welfare benefits) come in when jobs have been lost. We take the estimates for unemployment insurance and cash transfers from the original contribution by Hendren and Sprung-Keyser (2020). Unemployment insurance and cash transfers have an average marginal value of public funds of 0.61 and 0.79, respectively, which are similar, but smaller than the GRW – in particular when accounting for the GRW's positive spillover effects. For comparison, we also calculate the MVPF for the top marginal income tax rate cuts, finding values between 1.1 (assuming a low elasticity of taxable income of 0.1) and 2.2 (assuming a higher ETI of 0.5 as in Doerrenberg et al. (2017)).[22]

Cost per Job. As an alternative to the MVPF, we compute the cost per job created, another standard measure of the cost effectiveness of a policy. This metric has the drawback that it neglects other effects, such as workers forgoing unemployment benefits. Nevertheless, the measure is easy to interpret and allows a comparison with estimates from the previous literature. To calculate the cost per job, we take the estimate of the number of jobs created in manufacturing and other sectors as well as the direct government costs from our marginal value of public funds exercise and take the quotient. Appendix Figure A.11 shows the results for three scenarios. The cost per job is relatively high at €44,412 [€17,446,€114,028] if one neglects all spillover effects. Including sectoral spillover effects substantially reduces the costs per job to €27,113 [€10,250,€50,490] since both the number of jobs increase as well as the net government costs decrease. Accounting for trade spillover causes the estimate to decrease even further to €24,194 [€9,680,€50,981].

6.2 Implications for Regional Inequality

In a last step, we investigate the effectiveness of the GRW to achieve its politically stated goal, i.e. to reduce regional inequality. In a first step, we calculate the coefficient of variation of the county-level labor income a measure of regional inequality as suggested in Ehrlich and Overman (2020). We calculate the labor income per capita in county c and year t from the BHP and SIAB data as follows.

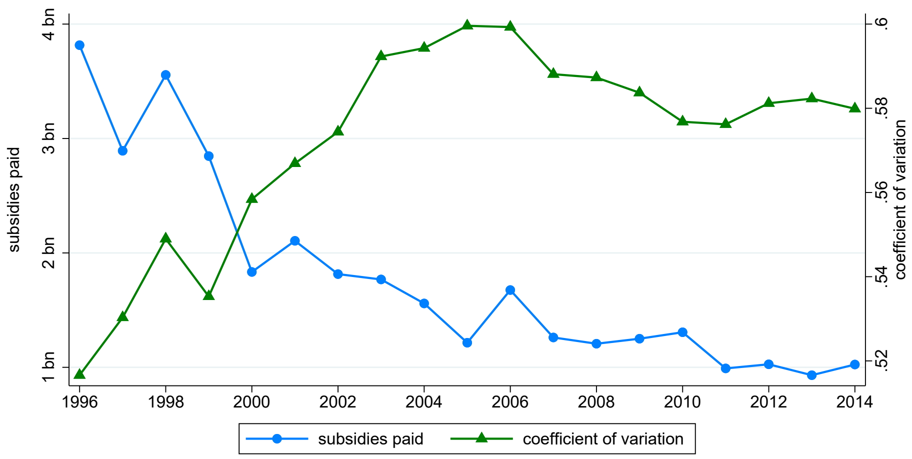
$$\text{labor income per capita}_{ct} = \frac{1}{N_{ct}} \sum_{s \in S} jobs_{cst} \cdot wage_{cst} \tag{9}$$

where N_{ct} denotes population in county c and year t and s stands for sector from set $S = \{\text{manufacturing, retail, construction, other}\}$. Moreover, $jobs_{cst}$ and $wage_{cst}$ are the number of jobs and the average wage in county c, sector s and year t, respectively. 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 (see Figure 7).

We are going to investigate how much of the increase in regional inequality could potentially be reversed by increasing the subsidy rate for low-income counties. Clearly, we cannot causally link the decrease in the generosity of the GRW to changes in inequality, but we can approximate its potential

[22] We apply the formula $\text{MVPF} = \frac{1}{1-\frac{\tau}{1-\tau}*\alpha*ETI}$ from Hendren and Sprung-Keyser (2020). The top marginal tax rate τ in Germany is 0.42 and is paid in incomes above 57,000€ and we set $\alpha = 1.5$ for labor income following Hermle and Peichl (2013).

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 labor income per capita as calculated in equation (9).

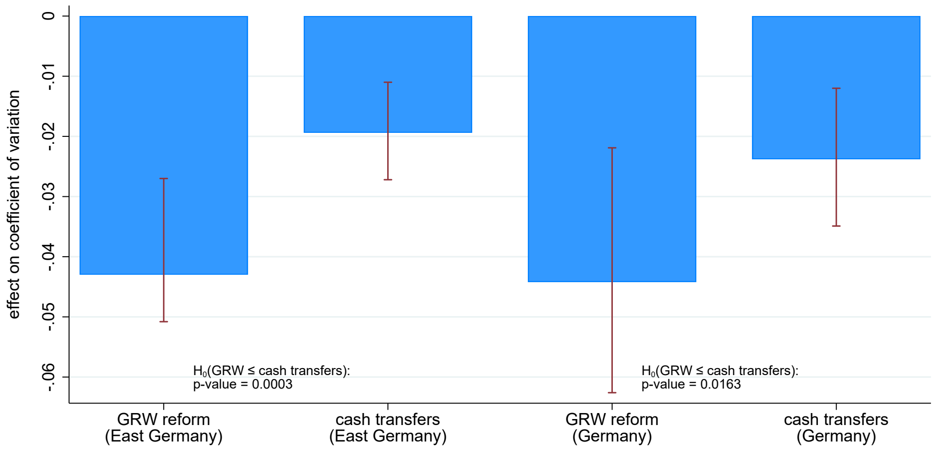
to mitigate the increase by extrapolating from our causal reduced-form evidence derived in Section 5 and using regional distributions of jobs and wages per sector in 2014. We simulate the GRW effect on regional inequality under various counterfactuals. In our baseline counterfactual, we increase the subsidy rate of the bottom 20% counties (15 in total) in the East German labor income distribution to their 1996 level.[23] This corresponds to a 21 percentage point increase of the subsidy rate for these 15 counties on average. We apply the same methodology as in the previous sections to calculate the additional manufacturing, retail and construction jobs from year 0 to year 10. We also calculate the number of jobs created through trade linkages in all East German counties by multiplying their import exposure to the treatment counties with our trade spillover estimate. In order to calculate the counterfactual regional dispersion under this regime, we modify equation (9) to account for these additional jobs in the treated counties. Comparing this counterfactual regional dispersion to the actually observed regional inequality in 2014 indicates by how much an increase in the GRW could curb regional inequality. We calculate 95% confidence intervals by bootstrapping the procedure. As Figure 8 shows, expanding the GRW in this way reduces the coefficient of variation in 2014 by 0.0430 [0.0270,0.0508], or about 7%.

Next, we simulate the counterfactual of a revenue-neutral, place-blind policy and calculate its impact on regional inequality. To that end, we uniformly increase welfare payments to every welfare recipient, independent of location. First, we calculate the net cost of the GRW policy by dividing the willingness to pay for the policy by the marginal value of public funds that we calculated in Section 6.1, yielding total costs of €0.681 billion. Taking into account that increased payments to the unemployed could have indirect costs on the government budget, for example by reducing labor supply, we need to adjust the amount spend. In our baseline, we are conservative and assume that the efficiency cost of the transfers are equal to the place-based policy even though the MVPF of cash transfers provided by Hendren and Sprung-Keyser (2020) is somewhat lower.[24] This yields

[23] We also repeat the same exercise for the bottom 15% and 25% of the labor income distribution (see Appendix Figures C.10a and C.10b). The resulting patterns are very similar to our baseline scenario.

[24] In Appendix Figure C.11a, we apply the MVPF of 0.79 that Hendren and Sprung-Keyser (2020) find, which further strengthens our results.

Figure 8: Counterfactual regional inequality



Source: BHP, SIAB *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. The second bar displays the effect a revenue-neutral policy that pays cash transfers to all unemployed within East Germany. The third and forth bar show the effects of the two policies if they were applied to Germany as a whole. East Germany excludes Berlin. Confidence intervals are based on 9,999 bootstrap draws. The p-values refer to one-sided tests whether the effect of the GRW policy is larger than the effect of cash transfers.

efficiency-adjusted costs of €0.668 billion.

Next, we divide the adjusted costs by the number of unemployed in East Germany and assign each unemployed person this amount as a cash transfer. In this policy counterfactual, cash transfers have a presented discounted value of €1,076. We simulate that in this counterfactual the coefficient of variation decreases by 0.0194 [0.0110,0.0272], which is substantially lower than the effect of the GRW policy (see Figure 8). This is due to the place-blind nature of the cash transfer policy which captures spatial inequality only in so far that the number of unemployed is higher in poorer areas. Since the counterfactual policy also increases the income of recipients in relatively rich regions, the effect on regional inequality is much smaller compared to the place-specific policy.

So far both counterfactual have been targeted at East Germany. In a last step, we also simulate how the two simulated policies would affect Germany as whole. As before, we increase the GRW subsidy by the same amount for the bottom 20% of the overall German labor income distribution in 2014. This corresponds to a 21 percentage points increase for 80 counties. The total cost of such a policy would be €4.579 billion, which we again adjust by the marginal value of public funds of our GRW policy to €4.487 billion. Dividing by the total number of unemployed in Germany in 2014 yields a presented discounted cash transfer of €1,548 per unemployed. As Figure 8 shows, the pattern is very similar when we extend the two policy counterfactuals to all German counties. The GRW policy would reduce spatial the coefficient of variation by 0.0442 [0.0219,0.0626], which equals roughly two thirds of the increase in regional inequality we observe from 1996 to 2014. We find a very similar pattern when we use the Gini coefficient as an alternative measure of inequality (see Appendix Figure C.11b). We also investigate the role of spillover on the effect of the policy on regional inequality. Appendix Figure A.12 shows that without accounting for any spillover effects the effect on regional inequality is substantially lower. Adding sectoral spillover further reduces regional inequality as it accounts for additional jobs created in poor regions. The effect of trade spillover 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 disproportionally connected to richer regions, trade spillovers dampen the reduction in regional inequality. We find that for East Germany the first case applies, while for all of Germany the latter applies. In general, the overall impact of trade spillover is rather modest in size. Last, we also compare the effect of both policies on the gap in labor earnings between East and West Germany as it was a stated goal of the GRW policy to equalize living conditions between the two. According to the BHP and SIAB data in 2014, East Germans have a 33.29% lower labor income per capita than West Germans. The GRW policy would reduce the gap by 4.97 [2.73,7.15] percentage points, whereas the cash transfer reform policy would reduce the gap only by 2.99 [1.50,4.43] percentage points.

7 Conclusions

In this paper, we investigate the direct, spillover and welfare effects of regional firm subsidies. 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 sizable 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 a 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 0.64 jobs lost in the retail and construction sectors. Counties with a high trade exposure to the treated counties also experience a slight decline in manufacturing employment. In terms of regional spillovers, we do not find any evidence for reallocation of labor within the local labor market. Last, we find that local policy makers increase the business and property tax in response to subsidy cuts. We use the framework by Hendren and Sprung-Keyser (2020) to show that the efficiency of the place-based policy is similar to unemployment insurance of welfare cash transfers when ignoring these spillover effects. Accounting for spillovers makes the regional subsidy slightly more cost-effective. Moreover, we show that the place-based policy is favorable in reducing regional inequality compared to place-blind cash transfers.

In the 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. For instance, Austin et al. (2018) argues 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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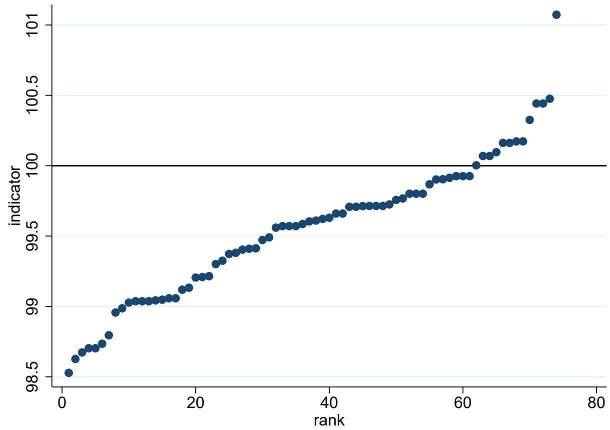
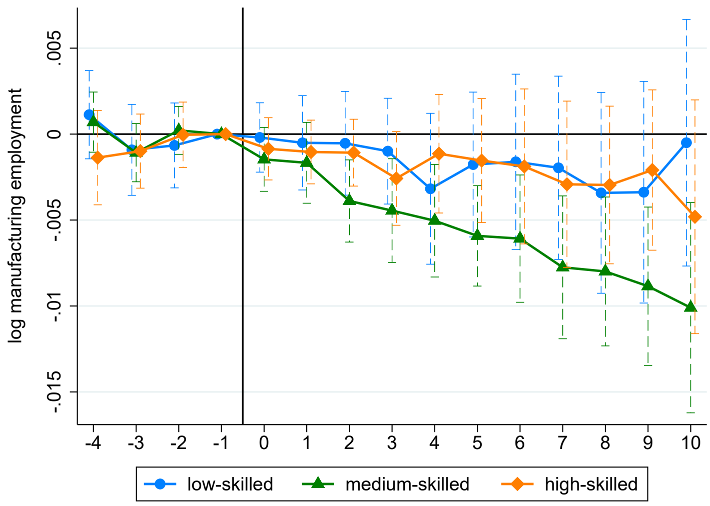
A Appendix

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



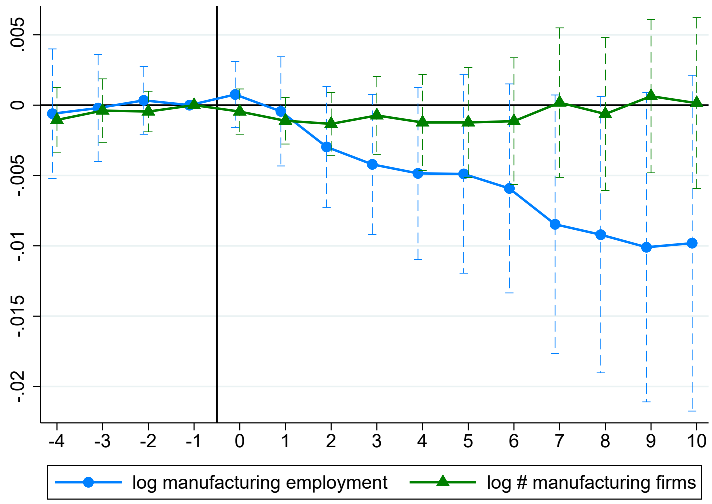
Source: Federal Ministry for Economic Affairs. *Notes*: This figure plots indicator values and the ranks of counties in the year 1997. The cutoff was formally at indicator value 100.

Figure A.2: Event study estimates: manufacturing employment by skill



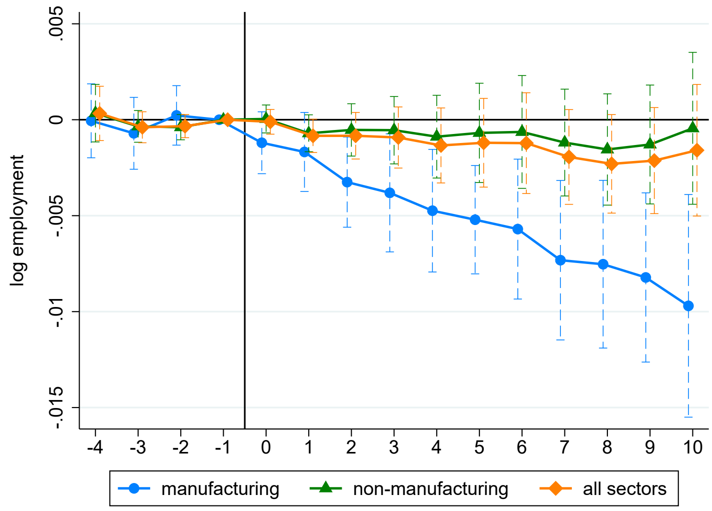
Source: BHP *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment by skill on leads and lags of a change in the maximum assistance rate as in equation (5). The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county and plant level. See Table C.4 for the point estimates.

Figure A.3: Event study estimates: number of manufacturing establishments and county-level manufacturing employment



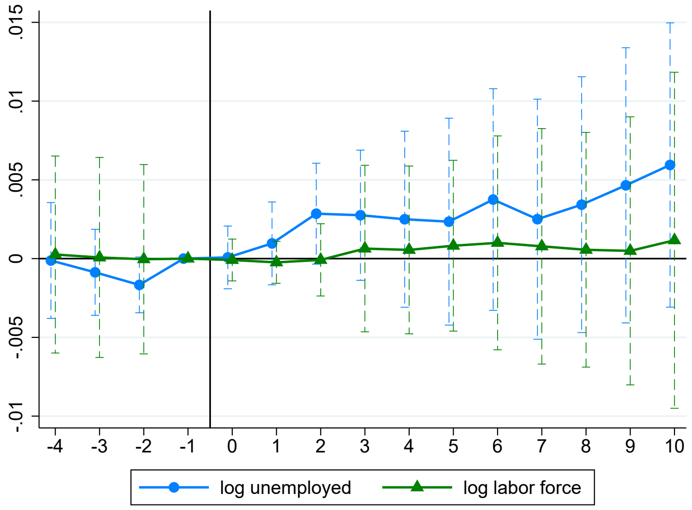
Source: BHP Notes: This figure plots coefficients along with 95% confidence intervals of a regression of changes in the log number of manufacturing establishments and log manufacturing employment at the county level on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county level. See Table C.5 for the point estimates.

Figure A.4: Event study estimates: total and non-manufacturing employment



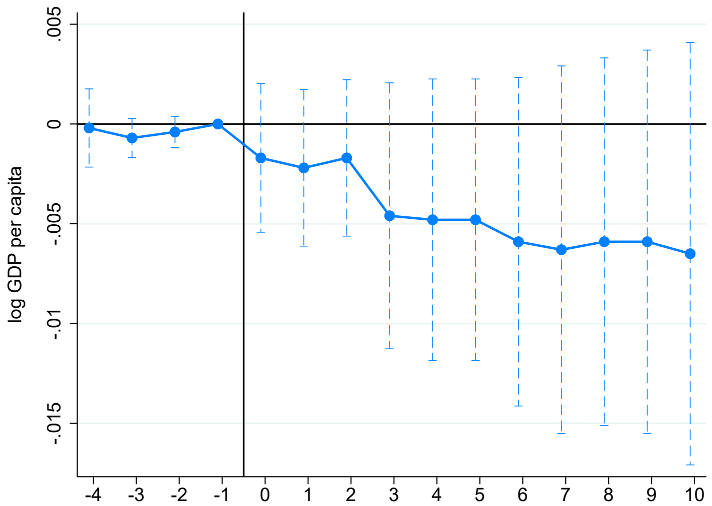
Source: BHP *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of log industry employment on leads and lags of a change in the maximum assistance rate as in equation (5). The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county and plant level. See Table C.6 for the point estimates.

Figure A.5: Event study estimates: unemployed and labor force



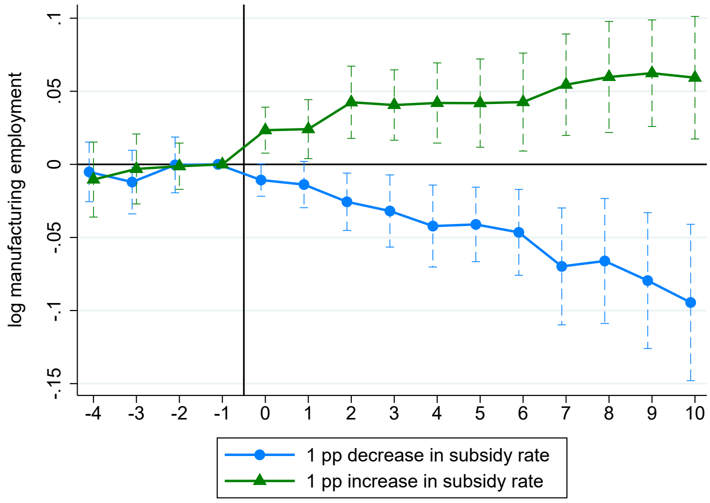
Source: Statistical Offices of German States *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of changes in the log unemployed and log labor force on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county and plant level. See Table C.7 for the point estimates.

Figure A.6: Event study estimates: GDP per capita



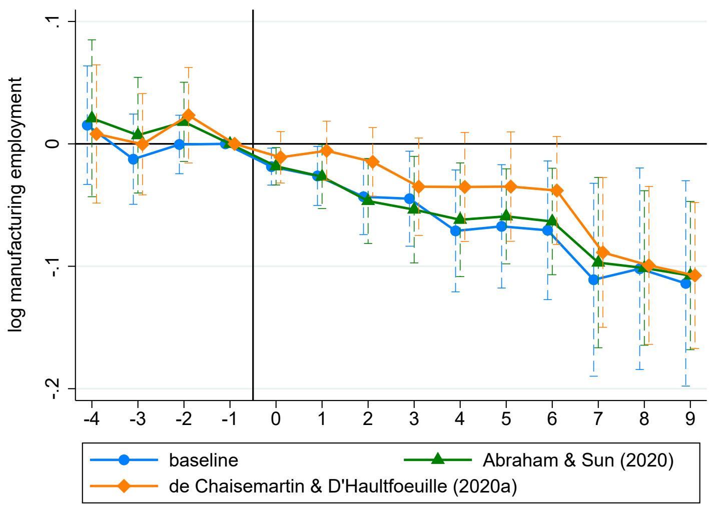
Source: Statistical Offices of German States *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of changes in log GDP per capita on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county and plant level. See Table C.8 for the point estimates.

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



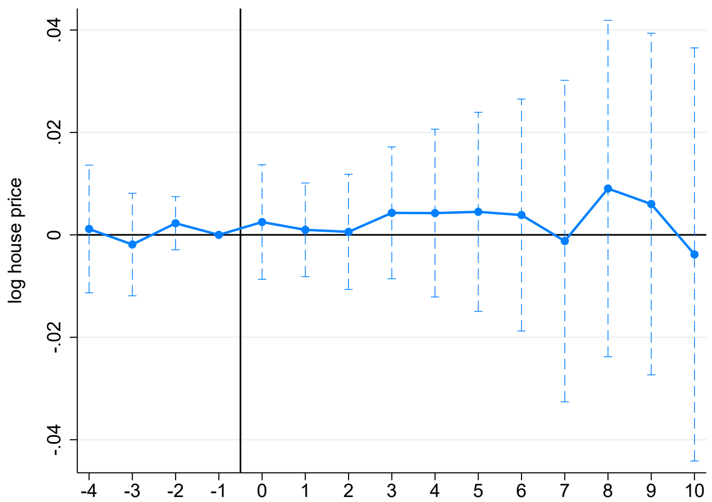
Source: BHP *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate. Treatment is discrete as in equation (1) and we include separate dummies for increases and decreases in the subsidy rate. The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county and plant level. See Table C.16 for the point estimates.

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



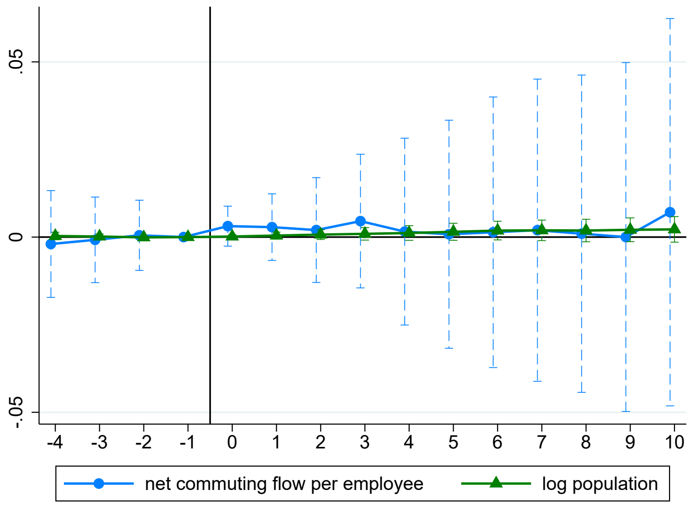
Source: BHP *Notes*: This figure plots coefficients along with 95% confidence intervals of the methods developed in de Chaisemartin and D'Haultfoeuille (2020a) and Sun and Abraham (2020) used on equation (1) with manufacturing employment as the outcome. We cut the sample in 2006 for all three estimators to only have one treatment per unit and retain never-treated units. We implement the estimator from de Chaisemartin and D'Haultfoeuille (2020a) using the Stata command *did_multipleGT* and 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 Table C.17 for the point estimates.

Figure A.9: Event study estimates: housing prices



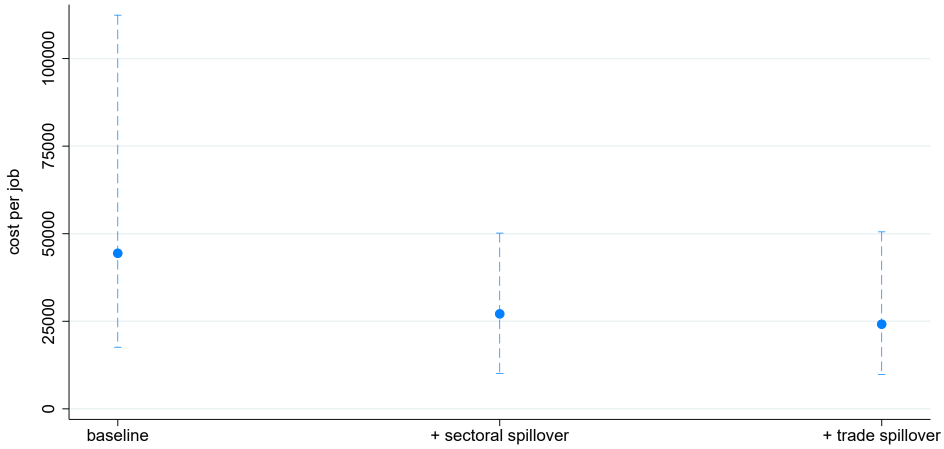
Source: IVD *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of changes in the log housing price on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county level. See Table C.23 for the point estimates.

Figure A.10: Event study estimates: population and commuting flows



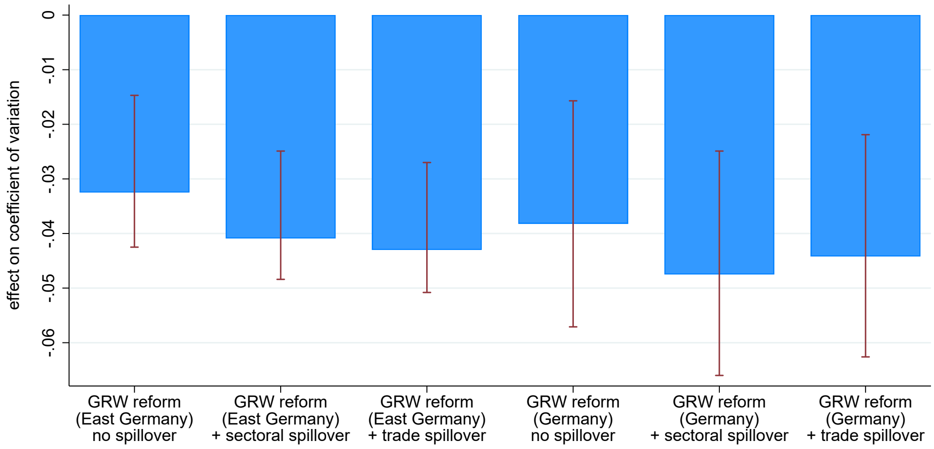
Source: Statistical Offices of German States, Federal Office for Building and Regional Planning *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of changes in log population and the inverse hyperbolic sine of the net commuting flow per employee on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county level. See Table C.25 for the point estimates.

Figure A.11: Cost per job



Source: own calculations *Notes:* Confidence intervals are based on 9,999 bootstrap draws.

Figure A.12: Counterfactual regional inequality: the role of spillover



Source: BHP, SIAB *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 bar add trade and sectoral spillover, respectively. The forth, fifth and sixth bar show the effects of these scenarios if they were applied to Germany as a whole. East Germany excludes Berlin. Confidence intervals are based on 9,999 bootstrap draws.

B Online Appendix: Data and Institutions

B.1 Data

Table B.1: Definition of variables and data sources

	year	description	source
plant level			
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: non-manufacturing	1996 - 2017	Total number of non-manufacturing employees at the plant level.	BHP
employees: all	1996 - 2017	Total number of employees at the plant level.	BHP
county level			
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 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 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 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 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 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 non-manufacturing wage	1996 - 2014	Median wage in 2010 € of non-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 wage	1996 - 2014	Median 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	1997 - 2017	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

continued

Table B.1 continued

	year	description	source
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 (*Steuermesszahl*).	Statistical Offices of the German States
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 (*Steuermesszahl*).	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 of 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	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
local labor market level			
employees: manufacturing	1996 - 2017	Total number of manufacturing employees at the local labor market-level.	BHP

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

Table B.2: Descriptive statistics

variable	mean	sd	N	years
plant level				
total investment (in thousand €)	931.16	7176.99	124754	1996 - 2016
equipment investment (in thousand €)	795.32	6618.93	124754	1996 - 2016
employees: manufacturing	21.82	87.53	407694	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: non-manufacturing	10.68	56.39	4055878	1996 - 2017
employees: all	11.70	60.00	4463572	1996 - 2017
county level				
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	1894.95	299.55	1444	1996 - 2014
median manufacturing wage: low-skill	1480.08	573.24	1424	1996 - 2014
median manufacturing wage: medium-skill	1925.31	272.36	1444	1996 - 2014
median manufacturing wage: high-skill	3420.31	635.79	1444	1996 - 2014
median non-manufacturing wage	1647.77	163.58	1444	1996 - 2014
median wage	1700.38	145.27	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 1,000 €)	146.87	42.20	797	1996 - 2012
local labor market level				
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

$$
\begin{aligned}
indicator_r^{1997} &= (infr_r^{1995})^{0.1} \times (wage_r^{1995})^{0.4} \times (unemp_r^{1995})^{0.5} \\
indicator_r^{2000} &= (infr_r^{1999})^{0.1} \times (wage_r^{1997})^{0.4} \times (unemp_r^{1996-1998})^{0.4} \times (empforecast_r)^{0.1} \\
indicator_r^{2007} &= (infr_r^{2005})^{0.05} \times (wage_r^{2003})^{0.4} \times (unemp_r^{2002-2005})^{0.5} \times (empforecast_r)^{0.05}
\end{aligned}
$$

where $infr_r^t$ measures the quality of a region r's infrastructure assessed at time t, $wage$ represents per-capita earnings, $unemp$ the average unemployment rate, and $empforecast$ is an employment rate projection.

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. 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	20,822€
Jerichower Land	Magdeburg	high	20,822€
Altmarkkreis Salzwedel	Magdeburg	high	20,822€
Boerde	Magdeburg	high	20,822€
Harz	Magdeburg	high	20,822€
Salzlandkreis	Magdeburg	high	20,822€
Stendal	Magdeburg	high	20,822€
Vogtlandkreis	Chemnitz	high	20,914€
Chemnitz, Stadt	Chemnitz	high	20,914€
Zwickau	Chemnitz	high	20,914€
Mittelsachsen	Chemnitz	high	20,914€
Erzgebirgskreis	Chemnitz	high	20,914€
Mansfeld-Suedharz	Halle	low	21,228€
Burgenlandkreis	Halle	low	21,228€
Halle (Saale), Stadt	Halle	low	21,228€
Saalekreis	Halle	low	21,228€
Elbe-Elster	Brandenburg-Suedwest	low	22,572€
Cottbus	Brandenburg-Suedwest	low	22,572€
Teltow-Flaeming	Brandenburg-Suedwest	low	22,572€
Dahme-Spreewald	Brandenburg-Suedwest	low	22,572€
Havelland	Brandenburg-Suedwest	low	22,572€
Brandenburg an der Havel, Stadt	Brandenburg-Suedwest	low	22,572€
Potsdam-Mittelmark	Brandenburg-Suedwest	low	22,572€
Oberspreewald-Lausitz	Brandenburg-Suedwest	low	22,572€
Spree-Neisse	Brandenburg-Suedwest	low	22,572€
Potsdam	Brandenburg-Suedwest	low	22,572€
...			

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

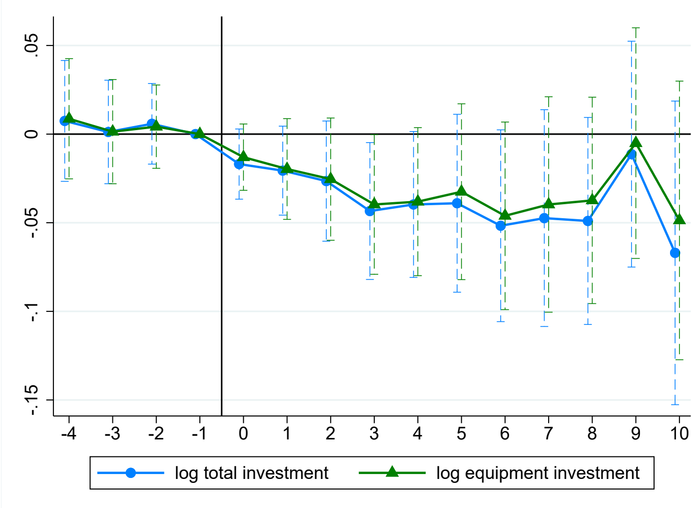
Table B.6: Automatically eligible and non-eligible industries for GRW subsidies

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, jewellery, 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

Source: Deutscher Bundestag (1997), Deutscher Bundestag (2000), Deutscher Bundestag (2007) *Notes*: Industries which are neither automatically eligible nor excluded from the subsidies have to show that the conditions mentioned in Section 2 are met.

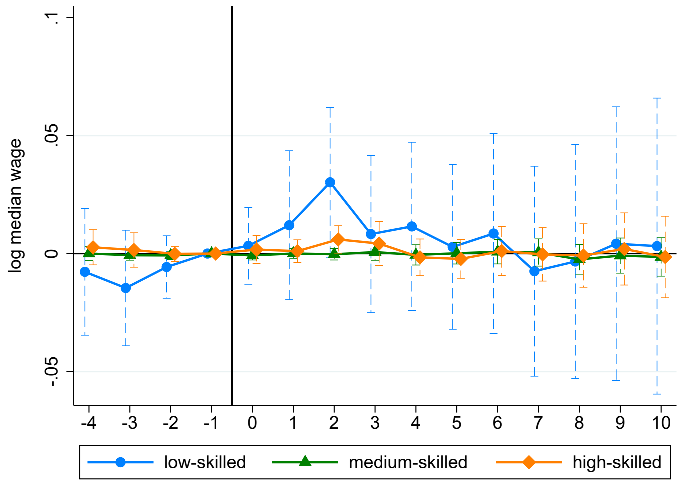
C Online Appendix: Additional Results

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). Clustering of standard errors is at the county and plant level. See Table C.2 for the point estimates.

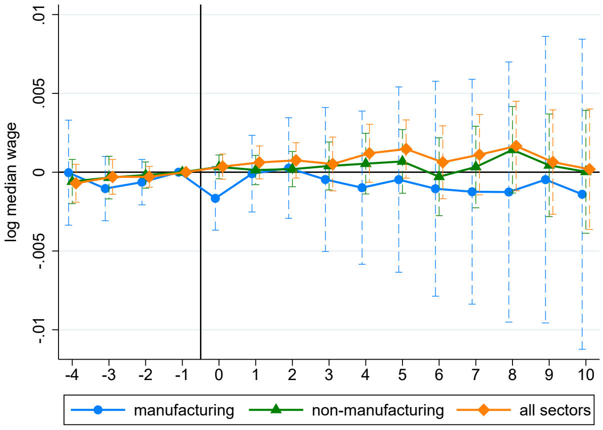
Figure C.2: Event study estimates: wages by skill



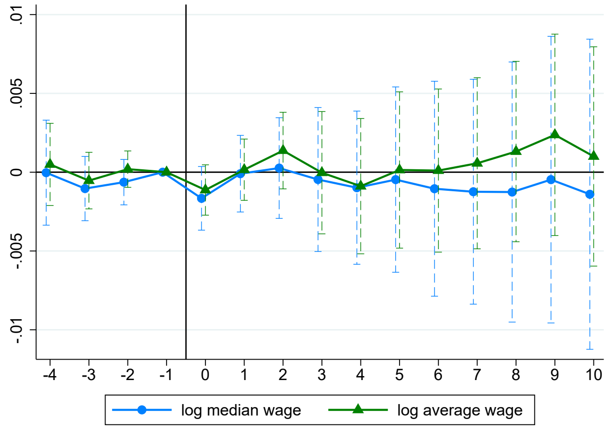
Source: SIAB *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of changes in log median wages by skill level on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs (M=30). Clustering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

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

(a) wages by sector



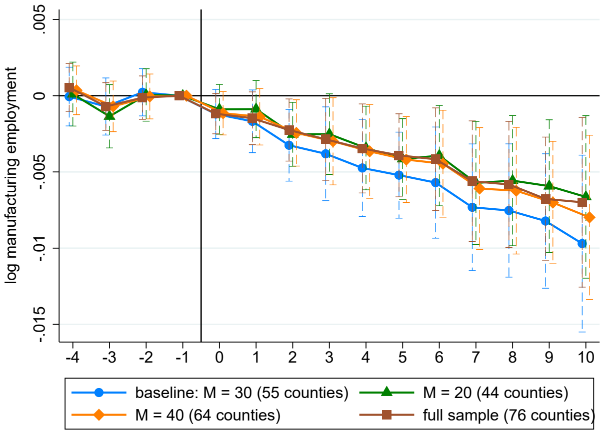
(b) average & median wage



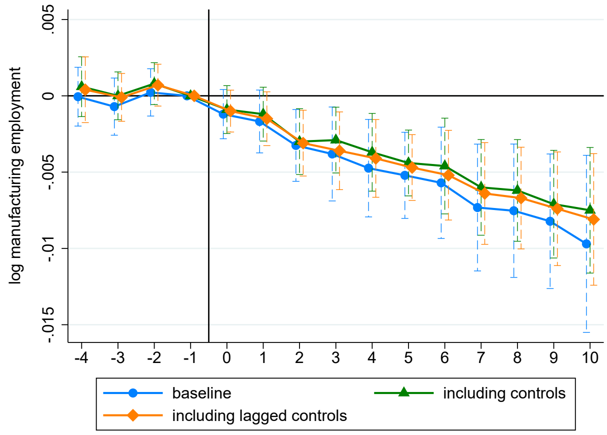
Source: SIAB *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of changes in log manufacturing wages by sector (Panel a) and log average wages (Panel b) on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the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

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

(a) manufacturing employment by cutoff sample

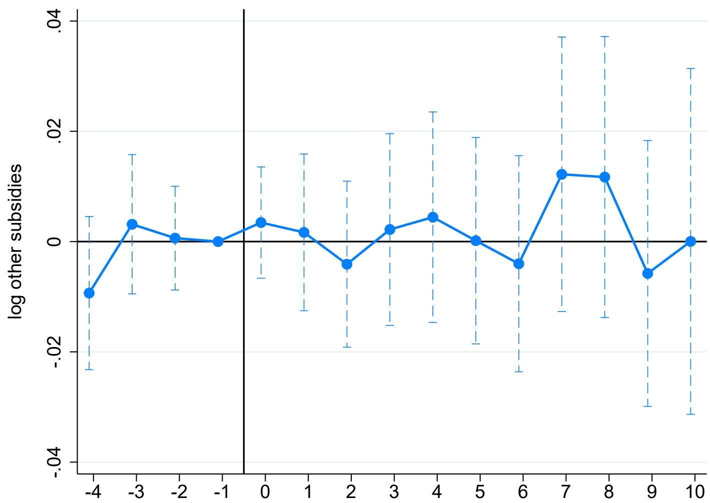


(b) manufacturing employment with controls



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). Clustering of standard errors is at the county and plant level. See Tables C.13 and C.14 for the point estimates.

Figure C.5: Event study estimates: other subsidies received

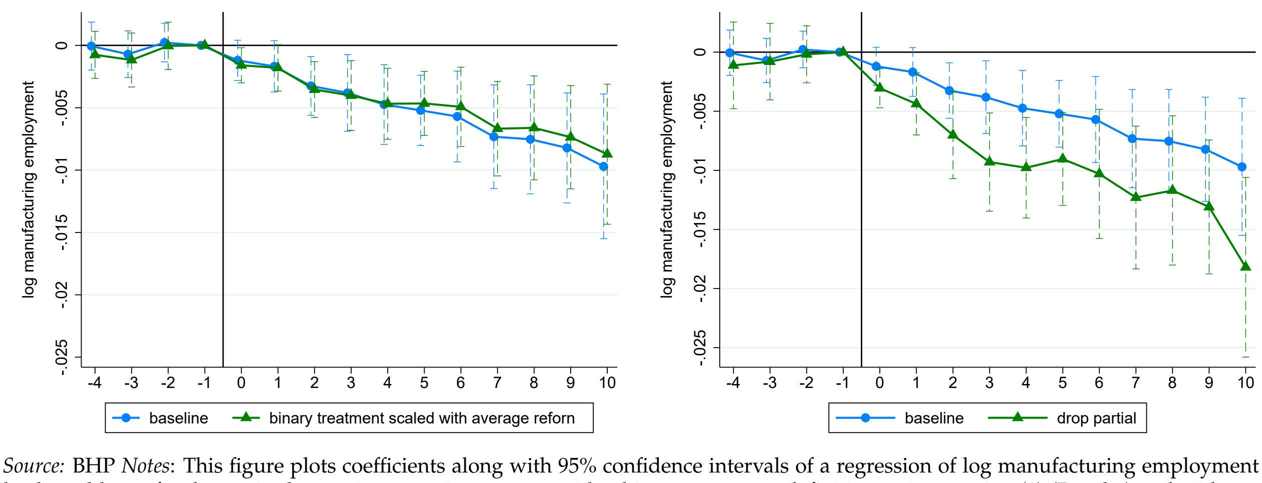


Source: Statistical Offices of German States *Notes*: This figure plots coefficients along with 95% confidence intervals of a regression of changes in log other subsidies on leads and lags of a change in the maximum assistance rate at the county level. The sample includes the 55 counties closest to cutoffs (M=30). Clustering of standard errors is at the county level. See Table C.15 for the point estimates.

Figure C.6: Event study estimates: manufacturing employment with binary treatment and without partially treated

(a) with binary treatment

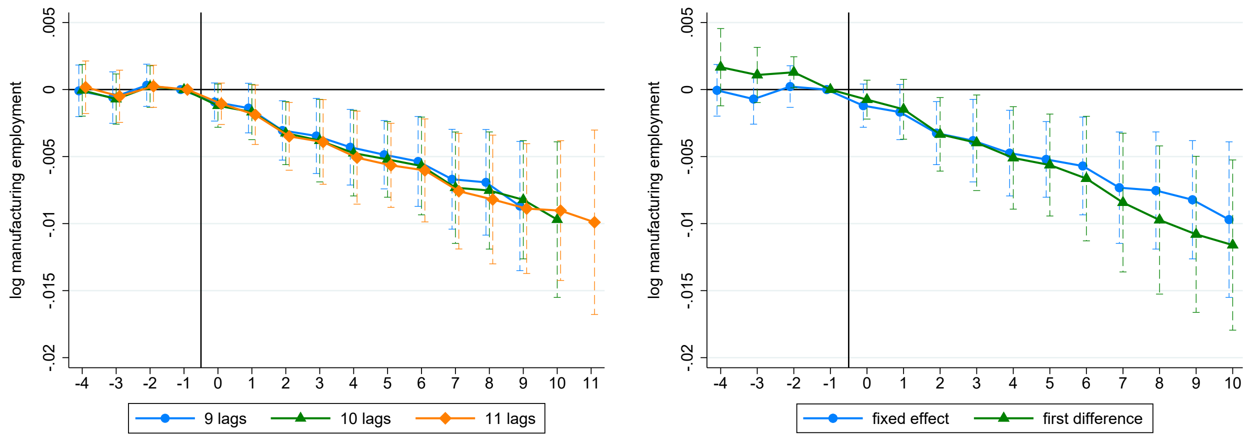
(b) without partially treated counties



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 without the partially treated counties (Panel b) as in equation (5). Clustering of standard errors is at the county and plant level. See Tables C.18 and C.19 for the point estimates.

Figure C.7: Event study estimates: manufacturing employment by number of lags and in first differences

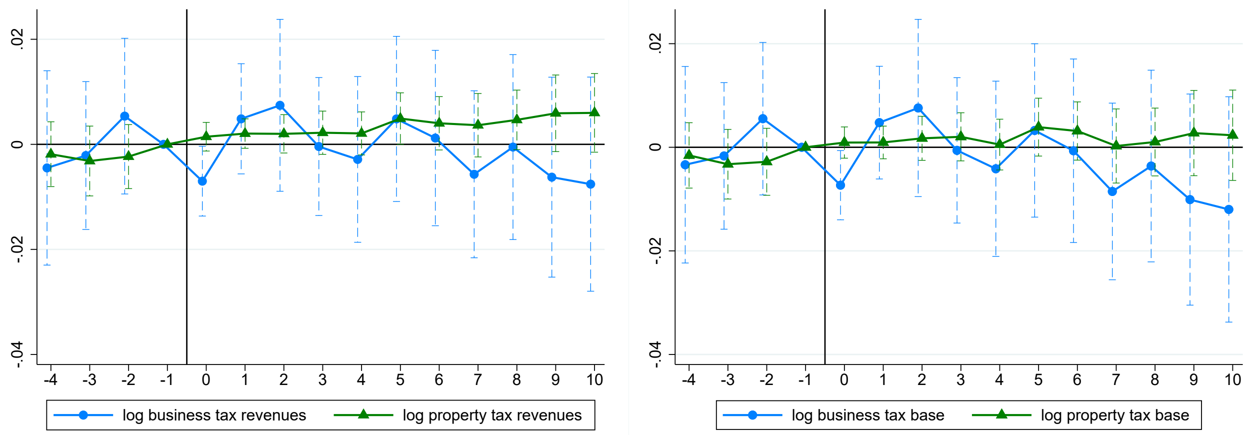
(a) by number of lags (b) fixed effect & first differences



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 in first differences (Panel b) as in equation (5). Clustering of standard errors is at the county and plant level. See Tables C.20 and C.21 for the point estimates.

Figure C.8: Event study estimates: local tax revenues and tax bases

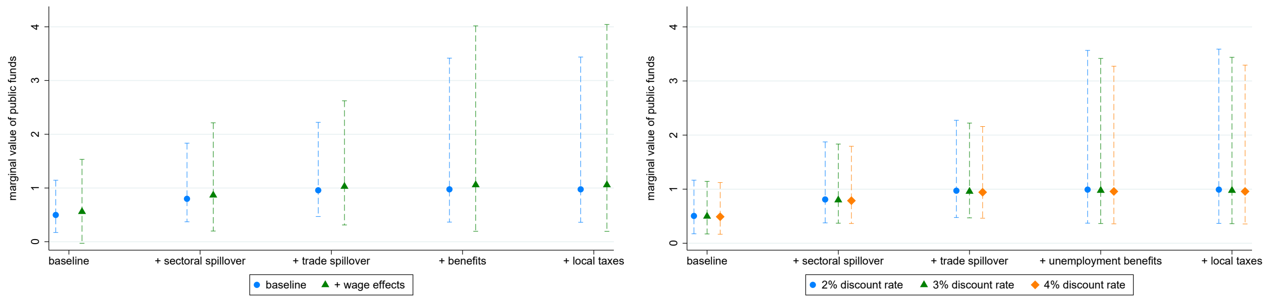
(a) local tax revenues per capita (b) local tax base per capita



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). Clustering of standard errors is at the county level. See Tables C.26 and C.27 for the point estimates.

Figure C.9: Marginal value of public funds including wage effects and by discount rate

(a) including wage effects (b) by discount rate

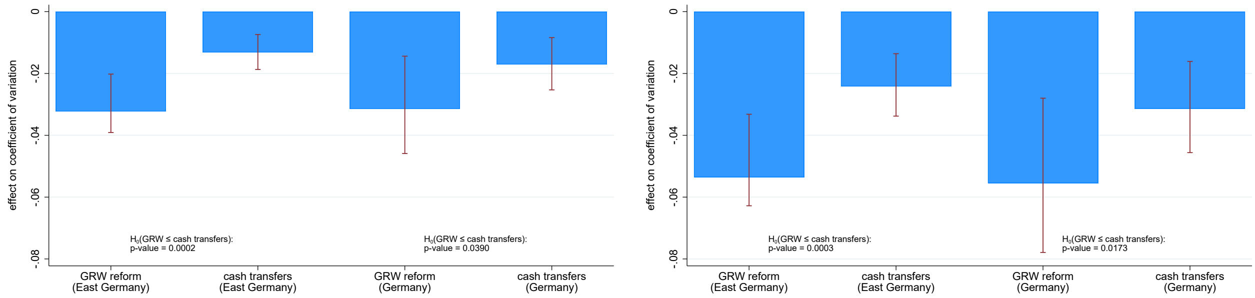


Source: own calculations *Notes:* Confidence intervals are based on 9,999 bootstrap draws.

Figure C.10: Counterfactual regional inequality: bottom 15% and bottom 25%

(a) bottom 15%

(b) bottom 25%

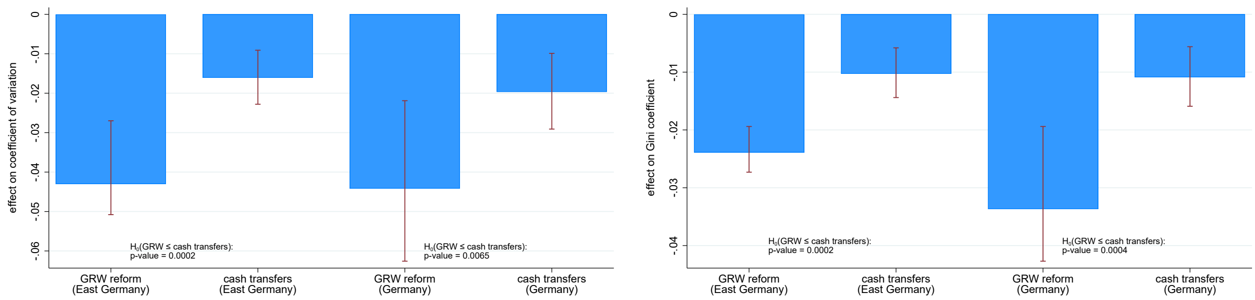


Source: BHP, SIAB *Notes:* The first bar displays the effect of an increase in the GRW subsidy back to 1996 levels for counties in the bottom 15% (Panel a) and bottom 25% (Panel b) of the labor income distribution on regional inequality within East Germany. The second bar displays the effect a revenue-neutral policy that pays cash transfers to all unemployed within East Germany. The third and forth bar show the effects of the two policies if they were applied to Germany as a whole. East Germany excludes Berlin. Confidence intervals are based on 9,999 bootstrap draws. The p-values refer to one-sided tests whether the effect of the GRW policy is larger than the effect of cash transfers.

Figure C.11: Counterfactual regional inequality: imposing different MVPFs and Gini coefficients

(a) imposing different MVPFs

(b) Gini coefficient



Source: BHP, SIAB *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. Panel (a) does not impose that the MVPF of the GRW and cash transfer policy have to be the same and Panel (b) uses the Gini coefficient as an alternative measure of regional inequality. The second bar displays the effect a revenue-neutral policy that pays cash transfers to all unemployed within East Germany. The third and forth bar show the effects of the two policies if they were applied to Germany as a whole. East Germany excludes Berlin. Confidence intervals are based on 9,999 bootstrap draws. The p-values refer to one-sided tests whether the effect of the GRW policy is larger than the effect of cash transfers.

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	-0.028
	(0.027)	(0.032)
1 pp subsidy cut: year 3 before reform	-0.003	-0.003
	(0.024)	(0.027)
1 pp subsidy cut: year 2 before reform	-0.028	-0.037
	(0.024)	(0.026)
1 pp subsidy cut: year 0 after reform	-0.089***	-0.090***
	(0.028)	(0.029)
1 pp subsidy cut: year 1 after reform	-0.114***	-0.102***
	(0.025)	(0.028)
1 pp subsidy cut: year 2 after reform	-0.105***	-0.089***
	(0.025)	(0.026)
1 pp subsidy cut: year 3 after reform	-0.131***	-0.108***
	(0.029)	(0.033)
1 pp subsidy cut: year 4 after reform	-0.095***	-0.079**
	(0.032)	(0.037)
1 pp subsidy cut: year 5 after reform	-0.142***	-0.134***
	(0.033)	(0.037)
1 pp subsidy cut: year 6 after reform	-0.157***	-0.138***
	(0.032)	(0.035)
1 pp subsidy cut: year 7 after reform	-0.131***	-0.121***
	(0.038)	(0.044)
1 pp subsidy cut: year 8 after reform	-0.075*	-0.056
	(0.041)	(0.044)
1 pp subsidy cut: year 9 after reform	-0.120***	-0.109**
	(0.039)	(0.043)
1 pp subsidy cut: year 10 after reform	-0.138**	-0.146**
	(0.062)	(0.071)
N	1,141	1,141

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) log total investment	(2) log equipment investment
1 pp subsidy cut: year 4 before reform	0.007	0.009
	(0.017)	(0.017)
1 pp subsidy cut: year 3 before reform	0.001	0.001
	(0.015)	(0.015)
1 pp subsidy cut: year 2 before reform	0.006	0.004
	(0.012)	(0.012)
1 pp subsidy cut: year 0 after reform	-0.017*	-0.013
	(0.010)	(0.010)
1 pp subsidy cut: year 1 after reform	-0.021	-0.020
	(0.013)	(0.015)
1 pp subsidy cut: year 2 after reform	-0.027	-0.025
	(0.017)	(0.018)
1 pp subsidy cut: year 3 after reform	-0.043**	-0.040*
	(0.020)	(0.020)
1 pp subsidy cut: year 4 after reform	-0.040*	-0.038*
	(0.021)	(0.021)
1 pp subsidy cut: year 5 after reform	-0.039	-0.033
	(0.026)	(0.025)
1 pp subsidy cut: year 6 after reform	-0.052*	-0.046*
	(0.028)	(0.027)
1 pp subsidy cut: year 7 after reform	-0.047	-0.040
	(0.031)	(0.031)
1 pp subsidy cut: year 8 after reform	-0.049	-0.037
	(0.030)	(0.030)
1 pp subsidy cut: year 9 after reform	-0.011	-0.005
	(0.033)	(0.033)
1 pp subsidy cut: year 10 after reform	-0.067	-0.049
	(0.044)	(0.040)
N	90,656	90,656

Notes: Standard errors in parentheses. See 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	312,503

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 skill

	(1) log manufacturing employment: low-skilled	(2) log manufacturing employment: medium-skilled	(3) log manufacturing employment: high-skilled
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	0.000	-0.000
	(0.001)	(0.001)	(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	-0.002	-0.001
	(0.001)	(0.001)	(0.001)
1 pp subsidy cut: year 2 after reform	-0.001	-0.004***	-0.001
	(0.002)	(0.001)	(0.001)
1 pp subsidy cut: year 3 after reform	-0.001	-0.004***	-0.003*
	(0.002)	(0.002)	(0.001)
1 pp subsidy cut: year 4 after reform	-0.003	-0.005***	-0.001
	(0.002)	(0.002)	(0.002)
1 pp subsidy cut: year 5 after reform	-0.002	-0.006***	-0.002
	(0.002)	(0.001)	(0.002)
1 pp subsidy cut: year 6 after reform	-0.002	-0.006***	-0.002
	(0.003)	(0.002)	(0.002)
1 pp subsidy cut: year 7 after reform	-0.002	-0.008***	-0.003
	(0.003)	(0.002)	(0.002)
1 pp subsidy cut: year 8 after reform	-0.003	-0.008***	-0.003
	(0.003)	(0.002)	(0.002)
1 pp subsidy cut: year 9 after reform	-0.003	-0.009***	-0.002
	(0.003)	(0.002)	(0.002)
1 pp subsidy cut: year 10 after reform	-0.001	-0.010***	-0.005
	(0.004)	(0.003)	(0.003)
N	114,771	299,229	123,354

Notes: Standard errors in parentheses. See 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: number of manufacturing establishments and county-level manufacturing employment

	(1) log manufacturing employment	(2) log manufacturing firms
1 pp subsidy cut: year 4 before reform	-0.001	-0.001
	(0.002)	(0.001)
1 pp subsidy cut: year 3 before reform	-0.000	-0.000
	(0.002)	(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	0.001	-0.000
	(0.001)	(0.001)
1 pp subsidy cut: year 1 after reform	-0.000	-0.001
	(0.002)	(0.001)
1 pp subsidy cut: year 2 after reform	-0.003	-0.001
	(0.002)	(0.001)
1 pp subsidy cut: year 3 after reform	-0.004	-0.001
	(0.003)	(0.001)
1 pp subsidy cut: year 4 after reform	-0.005	-0.001
	(0.003)	(0.002)
1 pp subsidy cut: year 5 after reform	-0.005	-0.001
	(0.004)	(0.002)
1 pp subsidy cut: year 6 after reform	-0.006	-0.001
	(0.004)	(0.002)
1 pp subsidy cut: year 7 after reform	-0.008*	0.000
	(0.005)	(0.003)
1 pp subsidy cut: year 8 after reform	-0.009*	-0.001
	(0.005)	(0.003)
1 pp subsidy cut: year 9 after reform	-0.010*	0.001
	(0.006)	(0.003)
1 pp subsidy cut: year 10 after reform	-0.010	0.000
	(0.006)	(0.003)
N	1,210	1,210

Notes: Standard errors in parentheses. See 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: total and non-manufacturing employment

	(1) log manufacturing employment	(2) log non-manufacturing employment	(3) log total employment
1 pp subsidy cut: year 4 before reform	-0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)
1 pp subsidy cut: year 3 before reform	-0.001	-0.000	-0.000
	(0.001)	(0.001)	(0.000)
1 pp subsidy cut: year 2 before reform	0.000	-0.000	-0.000
	(0.001)	(0.000)	(0.000)
1 pp subsidy cut: year 0 after reform	-0.001	0.000	-0.000
	(0.001)	(0.000)	(0.000)
1 pp subsidy cut: year 1 after reform	-0.002	-0.001	-0.001*
	(0.001)	(0.000)	(0.000)
1 pp subsidy cut: year 2 after reform	-0.003***	-0.001	-0.001
	(0.001)	(0.001)	(0.001)
1 pp subsidy cut: year 3 after reform	-0.004**	-0.001	-0.001
	(0.002)	(0.001)	(0.001)
1 pp subsidy cut: year 4 after reform	-0.005***	-0.001	-0.001
	(0.002)	(0.001)	(0.001)
1 pp subsidy cut: year 5 after reform	-0.005***	-0.001	-0.001
	(0.001)	(0.001)	(0.001)
1 pp subsidy cut: year 6 after reform	-0.006***	-0.001	-0.001
	(0.002)	(0.002)	(0.001)
1 pp subsidy cut: year 7 after reform	-0.007***	-0.001	-0.002
	(0.002)	(0.001)	(0.001)
1 pp subsidy cut: year 8 after reform	-0.008***	-0.002	-0.002*
	(0.002)	(0.001)	(0.001)
1 pp subsidy cut: year 9 after reform	-0.008***	-0.001	-0.002
	(0.002)	(0.002)	(0.001)
1 pp subsidy cut: year 10 after reform	-0.010***	-0.000	-0.002
	(0.003)	(0.002)	(0.002)
N	312.503	2.939.470	3.252.514

Notes: Standard errors in parentheses. See 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: unemployed and labor force

	(1) log unemployed	(2) log labor force
1 pp subsidy cut: year 4 before reform	-0.000	0.000
	(0.002)	(0.003)
1 pp subsidy cut: year 3 before reform	-0.001	0.000
	(0.001)	(0.003)
1 pp subsidy cut: year 2 before reform	-0.002*	-0.000
	(0.001)	(0.003)
1 pp subsidy cut: year 0 after reform	0.000	-0.000
	(0.001)	(0.001)
1 pp subsidy cut: year 1 after reform	0.001	-0.000
	(0.001)	(0.001)
1 pp subsidy cut: year 2 after reform	0.003*	-0.000
	(0.002)	(0.001)
1 pp subsidy cut: year 3 after reform	0.003	0.001
	(0.002)	(0.003)
1 pp subsidy cut: year 4 after reform	0.002	0.001
	(0.003)	(0.003)
1 pp subsidy cut: year 5 after reform	0.002	0.001
	(0.003)	(0.003)
1 pp subsidy cut: year 6 after reform	0.004	0.001
	(0.004)	(0.003)
1 pp subsidy cut: year 7 after reform	0.003	0.001
	(0.004)	(0.004)
1 pp subsidy cut: year 8 after reform	0.003	0.001
	(0.004)	(0.004)
1 pp subsidy cut: year 9 after reform	0.005	0.000
	(0.004)	(0.004)
1 pp subsidy cut: year 10 after reform	0.006	0.001
	(0.005)	(0.005)
N	990	1,210

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

Table C.8: 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	1,210

Notes: Standard errors in parentheses. See 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
	(0.001)
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	0.000
	(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	1,045

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	0.000	0.003
	(0.014)	(0.002)	(0.004)
1 pp subsidy cut: year 3 before reform	-0.015	-0.001	0.002
	(0.013)	(0.001)	(0.004)
1 pp subsidy cut: year 2 before reform	-0.006	-0.001	-0.000
	(0.007)	(0.001)	(0.002)
1 pp subsidy cut: year 0 after reform	0.003	-0.001	0.002
	(0.008)	(0.001)	(0.003)
1 pp subsidy cut: year 1 after reform	0.012	0.000	0.001
	(0.016)	(0.001)	(0.002)
1 pp subsidy cut: year 2 after reform	0.030*	-0.000	0.006**
	(0.016)	(0.001)	(0.003)
1 pp subsidy cut: year 3 after reform	0.008	0.001	0.004
	(0.017)	(0.002)	(0.005)
1 pp subsidy cut: year 4 after reform	0.012	-0.001	-0.002
	(0.018)	(0.002)	(0.004)
1 pp subsidy cut: year 5 after reform	0.003	0.000	-0.002
	(0.018)	(0.002)	(0.004)
1 pp subsidy cut: year 6 after reform	0.008	0.001	0.001
	(0.022)	(0.002)	(0.005)
1 pp subsidy cut: year 7 after reform	-0.007	0.000	-0.000
	(0.023)	(0.003)	(0.006)
1 pp subsidy cut: year 8 after reform	-0.003	-0.002	-0.001
	(0.025)	(0.003)	(0.007)
1 pp subsidy cut: year 9 after reform	0.004	-0.001	-0.002
	(0.030)	(0.004)	(0.008)
1 pp subsidy cut: year 10 after reform	0.003	-0.001	-0.002
	(0.032)	(0.004)	(0.009)
N	1,024	1,045	1,045

Notes: Standard errors in parentheses. See 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 wages by sector

	(1) log median non-manufacturing wage	(2) log median wage
1 pp subsidy cut: year 4 before reform	-0.001	-0.001
	(0.001)	(0.001)
1 pp subsidy cut: year 3 before reform	-0.000	-0.000
	(0.001)	(0.001)
1 pp subsidy cut: year 2 before reform	-0.000	-0.000
	(0.000)	(0.000)
1 pp subsidy cut: year 0 after reform	0.000	0.000
	(0.000)	(0.000)
1 pp subsidy cut: year 1 after reform	0.000	0.001
	(0.000)	(0.001)
1 pp subsidy cut: year 2 after reform	0.000	0.001
	(0.001)	(0.001)
1 pp subsidy cut: year 3 after reform	0.000	0.001
	(0.001)	(0.001)
1 pp subsidy cut: year 4 after reform	0.001	0.001
	(0.001)	(0.001)
1 pp subsidy cut: year 5 after reform	0.001	0.001
	(0.001)	(0.001)
1 pp subsidy cut: year 6 after reform	-0.000	0.001
	(0.001)	(0.001)
1 pp subsidy cut: year 7 after reform	0.000	0.001
	(0.001)	(0.001)
1 pp subsidy cut: year 8 after reform	0.001	0.002
	(0.001)	(0.001)
1 pp subsidy cut: year 9 after reform	0.000	0.001
	(0.002)	(0.002)
1 pp subsidy cut: year 10 after reform	0.000	0.001
	(0.002)	(0.002)
N	1,045	1,045

Notes: Standard errors in parentheses. See 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) log manufacturing wage
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	0.000
	(0.001)
1 pp subsidy cut: year 2 after reform	0.001
	(0.001)
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	0.000
	(0.003)
1 pp subsidy cut: year 7 after reform	0.001
	(0.003)
1 pp subsidy cut: year 8 after reform	0.001
	(0.003)
1 pp subsidy cut: year 9 after reform	0.002
	(0.003)
1 pp subsidy cut: year 10 after reform	0.001
	(0.004)
N	1,045

Notes: Standard errors in parentheses. See 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	0.000	-0.000	0.000
	(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)	(0.001)	(0.001)
1 pp subsidy cut: year 2 before reform	-0.000	0.000	0.000	-0.000
	(0.001)	(0.001)	(0.001)	(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	-0.002	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
1 pp subsidy cut: year 2 after reform	-0.002**	-0.003**	-0.003***	-0.002**
	(0.001)	(0.001)	(0.001)	(0.001)
1 pp subsidy cut: year 3 after reform	-0.003**	-0.003*	-0.004**	-0.003**
	(0.001)	(0.001)	(0.002)	(0.001)
1 pp subsidy cut: year 4 after reform	-0.003**	-0.003**	-0.005***	-0.004**
	(0.001)	(0.001)	(0.002)	(0.002)
1 pp subsidy cut: year 5 after reform	-0.004***	-0.004***	-0.005***	-0.004***
	(0.001)	(0.001)	(0.001)	(0.001)
1 pp subsidy cut: year 6 after reform	-0.004**	-0.004**	-0.006***	-0.004**
	(0.002)	(0.002)	(0.002)	(0.002)
1 pp subsidy cut: year 7 after reform	-0.006***	-0.006***	-0.007***	-0.006***
	(0.002)	(0.002)	(0.002)	(0.002)
1 pp subsidy cut: year 8 after reform	-0.006***	-0.006**	-0.008***	-0.006***
	(0.002)	(0.002)	(0.002)	(0.002)
1 pp subsidy cut: year 9 after reform	-0.007***	-0.006**	-0.008***	-0.007***
	(0.002)	(0.002)	(0.002)	(0.002)
1 pp subsidy cut: year 10 after reform	-0.007**	-0.007**	-0.010***	-0.008***
	(0.003)	(0.003)	(0.003)	(0.003)
N	401,290	244,169	312,503	355,601

Notes: Standard errors in parentheses. See 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 controls)

	(1) log manufacturing employment	(2) log manufacturing employment: including controls	(3) log manufacturing employment: including lagged controls
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	-0.000	-0.000
	(0.001)	(0.001)	(0.001)
1 pp subsidy cut: year 2 before reform	0.000	0.001	0.001
	(0.001)	(0.001)	(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)	(0.001)	(0.001)
1 pp subsidy cut: year 2 after reform	-0.003***	-0.003***	-0.003***
	(0.001)	(0.001)	(0.001)
1 pp subsidy cut: year 3 after reform	-0.004**	-0.003**	-0.004***
	(0.002)	(0.001)	(0.001)
1 pp subsidy cut: year 4 after reform	-0.005***	-0.004***	-0.004***
	(0.002)	(0.001)	(0.001)
1 pp subsidy cut: year 5 after reform	-0.005***	-0.004***	-0.005***
	(0.001)	(0.001)	(0.001)
1 pp subsidy cut: year 6 after reform	-0.006***	-0.005***	-0.005***
	(0.002)	(0.002)	(0.002)
1 pp subsidy cut: year 7 after reform	-0.007***	-0.006***	-0.006***
	(0.002)	(0.002)	(0.002)
1 pp subsidy cut: year 8 after reform	-0.008***	-0.006***	-0.007***
	(0.002)	(0.002)	(0.002)
1 pp subsidy cut: year 9 after reform	-0.008***	-0.007***	-0.007***
	(0.002)	(0.002)	(0.002)
1 pp subsidy cut: year 10 after reform	-0.010***	-0.008***	-0.008***
	(0.003)	(0.002)	(0.002)
N	312,504	312,504	312,504

Notes: Standard errors in parentheses. See 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: 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 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: manufacturing employment (increases & decreases)

	(1) log manufacturing employment	p-value: subsidy cut = - subsidy increase
average subsidy cut: year 4 before reform	-0.005	
	(0.010)	
average subsidy cut: year 3 before reform	-0.012	
	(0.011)	
average subsidy cut: year 2 before reform	-0.000	
	(0.010)	
average subsidy cut: year 0 after reform	-0.011*	
	(0.006)	
average subsidy cut: year 1 after reform	-0.014*	
	(0.007)	
average subsidy cut: year 2 after reform	-0.026**	
	(0.010)	
average subsidy cut: year 3 after reform	-0.032**	
	(0.013)	
average subsidy cut: year 4 after reform	-0.042***	
	(0.014)	
average subsidy cut: year 5 after reform	-0.041***	
	(0.013)	
average subsidy cut: year 6 after reform	-0.047***	
	(0.015)	
average subsidy cut: year 7 after reform	-0.070***	
	(0.020)	
average subsidy cut: year 8 after reform	-0.066***	
	(0.022)	
average subsidy cut: year 9 after reform	-0.080***	
	(0.024)	
average subsidy cut: year 10 after reform	-0.095***	
	(0.027)	
average subsidy increase: year 4 before reform	-0.010	0.211
	(0.013)	
average subsidy increase: year 3 before reform	-0.003	0.362
	(0.012)	
average subsidy increase: year 2 before reform	-0.001	0.372
	(0.008)	
average subsidy increase: year 0 after reform	0.023***	0.122
	(0.008)	
average subsidy increase: year 1 after reform	0.024**	0.335
	(0.010)	
average subsidy increase: year 2 after reform	0.043***	0.210
	(0.013)	
average subsidy increase: year 3 after reform	0.041***	0.546
	(0.012)	
average subsidy increase: year 4 after reform	0.042***	0.993
	(0.014)	
average subsidy increase: year 5 after reform	0.042***	0.964
	(0.015)	
average subsidy increase: year 6 after reform	0.043**	0.824
	(0.018)	
average subsidy increase: year 7 after reform	0.055***	0.498
	(0.020)	
average subsidy increase: year 8 after reform	0.060***	0.767
	(0.019)	
average subsidy increase: year 9 after reform	0.062***	0.384
	(0.019)	
average subsidy increase: year 10 after reform	0.059***	0.112
	(0.021)	
joint test of asymmetry for all post-reform effects		0.214
N	312,503	

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

Table C.17: Heterogeneous treatment effects: Sun and Abraham (2020) & de Chaisemartin and D'Haultfoeuille (2020a)

	(1) log manufacturing employment: baseline	(2) log manufacturing employment: Sun and Abraham (2020)	(3) log manufacturing employment: de Chaisemartin and D'Haultfoeuille (2020a)
average subsidy cut: year 4 before reform	0.015	0.021	0.008
	(0.025)	(0.033)	(0.029)
average subsidy cut: year 3 before reform	-0.013	0.007	-0.000
	(0.019)	(0.024)	(0.021)
average subsidy cut: year 2 before reform	-0.001	0.018	0.023
	(0.012)	(0.017)	(0.020)
average subsidy cut: year 0 after reform	-0.019**	-0.018**	-0.011
	(0.008)	(0.008)	(0.011)
average subsidy cut: year 1 after reform	-0.026**	-0.027**	-0.006
	(0.012)	(0.013)	(0.012)
average subsidy cut: year 2 after reform	-0.043***	-0.047**	-0.015
	(0.016)	(0.018)	(0.014)
average subsidy cut: year 3 after reform	-0.045**	-0.054**	-0.035*
	(0.020)	(0.022)	(0.020)
average subsidy cut: year 4 after reform	-0.071***	-0.062**	-0.035
	(0.025)	(0.024)	(0.023)
average subsidy cut: year 5 after reform	-0.067**	-0.059***	-0.035
	(0.026)	(0.020)	(0.023)
average subsidy cut: year 6 after reform	-0.071**	-0.063***	-0.038*
	(0.029)	(0.022)	(0.023)
average subsidy cut: year 7 after reform	-0.111***	-0.097***	-0.089***
	(0.040)	(0.036)	(0.031)
average subsidy cut: year 8 after reform	-0.102**	-0.101***	-0.099***
	(0.042)	(0.032)	(0.033)
average subsidy cut: year 9 after reform	-0.114***	-0.108***	-0.108***
	(0.043)	(0.031)	(0.030)
N	161,876	161,876	161,876

Notes: Standard errors in parentheses. The sample is ended in 2006 for all columns. See Figure A.8 for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.18: Event study estimates: manufacturing employment with binary treatment

	(1) log manufacturing employment
average subsidy cut: year 4 before reform	-0.006
	(0.007)
average subsidy cut: year 3 before reform	-0.009
	(0.009)
average subsidy cut: year 2 before reform	-0.000
	(0.007)
average subsidy cut: year 0 after reform	-0.012**
	(0.006)
average subsidy cut: year 1 after reform	-0.014*
	(0.007)
average subsidy cut: year 2 after reform	-0.027***
	(0.009)
average subsidy cut: year 3 after reform	-0.031***
	(0.011)
average subsidy cut: year 4 after reform	-0.036***
	(0.011)
average subsidy cut: year 5 after reform	-0.036***
	(0.010)
average subsidy cut: year 6 after reform	-0.038***
	(0.013)
average subsidy cut: year 7 after reform	-0.051***
	(0.015)
average subsidy cut: year 8 after reform	-0.051***
	(0.016)
average subsidy cut: year 9 after reform	-0.057***
	(0.016)
average subsidy cut: year 10 after reform	-0.067***
	(0.022)
N	312,503

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

Table C.19: 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	180,020

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

Table C.20: 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	-0.000	0.000
	(0.001)	(0.001)	(0.001)
1 pp subsidy cut: year 3 before reform	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)
1 pp subsidy cut: year 2 before reform	0.000	0.000	0.000
	(0.001)	(0.001)	(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	-0.002	-0.002
	(0.001)	(0.001)	(0.001)
1 pp subsidy cut: year 2 after reform	-0.003***	-0.003***	-0.004***
	(0.001)	(0.001)	(0.001)
1 pp subsidy cut: year 3 after reform	-0.003**	-0.004**	-0.004**
	(0.001)	(0.002)	(0.002)
1 pp subsidy cut: year 4 after reform	-0.004***	-0.005***	-0.005***
	(0.001)	(0.002)	(0.002)
1 pp subsidy cut: year 5 after reform	-0.005***	-0.006***	-0.006***
	(0.001)	(0.001)	(0.002)
1 pp subsidy cut: year 6 after reform	-0.005***	-0.006***	-0.006***
	(0.002)	(0.002)	(0.002)
1 pp subsidy cut: year 7 after reform	-0.007***	-0.008***	-0.008***
	(0.002)	(0.002)	(0.002)
1 pp subsidy cut: year 8 after reform	-0.007***	-0.008***	-0.008***
	(0.002)	(0.002)	(0.002)
1 pp subsidy cut: year 9 after reform	-0.009***	-0.009***	-0.009***
	(0.002)	(0.002)	(0.003)
1 pp subsidy cut: year 10 after reform		-0.010***	-0.009***
		(0.003)	(0.003)
1 pp subsidy cut: year 11 after reform			-0.010***
			(0.004)
N	312,503	312,503	312,503

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

Table C.21: 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	293,534

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

Table C.22: 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	-0.000	-0.001
	(0.001)	(0.001)	(0.001)
1 pp subsidy cut: year 3 before reform	-0.001	-0.001	-0.002*
	(0.001)	(0.001)	(0.001)
1 pp subsidy cut: year 2 before reform	0.000	-0.001	-0.002***
	(0.001)	(0.000)	(0.001)
1 pp subsidy cut: year 0 after reform	-0.001	0.000	-0.001
	(0.001)	(0.000)	(0.001)
1 pp subsidy cut: year 1 after reform	-0.002	-0.001	-0.003***
	(0.001)	(0.001)	(0.001)
1 pp subsidy cut: year 2 after reform	-0.003***	-0.001	-0.002*
	(0.001)	(0.001)	(0.001)
1 pp subsidy cut: year 3 after reform	-0.004**	-0.002*	-0.002**
	(0.002)	(0.001)	(0.001)
1 pp subsidy cut: year 4 after reform	-0.005***	-0.002*	-0.004**
	(0.002)	(0.001)	(0.001)
1 pp subsidy cut: year 5 after reform	-0.005***	-0.002*	-0.004**
	(0.001)	(0.001)	(0.002)
1 pp subsidy cut: year 6 after reform	-0.006***	-0.002*	-0.004**
	(0.002)	(0.001)	(0.002)
1 pp subsidy cut: year 7 after reform	-0.007***	-0.003*	-0.004**
	(0.002)	(0.001)	(0.002)
1 pp subsidy cut: year 8 after reform	-0.008***	-0.003*	-0.005**
	(0.002)	(0.002)	(0.002)
1 pp subsidy cut: year 9 after reform	-0.008***	-0.003	-0.004*
	(0.002)	(0.002)	(0.002)
1 pp subsidy cut: year 10 after reform	-0.010***	-0.003	-0.005*
	(0.003)	(0.002)	(0.003)
N	312,503	652,099	409,551

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

Table C.23: Event study estimates: housing prices

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

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

Table C.24: 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.25: 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	0.000
	(0.008)	(0.001)
1 pp subsidy cut: year 3 before reform	-0.001	0.000
	(0.006)	(0.000)
1 pp subsidy cut: year 2 before reform	0.001	-0.000
	(0.005)	(0.000)
1 pp subsidy cut: year 0 after reform	0.003	0.000
	(0.003)	(0.000)
1 pp subsidy cut: year 1 after reform	0.003	0.000
	(0.005)	(0.001)
1 pp subsidy cut: year 2 after reform	0.002	0.001
	(0.008)	(0.001)
1 pp subsidy cut: year 3 after reform	0.005	0.001
	(0.010)	(0.001)
1 pp subsidy cut: year 4 after reform	0.002	0.001
	(0.014)	(0.001)
1 pp subsidy cut: year 5 after reform	0.001	0.002
	(0.017)	(0.001)
1 pp subsidy cut: year 6 after reform	0.001	0.002
	(0.020)	(0.001)
1 pp subsidy cut: year 7 after reform	0.002	0.002
	(0.022)	(0.001)
1 pp subsidy cut: year 8 after reform	0.001	0.002
	(0.023)	(0.002)
1 pp subsidy cut: year 9 after reform	0.000	0.002
	(0.025)	(0.002)
1 pp subsidy cut: year 10 after reform	0.007	0.002
	(0.028)	(0.002)
N	1,045	1,210

Notes: Standard errors in parentheses. See Figure A.10 for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.26: 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
1 pp subsidy cut: year 4 before reform	-0.001*	-0.004	-0.003
	(0.001)	(0.009)	(0.010)
1 pp subsidy cut: year 3 before reform	-0.000	-0.002	-0.002
	(0.001)	(0.007)	(0.007)
1 pp subsidy cut: year 2 before reform	-0.000	0.005	0.006
	(0.000)	(0.008)	(0.008)
1 pp subsidy cut: year 0 after reform	0.000	-0.007**	-0.007**
	(0.000)	(0.003)	(0.003)
1 pp subsidy cut: year 1 after reform	0.000	0.005	0.005
	(0.001)	(0.005)	(0.006)
1 pp subsidy cut: year 2 after reform	-0.000	0.007	0.008
	(0.001)	(0.008)	(0.009)
1 pp subsidy cut: year 3 after reform	0.000	-0.000	-0.001
	(0.001)	(0.007)	(0.007)
1 pp subsidy cut: year 4 after reform	0.001	-0.003	-0.004
	(0.001)	(0.008)	(0.009)
1 pp subsidy cut: year 5 after reform	0.002	0.005	0.003
	(0.001)	(0.008)	(0.009)
1 pp subsidy cut: year 6 after reform	0.002	0.001	-0.001
	(0.001)	(0.009)	(0.009)
1 pp subsidy cut: year 7 after reform	0.003*	-0.006	-0.009
	(0.001)	(0.008)	(0.009)
1 pp subsidy cut: year 8 after reform	0.003**	-0.001	-0.004
	(0.001)	(0.009)	(0.009)
1 pp subsidy cut: year 9 after reform	0.004**	-0.006	-0.010
	(0.002)	(0.010)	(0.010)
1 pp subsidy cut: year 10 after reform	0.004**	-0.008	-0.012
	(0.002)	(0.010)	(0.011)
N	1,210	1,210	1,210

Notes: Standard errors in parentheses. See Figures 5c, C.8a and C.8b for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.27: 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
1 pp subsidy cut: year 4 before reform	-0.000	-0.002	-0.002
	(0.001)	(0.003)	(0.003)
1 pp subsidy cut: year 3 before reform	0.000	-0.003	-0.003
	(0.001)	(0.003)	(0.003)
1 pp subsidy cut: year 2 before reform	0.000	-0.002	-0.003
	(0.001)	(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	0.002	0.001
	(0.001)	(0.001)	(0.002)
1 pp subsidy cut: year 2 after reform	0.000	0.002	0.002
	(0.001)	(0.002)	(0.002)
1 pp subsidy cut: year 3 after reform	0.000	0.002	0.002
	(0.001)	(0.002)	(0.002)
1 pp subsidy cut: year 4 after reform	0.002	0.002	0.001
	(0.001)	(0.002)	(0.002)
1 pp subsidy cut: year 5 after reform	0.001	0.005*	0.004
	(0.001)	(0.002)	(0.003)
1 pp subsidy cut: year 6 after reform	0.001	0.004	0.003
	(0.002)	(0.003)	(0.003)
1 pp subsidy cut: year 7 after reform	0.003*	0.004	0.000
	(0.002)	(0.003)	(0.004)
1 pp subsidy cut: year 8 after reform	0.004*	0.005	0.001
	(0.002)	(0.003)	(0.003)
1 pp subsidy cut: year 9 after reform	0.003*	0.006	0.003
	(0.002)	(0.004)	(0.004)
1 pp subsidy cut: year 10 after reform	0.004*	0.006	0.002
	(0.002)	(0.004)	(0.004)
N	1,210	1,210	1,210

Notes: Standard errors in parentheses. See Figures 5c, C.8a and C.8b for detailed information. Statistical significance denoted as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table C.28: Event study estimates: trade spillover

	(1) log manufacturing employment
1% trade exposure to 1 pp subsidy cut: year 4 before reform	-0.000006
	(0.000007)
1% trade exposure to 1 pp subsidy cut: year 3 before reform	-0.000007
	(0.000005)
1% trade exposure to 1 pp subsidy cut: year 2 before reform	-0.000005
	(0.000004)
1% trade exposure to 1 pp subsidy cut: year 0 after reform	-0.000006
	(0.000003)
1% trade exposure to 1 pp subsidy cut: year 1 after reform	-0.000006
	(0.000005)
1% trade exposure to 1 pp subsidy cut: year 2 after reform	-0.000013**
	(0.000006)
1% trade exposure to 1 pp subsidy cut: year 3 after reform	-0.000011
	(0.000007)
1% trade exposure to 1 pp subsidy cut: year 4 after reform	-0.000017*
	(0.000009)
1% trade exposure to 1 pp subsidy cut: year 5 after reform	-0.000015
	(0.000011)
1% trade exposure to 1 pp subsidy cut: year 6 after reform	-0.000018
	(0.000013)
1% trade exposure to 1 pp subsidy cut: year 7 after reform	-0.000020
	(0.000012)
1% trade exposure to 1 pp subsidy cut: year 8 after reform	-0.000018
	(0.000013)
1% trade exposure to 1 pp subsidy cut: year 9 after reform	-0.000025*
	(0.000013)
1% trade exposure to 1 pp subsidy cut: year 10 after reform	-0.000047*
	(0.000024)
N	2,555,361

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