Essays in Local Public Finance and Political Economy

Inauguraldissertation
zur Erlangung des akademischen Grades
eines Doktors der Wirtschaftswissenschaften
der Universität Mannheim

vorgelegt von

Tobias Etzel

im Herbstsemester 2018

Abteilungssprecher:	Prof. Dr. Jochen Streb
Referent:	Prof. Dr. Hans Peter Grüner
Korreferent:	Prof. Dr. Sebastian Siegloch

Tag der Verteidigung: 11. Dezember 2018

Meinen Eltern

Acknowledgements

I would like to thank Hans Peter Grüner for his dedication as a mentor and supervisor, for valuable feedback, for giving me the freedom to pursue my research interests and supporting my interest in economic policy.

My co-author Sebastian Siegloch has also been an exemplary supervisor. I thank him for his inspiration, helpful feedback, patience, writing tips, for sharing data and for providing motivation.

I would also like to thank Eckhard Janeba, who has advised and encouraged me throughout the PhD. The conversations with him have had a profound impact on my research and policy interests.

Antonio Ciccone, Sebastian Findeisen, Andreas Peichl and Ulrich Wagner also provided help and encouragement – I appreciate it.

Without my friends at the CDSE, I would not have been able to write this dissertation and would have enjoyed it a lot less. I thank you all, Mario, Tim, Simona, Ruben, Niklas, Hanno, Vahe, Xin, Inken, Fabian, Lukas, Frederik, Johannes, Justin, Felix, Christoph, Marcel, Esteban and Majed.

Thanks go to Astrid Reich, Marion Lehnert and Sylvia Rosenkranz for their patient assistance in many administrative matters.

Furthermore, I would like to thank Gerhard Fisch, Hartmut Clausen, Karolina Lyczywek and Karin Mack at the Federal Ministry for Economic Affairs for sharing data, for helpful comments and for hosting me in Berlin. Moreover, thanks go to Andre Küffe for patient responses to GRW data requests. I also appreciate support from the staff at the IAB in Nuremberg.

Finally, I am grateful for the support from the Collaborative Research Center SFB 884 "Political Economy of Reforms", funded by the German Research Foundation.

Contents

List of Figures

List of Tables

General Introduction

This dissertation is composed of three chapters, which can be read independently. Each chapter is concerned with some aspect of public expenditure: the optimal size of equalization payments, the labor market impacts of subsidies to capital and a politico-economic determinant of government spending, electoral thresholds.

The first two chapters are concerned with policies that respond to the fact that economic activity is distributed unequally across space within countries. In addition to the use of (people-based) policies that do not explicitly favor certain regions (e.g. unemployment insurance), governments across the world react to inequality across space by making use of place-based policies, explicitly favoring certain regions. The goal is typically to facilitate economic convergence (at least partially) of regions rather than permanent subsidization.

Two questions emerge: do place-based policies succeed in improving the situation of recipient regions? And if so, are these policies efficient? I analyze two specific policies to contribute to the scholarly effort to answer these questions. Chapter 1 is concerned with the assessment of the efficiency of fiscal equalization payments in the presence of agglomeration economies. Chapter 2 (co-authored by Sebastian Siegloch) investigates the employment and wage effects of capital investment subsidies, paying special attention to inter-regional as well as intraregional, inter-sectoral spill-overs.

Chapter 3 is concerned with an aspect of political economy, analyzing the electoral institution of explicit electoral thresholds. Excluding parties failing to reach a minimum vote share from representation, such thresholds have the potential to decrease the number of parties mechanically and by affecting the behavior of parties and voters. Economists have hypothesized that a large number of parties involved in budgeting may lead to higher government spending. I explore the effects of removing a threshold in the election of municipal councils on the number of parties represented and on expenditure.

In the remainder of this introduction, I briefly present each chapter.

Chapter 1: Evaluating the Efficiency of Fiscal Equalization in the Presence of Agglomeration: The Case of Germany

The first chapter theoretically and empirically studies conditions for optimality of fiscal equalization payments. In many federal countries, such payments are an important source of revenue of local governments. While the public debate is often focused on distributional effects of equalization schemes, the question of the payments' efficiency is typically neglected. In the absence of government intervention, an efficient allocation of labor across space is in general *not* achieved by market forces due to the presence of externalities and distortions. Equalization payments can correct inefficient incentives on the part of workers to locate in unproductive or congested regions (e.g. via their effect on the ability of local governments to provide public goods). The existing literature quantifies optimal equalization payments by taking into account the distortive effects across space of federal income taxation and public good congestion. I consider an additional externality, agglomeration economies, i.e. the fact that productivity is a function of employment density. If the strength of agglomeration effects varies across space, there is scope for an improvement over the market outcome since firms do not take into account the positive productivity effects when choosing labor.

I begin by extending the canonical spatial equilibrium model with equalization payments for agglomeration economies. Using administrative employment and wage data from Germany, I then estimate optimal payments as implied by the model and contrast them with actual equalization payments. Finally, I compute the dead-weight loss resulting from deviations of actual from optimal equalization payments. I find that actual payments, contrary to what is optimal, incentivize workers to locate in low-productivity regions. This leads to a dead-weight loss of approximately 0.5% of GDP. I also find that accounting for agglomeration profoundly alters the calculus of optimal equalization payments. The dead-weight loss of inefficient equalization is understated by a factor of 25 if agglomeration externalities are neglected.

Chapter 2: Efficiency and Equity Effects of Place-Based Policies: Evidence from Capital Subsidies in East Germany

This chapter is co-authored by Sebastian Siegloch. We estimate the causal effects on employment of capital investment subsidies mostly targeted at East German manufacturing firms post reunification. The policy analyzed was intended to revitalize the East German economy after reunification. Given the substantial differences in productivity between East and West Germany more than 25 years after reunification, the high degree of subsidization and the large volume of other transfers paid to regions in the East have received a lot of criticism. Some of the skepticism about the subsidies' efficacy also derives from the mobility of workers and firms, potentially leading to re-allocation across space as in a zero-sum game.

Our variation comes from quasi-experiments in the regional targeting of capital subsidies. In the late 1990s, policy-makers decided that due to the unequal economic development in East German regions, the intensity of subsidization should not continue to be uniform across space. Based on an indicator measuring economic well-being, counties were ranked and counties whose indicator value was above a certain cutoff value were assigned lower funding priority. The implication was that the maximum assistance rate, i.e. the maximum fraction of the cost of investment projects that could be covered by subsidies, was lowered in the affected regions. Similar changes in subsidy prioritization occurred in later years.

We compare counties below and above the cutoff values, making use of administrative data on employment at the firm level and on wages, in addition to data on subsidies. The event study research design we use allows us to compare treatment and control regions in multiple years before and after a reform occurs. This enables us to verify the identifying assumption and to explore the dynamics of treatment effects. We find that in counties in which subsidy payments are decreased, manufacturing employment reacts negatively. Overall employment is not affected, suggesting that one of the main variables targeted by the policy does not react. Wages of low-skilled workers decrease. However, when we conduct the analysis at the level of labor market regions (which include a number of counties each), the magnitude of effects is considerably smaller, suggesting that large spill-over effects are at play.

Chapter 3: Political and Economic Effects of Explicit Electoral Thresholds: The Case of German Municipalities

Electoral thresholds prevent parties from entering parliaments in many countries. Knowing more about their effects is important since thresholds are thought to have both disadvantages and advantages. On the one hand, thresholds lead votes to be disregarded and restrict party competition. On the other, they may prevent extreme fragmentation and may prevent excessively high spending by governments. In chapter 3, I explore their impact on political outcomes, such as the number of parties represented, and on government expenditure.

Exploiting the quasi-experimental removal of thresholds at the municipal level at different times in different German states, I implement an event study research design complemented by difference-in-differences estimates. The abolishments were triggered by court decisions, providing exogenous variation. The panel data set used is constructed from municipal election results and data on municipal finances. I find that the number of competing parties and the number of parties represented increase significantly when a threshold is removed. Concentration, measured by the Herfindahl index, decreases. The political effects are significantly larger in more populous municipalities. One reason for this heterogeneity is the higher number of seats on councils of larger municipalities, since implicit thresholds, which arise from the allocation of vote shares to seats, are decreasing in the number of seats. As for economic effects, the literature on the common pool problem predicts that when the number of parties involved in the budget process increases, so will spending. The reason is that each party only partially internalizes the cost of projects which are financed from a common pool of resources but enjoyed primarily by their own voters. I find that spending indeed increases as a result of threshold removals, in line with the theoretical prediction.

Chapter 1

Evaluating the Efficiency of Fiscal Equalization in the Presence of Agglomeration: The Case of Germany

1.1 Introduction

Market incomes vary enormously across space not only between but also within countries. Governments and supra-national organizations across the world use fiscal equalization payments to partially equalize incomes across regions. Within the European Union, the Structural and Investment Funds provide targeted funds to eligible regions but are financed from the common budget. In many federal countries (e.g. in Germany and Canada), conditional and unconditional fiscal equalization payments provide revenues to local governments whose revenues would otherwise not be sufficient to fulfill their assigned responsibilities. The payments also redistribute income across regions, making them oftentimes very controversial. Catalonia, for example, has long complained about its status as net-payer into Spain's re-distributive scheme, contributing to the rise of the independence movement (Heinemann, 2017).

While equity considerations dominate the political debate about the extent of regional redistribution, equalization payments can, if set correctly, help to achieve an efficient allocation of labor across regions. The reason is that equalization payments affect the incentives of mobile workers choosing their region of residence. Even if regional governments act optimally, labor is in general not allocated efficiently among locations due to externalities and distortions. One such externality is the congestion of publicly provided goods, the severity of which in different regions depends on the allocation of workers across space. Federal income taxation is distortive across space since if workers care about real wages (i.e. wages net of housing rents), they are indifferent between high-wage (and therefore high-productivity) locations where rents are high and low-wage locations where rents are low. However, income tax payments are higher in regions were (nominal) wages are high. Federal equalization payments can correct these effects and ensure that an optimal outcome obtains. Assessing the efficiency of a country's equalization scheme amounts to estimating Pareto-optimal equalization payments and comparing them to actual equalization payments.

In this paper, I investigate the impact on optimal equalization payments of an externality neglected by the literature, agglomeration economies in production. Beginning with Ciccone and Hall (1996), a vast literature on the reduced-form effect of employment density on productivity has documented the importance of this externality (see Rosenthal and Strange (2004) for a summary). Such effects may result from various underlying mechanisms, three of which are proposed already in Marshall (1920): knowledge spill-overs among workers, the availability of intermediate goods whose production exhibits increasing returns to scale and labor market thickness improving the quality of matches.[1]

The model builds on the long run spatial equilibrium framework of Albouy (2012). On top of an exogenous regional productivity component e.g. due to the availability of natural resources, the presence of a research institution or airport, it is the density of employment that endogenously determines the productivity of firms producing tradable goods in a region. Since the positive impact on regional productivity is not taken into account by firms when they add a worker, wages only reflect part of the marginal product. If the responsiveness of output to the density of employment varies across regions, an inefficient allocation may then obtain since moving a worker from one region to another has the potential to reduce output in the origin region by less than the increase that materializes in the destination region. Appropriately chosen equalization payments can ensure internalization on the part of workers when they make their location choice. I estimate agglomeration effects along with other distortions and check whether equalization in Germany corrects for inefficient incentives.

Using German data, I analyze a country whose equalization scheme is particularly interesting since, in addition to payments between states (Bundeslaender), equalization occurs within-state. I aggregate equalization payments received by municipalities and counties to the level of labor market regions which are defined as groups of counties, taking into account commuter flows. Relying on the structural approach to estimate agglomeration elasticities at different levels of density introduced by Kline and Moretti (2014a), I analyze regional heterogeneity in agglomeration economies. In order to identify differential wage income tax payments, I estimate an individual-level wage equation using administrative micro data.

The empirical results show that agglomeration forces can fundamentally alter the distribution of efficient equalization payments. The estimated distortive effects of wage income taxation and congestion are dwarfed in magnitude by the agglomeration effects. Actual equalization

[1] Additional channels have been proposed and the relative importance of channels has been analyzed empirically (Rosenthal and Strange, 2004). However, for the present paper, what matters is the reduced-form relationship.

payments do not correct for inefficient incentives. On the contrary, I estimate a correlation coefficient between actual and efficient payments of -0.42. The main reason is the bias of payments towards East Germany, where productivity remains low even close to thirty years after reunification. Importantly, also the agglomeration effects are lower, meaning that reallocation of workers to the West increases output by more than the resulting reduction in the East. Since equalization payments inefficiently lower incentives to migrate to the West, East Germany is overpopulated relative to the optimum. I calculate a dead-weight loss of existing equalization payments of approximately 0.5% of GDP. Calculating optimal equalization payments under the assumption of zero agglomeration effects yields a dead-weight loss smaller by factor 25.

I contribute to the literature on optimal equalization payments by quantifying the role of agglomeration economies. Boadway (2004) provides a good summary of the canonical model and extensions. Flatters et al. (1974) and Boadway and Flatters (1982) develop the basic two-region model and establish the result that mobile labor is in general located inefficiently in equilibrium in the presence of congestion of the public good and that equalization payments can restore optimality. Albouy (2012) incorporates amenities, regionally varying productivities and introduces the distortion induced by federal wage income taxation based on nominal incomes in spatial equilibrium (analyzed first in Albouy (2009)). Albouy (2012) also provides the model's first empirical application. Looking at Canada, he finds that inefficient equalization among provinces results in a dead-weight loss of approximately 0.4% of GDP, ignoring agglomeration economies. Recently, Henkel et al. (2018) have analyzed the efficiency of fiscal equalization in Germany in a calibrated model, illustrating the potential importance of agglomeration forces. However, they do not estimate agglomeration's effects, the regional heterogeneity of which is crucial to assess the optimality of equalization payments.

A related strand of the literature considers the incentives on the part of local governments that arise due to fiscal equalization. Buettner (2006) theoretically predicts and empirically finds that German municipalities raise their local business tax rate when they face equalization payments that decrease more strongly in the local tax base.[2] Janeba and Peters (2000), Bucovetsky and Smart (2006) and Köthenbürger (2002) analyze this incentive in conjunction with tax competition and find that it can offset the incentive of individual regions to set tax rates below the efficient level in equilibrium.

I also add to the recent literature on local labor markets which is broadly concerned with place-based policies that affect a country's regions differentially, of which Kline and Moretti (2014b) and Moretti (2011) provide a good summary. Busso et al. (2013) investigate the efficiency of the Federal Urban Empowerment Zone program in the U.S. which includes, among its various instruments, a wage subsidy to poor neighborhoods of large cities. They find a low dead-weight loss due to a low elasticity of employment with respect to the subsidy. Glaeser and Gottlieb (2008) establish the result that reallocation of economic activity is

[2] In the typical scheme, equalization payments are lower when the tax base is higher.

a zero-sum game unless the magnitude of agglomeration economies varies across locations. Kline and Moretti (2014a) use a spatial equilibrium model to structurally estimate the size of agglomeration economies at different density levels for the U.S. as part of an evaluation of spatially targeted infrastructure investments. Following their approach, I consider how the optimal design of another place-based policy, fiscal equalization, is affected by agglomeration economies.

The remainder of this paper is organized as follows: I outline the model in section 1.2, which is followed by sections on institutional details and the data. Section 1.5 outlines the research design. Empirical results are presented in section 1.6. I then compute the policy's dead-weight loss in section 1.7. Section 1.8 concludes.

1.2 Model

I extend Albouy (2012)'s spatial equilibrium model with equalization payments, allowing for agglomeration in the production of tradable goods. In the tradition of Flatters et al. (1974), it is the impact that equalization payments have on the allocation of labor across space that is of central interest, rather than equity considerations. Essential is therefore the location choice mobile workers make. In the spirit of Rosen (1979) and Roback (1982), the model's equilibrium is characterized by a no-arbitrage condition, according to which utility does not vary across space such that there are no incentives to move. I characterize efficient equalization payments by first solving the social planner's problem and then using budget constraints of workers and governments and market prices.

There is a continuum of mass one of workers, of which N^j choose region j[3] and derive utility $U\left(x, y, g^j, Q^j\right)$ from exogenous amenities Q^j, the consumption of a tradable good x (the numeraire), housing y and a publicly provided good G^j. The publicly provided good is congested according to the parameter $\omega \in [0, 1]$, which includes the possibilities of it being a purely public or purely private good: $g^j = \frac{G^j}{(N^j)^\omega}$. Amenities reflect quality of life, e.g. due to proximity to a natural body of water. Each worker inelastically supplies one unit of labor in the region of residence.

Production of the tradable good X using as inputs mobile capital K_X, mobile labor N_X and the fixed factor land L_X in region j is described by $F_X\left(K_X^j, L_X^j, N_X^j, A_X^j\right)$, where $A_X^j = A_X\left(N_X^j\right)$ denotes a region's productivity. Here, I endogenize productivity and allow for agglomeration externalities in reduced form as in Glaeser (2008) by assuming that firms take regional population as given when they choose inputs. The supply of capital is fixed at the national level. Land is variable from a firm's perspective, consistent with constant returns to scale. However, at the regional level, the fact that land is a fixed factor leads to decreasing returns to scale. Since agglomeration economies are not modeled explicitly, all

[3] In the empirical part, the baseline geographical entity is the labor market region, whose definition ensures that the lion's share of commuting occurs within region (see section 1.3 for institutional details).

above-mentioned channels are consistent with this specification (e.g. knowledge spill-overs). Land owners are the residual claimants in this model and the portion of the marginal product not reflected by wages therefore accrues to them. Production of housing (F_Y) and the government good (F_G) also use capital (K_Y and K_G), land (L_Y and L_G) and labor (N_Y and N_G) but are characterized by exogenous productivity levels.[4] Prices are taken as given on all markets. Production in all sectors takes place within region.

Households pay federal wage income taxes (depending on local wages) and own a portfolio of land and capital. The portfolio pays the same return regardless of where agents reside and is therefore not modeled. Importantly, they also receive a location-dependent equalization payment F^j (which may be positive or negative) from the federal government.[5] Local governments efficiently produce the publicly provided good G^j and raise revenues using source-based taxes on land and capital (τ_L^j and τ_K^j).[6] The federal government uses wage income taxes (τ_w^F) to finance equalization payments to workers.[7] Other expenses by the federal government are assumed to benefit workers equally in all regions and are therefore irrelevant for workers' location decision. The federal government is assumed to move first, anticipating the other actors' reactions, followed by local governments. Firms and workers finally choose production levels and location, respectively.[8]

Karush-Kuhn-Tucker necessary first order conditions characterize the social planner's solution (see Appendix for details). Using market conditions[9] for prices, I arrive at the following condition that equalization payments F^j received by workers in region j satisfy if they are

[4] The evidence in Brülhart and Mathys (2008) suggests that agglomeration economies do not play a role in the construction sector. In addition, the channels underlying agglomeration effects mentioned above seem to matter little in the public sector.

[5] While unrealistic, this assumption is necessary to derive the dead-weight-loss due to inefficient payments in section 1.7. Nevertheless, I explore the implication of local governments receiving the payment instead in the Appendix.

[6] More precisely, I assume provision of the publicly provided good consistent with the Samuelson condition amended for congestion, following the literature (Boadway, 2004) Doing so allows for a direct comparison of my empirical results with existing estimates that are based on models that ignore agglomeration economies. The drawback of imposing Samuelson provision is that it does not characterize optimal local government behavior under distortionary taxation. In addition, local governments are effectively assumed to behave strategically naive. However, in a model without local public goods, uniqueness would still obtain under my assumptions due to the fixed factor land. Furthermore, ignoring local public goods would leave the main results unaffected, as is evident from the empirical analysis.

[7] Since equalization payments may be positive or negative, a net-scheme requiring no revenues on the federal government's part is also feasible.

[8] This assumptions is not innocuous. If local governments move first and the federal government cannot commit to its choice, local governments might overspend, counting on a bailout by the federal government (see Köthenbürger (2007) for details).

[9] Sufficiency, the existence of an interior solution and uniqueness of the market outcome are discussed in the Appendix for particular functional form assumptions. In particular, I assume that local labor demand is downward sloping by assuming that crowding of the fixed factor land outweighs endogenous productivity gains when employment in the tradable sector increases. This assumption is backed by my empirical results. Note that the empirical strategy does not rule out multiple equilibria ex-ante but that the results suggest uniqueness.

efficient, i.e. if they act to set migration incentives appropriately[10]:

$$F^{j\star} = \tau_w^F w^j - \omega \frac{\tau_L^j r^j L^j + \tau_K^j i K^j}{N^j} + \frac{\partial F_X^j}{\partial A_X} \frac{\partial A_X^j}{\partial N_X} + \overline{F} \tag{1.1}$$

where w^j is the wage in region j, r_j is the price of land[11] and i the price of capital. The location-independent transfer $\overline{F}$ ensures that the federal budget constraint is satisfied (see Appendix for details).

This equation tells us that equalization payments should offset differential federal wage income tax payments which lure households away from highly productive (and thus high wage) areas. These tax payments based on nominal income make high-wage regions unattractive to workers even though the real wage (after housing cost) is equal across locations in spatial equilibrium, distorting the allocation of labor towards low-productivity regions. Congestion of the publicly provided good should be penalized (to the extent that it varies across regions) in proportion to local government spending since agents do not take into account that their consumption is (partially) at the expense of existing residents. If ω is small, meaning that congestion is relatively unimportant, the corresponding term receives a lower weight.

The novelty is the term capturing the increase in output as a result of higher density. The intuition is that moving a worker from one region to another produces a density-induced output loss in one region and a gain in another region. If the loss perfectly offsets the gain, a correction is unnecessary. If however the absolute value of the two effects differ, a reallocation is beneficial. Optimal equalization payments thus include a Pigouvian correction for public good congestion and for agglomeration externalities. Note that the price of housing does not appear, consistent with the fact that housing markets are not distorted.

The elements of this equation are either observable directly or estimable such that the hypothesis of actual equalization payments in Germany being efficient is empirically testable. Before I proceed by quantifying the components of equation (1.1), I will briefly outline the relevant institutions and the data.

[10] In the tradition of the literature, I focus on the allocation of labor (Boadway, 2004) However, I briefly discuss the condition relating to capital in the Appendix.

[11] More precisely, there is a price for each of the fixed factors used in the three sectors of production: L_X, L_Y and L_G. I assume that they are all taxed at the same rate. $\tau_L^j r^j L^j$ represents total local tax revenues from the taxation of land.

1.3 Institutional details

In Germany, fiscal equalization takes place both between and within states (Bundeslaender).[12] Transfers between states matter for this paper only to the extent that they affect local governments via within-state equalization schemes, which are less well-known but provide an important source of revenue for municipalities[13] and county (Kreis-) governments (Buettner and Holm-Hadulla, 2008). There are some minor differences in the design of these schemes across states, but the basic features are very similar: municipalities and counties pay into the system or receive payments according to whether their fiscal capacity exceeds a measure of their fiscal need and the state typically contributes own funds to the scheme.

The system of tax revenue sharing is also an important source of revenue for local governments (also see descriptive statistics in the next section). Quantitatively, the two most important revenues for municipalities of this category are their shares of the wage income tax and value added tax. Together, they account for 43% of municipal revenue from taxes.[14] I treat these revenues as equalization payments, implicitly assuming that the sharing agreement can be adjusted unilaterally by the federal government. In the Appendix, I relax this assumption.

I aggregate equalization payments received by municipalities and counties[15] to the labor market region, regardless of whether they originate at the federal or state level.[16] Labor market regions are the natural entities for the analysis of agglomeration economies: they consist of one or more counties and are defined based on commuting patterns with the goal of obtaining regions resembling the theoretical concept of local labor markets more closely than counties do (a county always belongs to only one labor market region entirely). The drawback is that these regions are not of administrative nature and have no budget

[12] In a nutshell, between-state equalization for the time period observed comprises four steps: first, federal and state governments and municipalities share the revenues from a number of taxes (e.g. from income tax revenues, the federal government receives 42.5%, state governments receive 42.5% and municipalities 15%). Second, the states' share is distributed among the individual states, partly based on where tax revenue is collected and partly based on need, which achieves equalization in revenues to a certain degree. In the next step, explicit redistribution between states occurs based on a comparison of per capita financial capacity, which leads to further but not to complete equalization in revenues. Finally, the federal government grants further payments to particularly poor states and local governments (see https://www.bundesfinanzministerium.de/Content/DE/Standardartikel/Themen/Oeffentliche_Finanzen/Foederale_Finanzbeziehungen/Laenderfinanzausgleich/Eng-Der-Bundesstaatliche-FAG.pdf?__blob=publicationFile&v=1, retrieved February 1, 2018). A recent reform of the between-state scheme, passed in 2017 and hence not applicable to the time period observed in this paper (1995-2010), has formally ended cross-payments by states (Hentze, 2017) and each state will receive more funds under the new scheme at the expense of the federal government. However, quantitatively, major changes in differential payments received by states are not to be expected.

[13] E.g. on average, 20% of revenues came from such a scheme in the state of North Rhine-Westphalia in the year 2005.

[14] Own calculation based on the municipalities' official budgetary reports: http://www.staedtetag.de/imperia/md/content/dst/veroeffentlichungen/gemeindefinanzbericht/gemeindefinanzbericht_2017_langfassung.pdf, retrieved February 1, 2018.

[15] See Appendix for a detailed list of the payments considered.

[16] To the extent that goods and services (rather than payments) are provided by state governments directly, these are not accounted for in my calculations. Similarly, differences in the efficiency of public good provision may be relevant for workers' location choices. However, both aspects are reflected in the model by Q^j.

themselves. Note that in the model, all payments are federal payments. For the purpose of this paper, I therefore disregard the role of states and assume that the federal government can control payments to local governments.

Municipalities can influence their revenues by choosing multipliers for the property tax (Grundsteuer) on land and the local business tax (Gewerbesteuer) on profits.[17] The local business tax rate is the product of a federal rate, uniform across locations, and the local rate chosen by a municipality's council while the tax base is also determined federally. Similarly, the property tax rate is determined by the product of a federal rate and a locally chosen multiplier.[18] Combined, they make up 55% of municipal revenue from taxes.[19] I take the local business tax and the property tax as the equivalents to the model's source-based taxes on capital and land.

1.4 Data

I rely on publicly available data by the statistical offices[20] to compute equalization payments and tax revenues at the labor market region level, using data for the time period 1995-2010 (in the baseline of the empirical part, I look at the year 2005 but explore the sensitivity of my analysis to this choice). Equalization payments are received by municipalities and by counties and I aggregate both payments to the labor market region level. Source-based tax revenues accrue to municipalities in the form of local business taxes and property taxes. I also aggregate them to the labor market region level.

As for labor market specific wage premia, I rely on the weakly anonymous Sample of Integrated Labor Market Biographies (SIAB)[21] provided by the Institute for Employment Research (IAB) (Antoni et al., 2016) in Nuremberg. This data set consists of a two percent random sample of worker biographies recorded in the German social security system going back to 1975 (for West Germany), resulting in biographies of roughly 1.8 million workers. Public servants and self-employed are not included since they do not normally pay social security contributions. Detailed information is included on wages, the place of work, the employer's sector, education and other relevant individual characteristics.

In terms of firm data, I use the waves of 1980, 1990, 2000 and 2010 of the weakly anonymous Establishment History Panel (BHP)[22] also provided by the IAB (Schmucker et al., 2016),

[17] The local business tax is paid by all firms outside the agricultural and public sectors. In addition, corporate firms pay corporate taxes (Koerperschaftsteuer) and non-corporate firms pay personal income taxes (Einkommensteuer).

[18] More specifically, there is a property tax on agricultural land (Grundsteuer A) and a property tax on non-agricultural land (Grundsteuer B), each with its own multiplier.

[19] Own calculation based on the municipalities' official budgetary reports (see footnote above)

[20] `http://www.regionalstatistik.de`, available permanently

[21] Data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the IAB and remote data access.

[22] See footnote above on data access.

to estimate agglomeration elasticities. This fifty percent random sample of establishments is based again on social security records.. The annual number of establishments observed varies between 640 000 and 1.5 million. The BHP allows for aggregation of employment and wages by labor market region and – crucial for this paper – by sector. I exclude the construction sector and governmental institutions from the set of all industries to obtain employment in the tradable sector since this follows the model most closely.

Finally, the German internal migration database (Sander, 2014) forms the basis for estimating the elasticity of population with respect to equalization payments, which will be required in section 1.7. This panel of annual movements (changes in permanent residence) between each pair of counties makes use of the German population register. I use county border definitions valid in the year 2014 and map data recorded for deviating border definitions to the 2014 definitions.

Table 1.1: Summary statistics at the labor market region level

Variable	Obs	Mean	Std. Dev.
Land	4128	1374	937
Population	4128	318528	405329
No. of counties	4128	1.56	1.03
Local government revenue (per capita)			
Fees	4128	217	103
Credit	4128	128	119
General equalization payments	4128	689	212
Investment subsidies	4128	153	101
Wage income tax (via sharing)	4128	273	101
VAT (via sharing)	4128	27	16
Local business tax	4128	247	129
Property tax A	4128	7	5
Property tax B	4128	104	29
Yearly gross earnings	23175028	22197	20707

Notes: There are 258 labor market regions which are observed for 16 years: 1995-2010. Gross wages are based on 1975-2010. Land is measured in square kilometers, all monetary variables are measured in €.

Table 1.1 presents descriptive statistics at the labor market region level. On average, about 320 000 people live in a region consisting of 1.6 counties. By far the most important source of revenue are equalization payments, amounting to roughly €690 per capita, followed by wage income taxes (via revenue sharing) at about €270 per capita and local business tax revenue. Yearly earnings (the average of which is €22 200) are based on both full-time and part-time employment.[23]

[23] Note that wage data from the SIAB is right-censored, i.e. payments are only recorded up to the highest amount relevant for the social security system.

1.5 Empirical research design

In accordance with the literature, I define the net fiscal benefit (NFB) as the difference between actual equalization payments and optimal payments from equation (1.1), ignoring the location-independent transfer:

$$NFB^j := F^j - \tau_w^F w^j - \frac{\partial F_X^j}{\partial A_X} \frac{\partial A_X^j}{\partial N_X} + \omega \frac{\tau_L^j r^j L^j + \tau_K^j i K^j}{N^j} \tag{1.2}$$

Using this definition, if equalization payments are optimal, it must be true that $NFB^j = \overline{F}$ for all j and some constant $\overline{F}$.

In this section, I describe how I empirically quantify the NFBs' components. Below, I outline the strategies used to obtain estimates of wage income tax payments and the agglomeration effects. Actual equalization payments F^j are observable directly. The same applies to per-capita source-based tax revenues from the property tax and the local business tax. Finally, I set ω equal to one following Bergstrom and Goodman (1973), exploring the sensitivity of results to this assumption in the Appendix.

1.5.1 Wage income tax payments

Labor market region specific wage premia w^j have to be estimated to quantify differential wage income tax payments and agglomeration effects. In order to obtain the causal effect of working in a region j on the wage earned, I follow Glaeser and Maré (2001) and Hirsch et al. (2016) and estimate

$$\ln \overline{w_{ijt}} = \delta_j + \delta_i + \mu_t + X_{it}\beta^w + \epsilon_{ijt}^w \tag{1.3}$$

where $\overline{w_{ijt}}$ is the wage of individual i in region j at time t. The inclusion of individual-specific fixed effects δ_i implies that identification of the log labor market specific wage premium $\delta_j = \ln w^j$ comes from workers who move across regions. This specification ensures that the estimated wage premia are not contaminated by sorting of workers based on unobservables (e.g. highly productive workers sorting into high-productivity areas). I also add individual controls X_{it}. $\ln w^j$ represents the portion of the log wage in region j that is due to location. This labor market region-specific premium may be due to productivity differences that are the result of agglomeration economies.[24]

The SIAB includes worker characteristics such as education, age and gender but falls short of providing detailed wage income tax-relevant information. Mainly, it is the lack of tax code information that prevents calculation of the tax payment. I approximate the wage income tax payment using a linear tax rate of about 25%, also accounting for social security

[24] Productivity is a function of population, which is reflected in wages when firms take productivity as given, in contrast to $\frac{\partial F_X^j}{\partial A_X} \frac{\partial A_X^j}{\partial N_X}$. Model details can be found in the Appendix.

payments which depend on the wage.[25] In the Appendix, I provide approximations based on assumptions on individuals' tax code. Note that the distortion induced by the wage income tax as described does not depend on the progressivity of the actual tax schedule (however, progressivity leads to a larger variance of tax payments).

1.5.2 Agglomeration effects

In order to be able to estimate the agglomeration effects in the tradable sector, I follow Kline and Moretti (2014a) and assume production of tradable goods to be Cobb-Douglas: $F_X(.) = A_X^j \left(K_X^j\right)^\alpha \left(L_X^j\right)^\beta \left(N_X^j\right)^{1-\alpha-\beta}$, where productivity is assumed to obey $\ln A_X^j = g\left(\frac{N_X^j}{R^j}\right) + \nu^j$. The constant region-specific productivity effect ν^j is due to fixed factors such as the availability of natural resources, of an airport or the proximity to a research institution. In contrast, agglomeration is an endogenous force that depends on the density of employment (R^j denotes an area's size) in the tradable sector which may affect productivity through a variety of different channels, as discussed above. According to this definition, the magnitude of agglomeration economies is region-specific only to the extent that density varies across locations, which facilitates estimation. This functional form implies

$$\frac{\partial F_X^j}{\partial A_X} \frac{\partial A_X^j}{\partial N_X} = \sigma^j \frac{w^j}{1-\alpha-\beta} \tag{1.4}$$

where $\sigma^j = \frac{\partial \ln A_X^j}{\partial \ln\left(\frac{N_X^j}{R^j}\right)} = g'\left(\frac{N_X^j}{R^j}\right)\frac{N_X^j}{R^j}$ is the elasticity of productivity with respect to the density of employment in the tradable sector and w^j is again the labor market region-specific wage premium.

It is the functional form of $g(.)$ that is of primary interest here. If $g(.)$ is log-linear in density of employment, implying a constant σ, reallocation of labor among regions exhibiting the same wage is a zero-sum game: the resulting increase in productivity in one region is exactly offset by the decrease in productivity in the other region.[26]. When wages differ, reallocation is not a zero-sum game and the variance of wages determines the variance of agglomeration effects. If $g(.)$ is non-linear in logs, however, moving a worker from a low-σ^j region to a high-σ^j region may be beneficial as output increases by more in one region than it decreases in another. In addition, σ^j is informative about the slope of local labor demand in a given region at different levels of density[27]

[25] In the year 2005, an unmarried person earning €22200 paid approximately 25 % in taxes and social security contributions (if pension insurance contributions are subtracted, as these determine pension entitlements at least partially).

[26] Note that σ is unrestricted. In particular, the assumption of log-linearity required for sufficiency and the existence of an interior solution of the planner's problem as well as for uniqueness of the market outcome as stated in equation (A.9) of the Appendix is not required for estimation of σ

[27] This is the case since labor demand contains the level of density in $\ln A_X^j$. Details can be found in the Appendix.

In order to estimate this elasticity, I interpret the static model's equilibrium in section 1.2 as the steady state of a dynamic model which obeys the production function in each time period t, measured in decades, with the addition that agglomeration is allowed to operate with a decadal lag (see equation (A.10) in the Appendix). As in Kline and Moretti (2014a), this lag makes the model estimable and prevents unrealistic large changes in density between two periods that could occur without a lag.

Since the functional form of the productivity-density relationship is unknown, it is desirable to estimate this relationship in a very flexible manner. I assume that the agglomeration function can be written as $g\left(\frac{N^j_{X,t}}{R^j}\right) = \sum_{k=1}^{3} \theta_k g_k\left(\frac{N^j_{X,t}}{R^j}\right)$. The spline functions $g_k(.)$ are chosen as the logarithm of tradable employment density at different levels of density[28], consistent with $g(.)$ being piecewise log linear. I divide the density range into three intervals $(k = 1, 2, 3)$.[29] The θ_k then represent the agglomeration elasticity for the relevant interval. It is the model's labor demand equation that suggests identification of agglomeration elasticities by using data on employment and wages.[30] Differencing the labor demand equation over time[31] yields the estimating equation

$$\Delta \ln N^j_{X,t} = -\frac{1-\alpha}{\beta}\Delta \ln w^j_t + \sum_{k=1}^{3} \frac{\theta_k}{\beta}\Delta g_k\left(\frac{N^j_{X,t-1}}{R^j}\right) + \Delta\phi_t + \gamma^j_t \tag{1.5}$$

where Δ represents the difference of a variable over time, ϕ_t are period fixed-effects and γ^j_t is an error term. The change in employment over time is therefore regressed on the change in wages over time (a movement along the labor demand curve for a given density interval) plus the agglomeration-induced change in labor demand, which is a function of past employment levels.

Identification is difficult: firstly, bias may result from correlation of the error term with shocks that affect labor supply such as shocks to amenities. For example, a natural disaster may affect labor supply decisions while simultaneously shocking labor demand. As a remedy, I calibrate the labor demand elasticity for a given level of productivity instead of estimating it. In the next section, where I present the empirical results, I discuss the robustness of my estimates with respect to different choices of the elasticity's magnitude.

Furthermore, serial correlation of the error term may threaten identification since trends in employment might be confused with agglomeration effects. This is due to the fact that serial correlation would induce correlation of the spline components and the error term. Therefore, in addition to estimating equation (1.5) using OLS, I follow Kline and Moretti

[28] For robustness, I also conduct the estimation using linear splines. Results are presented in the Appendix.

[29] In the baseline, I use the 50th and the 75th percentile of the density distribution in the first year (1980) as knots of division in order to get approximately the same number of observations in all intervals but explore the sensitivity of estimates to this choice in the Appendix.

[30] I use decadal wages in the tradable sector from the BHP. Note that in the model, wages are equalized within region across sectors.

[31] See equation (A.7) of the Appendix.

(2014a) and instrument the spline functions of employment density with its decadal lags, i.e. $\Delta g_k\left(\frac{N^j_{X,t-2}}{R^j}\right)$.

In order for these instruments to be valid, employment density in period $t-1$ is required to be correlated with employment density ten years earlier (instrument relevance) but this twice lagged density must not be correlated with today's value of γ^j_t (the exclusion restriction). The former is implied by the model since density affects employment with a lag via agglomeration. The latter assumes that productivity shocks today are not related to productivity shocks two decades ago.

The instruments chosen exploit the fact that even if productivity shocks are correlated in the short run, they may not correlated over the course of twenty years. While the literature provides evidence for this hypothesis in general (Eeckhout, 2004), in the context of Germany's economic development since 1990, East German regions constitute a special case. Their catching up to GDP levels closer to those of West German regions indeed suggests the presence of persistence in shocks (Burda and Hunt, 2001). I therefore conduct the estimation for West German regions separately in addition to an estimation using all regions and also include state-decade fixed effects throughout. Below, I interpret the differences in results from OLS and 2SLS estimation.

1.6 Empirical results

In this section, I present my empirical results of the components of the net fiscal benefit (NFB) as defined in equation (1.2), beginning with quantities that are observable directly and do not require estimation, namely equalization payments and source-based tax revenues. The distribution of equalization payments relative to the mean in the year 2005 is presented in figure 1.1. Payments do not vary substantially over the period 1995-2010 (as demonstrated in figures A.10a and A.10b) and the same holds for the other terms of equation (1.2).[32] As expected, the per capita amounts are high predominantly in East Germany, where the states receive large payments from the state-level equalization scheme which they evidently pass on to a considerable extent to local governments. Nevertheless, there is substantial heterogeneity in payments received by regions in West Germany. The distribution of payments is summarized in table 1.2 along with distributions of the other components of the NFB which I will explain in what follows. Source-based tax revenues from local business and property taxes exhibit the lowest degree of variability. Figure A.3a (in the Appendix) shows the geographical distribution which is clearly inversely related to equalization payments.

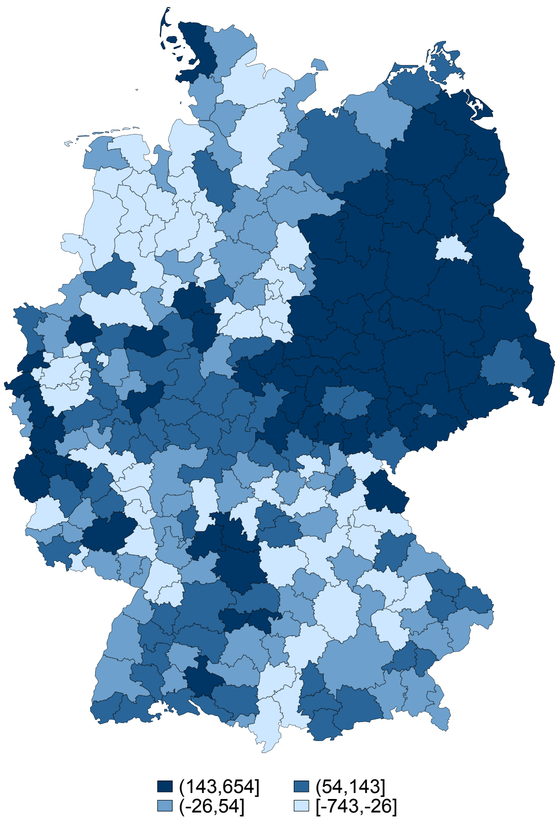
[32] Since I check an optimality condition that corresponds to a long-run equilibrium, it is important that the model's empirical counterparts are sufficiently stable over time.

Table 1.2: Components of the NFB

Variable	Obs	Mean	Std. Dev.
Equalization payments	258	1073	166
Source-based revenues	258	382	130
Wage income tax payment	258	3925	574
Agglomeration effect	258	16257	2376

Notes: The amounts are in Euro per capita, measured at the labor market region level for the year 2005 (wage premia are estimated using data covering 1975-2010, agglomeration elasticities are estimated using data covering 1980-2010).

Figure 1.1: Equalization payments (year 2005)



Notes: This map shows equalization payments in Euro per capita relative to the population weighted mean for each labor market region. The distribution is partitioned into four intervals containing an equal number of observations. Darker colors indicate higher values.

1.6.1 Wage income tax payments

The second term of the NFB is the wage income tax payment that is due to the labor market region-specific wage. The labor market region-specific wage premia w^j are estimated with high precision, as can be seen from table A.1. The estimates in column 1, which are used henceforth, are based on the log wage equation (1.3) without controls. Since the dependent variable is the log wage, the estimates can be interpreted as percentage deviations from the base category (the labor market region Husum). For example, wages in Hamburg exceed those in Husum by on average 9% while wages in Dresden are lower by 39%. Adding time-varying controls such as education or (one-digit) industry does not meaningfully alter the estimates (recall that identification comes from individuals that move from one labor market region to another).[33]

Using these estimates, I first calculate the wage premium in levels. The mean gross labor market wage premium is about €16 000.[34] Figure 1.2 presents the geographical distribution of the results relative to the (population weighted) mean. Here, we see that wage premia are highest in the South and in some parts of the West but lowest in the East. Munich, Frankfurt and Wolfsburg[35] are the labor market regions exhibiting the highest wage premia.

Wage income tax payments, the mean of which is shown in table 1.2, are approximated as a quarter of the wage premium as explained in section 1.5. In the Appendix, I also calculate the tax payment using actual tax rates in place in the year 2005.[36] Figures (A.6a) and (A.6b) show the distributions of wage income tax payments relative to the mean for the approximation and the exact calculation. While the magnitudes are overall very similar, the distribution of exact tax payments is skewed to the right, reflecting progressivity.

1.6.2 Agglomeration effects

As for the effects of agglomeration, I first present OLS and IV estimates of the agglomeration elasticity with respect to the density of tradable employment based on equation (1.5).[37] Column 1 of table 1.3 shows OLS estimates of $\frac{\theta_k}{\beta}$ for three different levels of density (low, medium, high). The results support the conjecture of an agglomeration elasticity constant across density levels. Furthermore, the estimates confirm that $\sigma < \beta$ holds[38], which implies that labor demand in a location is downward sloping.[39] The IV estimates of column 2 confirm this hypothesis: the coefficients are generally similar and differ only in absolute but not in relative magnitude. An F-test cannot reject the null hypothesis of equal coefficients.[40]

[33] The estimated effects are stable over time, as can be seen in figures A.9a and A.9b.

[34] Overall mean gross earnings (which include the worker-specific and year-specific effects next to the region-specific part) are equal to about €22 200. I have part-time workers in the baseline sample but explore the robustness to excluding them (compare table A.1).

[35] Wolfsburg is located to the West of Berlin and is home to the VW automotive manufacturing company.

[36] I add the mean individual fixed effect to region-specific wage premia before calculating taxes. Lacking information on marital status, I assume individual filing. In these calculations, I do not take social security payments into account.

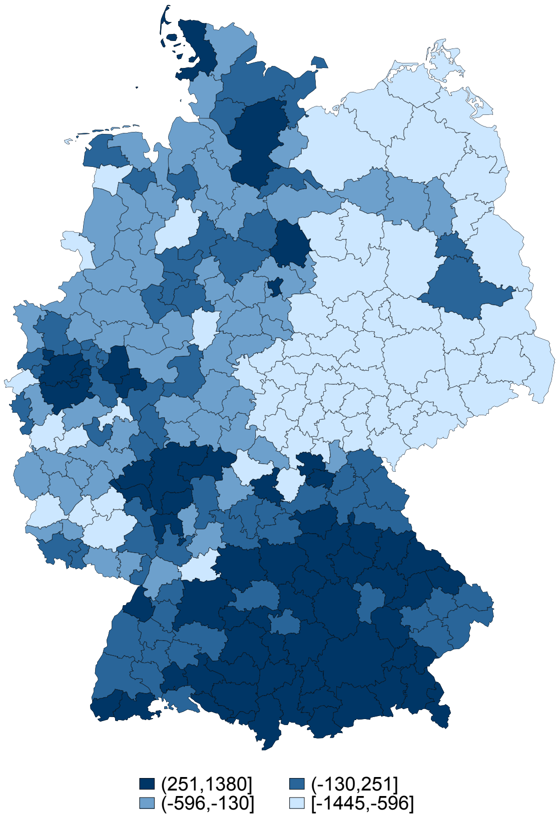
[37] Recall that I use decadal data from 1980 onwards. This is necessitated by the instruments used (employment densities lagged twice and three times). Therefore I cannot include East Germany. However, I report OLS results using all German regions in the Appendix. In addition, note that I cannot separately estimate agglomeration elasticities for different time periods. However, the agglomeration effects relevant for optimal equalization payments also depend on wage premia, which are estimated to be very stable over time, as mentioned above.

[38] Since $\frac{\hat{\theta_k}}{\beta} < 1$ for $k = 1, 2, 3$.

[39] The fact that agglomeration elasticities are constant across density levels matters within region since if they were not, labor demand might jump if a certain density value is exceeded (the labor demand elasticity is equal to $\frac{\sigma-\beta}{1-\alpha}$, as explained in Appendix section A.1.2).

[40] p-value: 0.98

Figure 1.2: Wage income tax payments



Notes: This map shows wage income tax payments relative to the population weighted mean for each labor market region. The distribution is partitioned into four intervals containing an equal number of observations. Darker colors indicate higher values.

The fact that the IV estimates are slightly larger in absolute value suggests a negative serial correlation of the decadal productivity shocks.

The IV estimates taken as 0.5 imply, according to equation (1.4), an agglomeration-induced productivity effect of approximately $1.02w^j$, i.e. approximately the full labor market region specific wage premium.[41] This means that re-allocation of workers is *not* a zero-sum game even though agglomeration elasticities are constant, the reason being that wages differ across locations. The conclusion from this exercise is that the other effects of equation (1.2) are dwarfed in magnitude by the agglomeration effects as is evident from table 1.2 (the wage income tax effect is about one fourth the size of the agglomeration effect due to the assumption of an average tax rate of 25 %). In terms of the geographic distribution of the effects, note that the effects are multiples of the wage income tax effects shown in figure (1.2) (see Appendix for a visualization of agglomeration effects). The IV estimates further imply an agglomeration elasticity of about 0.24 similar to the estimate of 0.2 obtained by Kline and Moretti (2014a) for the US and the estimate of 0.13 obtained by Brülhart and Mathys (2008) for Europe. This means that an increase in employment density by 1% leads to an increase in productivity of 0.24%.

Weak instruments may severely bias (Angrist and Pischke, 2008) the estimates obtained from

[41] Using $\alpha = 0.3$ and $\beta = 0.47$ as in Kline and Moretti (2014a).

Table 1.3: Estimates of agglomeration elasticities

	Δ log employment OLS	Δ log employment IV
Δ log density spline low	0.468***	0.530***
	(0.069)	(0.160)
Δ log density spline medium	0.419***	0.499***
	(0.098)	(0.156)
Δ log density spline high	0.392***	0.505**
	(0.094)	(0.202)
Δ log wage	-1.5	-1.5
N	410	205
State-Decade FE	yes	yes
Adj R-squared	0.72	0.90
First-stage F-statistic		12.10
Spline knots (percentiles)	50,75	50,75

Notes: Standard errors in parentheses (* $p < .1$, ** $p < .05$, *** $p < .01$). Clustering of standard errors is at the labor market region level.

two-stage least squares. I therefore report the F-statistic on the excluded instruments. The Cragg-Donald Wald F-test produces a value of 12.10 which is well above the critical value for three instruments of 4.23.[42] I calibrate the short-run labor demand elasticity (i.e. the elasticity for a given level of productivity) rather than estimating it due to the concern of endogeneity, as explained above. In the baseline, I use a value of -1.5 as in Kline and Moretti (2014a) but consider lower values (at -1.25 and -0.75) in the Appendix, which changes the estimates only marginally. The meta-analysis of Lichter et al. (2015) suggests a smaller absolute value than 1.5. However, larger values are plausible for this analysis since I am interested in the regional rather than national elasticity and, in addition, make use of decadal data.

Robustness checks presented in the Appendix include the omission of state-year fixed effects (instead using year fixed-effects), the usage of different values for the calibration of labor demand elasticity and spline knots in tables A.2 and A.3, basically leaving the estimates unaffected. The fact that estimated agglomeration elasticities are lower (while still constant across density intervals) when East German regions are included (see table A.4) points to the possibility that the baseline estimates of agglomeration effects are conservative. The reason is that I apply elasticities estimated using only West German regions to the East in the baseline. Using lower values of σ for the East would lead to even smaller estimates of agglomeration effects there. Finally, using linear spline components produces results that

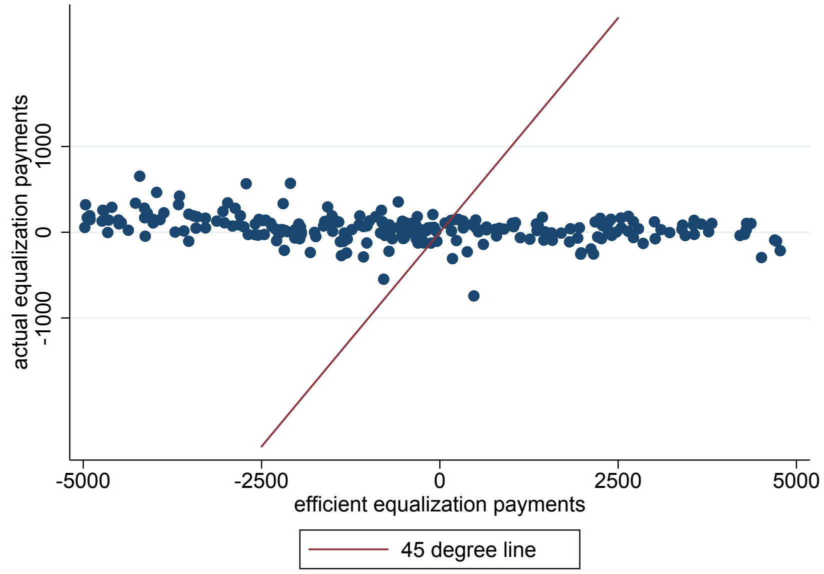
[42] Critical values are reported by Stock and Yogo (2002), suggesting a strong first stage. Batten and Martina (2007) extend the critical values to the case of three instruments and three endogenous regressors.

are in line with a concave agglomeration function $g(.)$ since the slope declines over density intervals. This result bolsters the choice of log spline components in the baseline.

1.6.3 Efficiency

I add up the right-hand side terms of equation (1.1) to obtain efficient payments for given prices. Figure 1.3 plots efficient against actual equalization payments (excluding outliers), revealing that actual payments are far from optimal: if they were optimal, they would lie on the 45 degree line. Indeed, the correlation coefficient of the two magnitudes is -0.42.[43]

Figure 1.3: Efficient and actual equalization payments (year 2005), excluding outliers



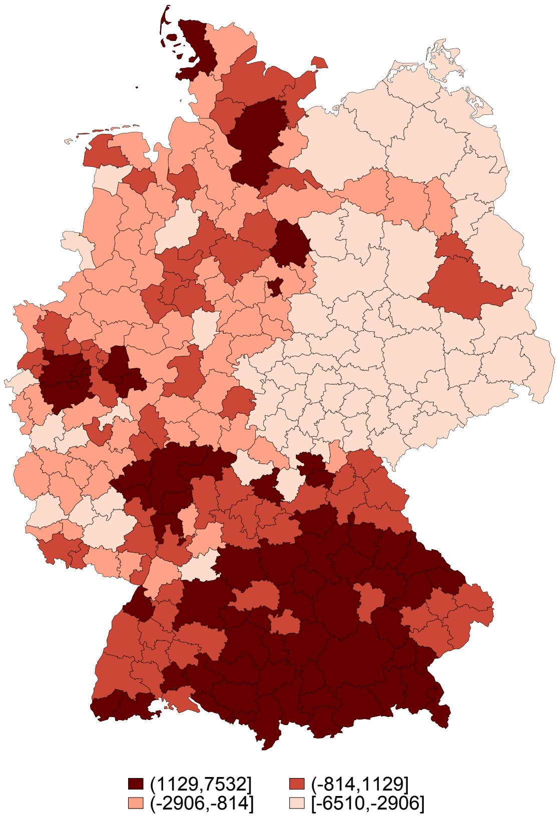
Notes: The amounts are in Euro per capita relative to the population weighted mean. Only efficient equalization payments in the interval $(-5000, 5000)$ are shown.

Figure 1.4 shows the geographic distribution of efficient payments which is in stark contrast to the distribution of actual payments shown in figure 1.1. High equalization payments should be paid to labor market regions in the South of Germany and low payments to the East – in reality, the opposite happens.[44] If agglomeration effects are ignored, this conclusion remains valid due to the geographical distribution of wage income tax payments (see figure A.4b for a visualization of efficient payments without agglomeration). However, the variance of efficient payments increases dramatically due to agglomeration. The reason is simple: endogenous productivity gains are highest where location-specific wage components are highest (which is in the South), reinforcing the wage income tax channel.

Relative to the optimum, this means that equalization payments incentivize workers to stay where they are unproductive and hence regions receiving high per-capita payments are over-populated. The fact that payments are not optimal manifests itself in net fiscal benefits

[43] See Appendix figure A.8 for a scatter plot that includes all observations.
[44] Setting $\omega = 0$ changes little, which can be seen in figure (A.4a).

Figure 1.4: Efficient equalization payments (year 2005)



Notes: This map shows efficient equalization payments in Euro per capita relative to the population weighted mean for each labor market region. The distribution is partitioned into four intervals containing an equal number of observations. Darker colors indicate higher values.

unequal across regions, the distribution of which is shown in Appendix figure A.7b. It is the variance of NFB that matters for the dead-weight loss resulting from inefficient equalization payments, as will be explained in the next section.

Efficient payments calculated are however only valid for given wages which are a function of equalization payments via population, i.e. we can so far only check whether existing payments are optimal. Since efficient equalization payments depend on equalization payments themselves, I make use of a first-order Taylor polynomial to approximate optimal equalization payments not conditional on wages in Appendix section (A.1.3).

1.6.4 Equity

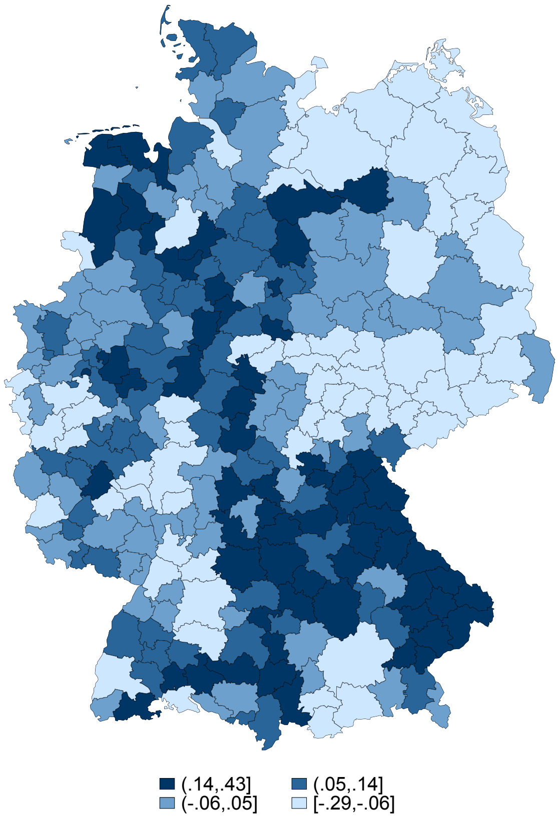
Given that fiscal equalization does not seem to be efficient, I now turn to equity. The discussion in section 1.3 suggested that it is equity considerations that motivate equalization payments in Germany. But are the payments indeed equitable and if so, by what criterion?

Figures 1.1 and 1.2 clearly show that regions exhibiting low wage premia receive more equalization payments per capita. However, lower housing cost could compensate for these low wage premia[45] and this is indeed the case in a number of regions. Figure (1.5) plots the

[45] The effect of differences in other prices across regions in Germany are small compared to the effects of

difference in logs of wages and annual rents (per square meter) for each region.[46] Relative to figure (1.2), some clear differences emerge: not surprisingly, high-rent labor market regions exhibiting the highest (nominal) wage premia, such as Frankfurt, Stuttgart and Munich are not at the top of the real-wage distribution. However, real wages in those regions are exceeded by those of a number of regions in East Germany. This finding alone is highly policy-relevant since eligibility of place-based policies such as capital subsidies continues to be determined by comparison of nominal wages, neglecting differences in cost of living (Deutscher Bundestag, 2007). Still, low real wage premia-regions are predominantly found in the East where per-capita equalization payments are large.

Figure 1.5: Log wage premia relative to rents (year 2005)



Notes: This map shows the difference of log wages and log annual rents per square meter relative to the population weighted mean for each labor market region. The distribution is partitioned into four intervals containing an equal number of observations. Darker colors indicate higher values.

On the whole, equalization payments do seem to support equity in terms of realized income (and possibly amenities) but, in the context of the present model, they are redundant in the sense that it is their own existence that makes them equitable: in a spatial equilibrium

differences in the cost of housing. This is due to the fact that the cost of housing varies more strongly than other prices and the expenditure share of housing is significantly larger than shares of other consumption categories (Kawka, 2010).

[46] This log difference is referred to as real wage in what follows. With Cobb-Douglas utility, this real wage determines agents' utility in a given region, next to amenities and consumption of the publicly provided good.

without equalization payments, labor would adjust such that differences in the real wage are offset by amenities or public good provision.[47]

1.7 Dead-weight loss

Given that actual equalization payments do not satisfy the model's optimality condition which requires that NFBs are equal across regions, it is then the magnitude of the resulting loss in efficiency that is of central interest. If equity considerations guide policy makers when they design equalization schemes, the loss in efficiency can be interpreted as their implicit willingness to pay for achieving equity-motivated goals.[48]

I treat NFBs differing across regions as a locational subsidy (or, if negative, as a city-specific head tax). Recall that the NFB measures the deviation of equalization payments from their optimal value. It is here that the assumption of equalization payments being received by workers instead of local governments is crucial (see section 1.2). Albouy (2009) and Albouy (2012) derive the dead-weight-loss (DWL) for this setting[49] and arrive at the following equation

$$\frac{DWL}{mN} = \frac{1}{2} \times \epsilon \times var\left(\frac{dNFB}{m}\right) \tag{1.6}$$

where ϵ represents the elasticity of population with respect to equalization payments, N is total population, m is average income and the variance term measures the dispersion of NFB across locations. In line with Harberger (1964), the dead-weight loss is increasing in the elasticity and the expected value of the square of differential net benefits (relative to the mean).

1.7.1 Population elasticity

A key component of the deadweight loss formula is the percent change in population due to a permanent increase in equalization payments. The ideal experiment would randomly assign permanent changes in equalization payments, allowing the identification of long-run responses in population. Lacking such an experiment, I resort to the analysis of yearly population movements across labor market region borders. Such movements occur only out of equilibrium, since agents are indifferent between locations under the no-arbitrage

[47] In reality, moving costs might be substantial but should matter less in the long run which is the focus of the present paper

[48] To a certain extent, pursuing equity is mandated: the German constitution requires the Federal government to ensure that equivalent (not equal) conditions of living prevail in all parts of the country (see Article 72 (2) of the "Grundgesetz"), although this mandate does not specify precisely when conditions can be considered equivalent.

[49] Computing the DWL amounts to replacing an equalization scheme that ensures uniform NFBs across regions by a utility-equivalent scheme with NFBs that differ across regions.

equilibrium condition. The empirical approach I pursue below can be described as follows: I compare regions that vary in local government expenditures but are similar in terms of other observable characteristics, assuming that movements across region borders are due to the differences in local government expenditures. I approximate the elasticity by investigating the effect on population of local government expenditures per capita instead of equalization payments due to the endogeneity of those payments since low-performing regions receive equalization payments but lose population because of weak economic fundamentals, obfuscating the relationship of interest.[50]

I first investigate what the effect of per-capita local government expenditure differentials is on movements in the cross section of region pairs, controlling for wage (w) and unemployment (u) differentials between two regions.[51] In other words, I want to determine the yearly flow in response to an expenditure differential. I then simulate the long-run adjustment by multiplying this yearly flow by 10.[52]

The estimating equation reads

$$\ln M_{ij,t} = \rho_{ij}^M + a\Delta_{ij}\left(exp_t\right) + b\Delta_{ij}\left(w_t\right) + c\Delta_{ij}\left(u_t\right) + \epsilon_{ij,t}^M \tag{1.7}$$

where $M_{ij,t}$ is the flow from region i to j in year t, exp is per-capita local government expenditure and Δ_{ij} denotes the difference in a variable between the two regions. The time-constant region pair-specific flow ρ_{ij}^M accounts for the fact that certain region pairs exhibit movement patterns which are stable over time independent of economic conditions (e.g. Berlin attracts people from all areas of Germany each year regardless of wages or expenditure). I rely on the between-estimator to estimate a, assuming uncorrelatedness between the time-constant movement component and expenditure differentials. The between-estimator exploits only the variation between region pairs, neglecting variation within pairs over time, which is consistent with the assumption that it is the long-run differences in expenditure that guide agents' behavior. By regressing the logarithm of the flow on expenditure differences as a multiple of 1% of average income, I obtain the percentage increase in the flow.

Estimating equation (1.7) gives the results in table 1.4. The estimates can be interpreted as follows: a per-capita expenditure differential of 1% of average income in favor of the origin region relative to the destination region induces a reduction in the population outflow from the origin region of 11 % over ten years, using the results from column 3. Equivalently, this can be interpreted as a decrease in the population inflow into the destination region of 11

[50] This assumption may not be innocuous: an increase in equalization payments could lead to a less than one-for-one increase in expenditures, e.g. if local governments decide to cut taxes instead.

[51] The theoretical model abstracts from unemployment since adding it would make the analysis more complicated but add little to the analysis of agglomeration economies which depends on the density of *employed* workers. However, neglecting unemployment when estimating the population elasticity might induce bias.

[52] Adjustment horizons extending over a larger number of years yield even higher values of the population elasticity, leading to a larger dead-weight loss.

% over ten years. The control variables' signs are reassuring: an increase in the (market) income differential (of 1% of average income) in favor of the origin region reduces the outflow while an increase in the unemployment rate (measured in percentage points) in favor of the origin region increases the outflow.

Table 1.4: Estimates of population flows in response to expenditure differentials

	(1)	(2)	(3)
Δ Expenditure	-0.004*	-0.007***	-0.011***
	(0.003)	(0.002)	(0.002)
Δ Income		-0.005***	-0.003***
		(0.000)	(0.000)
Δ Unemployment rate			0.010***
			(0.002)
Observations	820561	554595	554595
Adjusted R^2	0.000	0.009	0.009

Notes: Standard errors in parentheses (* $p < .1$, ** $p < .05$, *** $p < .01$). The dependent variable is the log of the population flow from region i to region j. The expenditure differential is the difference in expenditure between region i and region j and similarly for the other independent variables.

Next, I apply this estimate to the overall net inflow at the labor market level. That is, I aggregate all region pair specific-flows year to get a region's inflow and outflow (the means of which are shown in table 1.5). Then, I apply an 11% reduction to the outflow and an 11% increase to the inflow. Next, I compute the implied change in the overall net inflow. This is the population change due to a per-capita expenditure differential of 1% of average income. Dividing by population and averaging over all regions produces an estimate of the elasticity of population with respect to an increase in equalization payments of 0.5.

Table 1.5: Predicted population effects (year 2005)

Variable	Mean	Std. dev.
Inflow	7524	9538
Outflow	7524	8773
Implied population change (in levels)	1655	2008
Implied population change (as fraction of population)	0.005	0.001

This estimate of the elasticity lies between existing estimates for Canada explicitly using equalization payments as the independent variable, which are however measured at the province level. My estimates are about three times as large as Bakhshi et al. (2009)'s, who use a specification similar to the one in equation (1.7) but do not look at the *differential* in payments. In addition, they exploit within-province variation in payments which is problematic if it is the long-run differences that drive migration patterns. Wilson (2003) finds an elasticity of 3.2 over a period of six years based on changes in the equalization scheme.[53]

1.7.2 Dead weight loss calculation

Computing the DWL is now straightforward and the results are presented in table 1.6. Depending on whether agglomeration is taken into account or ignored in the NFB calculation, the results vary substantially. Using $\epsilon = 0.5$ yields a DWL of roughly €11 billion or 0.5 % of GDP.[54] On the other hand, the efficiency loss amounts to only 0.02% if it is computed without the NFB term relating to agglomeration. This means that neglecting agglomeration understates the efficiency loss by a factor of 25. For the case of Canada, this would suggest a DWL of approximately C$ 100 billion instead of C$ 4 billion, as estimated by Albouy (2012), who ignores agglomeration.[55]

Table 1.6: Dead weight loss calculations (year 2005)

Variable	With agglomeration	Without agglomeration
$var\left(\frac{dNFB}{m}\right)$	0.02	0.0008
elasticity ϵ	0.5	0.5
$\frac{DWL}{mN}$	0.005	0.0002
mN	€2200 billion	€2200 billion
DWL	€11 billion	€0.44 billion

1.8 Conclusions

In this paper, I have estimated the contribution of agglomeration economies to the efficiency calculus of equalization payments. Efficient equalization payments compensate workers for wage income tax payments that are higher in high-productivity regions and offset the externality that results from public good congestion of movers. In addition, they should take into account endogenous productivity effects due to agglomeration varying across space: in some regions, increasing the density of employment has a larger impact on productivity and this is where workers should be incentivized to locate. Neglected by the existing literature, this type of externality has a profound impact. Since the current equalization scheme in Germany incentivizes workers to locate in low-productivity areas, contrary to what is optimal, the policy creates a dead-weight loss. Not accounting for agglomeration understates the dead-weight loss by a factor of 25.

[53] Labor mobility is typically thought to be higher in North America than in European countries. See e.g. http://siteresources.worldbank.org/ECAEXT/Resources/258598-1284061150155/7383639-1323888814015/8319788-1324485944855/10_us.pdf, retrieved February 12, 2018.

[54] GDP is used to approximate mN. The share of GDP of equalization payments as defined in this paper amounts to 3.8% in 2005.

[55] Albouy (2012)'s calculation is based on the above-mentioned population elasticity estimate of 3.2.

These results are policy-relevant: by providing more realistic estimates of equalization's efficiency cost, I have highlighted the equity-efficiency tradeoff inherent in such schemes. This can inform the public debate about regional re-distribution that is oftentimes focused primarily on equity or on the incentive effects that arise for local governments. In addition, if it is true that knowledge-intensive industries will make up an even larger share of production in the future, it may well be that agglomeration economies, too, will play en even larger role.

An important limitation of the current analysis the assumption of perfect mobility. Even though mobility is likely to be quite high over the long run (i.e. across decades), which is the focus of the current model, and the model can allow for a fraction of the population to be immobile, it would be interesting to consider imperfect mobility by incorporating idiosyncratic preferences for locations in the model as in Suarez Serrato and Zidar (2016). On top of idiosyncratic preferences, unemployment insurance might be a reason for immobility in the short run. Incorporating immobility would allow for an analysis of the equity-efficiency trade-off when the federal government wants to achieve redistribution of income to immobile workers.

While congestion of the publicly provided good has been modeled explicitly above, I have abstracted from endogeneity of consumption amenities. A higher density of population may on the one hand make a region more attractive since e.g. specialty restaurants or certain cultural institutions are not profitable in low density areas. But there can also be downsides such as a higher level of criminal activity or environmental pollution (Diamond, 2016). Future work could consider the impact of these effects on optimal equalization payments.

Chapter 2

Efficiency and Equity Effects of Place-Based Policies: Evidence from Capital Subsidies in East Germany[1]

2.1 Introduction

In many countries and federations, place-based policies are a means to support regions that are economically lagging behind. Identification of these regional transfers is challenging, which explains why systematic empirical evidence is still scarce. It is even more difficult to disentangle the underlying mechanisms of the local subsidies as agglomeration effects, inter-regional or inter-sectoral shifts of economic activity might simultaneously be at play and have opposing effects. However, understanding these mechanisms is crucial for optimally designing the policy as different channels have different consequences for the eventual welfare effects of the policy – at the local, regional and national level.

In this paper, we analyze the effects of a prominent German place-based policy put into place after the reunification to revitalize the East German economy after 50 years of socialist economic policy. Besides estimating the policy effect on treated regions, we explicitly shed light on the underlying mechanisms and spatial spill-overs of the policy.

The policy under study is called *Gemeinschaftsaufgabe Verbesserung der regionalen Wirtschaftsstruktur* – throughout the paper, we will refer to it using the German acronym GRW. The GRW constitutes Germany's main regional policy scheme for underdeveloped regions (Deutscher Bundestag, 1997) with 90% of the capital subsidies going to East Germany after reunification. The GRW's main instrument are capital investment subsidies for manufacturing firms in eligible regions, which can be used for purchasing new machines or building new production sites. The explicit goal of the policy is to incentivize investment, thereby creating new jobs and stimulating regional growth.

[1] This chapter is co–authored by Sebastian Siegloch.

Our identifying variation comes from multiple changes of the maximum fraction of covered investment costs. These changes in the maximum subsidy rate additionally varied across sectors, firm sizes and – importantly – East German counties according to an indicator of regional economic performance. This indicator is based on pre-determined performance measures on a higher regional level and thus difficult to manipulate for the counties. 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 dataset, provided by the Institute for Employment Research (IAB) of the German Federal Employment Agency. For the years 1996-2013, we have access to a fifty percent random sample of establishments in East Germany. The data cover the annual number of employees at an establishment as well as the county in which it is located. In addition, we rely on IAB data on wages included in the Sample of Integrated Labour Market Biographies. Official subsidy data 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 the GRW subsidy 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 by analyzing inter and intra-regional spill-overs. In order to do so, we study the policy effects on uncovered sectors and regions by varying the level of regional aggregation (i.e. by looking at labor market regions instead of counties).

Our main empirical findings are as follows. Changes in the main policy instrument, the maximum subsidy rate, significantly affect actual subsidies paid (the lower the maximum rate, the lower subsidies paid). As for employment, a one percentage point decrease in the maximum subsidy rate leads to a decrease in county-level manufacturing employment of 1.4% five years after the reform. After ten years, the effect stabilizes at a level of manufacturing employment lower by 1.8%. However, overall employment in counties experiencing a decrease in the maximum rate does not decrease and employment in sectors other than manufacturing actually seems to increase slightly. While GDP is not affected, manufacturing wages decrease but only for the low skilled. As a consequence, county-level inequality increases. Finally, when we aggregate manufacturing employment to the level of the labor market region, the effects are smaller by about 64%, suggesting that the negative employment effect in treated counties is to a significant degree driven by reallocation of labor within the commuting zone.

We contribute to the existing and recently growing literature on place-based policies in several ways.[2] We complement the finding by Kline and Moretti (2014a) who study the long-term effects of the Tennessee Valley Authority, the most prominent regional subsidy program in U.S. history.[3] They find that agglomeration economies yield a long-term positive effect on manufacturing employment in treated regions, which persists even after the program ended. Using a place-based subsidy paid to West German regions close the Iron Curtain from the 1970s to until reunification, Ehrlich and Seidel (2018) corroborate the positive long-term effects of temporary subsidies. Moreover, they point to an important channel that can explain the persistence by presenting evidence that subsidies increase local public investment levels beyond the existence of the program. 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.

In a recent paper, Overman (2018) have analyzed an industrial policy in the UK, which is similar to the GRW. Exploiting changes in regions' eligibility for subsidy rates, they find manufacturing employment effects that are quite similar to ours. We complement their analysis by focusing on dynamic (pre-)treatment effects and studying in particular the long-run effects of the policy. Moreover, we also study the effect on wages by skill-group and underlying inequality effects, focusing on the redistributive effects of place-based policies, which are often one of their main policy goals (Neumark and Simpson, 2015). Despite this explicit goal, a comprehensive evidence of the distributional effects of place-based policies is still scarce. One goal of this paper is to start closing this gap by studying a case where both regional inequality in economic performance, i.e. the East-West productivity gap after reunification, and within regional inequality was massive.

Another aim of the current study is to add to our understanding of inter and intra-regional spill-overs of place-based policies – a key issue, which only recently has received increased attention. Our evidence complements the contemporaneous and independently conducted study by Dettmann et al. (2016), which focuses on GRW subsidies to *West German* counties. The paper looks at the effect at the regional level, finding no aggregate effect. While the context of the GRW for West Germany is markedly different, as the intensity of subsidization is much lower compared to East German, our results underline the importance of looking at firm level data as important inter-regional spill-over might not be detectable at the aggregate regional level.

Intra-regional employment spill-overs might also explain the contrasting evidence found in a series of papers investigating effects of the EU Structural Funds (ESF), a regional subsidy paid by the European Union (Becker et al., 2010, 2012, 2013). Interestingly, the ESF had no effects on employment, while it increased GDP per capita.[4]

[2] For a current survey, see Neumark and Simpson (2015).

[3] Neumark and Kolko (2010) provide a short-term analysis of state enterprise zones.

[4] Another reason for the different results might be that the ESF comprise different programs, some of

In terms of inter-regional spillovers, the existing evidence so far is mixed. While Overman (2018) and Alder et al. (2016) find no effect on neighbors of treated regions, Ehrlich and Seidel (2018) suggest that positive employment effects in treated regions might at least in part be at the expense of other regions. Our findings show that more than half of the employment effect of the place-based policy studied is absorbed when moving the analysis to the level of the commuting zone, suggesting substantial regional, yet still quite local spill-overs.

The remainder of this paper is organized as follows: We explain the institutional setting in section 2.2, followed by a section on the research design. Section 2.4 presents the data. Empirical results are presented in section 2.5. Section 2.6 concludes.

2.2 Institutional background

In this paper, we study a specific German regional economic policy, called *Gemeinschaftsaufgabe Verbesserung der regionalen Wirtschaftsstruktur* (GRW). The GRW is jointly coordinated and financed by the federal government and state governments. Since 1969, the policy's goal has been to equalize standards of living across Germany not via transfer payments to individuals but by way of stimulating regional business activity leading to the creation of jobs, in particular in the manufacturing sector.

In the 1970s and 1980s, the GRW targeted economically underdeveloped regions in West Germany. With the reunification in 1990, East German regions, which were considerably less industrialized than Western ones, became eligible for GRW subsidies. As a result, 89% of the GRW funds since 1990 have been targeted at former East Germany. As such, the GRW was seen as one of the main instruments aiming at re-industrializing East Germany and bringing it to Western levels.[5]

From 1993 to 2013, each year on average 1 billion euros of subsidies were paid out to East German firms. While the GRW incorporates a number of instruments itself, by far the most important one are capital and wage subsidies paid out to firms, making up roughly two thirds of the total budget (Deutscher Bundestag, 1997).[6] These grants are used to cover a certain

which are directed at firms and others at local governments and it is therefore impossible to evaluate the effect of the individual programs.

[5] Other policy measures targeted at firms 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 firms. We check that the reforms exploited for identification did not affect funds paid to municipalities.

[6] As part of the GRW, infrastructure subsidies are granted to municipalities independently of capital and wage subsidies, accounting for the remaining budget. As shown below, we exploit reforms at the county level for identification, which did not affect municipal infrastructure funding, the intensity of which was not changed over the course of the years. Other quantitatively less important GRW instruments are the financing of employee training, counseling, research and marketing for small and medium sized firms.

cost share of a firm's investment project. Typical eligible investment projects comprise the acquisition of machinery, buildings but also licenses and patents. The wage bills of workers can be subsidized only for workers directly working on the corresponding investment project.

Eligibility of a project is determined by a number of requirements which need to be fulfilled. On a broad level, a project is eligible for funding if (a) the yearly investment cost exceeds the average amount of the firm's capital consumption in the preceding three years by at least 50% or if (b) the number of regular (i.e. not short-term) employees is increased by at least 15% due to the project. In addition, the following requirements have to be fulfilled: (i) the project's duration must not exceed three years, (ii) firms applying have to predominantly sell their products or services outside of their county. The latter requirement means that products worth at least one half of firm revenues must be sold outside of the respective county. The rationale behind this requirement is that export-oriented firms generate additional income within a county, which is in turn spent partly on regionally traded goods and services, leading to a multiplier effect. Requirement (ii) causes that 74% of funds go to manufacturing firms (Appendix table B.5 shows industries for which requirement (ii) was assumed to hold and firms did not have to provide evidence). Nevertheless, apart from a small number of specific industry branches, all industries were de jure eligible for the subsidies.

In order to receive the subsidy, firms need to apply at their respective state government, specifying the investment project and demonstrating that the eligibility criteria are fulfilled. States have an annual budget on subsidies to be paid out and can discretionarily grant or deny a firm's application. Note that in more than 90% of cases, states do not exhaust their annual budgets, which suggests that there was de facto no rationing of the funds and no rivalry between projects.[7]

Upon successful application, firms would receive subsidies to cover a certain share of the investment cost stated in the application. There was a binding maximum subsidy rate imposed by federal law, which varied by county, year and firm type. Note that states could undercut the maximum subsidy rate for a given project if desired. We show below that exogenous changes in a county's maximum subsidy rate strongly affect total subsidized investment. The yearly average share of the investment cost of granted projects covered by subsidies was approximately 20% at the county level.

In the empirical part of the paper, we exploit (the exogenous components of) the variation in maximum subsidy rates to estimate the causal effects of the policy. In the following, we describe this variation in detail. At the onset of the GRW post-reunification program, firms in all East German counties were treated equally, with the maximum subsidy rate for small and medium-sized firms being 50% and 35% for large firms. Firm size is defined by the number of employees: small firms have less than 51 workers, medium-sized firms

[7] Unfortunately, no systematic information on the number of denied applications exists. Bronzini and de Blasio (2006) exploit such information in an analysis of investment subsidies for firms in Italy. However, they focus on firm-level outcomes instead of regional outcomes.

51 to 250, and large one above 250. Starting in the mid-1990s, policy makers realized that economic development was very unequal across East German regions. As a result, policymakers introduced differential maximum rates between counties in 1997 based on a county's neediness. Out of the 76 counties, only 49 were were categorized to have the highest funding priority. For these, maximum subsidy rates remained unchanged. For the remaining 27 counties, maximum subsidy rates were cut by 7 percentage points across all three firm size groups (see Table 2.1, regimes 1 vs. 2).

Table 2.1: Subsidy regimes

	Regime 1 pre 1997		Regime 2 1997-1999		Regime 3 2000-2006		Regime 4 2007-2010		Regime 5 2011-2014	
Relative priority	high	low	high	low	high	low	high	low	high	low
small firms	50%	n/a	50%	43%	50%	43%	50%	n/a	50%	40%
medium firms	50%	n/a	50%	43%	50%	43%	40%	n/a	40%	30%
large firms	35%	n/a	35%	28%	35%	28%	30%	n/a	30%	20%
# counties	76	n/a	49	27	40	36	76	n/a	59	17

Sources: Deutscher Bundestag (1996), Deutscher Bundestag (1997), Deutscher Bundestag (2000), Deutscher Bundestag (2007)

The allocation of counties to highest or lower funding priority was conducted by the Federal government based on an indicator of economic performance in preceding years. Importantly, the performance indicator was calculated at the labor market region level. Labor market regions are comparable to commuting zones in the U.S.. Counties are perfectly nested within labor market regions.[8] The indicator for labor market region r and the year 1997 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[9] in 1995, $wage$ represents per-capita earnings in 1995 and $unemp$ measures the unemployment rate in 1995.

All 53 labor market regions were ranked according to this indicator. Counties whose labor market region had an index-value below a certain threshold (here: normalized to 100) were classified as highest funding priority, counties whose labor market region had an index-value above received a cut in the maximum subsidy rate (see Figure 2.1). In Appendix Table B.1, we take a more detailed look at the counties around the 1997 cutoff.

Over the years, multiple changes in county border definitions occurred due to mergers of

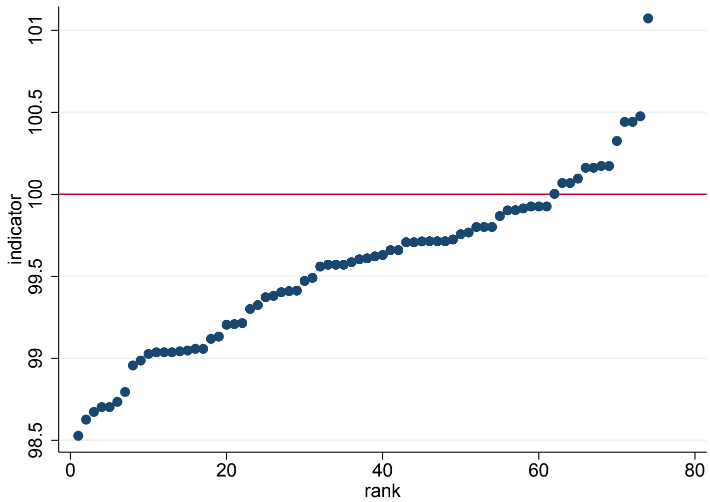
[8] The only exception is the labor market region of Berlin, where the border of the labor market region cuts through certain counties. We drop Berlin from the analysis.

[9] The infrastructure sub-indicator is based on measures of reachability of an airport and of close large cities by car or train, of the traveling time for trucks to the next trans-shipment center, the share among all employees 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.

counties. Throughout, we make use of the county border definitions of the year 2014. If a county that is treated in a given regime is merged with an untreated county, this has the effect that the resulting, larger county is only partially affected by the reform. We exclude all counties that were only partially affected by a reform in a robustness test.

For a perfectly deterministic rule, we would expect all counties below the threshold to receive the high maximum rates. However, there are some counties below this value that lose eligibility for high rates. This is mainly due to the fact that counties bordering Berlin were partially reformed: municipalities close to Berlin received a cut but municipalities further away did not receive a cut, the rationale being that there are a lot of cross-county commuters close to Berlin. We classify the affected counties as downgraded but exclude all counties that were only partially affected by a reform in a robustness test.[10] In addition, the Federal government (jointly with state governments) reserves the right to deviate from the ranking when they see fit. However, this happens rarely (e.g. in 1997, two counties were affected). Nevertheless, we pay special attention to ensuring the comparability of treatment and control groups in our empirical analysis.

Figure 2.1: Ranking of counties based on 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 (however some counties below the cutoff still lost eligibility for the highest assistance rates).

In the year 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.1). As shown in Table 2.1, 40 counties received highest priority with additional counties switching from high to low priority (compared to 1997).

In 2007, the ranking of counties was renewed. At this time, all German counties (East and West) were ranked, while in previous years East Germany regions were assessed separately. Due to the relatively richer West German regions, all East German counties received high

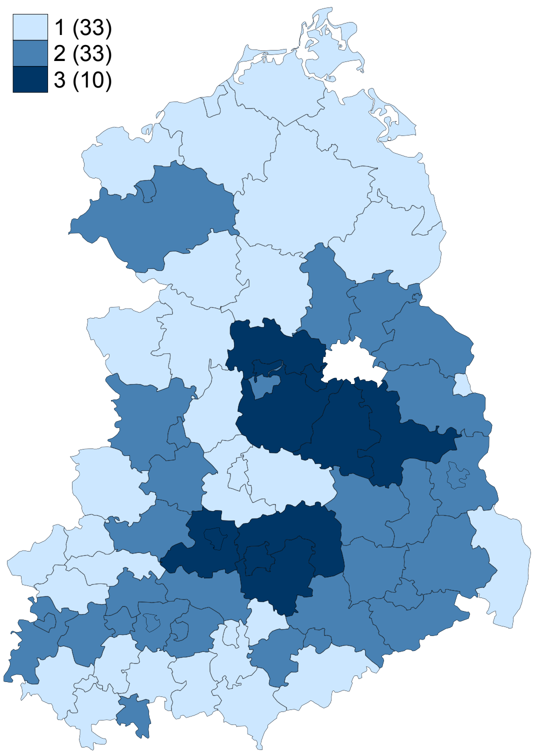
[10] County mergers, explained above, are also responsible for exceptions. When counties merge, we define the indicator as the average of the merging counties' indicator values.

priority status. At the same time, the maximum subsidy rate for medium and small firms were cut by 10 and 5 percentage points, respectively (cf. Table 2.1).

The last reassessment analyzed occurred in 2011. 17 counties were downgraded in their priority status. The reason for this change was the EU's enlargement from 15 to 25 member states which resulted in a decline of EU average regional GDP per capita. According to EU regulations, regions above the 75th percentile of GDP per capita lose eligibility for the highest maximum rates. This cutoff effectively replaces the indicator cutoff in other years since the EU rule trumps any national allocation schemes.

The various reforms generate substantial variation in maximum subsidy rates across East German counties. Figure 2.2 illustrates that all counties experience at least one change in the subsidy rate, while more than 50% experience two or three changes. We exploit these changes in our empirical research design presented in Section 2.3.

Figure 2.2: Number of reforms in the maximum subsidy rates



Sources: Deutscher Bundestag (1997), Deutscher Bundestag (2000), Deutscher Bundestag (2007). *Notes*: This figure shows the number of reforms in the maximum subsidy rates a county experienced. Berlin is excluded from the analysis.

2.3 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 2.2 is quite complex, we develop our preferred empirical model step-by-step for didactic reasons. We start with a situation of one policy reform, say the switch from regime 1 to 2 described in Table 2.1. In this case, the treatment group are the 27 counties which are assigned a low priority status in 1997 and which therefore experience a reduction in the maximum subsidy rate.

Basic model

The simplest version of the event study design for such a set-up is defined as follows:

$$\ln y_{c,t} = \sum_{k=-B}^{A} \beta^k D_{c,t-k} + \gamma_c + \psi_{s,t} + \varepsilon_{c,t}. \tag{2.1}$$

We regress an outcome y in county c and year t on a set of dummy variable, $D_{c,t-k}$ indicating whether a change in the maximum subsidy rate occurred for the county $k \in [-B, ..., A]$ periods ago. As described in Section 2.2, the vast majority of subsidy rate changes were decreases. Consequently, we define the indicator variable as follow: $D = 1$ for the typical change, hence a rate cut, $D = -1$ for an increase and $D = 0$ if the maximum subsidy rate did not change. Parameters $-B$ and A denote the ends of the event window. Coefficient β^{-3}, for instance, measures the effect of a change in the subsidy rate three years into the future on the outcome in year t, and β^2 the lagged subsidy effect after two years. Hence, the event study design enables us to test for flat pre-trends ($k \leq -1$) and informs about the adjustment paths of the treatment effect ($k \geq 0$). Following standard practice, the end points of the event window $-B$ and A account for all changes that will occur in $|-B|$ or more periods in the future, and that have occurred A or more periods ago (McCrary, 2007). This adjustment makes the system of event dummies perfectly collinear such that we omit the dummy for the pre-treatment year $k = -1$ from the regression. All other estimates are to be interpreted relative to the pre-treatment period. The model includes county fixed-effects γ_c and state (s) times year fixed effects $\psi_{s,t}$.[11]

While we described the basic event study design using the example of one reform, it is obvious that the set-up is flexible enough to account for the multiple reforms we experience during our sample period from 1996 to 2013. Consider the year $t = 2007$ and a county c that is treated, i.e., above the cut-off, in regime 2. For this county, event dummy $D_c, t - 10 = 1$ would be switched on since the 1997 reform occurred ten years ago. At the same time, $D_c, t - 0 = -1$ since all counties were high-priority in 2007. We are able to separate the effects, as long as there is per county variation in the number of treatment dummies that are switched on simultaneously (Fuest et al., 2018). In the following, we will adjust this simple version of the event study in two ways.

[11] Standard errors are clustered at the county level throughout.

Size-adjusted model

First, we take into account that our reforms induced subsidy rate changes of different sizes. This is particularly important in our setting of multiple reforms. The reforms differentially affected maximum subsidy rates for different firm sizes. For instance, rates for small firms may increase in a given year and region while rates for large firms decrease. To exploit these changes, we define treatment intensity $I_{c,t}$ of county c and year t as

$$I_{c,t} = \Delta s_{c,t}^{small} \omega_c^{small} + \Delta s_{c,t}^{med} \omega_c^{med} + \Delta s_{c,t}^{large} \omega_c^{large}. \tag{2.2}$$

The intensity measure is a weighted average of the change in maximum subsidy rate $\Delta s_{c,t}^{s} = s_{c,t}^{s} - s_{c,t-1}^{s}$ across firm types, $s \in [small, med, large]$. Respective weights are denoted by ω and defined as the respective share of firm size specific covered employment in total covered employment in county c in the first available data year $\bar{t}$, hence pre-treatment.[12] Covered employment, in turn, refers to employment in industries that are eligible for the subsidy and is approximated by manufacturing employment. Denoting covered employment as E, formally:

$$\omega_c^s = \frac{E_{c,\bar{t}}^{s}}{E_{c,\bar{t}}^{small} + E_{c,\bar{t}}^{med} + E_{c,\bar{t}}^{large}} \quad \forall s \in [small, med, large]$$

Based on these definitions, the adjusted event study design, accounting for the intensity of the treatment is given by:

$$\ln y_{c,t} = \sum_{k=-B}^{A} \beta^k \left[D_{c,t-k} \cdot I_{c,t-k} \right] + \gamma_c + \psi_{st} + \varepsilon_{c,t}. \tag{2.3}$$

Compared to the simple, conventional model given in equation (2.1), this variant of the event study replaces the homogeneous zero/one treatment indicator with an indicator that is specific to the event. By defining event indicators in terms of event sizes, we sacrifice the purely non-parametric nature of the event study.[13] On the other hand, with the – rather weak and standard assumption of linearity – we can make heterogeneous reforms comparable and therefore poolable across space and time.

Improving comparability

In the second step, we want to improve the comparability between treatment and control group. So far we have implicitly assumed treatment assignment as random, which yields unbiased estimates of the treatment effects $\beta^k \quad \forall k \geq 0$. We know, however, that treatment

[12] In our case, $\bar{t} = 1995$. We drop year 1995 from the data after calculating the shares.

[13] Note, however, that quantifying the magnitude of an effect using a traditional event study design usually involves an implicit linear assumption as effects are assessed by the size of the average reform size.

was not random but based on past economic performance of the counties. Hence, it is possible that treatment and control counties were on different trends. While pre-treatment coefficients, $\beta^k \quad \forall k < 0$, generally inform about different pre-trends, we can further increase the comparability of treatment and control group by restricting the sample to counties closer to the cut-off. Denote $\mathbb{T}^{r,M}$ ($\mathbb{C}^{r,M}$) the set of M counties closest to the performance cut-off from above (below) following the indicator determined in reform r. Let $\mathbb{S}^{r,M} = \mathbb{T}^{r,M} \cup \mathbb{C}^{r,M}$ the set of $2M$ counties around the cut-off following reform r. The general rationale of the following extension of the simple event study is to restrict the sample to counties close to the cut-off making treatment and control counties more comparable. If common trends assumptions were not fulfilled in the full sample, pre-trends should become flatter. In terms of post-treatment effects, we would expect weakly larger effects due to this restriction since well performing counties that receive a cut in the maximum subsidy rate would do better in economic terms than under performing counties that do not receive a reduction. This general difference in trends is arguably less pronounced around the cut-off.

Restricting the sample to counties around the cut-off is complicated by the presence of multiple reforms. We use an example to illustrate this point: Let us consider three regimes (1,2,3) and two reforms (1997 and 2000). In the year 1997, 27 counties were downgraded in their priority and experienced a subsidy rate cut (cf. Table 2.1). In 2000, some of the remaining high-priority counties were downgraded, as well. We now have 3 groups of counties: (i) counties that were always high priority (N=40); (ii) counties that stayed high priority in regime 2 but were downgraded in regime 3 (N=9); (iii) counties that were downgraded in regime 2 (N=27). If we choose $M = 13 \; \forall r \in \{1997, 2000\}$[14], reform 1 will yield 13 treated and 13 control counties.[15] However, out of the counties in $\mathbb{S}^{1997,13}$, some are close to the cut-off in 2000 while others are not in the restricted sample ($\notin \mathbb{S}^{2000,13}$) either because they are too far away from the cut-off from above or below. These transitions further complicate the definition of samples close to the cut-off. We deal with this issue by including only counties which are at least once close to a cut-off. Table B.4 illustrate the year-specific ranks of all counties along with their treatment intensity.

We implement this variation by estimating equation (2.3) on the restricted sample

$$\ln y_{c,t \;|\; \mathbb{S}^M} = \sum_{k=-B}^{A} \beta_k \left[D_{c,t-k} \cdot I_{c,t-k}\right] + \gamma_c + \psi_{s,t} + \varepsilon_{c,t} \tag{2.4}$$

where $\mathbb{S}^M$ is the union of all reform specific samples: $\mathbb{S}^M = \mathbb{S}^{1997,M} \cup \mathbb{S}^{2000,M} \cup \mathbb{S}^{2011,M}$.[16]

[14] This will be our preferred restriction in the empirical analysis below.

[15] In fact, as described in section 2, there are a few counties that are downgraded even though they are below the cutoff. However, these exceptions are quite rare (see cutoff tables in the Appendix).

[16] Note that there is no relevant cutoff for the year 2007 since all counties in East Germany were affected by the reform.

Testing for regional spill-overs

Regional spill-overs can be a result of place-based policies. In our setting such spill-overs would imply that counties in the control group are affected by the policy changes as well. Theoretically, these spill-overs can be positive in case local demand beyond the county lines is stimulated or negative if economic activities move from control to treated counties as a consequence of the policy. It is important to bear in mind that $\hat{\beta}_k$ provides the overall policy effect on treated counties, relative to non-treated. In other words, a significant policy effect could be driven by a positive effect on treated counties and zero effect on non-treated, a zero-effect on treated counties and a negative effect on non-treated due to spill-overs, or some combination of both effects. What is more, if non-treated, neighboring counties benefit from the policy, e.g. because of agglomeration forces that do not stop at county borders, the positive policy effect is underestimated. We test for those kinds of spill-overs by moving the analysis to a higher level of aggregation. Explicitly, we aggregate employment to the labor market region level and re-estimate the model. The difference between the estimate at the county-level and the estimate at the labor market region level gives an indication of reallocation of economic activity within the commuting zone.

2.4 Data

2.4.1 Subsidy data

We make use of administrative subsidy data provided by the Federal Ministry for Economic Affairs (via the division Bundesamt fuer Wirtschaft und Ausfuhrkontrolle). For the years 1996-2013, we have the universe of GRW subsidy cases in East Germany including investment volume, subsidy amount, duration and the receiving establishment's county and industry. Firms are asked how many jobs were saved and created due to the subsidy's payment. This information is, however, not verifiable and we will not rely on theses answers in our analysis. Matching these data to an establishment's employment outcome is prohibited due to data protection laws. Therefore, we investigate the employment response of establishments in a treated area but are unable to identify which establishment did in fact receive subsidies and which didn't. As mentioned above, 74% of all subsidies were paid to manufacturing firms.

Table 2 shows that the average yearly subsidy payments received by a county amount to EUR 13 million, supporting investment projects worth EUR 64 million. This implies an overall average assistance rate of about 20%. This is due to the fact that in many cases, maximum rates are not exhausted. Importantly, these figures 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

Table 2.2: GRW descriptive statistics (1996-2013)

Variable	Obs	Mean	Std. Dev.
Granted funds (in millions of EUR)	1364	13	20
Investment volume (in millions of EUR)	1364	64	121
Saved jobs	1364	824	940
Additional jobs	1364	297	352
Sub-sample for which industry classifications are available			
Granted funds (in millions of EUR)	1247	11	20
Investment volume (in millions of EUR)	1247	46	107
Saved jobs	1247	615	860
Additional jobs	1247	169	236
Manufacturing share of funds	1247	.74	.33

Source: Federal Ministry for Economic Affairs (Bundesamt fuer Wirtschaft und Ausfuhrkontrolle) *Notes*: Information on jobs saved and created is provided by subsidy recipients. We do not make use of this information, which is not verifiable.

usage, such as sectoral restrictions and maximum assistance rates are thus identical for ERDF and GRW funds.

2.4.2 Employment and wage data

As for employment data, we use the Establishment History Panel (BHP) based on social security records and provided by the Institute of Employment Research in Nuremberg (Schmucker et al., 2016). We have access to a fifty percent random sample of establishments in Germany for the period of 1995-2013. This data set includes the yearly number of employees by skill at an establishment as well as the county in which it is located and the industry classification. Since we have no information on whether more than one establishment belongs to the same firm, we use the terms "firm" and "establishment" interchangeably, always referring to the latter. We also make use of the IAB's Sample of Integrated Labour Market Biographies (SIAB), to analyze wages (Antoni et al., 2016). A 2% sample of individual earnings biographies is available, which includes individual characteristics as well as employer information from the BHP.[17]

2.4.3 Observable confounders

We test both the credibility of our identification and the robustness our findings by taking a closer look whether differences in observable confounders determine treatment status and affect treatment effects. The identifying assumption of our main empirical model given in equation (2.4) is that treatment and control groups are similar prior to the reform, which is implied by flat pre-trends. In other words, there must not be systematic differences in local

[17] 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.

business cycles that determine treatment status. We test whether identification is achieved by looking at pre-trends in county level GDP per capita and the local unemployment rate, which are publicly provided by state and federal statistical offices. Similarly, we estimate the treatment effect on our main outcomes conditional on (pre-determined) business cycle controls.

In addition, we have data on appropriations of money for investment received by municipalities and counties, which we investigate as an additional outcome variable since these funds may be correlated with GRW payments. To the extent that the funds were received not by firms but by municipalities and counties, these figures include infrastructure subsidies as well as other equalization transfers including numerous Federal funds intended to assist East German regions post-reunification. Descriptives are shown in table 3 and reveal a very high average unemployment rate in East counties of about 17 %. Note also that manufacturing only makes up 18% of total employment on average.

Table 2.3: Descriptive statistics (1996-2013)

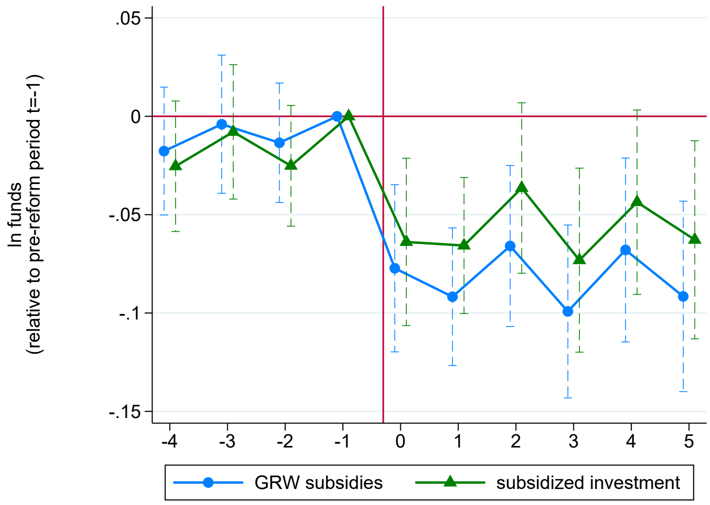
Variable	Obs	Mean	Std. Dev.
Unemployment rate	1368	.17	.04
GDP per capita (in €)	1336	20730	5080
Investment subsidies received by municipalities (in €1000)	1222	52641	32924
Employees	1368	76498	49344
Share: manufacturing	1368	.18	.07
Share: service	1368	.33	.06
Share: construction	1368	.11	.04
Share: commerce	1368	.24	.03
Share: finance	1368	.12	.05
Share: agriculture	1368	.03	.02
Share: small establishments	1368	.96	.01
Share: medium-sized establishments	1368	.03	.01
Share: large establishments	1368	.004	.002
Monthly wage (in €)	3732703	1467	1343

Sources: Statistical Offices of German States (Laender), BHP, SIAB. *Notes*: There are 76 counties in East Germany (excluding Berlin) according to 2014 county definitions. Monetary variables are in 2010 euros. Wages of part-time workers are included.

2.5 Empirical results

In this section, we present the reduced form effects of the place-based policy. Subsection 2.5.1 focuses on the treatment effect on the treated, that is focusing on the labor market effects in manufacturing firms in treated counties. Subsection 2.5.2 sheds light on the aggregate effects of the policy beyond treated industries and regions.

Figure 2.3: Event study estimates: first stage at county level



Source: Federal Ministry for Economic Affairs (Bundesamt fuer Wirtschaft und Ausfuhrkontrolle). *Notes*: This figure plots coefficients along with 95 % confidence intervals of a regression of log subsidies paid to counties and log subsidized investment on leads and lags of a change in the maximum assistance rate as in equation (2.3). The sample includes all counties in East Germany. State-year fixed effects are included. Clustering of standard errors is at the county level.

2.5.1 Treatment on the Treated

Main effects

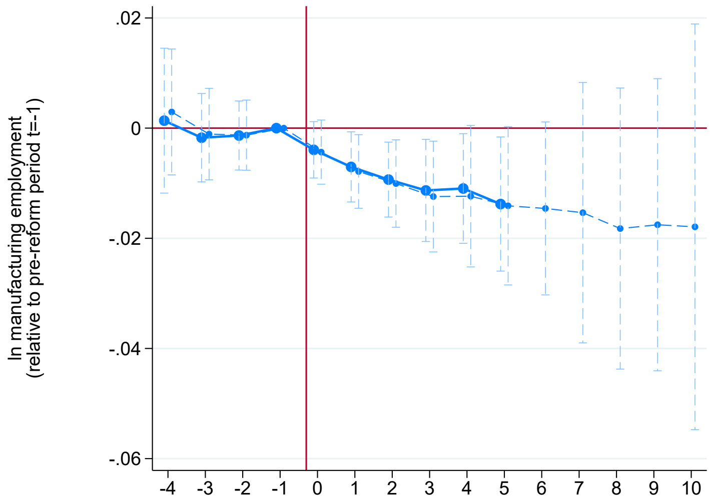
In a first step, we assess whether the reforms of the maximum subsidy rates affect the subsidies paid out, that is, we test our first stage. Figure 2.3 shows the effects on log GRW funds at the county level. We find that subsidy payments in treated counties decrease by 9.2% five or more years after a reform when the maximum subsidy rate decreases by one percentage point. Reassuringly, before a reform occurs, treatment and control groups exhibit a very similar development.[18]

Next, we test whether the strong decrease in subsidies paid decreased subsidized investment. In theory, it is possible that total subsidized investment remains constant, but a smaller share of the investments are subsidized due to the lower maximum rates. The event study shows that the latter effect is not at play. Instead, the total investment volume subsidized by the GRW decreases after the reform. In terms of magnitudes we detect a decrease of 6.3% after five years.

Having established a first-stage, we move to the employment effect, the main outcome according to the explicit goal of the policy. Theoretically, a decrease in subsidized investment does not necessarily lead to a decrease in employment for two main reasons: (i) it is possible that subsidized crowds out non-subsidized investment, (ii) more expensive capital might be substituted by labor. Figure 2.4 shows the effect of a reform in the maximum subsidy

[18] We find no spill-overs in GRW subsidies across counties. In other words, the decrease in subsidies in reformed counties does not lead to an increase in subsidies in neighboring counties (see Section 2.5.2 for more details on spill-overs).

Figure 2.4: Event study estimates: manufacturing employment at county level



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 (2.4). The sample includes the 49 counties closest to cutoffs. State-year fixed effects are included. Clustering of standard errors is at the county level.

rate on aggregate county-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 1.4% after five years for our baseline of 49 counties.[19] These estimates are remarkably similar to the main finding of Overman (2018) of a 10% increase in manufacturing employment in response to an increase in the maximum subsidy rate of ten percentage points.

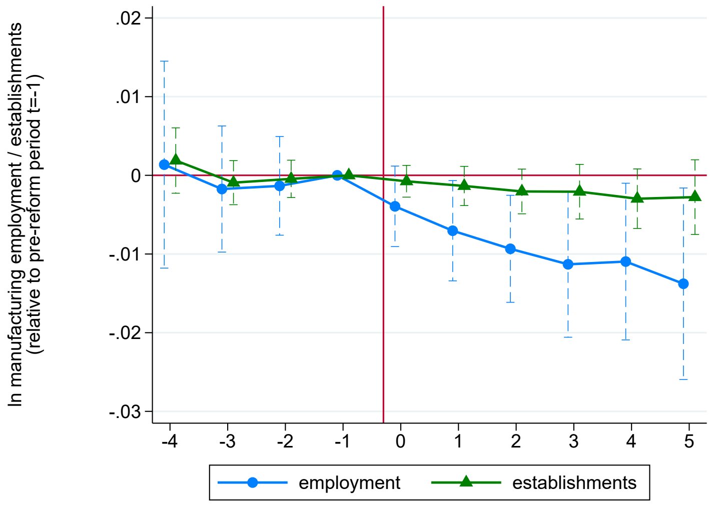
The employment effect gradually evolves following a concave pattern – with the effect after five years accounting for 80% of the effect after ten years. As confidence bands widen in the long-run, we consider the specification with five lags as our (conservative) baseline estimate for the remainder of the paper. We find that the decrease in employment is similar for the low-skilled, medium-skilled and high-skilled (see Appendix Figure B.2).

The negative effect on total manufacturing employment at the county-level could be due to adjustments at the extensive and intensive margin. Looking at the number of manufacturing establishments, figure 2.5 shows small negative effects. The finding of a decrease in the number of manufacturing establishments of roughly 0.3% for a one percentage point decrease in the maximum subsidy rate after five or more years suggests that approximately one fourth of the total employment response is due to the extensive margin. We do not find evidence for relocation of firms due to the reforms. The number of newly created manufacturing establishments decreases in treated regions, effects are, however, not significant at conventional levels. Establishment destruction is not affected.

Last, the decrease in labor demand should lead to decreasing wages in manufacturing. Using

[19] For each regime, we pick the 13 counties which are closest to the cut-off from below and the 13 counties that are closest from above. Aggregating over regimes, we end up with 49 counties that are at least once close to the cut-off.

Figure 2.5: Event study estimates: number of manufacturing establishments



Source: BHP. Notes: This figure plots coefficients along with 95 % confidence intervals of a regression of the log number of manufacturing establishments on leads and lags of a change in the maximum assistance rate as in equation (2.4). The sample includes the 49 counties closest to cutoffs. State-year fixed effects are included. Clustering of standard errors is at the county level.

the SIAB, we calculate average wages at the county level. Interestingly, Figure 2.6 shows that wages for low-skilled workers decrease, while wages for high-skilled and medium-skilled workers are largely unaffected or even increase slightly. As employment by skill responds homogeneously (cf. Appendix Figure B.2), it seems that the differential wage effect is driven by differential labor supply responses, with low skilled labor being less elastic, e.g. due to lower mobility.

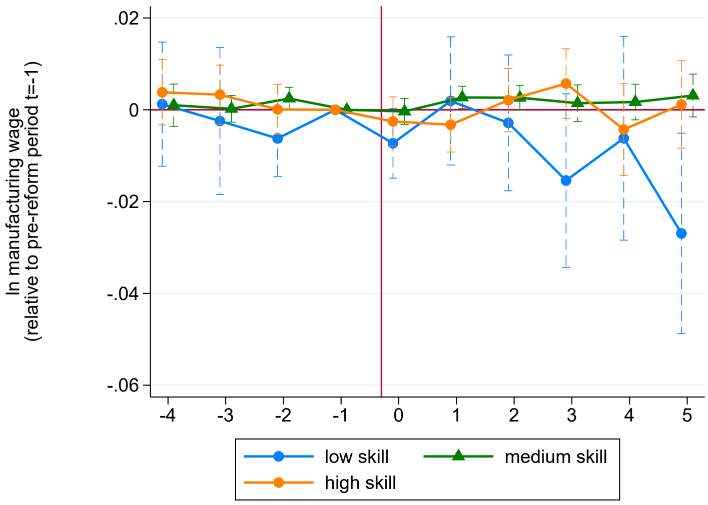
Sensitivity checks

In the following we present various tests demonstrating the robustness of our main results. 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 26 counties around the cut-off per regime. This is clearly an arbitrary choice, induced by the trade-off between comparability and statistical power. Appendix Figures B.3 and B.4 present results for employment and low-skilled wages for different cut-off samples including for the full sample.[20] Interestingly, the magnitude of the employment effect decreases as we restrict the number of counties around the cutoff. As we move away from the cut-off, we start to include better performing counties in our estimation sample which are likely to be on a different trend and perform better after in the post-treatment period without being treated. Including these counties biases the treatment effect toward zero.

Second, we use the refined treatment intensity in our baseline to more accurately account

[20] Restricting the sample to counties which are among the 20 counties closest to the cutoff for at least one reform yields 44 counties. Looking at 16 counties closest to the cutoff yields 38 counties.

Figure 2.6: Event study estimates: wages by skill in manufacturing



Source: SIAB. *Notes*: This figure plots coefficients along with 95 % confidence intervals of a regression of log manufacturing wages by skill on leads and lags of a change in the maximum assistance rate as in equation (2.4). The sample includes the 49 counties closest to cutoffs. State-year fixed effects are included. Clustering of standard errors is at the county level.

for the size of different reforms, sacrificing some of the non-parametric appeal of event study designs. As a check, we also implement the standard event study design using a discrete treatment indicator following equation (2.1).[21] Appendix Figures B.5 and B.6 compare our baseline estimate with the estimates from discrete treatment specification scaled by the average effect change in the maximum subsidy rate. Results are very similar, but exploiting information on the size of the change increases the precision. Furthermore, Appendix Figures B.7 and B.8 show that results of the discrete treatment specification also follow a similar pattern when varying the cut-off sample.

Excluding the few never-treated counties[22] from the analysis almost does not change the results, as can be seen from Appendix Figure B.9. This suggests that the treatment effects obtained by comparing ever-treated counties with never-treated counties is similar to the effect of comparing within the set of ever-treated counties, but exploiting the fact that those counties are treated at different points in time. Dropping partially treated counties also yields larger effects, suggesting that our baseline estimate is conservative: 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 (cf. Appendix Figure B.9).

In order to make sure that our results are not driven by the prevalence of multiple treatments, we re-estimate our main model only using the first reform. We find larger effects, but we are

[21] In 2007, the change in maximum subsidy rates is not uniform across firm size groups (cf. table 2.1). We base the definition of discrete treatment on the change in the maximum rate of small firms since 96% percent of firms are small. High-priority counties in regime 3 are therefore classified as not experiencing (discrete) treatment, whereas low-priority counties in regime 3 are classified as experiencing a (discrete) increase in the maximum rate.

[22] We define a county as never-treated if it is never discretely treated (see also footnote above).

only able to look at the first two years after the reform, after which the next reform happens (see Appendix Figure B.10).

Adding control variables that pick up the local business cycle (and consequently affected treatment status via the eligibility indicator) reassuringly changes the results little, as demonstrated in Appendix Figures B.11 and B.12.[23] We also test the effect of the GRW reforms on other subsidies received by municipalities. Figure B.13 shows that the reforms did not have a significant effect on other subsidies received by municipalities. The slight upward trend implies that, if anything, our estimates of the GRW effect are conservative.

2.5.2 Aggregate policy effects

While the previous section has demonstrated a local industry-specific treatment effect of the policy, we now check whether the place-based policy had an aggregate effect across industries and counties. Hence, we focus on spill-overs within and across counties.

In order to test for possible spill-overs within county, we start by assessing whether total county-level employment responded to the reform. As discussed in Section 2.2, policymakers hoped that the policy would raise local demand for goods and services in other sectors as well. Figure 2.7 shows that this was not the case. Non-manufacturing sectors did not respond to the change in the subsidy rate. As the average share of manufacturing employment is at around 18%, we do not detect an effect on total county-level employment either. Breaking down the effect on non-manufacturing employment by sectors, Appendix Figure B.14, however, provides some suggestive evidence of spill-overs. The construction sectors tends to show short-run negative effects, which seems reasonable as a large portion of the subsidized investments were used on buildings. Likewise, we detect a negative effect on trade/commerce, which could be reconciled with temporary decrease in overall demand. However, there are no spill-overs to service sector firms, which are a large share of the local economy and drive the positive effect of non-treated industries shown in Figure 2.7. A reason for the weak and rather short-lived within-county across-industry spill-overs might be that we look at a subsidy rate cut, rather than an increase. Unfortunately, the institutional set-up of the GRW does not allow us to develop a research design that is able to test for potential asymmetries in the response.

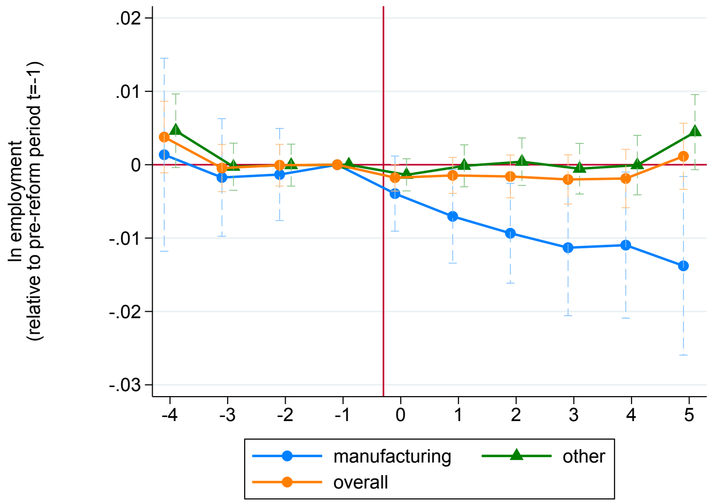
In line, with the zero effect on total employment, we find that county-level log GDP conditional on population does not respond to the policy (see Appendix Figure B.15). What is more, unemployment is not affected either (see Appendix Figure B.16).[24]

Looking at wages in non-treated sectors, we see a similar picture as for manufacturing wages: low-skilled workers are affected negatively while wages of medium and low-skilled workers do not respond (cf. Appendix Figure B.17). This is in line with the negative employment

[23] This is not surprising given that we find no treatment effect on these variables (GDP and unemployment) in the next subsection.

[24] Note that we do find insignificant, yet slightly negative effects, however the effects size is very small.

Figure 2.7: Event study estimates: employment by sector



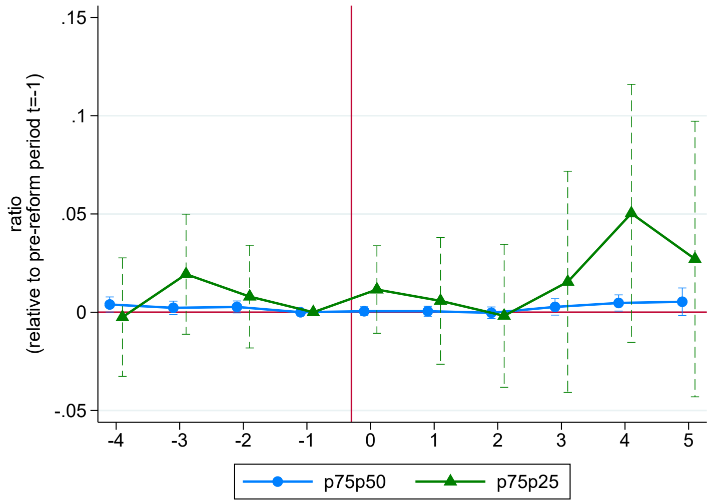
Source: BHP. *Notes*: This figure plots coefficients along with 95 % confidence intervals of a regression of log sectoral employment on leads and lags of a change in the maximum assistance rate as in equation (2.4). The sample includes the 49 counties closest to cutoffs. State-year fixed effects are included. Clustering of standard errors is at the county level.

effects for the construction and trade sectors that employ many low-skilled workers. As low skilled wages decrease across sectors, within-county inequality increases. Figure 2.8 shows the effects on the ratio of different percentiles of the wage distribution. Appendix Figure B.18 shows a similar effect on the Gini coefficient.

Finally, we test whether negative manufacturing employment effects in treated counties have effects that go beyond county borders.[25] We aggregate county-level manufacturing employment to the labor market region level and use the weighted average of counties' treatment intensities to re-estimate equation (2.3) on the baseline sample. Figure (2.9) shows that the treatment effect on employment shrinks by about 64%, which implies that most of regional treatment effect is due to a reallocation of workers across counties within a commuting zone.

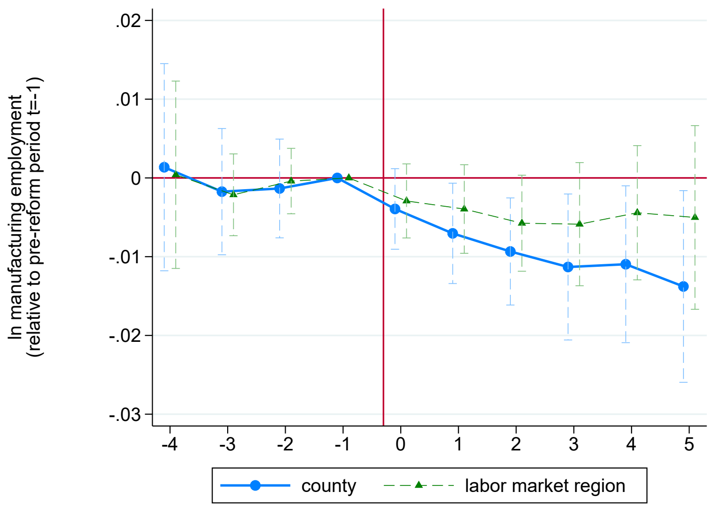
[25] Recall that untreated counties do not receive higher subsidy payments due to the cuts in treated counties.

Figure 2.8: Event study estimates: measures of inequality



Source: SIAB. *Notes*: This figure plots coefficients along with 95 % confidence intervals of a regression of inequality measures on leads and lags of a change in the maximum assistance rate as in equation (2.4). The sample includes the 49 counties closest to cutoffs. State-year fixed effects are included. Clustering of standard errors is at the county level.

Figure 2.9: Event study estimates: labor market region 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 at the labor market region level. Treatment intensities of counties are weighted by the number of employees. The sample includes the labor market regions that contain the 49 counties closest to cutoffs. State-year fixed effects are included. Clustering of standard errors is at the labor market region level.

2.6 Conclusions

In this paper, we investigate the labor market effects of subsidies to capital in the manufacturing sector, using data from Eastern Germany post reunification. We exploit various policy reforms that – in most cases – reduced the maximum share of investments that are eligible for the subsidy. These changes in the maximum subsidy rate varied across sectors, firm sizes and – importantly – East German counties according to an indicator of regional economic performance. We compare counties that are below the threshold yielding a higher subsidy rate to counties that are above.

We find that a cut in subsidies has strong negative effects on local manufacturing employment. A one percentage point decrease in the maximum subsidy rate leads to a decrease in county-level manufacturing employment of 1.4% five years after the reform. After ten years, the effect stabilizes at a level of manufacturing employment lower by 1.8%. The negative demand shock leads to a negative effect on wages for low-skilled workers. As wages for high- and medium-skilled workers are not affected by the policy, inelastic labor supply of low-skilled workers might be able to explain this pattern. With low-skilled wages decreasing, inequality increases due to the subsidy cut.

We also test for sectoral and regional spill-overs. While overall employment in counties experiencing a decrease in the maximum rate is not affected, we find suggestive evidence for small short-run responses for construction sector employment as a consequence of the reduction in subsidized investment. In terms of inter-county spill-overs, we find that about two thirds of the negative employment effect are absorbed within the labor market region, which suggests that reallocation of labor within the commuting zone is important.

Our institutional framework only allows us to study the effect of *cuts* in the subsidy rate. It is very well possible that treatment and spill-over effects are different for *increases* in subsidization. Further research could also focus on the analysis of channels driving inter-regional spill-overs. Individual data on residence and place of work would allow the investigation of commuting behavior. In addition, if researchers were allowed to link data on firm-level investment and output to firm-level subsidy data in the future, the role of spill-overs within region could be assessed in more detail (e.g. it would be interesting to know the effect of a firm receiving subsidies on other firms within industry).

The policy studied in this paper being part of the broader effort to help East German regions catch up with West German regions, we relate to the ongoing discussion about whether this effort has been successful. Burda (2006) and Uhlig (2006) paint a mixed picture, highlighting that labor productivity in the East had only reached two thirds of productivity in the West by the early 2000s. Today, large differences remain: unemployment rates have converged but nominal wages in the East have reached only roughly 80% of West wages. In addition, firms in East Germany are still predominantly of small size, resulting in low R&D expenditure and contributing to a persisting gap in productivity.[26] These developments cast into doubt the efficacy of continuing large-scale transfers to Eastern regions. Given the finding of our paper of a small overall effect of the GRW, skepticism may be in order.

[26] See https://www.beauftragter-neue-laender.de/BNL/Redaktion/DE/Downloads/Publikationen/Berichte/jahresbericht-de-2018.pdf?__blob=publicationFile&v=2, retrieved September 27, 2018.

Chapter 3

Political and Economic Effects of Explicit Electoral Thresholds: The Case of German Municipalities

3.1 Introduction

Electoral rules in many countries require parties to obtain a certain vote share in order to be eligible for seats in parliament. Turkey, for example, has a threshold of 10% in place for parliamentary elections, while in New Zealand and Germany, a 5% threshold applies. The existence of an electoral threshold implies that votes for parties failing to reach the minimum requirement are disregarded in the allocation of seats, a violation of the principle of "every vote counts". In addition, competition among parties is restricted. These drawbacks are often justified by the fact that it may become difficult to form majorities needed to pass legislation as the number of parties represented in a parliament increases.[1]

Economists have argued that a small number of represented parties may be desirable for another reason: the common pool problem, leading to inefficiently high public expenditure, is exacerbated as the number of legislators in parliament increases (Weingast et al., 1981). Applied to the number of parties involved in the budget process on a municipal council, the rationale is that individual parties internalize the cost of policies that favor their voters only partially, as part of the cost is borne by supporters of other parties (e.g. via taxation).

[1] However, this justification may not be accepted by courts for sub-national or supra-national elections. In 2011, the German Federal Constitutional Court ruled that a five percent electoral threshold in place in Germany for elections to the European parliament was unconstitutional on the grounds that since no government is elected by the European parliament, fragmentation is not as harmful as at the national level. As a reaction, a number of parties jointly passed a proposal to establish a three percent threshold instead which was then also ruled unconstitutional. The European parliament election in 2014 took place without a threshold (see http://www.bundesverfassungsgericht.de/pressemitteilungen/bvg11-070.html and http://dipbt.bundestag.de/extrakt/ba/WP17/537/53796.html, retrieved August 28, 2018).

Electoral thresholds can therefore be seen as a means to limit the common pool problem since the inefficiency is less severe the fewer parties are represented on a council.

In this paper, I analyze the removal of explicit electoral thresholds at the municipal level using German data to answer two questions: how large is the increase in the number of parties when a threshold is removed and what is the effect on economic outcomes such as expenditure? While some states in Germany never had a threshold in place for municipalities, others have abolished theirs at different points in time since 1990. The removals were necessitated by court decisions, on the basis that severe threats to the functioning of councils were not to be expected. Anecdotal evidence suggests that the number of parties represented at the municipal level has increased substantially in states that removed an electoral threshold. For example, there are currently 15 parties represented in Frankfurt's municipal council, where an explicit threshold of 5% has been abolished in 1999, whereas only 4 parties were represented in 1993. However, it is unclear what portion of this increase is simply due to an increase in the number of parties over time regardless of electoral thresholds.

In order to estimate the reforms' causal effect on political and economic outcomes, I rely on panel data on electoral outcomes and municipalities' budgets. Treating the abolishment of thresholds at different times as quasi-experimental allows the application of variants of the difference-in-differences estimator.

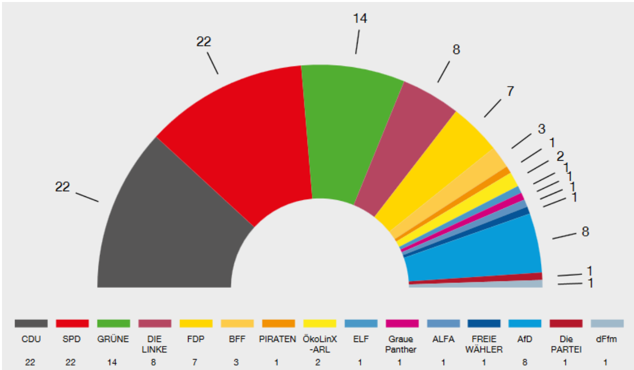
Fundamentally, the evaluation of electoral reforms at the municipal level sacrifices external validity for internal validity: in contrast to cross-country studies, the advantage of the present paper is that municipalities within Germany are characterized by a large degree of institutional homogeneity. On the other hand, municipalities do not have legislative powers, an important difference from national parliaments.[2] The councils of large municipalities more closely resemble parliaments than those of small municipalities: crucially for this paper, the number of seats is larger. This causes implicit thresholds, which may prevent party entry even in the absence of explicit thresholds, to be lower than in small municipalities (such thresholds emerge from the mapping of continuous vote shares to a discrete number of seats). In addition, stakes are higher and also remuneration for council members. I therefore pay special attention to larger municipalities throughout the paper.

The analysis is complemented by survey questions submitted to the German Internet Panel (GIP) to explore potential mechanisms and external validity. In particular, voter knowledge is tested since next to mechanical effects of the reforms, it is likely that a change of behavior occurred on the parts of voters and parties.

In a first step, I investigate the effect of a threshold removal on political outcomes, beginning with the number of parties represented on a council. Having demonstrated flat pre-trends, a difference-in-difference estimation yields an average increase of 0.3 parties. In the largest municipalities, however, the increase is 2.8. One reason is the existence of implicit thresholds, affecting small municipalities more strongly. In a second step, I look at municipal expenditure,

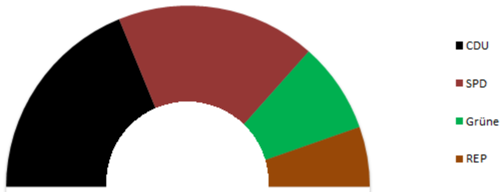
[2] Municipal councils may however set tax multipliers, e.g. for local business taxes.

Figure 3.1: Seats obtained by party in Frankfurt's 2016 municipal council election



Source: https://www.frankfurt.de/sixcms/media.php/738/FWA64_Stadtverordnetenwahl_2016.pdf, retrieved August 27, 2018.

Figure 3.2: Seats obtained by party in Frankfurt's 1993 municipal council election



Source: https://www.frankfurt.de/sixcms/media.php/678/Kap01.pdf, retrieved August 27, 2018.

motivated by the literature on the common pool problem predicting higher spending when the number of legislators increases. I find an increase of 3% on average. Mirroring the heterogeneity of the political results, the increase is higher at 15% in the largest municipalities. Revenue increases accordingly.

I contribute to the literature firstly by analyzing the political effects of *explicit* electoral thresholds and secondly by considering the thresholds' economic effects. Furthermore, the threshold variation used may be of use to other researchers looking for exogenous variation in representation of particular small parties, such as extremist parties.

As for the political effects of electoral thresholds, the closest papers to mine are Baskaran and da Fonseca (2016a), Baskaran and da Fonseca (2016b) and Pellicer and Wegner (2014). The first two papers are concerned with the political effects of implicit electoral thresholds in the German state of Hesse. In a regression discontinuity design, the authors investigate the abolishment of an explicit municipal threshold which has differential effects on municipalities by population size due to different *implicit* thresholds. They find an increase in vote and seat shares of smaller parties when the implicit threshold is lower but no effect on the number of parties represented. However, their setup only allows for the estimation of relative effects, i.e. how the effects vary between differently sized municipalities. In contrast, I estimate the total effect, using municipalities in other states as a control group. Thus, I am able to answer a more relevant question, namely what the effect of an explicit electoral threshold is relative to no explicit electoral threshold. This is particularly important since in addition to the effect varying by municipality size, there might be an effect due to changing behavior of voters and parties which is constant across municipalities of different size. Pellicer and Wegner (2014) analyze the political effects of a three percentage point increase in the explicit threshold in a subset of municipalities in Morocco. Comparing the reformed municipalities with municipalities that hold majoritarian elections in a difference-in-differences setup, they find a decrease in the number of parties. However, it is unclear whether voters in a semi-authoritarian country actually believe that their vote matters.

A number of contributions in political science compare the degree of fragmentation of parliaments in countries with different explicit thresholds (Moser, 1999; Birch, 2001; Lijphart and Aitkin, 1994). Such cross-country comparisons suffer from well-known omitted variable bias due to institutional and other differences on top of the differences in electoral thresholds analyzed. In contrast, my analysis relies on reforms at the sub-national level in Germany, ensuring a high degree of homogeneity of treated and untreated units.

As for the economic effects of electoral thresholds, this paper is the first contribution to the best of my knowledge. If thresholds have an impact on spending only via the number of parties represented, the reforms can also be used as instruments to estimate the effect of one additional party represented on a council on spending. The empirical political economy literature is broadly concerned with economic effects of institutions, in particular with determinants of the size of budgets (Besley and Case, 2003). More specifically, there are a number of contributions concerned with the causal relationship between the size of municipal councils (i.e. the number of seats) and expenditure. Egger and Koethenbuerger (2010), analyzing the German state of Bavaria, exploit the fact that council size there is a discontinuous function of population, allowing the implementation of a regression-discontinuity design. They find that an increase in the size of a municipal council has a positive effect on expenditure, in line with the prediction of a common pool problem. On the other hand, Pettersson-Lidbom (2012) finds evidence for a negative effect of council size on expenditure exploiting similar population cut-offs in Finland and Sweden for identification.

Turning to the effect of the number of parties as opposed to the number of members on a council, Jarman (2016) finds an increase in investment spending of almost 50 % when one additional party is represented on a council. Analyzing the German stata of Thuringia, he compares municipalities where a party just made it on a council with municipalities where a party just didn't make it in an RDD setup. Schaltegger and Feld (2009) investigate the effect of cabinet size and coalition size on expenditure for Swiss Cantons, finding a positive effect of cabinet size but mixed evidence with respect to the size of coalitions. However, lacking an instrumental variable, they resort to fixed effects estimation such that it is doubtful whether they can recover a causal effect. By using a different and credible source of variation, I therefore add to a scant literature on the effect of the number of parties on spending.

In the remainder of the paper, I first present the institutional setting. Hypotheses to be tested are outlined in section 3.3, followed by a section on data used. Section 3.5 explains the research design. Empirical results can be found in section 3.6 before section 3.7 concludes.

3.2 Institutional background

The roughly 11 000 municipalities in Germany are responsible for the provision of local services such as, for example, canalization, kindergartens, graveyards, for land use regulation and may offer public transport and cultural services. Expenses are financed via horizontal and vertical equalization transfers, taxes (the local business tax being the most important), fees and loans.

Municipalities are jointly governed by mayors and municipal councils. The mayor is typically the head of the municipal administration and puts into execution decisions made by the municipal council, in addition to representing the municipality. In contrast to the federal and to state parliaments, municipal councils are also part of the executive branch, i.e. no laws are passed.[3] Councils are the decision-making bodies. In particular they pass the budget and decide on property and local business tax multipliers next to monitoring the mayor's and the administration's activities. Simple majorities are sufficient for most decisions including the passage of the budget.[4]

Members of the municipal councils are elected every five years (the only exception being Bavaria, where elections occur every six years). Party-list proportional representation with open lists is the most common system used. The exceptions are the Saarland (which has closed lists), North Rhine-Westphalia and Schleswig-Holstein (the latter two states use a combination of first-past-the-post voting and party-list proportional representation). Depending on the state, the D'Hondt method, the largest remainder method or the Sainte-Laguë method are used as rules for allocating seats. Next to parties which also run in state and federal elections, there is a large number of so-called "voter groups" running only in municipal elections (obtaining party status is more demanding in terms of meeting formal requirements such as detailed declaration of origin and use of funds).[5]

3.2.1 Threshold reforms

Seven German states have removed a municipal electoral threshold (which was at 5% except for the state of Rhineland-Palatinate where the threshold was at 3%) since reunification in 1990, as illustrated in figure 3.3. As for the other states, they did not have a threshold in place as of 1990.

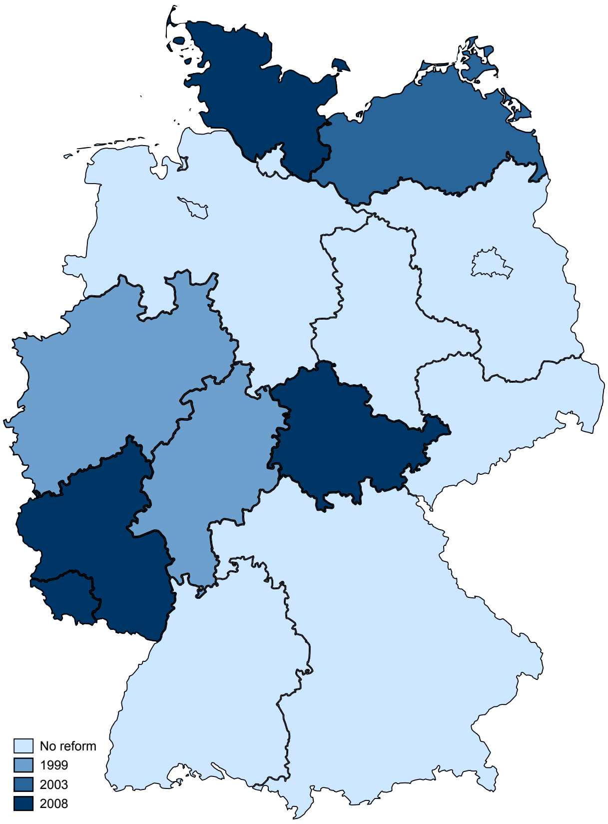
The reforms were initiated either by the Federal Constitutional Court, by one of its state

[3] I disregard the city states Berlin, Hamburg and Bremen where the municipal council is essentially the state parliament, which means that they are not comparable with municipalities in other states since state parliaments possess legislative powers.

[4] See e.g. http://www.kommunalwahl-bw.de, retrieved August 22, 2018. With the exception of the state of Hesse, all states follow essentially the same constitutional rules formulated in the "Süddeutsche Ratsverfassung".

[5] Independent candidates not affiliated with a party may also run in council elections in some states. I ignore them in what follows since they do not obtain a significant share of votes. See e.g. http://www.wahlergebnisse.nrw.de/kommunalwahlen/, retrieved August 22, 2018.

Figure 3.3: Timing of threshold removals by German states



Notes: This figure shows the year in which a German state removed an explicit electoral threshold for municipal elections post-1990. City states are excluded from the analysis.

level counterparts or by a state level government.[6] The Federal Constitutional Court argued that a threshold violates both equal opportunity among parties and equal treatment of votes (since votes for small parties are effectively discarded). At the same time, in contrast to the state and federal levels, stable majorities are not deemed essential since no laws are passed by municipal councils. Further reasons are the relatively strong position of mayors which guarantees, according to the court, that administration of a municipality is not jeopardized by a fragmented council and the existence of implicit thresholds, discussed below. In addition, the experience of states with no threshold in place is cited. There, the court argued, the number of parties represented had not led to deficiencies in the functioning of municipal councils.

The plaintiffs were typically members of a small party, e.g. in 2008 the Green party and the party "Die Linke" jointly brought the state of Schleswig-Holstein to trial before the Federal Constitutional Court. This is likely due to the fact that small parties expected to benefit from the threshold removal. However, it seems safe to assume that the particular timing of a suit was unrelated to the electoral outcomes in the following municipal elections since the duration of the trial and the performance at a particular election is hard to foresee. Small

[6] See Verfassungsgerichtshof Nordrhein-Westfalen (1999), Land Hessen (2000), Landesverfassungsgericht Mecklenburg-Vorpommern (2000), Bundesverfassungsgericht (2008), Verfassungsgerichtshof Thüringen (2008), Land Rheinland-Pfalz (2008) and Landtag des Saarlandes (2008) for details.

parties have gained higher vote shares over the past two decades at both the federal and state levels, but this trend has been similar in all states.[7]

3.2.2 Implicit thresholds

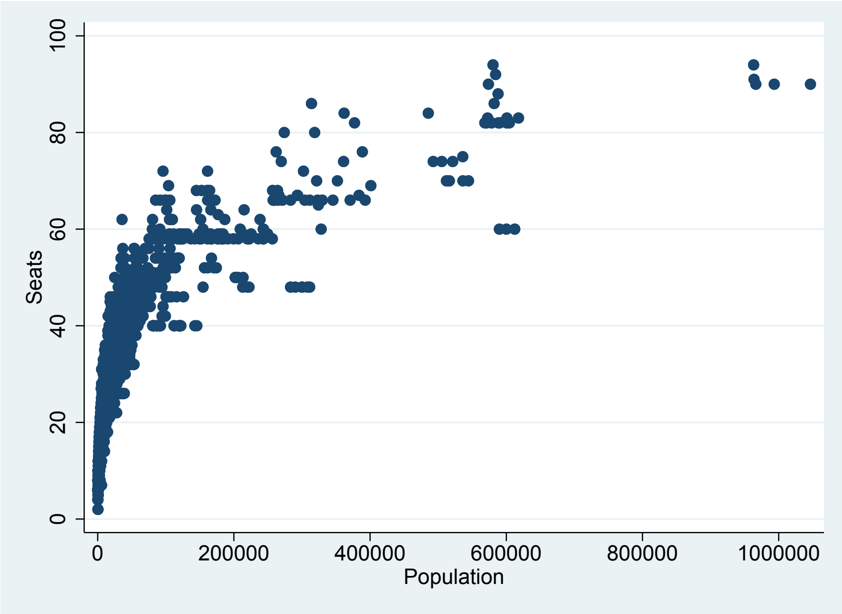
When an explicit threshold is removed, implicit thresholds may still prevent a party from entering a municipal council. An implicit threshold specifies the minimum vote share required to gain the first seat due to the fact that continuous vote shares are mapped into a discrete number of seats. This threshold is a function of the seat share allocation rule, of the number of seats and the actual vote shares realized. Ex-ante, one can only compute a range for the implicit threshold or make assumptions about the vote share distribution to obtain a distribution of implicit thresholds. Consider the extreme example of a council consisting of only one seat and three parties competing (with no explicit threshold in place). If two parties each are just short of one third of all votes, the third party needs to receive just above one third of the vote share to obtain the seat. On the other hand, if one party receives no votes, the minimum vote share to obtain the seat is one half.

As for German municipalities, the total number of seats in a municipal council depends on the number of inhabitants. The council size-population relationships varies across states, but generally the number of seats is discontinuously increasing in population (see figure 3.4 below). Also, in some states, there is discretion such that municipalities can choose the number of seats from a narrow range. Approximately, in most cases the realized implicit threshold lies between the average vote share per seat (e.g. 5% for 20 seats) and one half of the average vote share per seat.[8] This implies that as the number of seats increases, the approximated implicit threshold declines (i.e. entry becomes more likely, ceteris paribus). Due to the nature of the population-seat relationship, this in turn implies that as the population increases, the approximated implicit threshold declines.

[7] See e.g. http://www.wahlrecht.de/ergebnisse/.

[8] See Lijphart and Gibberd (1977) and Kopfermann (1991) for threshold theory and http://www.wahlrecht.de/verfahren/faktische-sperrklausel.html, retrieved August 22, 2018, for simulations applicable to German municipalities.

Figure 3.4: Population and council size



Notes: This figure plots the number of seats on a council against population for German municipalities included in *sample 1* (see section 3.4 for more information on the data set).

3.3 Hypotheses

In this section, I present hypotheses based on theoretical considerations about the effects of the threshold removals to be tested using the data described in section 3.4 and the methodology described in section 3.5.

Hypothesis 1: *The removal of an electoral threshold leads to an increase in the number of parties represented on a council.*

This hypothesis is straightforward: votes that are ignored pre-reform due the existence of an explicit electoral threshold may lead to additional parties gaining representation post-reform. This mechanical effect may be accompanied by additional effects due to changing behavior of parties and voters. For example, parties may decide to participate in an election that would not have run in the presence of a threshold.[9] Even if the same parties participate in elections, voters may be more willing to vote for small parties in the absence of a threshold.[10] This requires, however, that voters are aware of the reforms. Below, I present survey evidence on the salience of electoral thresholds.

[9] Note that since vote shares are not available for all parties, I cannot disentangle mechanical and behavioral effects.

[10] I do not formally investigate strategic behavior. The probability of being pivotal for a given outcome is hard to compute but likely very small even in municipal elections. Still, voters do turn out. It is possible that some voters are not aware of the low probability of being pivotal. In addition expressive motives may play a role for part of the electorate (Brennan and Lomasky, 1993).

Hypothesis 2: *A threshold removal leads to a smaller increase in the effective number of represented parties than in the absolute number of represented parties.*

Whether fragmentation of councils increases substantially depends on the number of seats that additional parties obtain. If mostly small parties enter municipal councils due to the reform, which is to be expected since large parties would have gained representation even with thresholds in place, it may well be that fragmentation does only increase marginally. I use the Herfindahl index $\sum_i s_i^2$, where s_i is the seat share of party i, as an additional outcome. It measures the concentration of power, reaching its maximum at 1 if one party holds all seats on a council, akin to the concentration in a market. I also look at the *effective* number of parties (Laakso and Taagepera, 1979), which is the inverse of the Herfindahl index and constitutes a particularly intuitive measure of fragmentation: when seat shares are equal across parties, the effective number of parties equals the actual number of parties. Therefore, we can interpret this measure as indicating the number of equal-sized parties with the same degree of fragmentation as induced by the actual number of parties.

Hypothesis 3: *The political effects differ by municipality size.*

The discussion of implicit thresholds above has shown that the reforms' mechanical effects vary across municipalities of different size. Larger municipalities which have larger municipal councils are characterized by lower implicit thresholds, making it easier for small parties to enter when an explicit threshold is removed. Behavioral effects, to the extent that they exist, may vary with size, as well. For instance, if parties are aware of implicit thresholds, they might decide to focus their efforts on large municipalities since the probability of obtaining representation there is higher. Furthermore, behavioral effects could be stronger in larger municipalities independently of implicit thresholds. For example, a more diverse electorate in large cities might lead to a demand for special-interest parties. In addition, the stakes are higher in the sense that councils' decisions affect are larger number of inhabitants. Finally, remuneration for members of individual councils is higher in large municipalities.

Hypothesis 4: *The removal of an electoral threshold leads to an increase in municipal expenditure (and more so in larger municipalities).*

The theoretical prediction about the effect of removing an electoral threshold on expenditure is not clear-cut: on the one hand, the literature on the common-pool problem predicts increasing expenditure. Beginning with Weingast et al. (1981), this strand of the literature posits that legislators only partially internalize the cost of policies that favor their constituents since part of the cost is borne by other constituencies. An increasing number of legislators aggravates this problem. This idea equally applies to the number of parties on a municipal council since typically, passing a budget requires that several parties agree on a proposal.[11] If the number of parties on a council increases, individual parties obtain a smaller number of seats such that the approval of a larger number of parties tends to be required to pass a budget, exacerbating the common-pool problem. Larger municipalities should be affected more strongly due to the larger expected number of additional represented parties (see Hypothesis 3). In addition, note that the *threat* of competition alone may have an effect on the behavior of parties represented in the council when the goal of preventing entry leads an incumbent party to target a broader base of voters.[12]

On the other hand, extreme fragmentation of power might lead to government inaction.[13] For instance, survey evidence on the reform in North Rhine-Westphalia hints at an increasing duration of council meetings (Bogumil et al., 2015). If government inaction is severe, this would speak against an increase in expenditure (and could lead to a decrease) since passing the budget requires a majority in the municipal council: if a municipal council is unable to pass a budget, a provisional budget automatically takes effect. Such a bare-bone budget allows for expenditure due to legal obligations that are based on decisions passed in the previous budgetary period but not for spending on new projects. In fact, the potential inability to pass legislation motivates the thresholds still in place at the state and federal level in Germany, as mentioned above. The veto player theory by Tsebelis (1995) formalizes this idea. A key result is that policy change becomes less likely as the number of veto players (here: parties) increases.

However, a permanent inability to pass a budget is unlikely since none of the parties would then be able to realize projects requiring funding. Inability to reach agreement might therefore be more relevant for other, non-budgetary policy proposals. Therefore, I take the prediction of the common pool literature of increasing expenditure to the data, which is in accordance with other empirical contributions concerned with the economic effects of the composition of municipal councils (see e.g. Egger and Koethenbuerger (2010)).

[11] Formal coalition agreements are mostly found in larger municipalities.

[12] Relatedly, Lizzeri and Persico (2005) consider the efficiency rationale for electoral thresholds by analyzing electoral competition among parties which promise either targetable transfers or public good spending. They find that the equilibrium is not surplus-maximizing if the number of parties is large enough. However, they do not consider the role of coalitions. Increasing political competition could be efficiency-enhancing in other dimensions. Ashworth et al. (2014) find that efficiency of public good provision increases, akin to goods markets, since re-election is less certain.

[13] It is also possible that the relationship between the number of parties and expenditure is non-monotonous, e.g. increasing first but decreasing as more and more parties enter.

3.4 Data

3.4.1 Political outcomes – *Sample 1*

I construct a panel of political outcomes using municipal election results provided separately by the individual state statistical offices.[14] There are two restrictions when constructing a panel of political outcomes: in a number of states, information on the number of seats obtained is available only for relatively large parties whereas for smaller parties, only the combined number of seats obtained by these parties is reported. These results are therefore uninformative when it comes to calculating the overall number of parties represented (the same issue applies to the number of parties competing and measures of concentration). Furthermore, election years vary across states, e.g. in North-Rhine Westphalia, municipal elections since reunification took place in 1994, 1999, 2004, 2009 and 2014, whereas in Hesse, they took place in 1993, 1997, 2001, 2006 and 2011 (see Appendix table C.7 for a list of election years of all states).

Mergers of municipalities are a further complication. I use municipality border definitions of the year 2014 and proceed as follows: if a municipality is never affected by a merger, I use all available years. If a merger of two or more municipalities takes place e.g. in the year 2004 but the resulting municipality is not merged thereafter, I use election data for the resulting municipality from 2004 onwards (similarly, for multiple mergers, I only use observations from the year onwards in which they are consistent with the border definitions of 2014).

Given these limitations, the panel of political outcomes is constructed by excluding states that do not hold elections in the years 1994, 1999, 2004, 2009 and 2014 (the majority of states holds elections in these years) and then excluding states where detailed information is available on the number of seats obtained by each party. The resulting sample includes municipalities of the states North Rhine-Westphalia, Baden-Wuerttemberg, Saarland, Saxony and Thuringia. In what follows, I refer to this set of observations as *sample 1*. Descriptive statistics are presented in table 3.1, illustrating that there are on average 3.8 parties represented in a municipal council. However, the *effective* number of parties[15], interpretable as the number of equal-sized parties leading to the same degree of fragmentation as induced by the actual number of parties, is lower at about 2.8.

3.4.2 Economic outcomes – *Sample 2*

Data on expenditure, revenues and local tax multipliers is publicly available for all German municipalities (Landesbetrieb Information und Technik Nordrhein-Westfalen, 2003-2011). I

[14] Results are available publicly, see Appendix for detailed data sources.
[15] $\frac{1}{\sum_i s_i^2}$, where s_i is the seat share of party i.

Table 3.1: Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max	P25	P75
Sample 1							
Population	9689	14786	46968	60	1046680	1503	12307
Represented parties	9689	3.75	1.51	1	13	3	5
Effective represented parties	9689	2.78	0.91	1	8	2	3.36
Competing parties	9638	4.01	1.78	1	17	3	5
Council seats	9689	19.75	12.61	2	94	12	26
Sample 2							
Population	173591	6727	28382	3	1429584	663	5136
Expenditure (in €1000)	173591	15231.78	103653.50	3.00	6569848	850.79	8641.04
Revenues (in €1000)	172912	14761.56	100247.80	.14	6460044	877.97	8628.23

use data from all states for the period 1995-2014[16] except for the city states Berlin, Hamburg and Bremen, as mentioned above. Using a large number of states unaffected by the reforms adds credibility to the identifying assumption of common trends in economic outcomes, explained in more detail below, since as the number of states increases, idiosyncratic state-year shocks e.g. to expenditure matter less.[17] In addition, reforms occur in states not covered by the data set on political outcomes.

In contrast to *Sample 1*, I make use of municipalities in all years available rather than in election years only since the reforms' effects on economic outcomes can materialize immediately rather than with a lag of five years (the number of years between elections). This sample will be referred to as *sample 2* below. Mergers are not an issue for economic outcomes such as expenditure since these can be consistently aggregated when municipalities merge (unlike election results): in the case of expenditures, the sum is easily obtained. As is evident from table 3.1, the average municipality is smaller in *sample 2*, containing more observations, than in *sample 1*. Expenditure and revenue figures exhibit a large degree of variability but for the majority of municipalities, the values lie between €1000 and €2000 per inhabitant.

3.5 Research design

Exploiting the quasi-experimental removal of explicit electoral thresholds in German states at different points in time, I implement variants of the classical difference-in-difference estimator. The affected states constitute the treatment group while other states, which either never had a threshold in place or are treated at a different point in time, serve as the control group. It is also possible to restrict the sample to municipalities of states that experienced a reform

[16] Some years are not available for all municipalities but it seems safe to assume that missing values are randomly allocated.

[17] Note that common trends e.g. in the number of parties represented are likely to be satisfied in the smaller *sample 1* since idiosyncratic state-year shocks are unlikely to occur for the political outcomes considered.

at some point in time. The control group for a given reform then consists of municipalities reformed in different years.[18] The identifying assumptions are that treated municipalities and never-treated control municipalities would have followed a common trend over time in the absence of treatment and that the timing of treatment is random. Therefore, if pre-treatment trends were significantly different for treatment and control groups, this would cast doubt on the validity of the research design. In addition to implementing the classical version of the difference-in-differences estimator, I make use of an event study design which allows me to both analyze pre-trends as well as the dynamics of the reforms' effect.

Looking at electoral outcomes first, implementing an event study amounts to regressing outcomes y_{ist} in municipality i, state s and *election* year t on dummies indicating how far away (in time) a reform will take place or has taken place:

$$y_{ist} = \sum_{d=-3, d\neq-1}^{3} \beta_d \cdot \mathbf{1}(t - e^s = d) + \gamma_i + \alpha_t + \epsilon_{ist} \tag{3.1}$$

where e^s indicates the reform year in a municipality's state and the time-constant fixed-effect γ_i absorbs permanent differences in the outcome variable across municipalities. α_t accounts for election year specific effects common to all states. The coefficients β_d measure the effect of a reform that took place d periods ago (or will take place $|d|$ periods from now, for negative values). Following standard practice, I drop the dummy representing the pre-treatment election such that the estimates of coefficients corresponding to the other dummies are to be interpreted relative to the pre-treatment election.[19] In addition, the dummies corresponding to β_3 (β_{-3}) are switched on if a reform is more than three election years away. Since elections take place only every five years, I investigate outcomes in *election* years prior to the reform and after the reform. In a variation of this event study, I separately estimate time fixed-effects for small and large municipalities due to the fact that size distributions vary across states and trends are likely to vary by municipality size (e.g. an increase in the number of parties over time may occur only in large municipalities).

When looking at economic outcomes, I consider a specification that differs slightly from the one in equation (3.1): t now represents years since, as opposed to election results, data points are available yearly. Furthermore, I include state-specific linear trends $\delta_s \cdot t$. Accounting for secular state-specific trends may be particularly important for outcomes such as expenditure since decisions made by the state government such as cuts to funding for municipalities might otherwise be confused with the electoral reforms' effects. The baseline specification for economic outcomes reads

[18] This approach will be one of the robustness tests presented below.

[19] Not dropping one of the event study's dummies leads to multicollinearity.

$$y_{ist} = \sum_{d=-4, d\neq -1}^{10} \beta_d \cdot \mathbf{1}(t - e^s = d) + \gamma_i + \alpha_t + \delta_s \cdot t + \epsilon_{ist} \tag{3.2}$$

where the notation is analogous.[20] Again, I also separately estimate time fixed-effects (and time trends) for small and large municipalities. Note that including state-specific trends is not possible when analyzing electoral outcomes due to limited data availability. Differentiating between state-specific trends and treatment effects requires sufficient observations pre-treatment (Angrist and Pischke, 2008) but this number is very small when elections do not take place every year. However, the empirical results presented below suggest that pre-trends are very similar for political outcomes.

Finally, I make use of classical difference-in-differences models closely corresponding to the event study specifications. Based on the model in equation (3.2), I estimate

$$y_{ist} = \beta D_{ist} + \gamma_i + \alpha_t + \delta_s \cdot t + \epsilon_{ist} \tag{3.3}$$

where D_{it} is equal to one if a reform occurs at time t or has already occurred and zero otherwise (and similarly for equation (3.1)). Borusyak and Jaravel (2017) caution that $\hat{\beta}$ does not necessarily represent the simple average of the dynamic treatment effects of a reform. By contrast, the estimate overweights short-run effects. However, this issue is most severe if identification comes solely from the different of timing of reforms. The presence of never-treated control units, of which I have a large number in the sample[21], reduces the bias which is zero if time effects are only identified from never-treated units. In addition, one important question is whether the reform's effect is heterogeneous with respect to municipality size. If weighting leads to a short run bias of estimates for both small and large municipalities, comparing the two effects is still meaningful.

Standard errors are clustered at the municipality level throughout to allow for serial correlation of error terms. For robustness, I also perform clustering at the state-year level.[22]

[20] Expenditure data beginning in 1995, four leads are chosen since the first reform occur sin year 1999. Ten lags are chosen due to the constancy of the effect seven years after a reform and later, as shown below.

[21] Never treated units make up 49% of *sample 1* and 47% of *sample 2*.

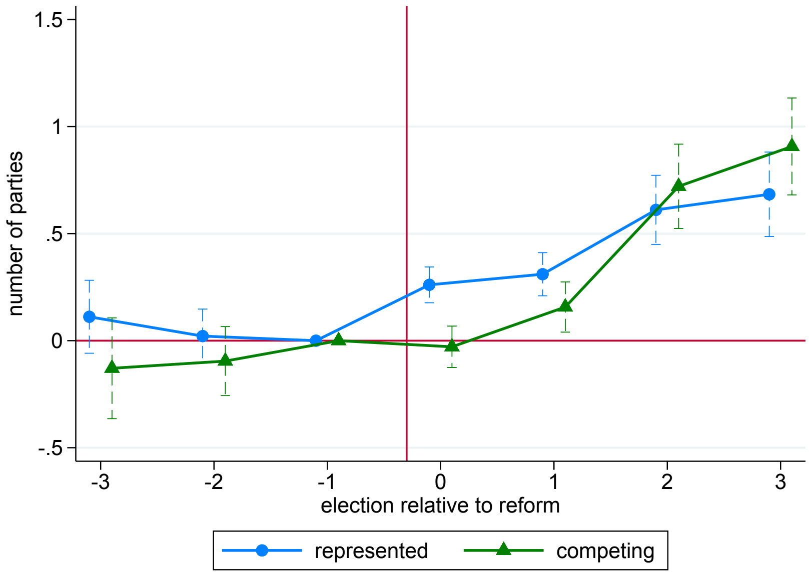
[22] Clustering at the state-year level is feasible only when the outcome is municipal expenditure since *sample 1* contains only twenty state-year cells. Clustering at the state level would be preferable, as highlighted by Bertrand et al. (2004), since serial correlation might be present at the state level but is infeasible due to the small number of states.

3.6 Empirical results

3.6.1 Political outcomes

In this section, I report the empirical results based on the estimation strategy outlined in the previous section, starting with political outcomes. Figure 3.5 plots the coefficient estimates of an event study based on equation (3.1) where the outcomes are the number of parties competing and represented in municipal councils. Elections occur every five years which is why it is number of elections the reform is away that can be found on the horizontal axis. Omitting the pre-reform election allows for an easy interpretation of the coefficients. Reassuringly, relative to the pre-reform election, treated municipalities' trajectories in terms of the number of parties before the reform do not differ from control municipalities.

Figure 3.5: Event study estimates: effect of reforms on number of parties



Notes: This figure shows event study estimates of equation (3.1) along with 95% confidence intervals. Size-year fixed effects are included. *Sample 1* is used for all political outcomes. The outcomes shown are the number of parties represented on a council and the number of parties competing. *Election* years are on the horizontal axis. Standard errors are clustered at the municipality level.

The effect on the number of parties represented is significantly positive at the reform election (i.e. when the threshold is just removed). Over time, the effect increases and at the third post-reform election, on average 0.7 additional parties are represented relative to the control group. The fact that it takes more time for the number of parties competing to increase is intuitive: mechanically, if the number of competing parties remains constant, the number of parties represented increases due to the threshold removal (in addition to behavioral effects). But for the number of parties competing to increase, a change in behavior is essential and parties and voters may only learn about the change in institutions over time. As expected, hypothesis 1 is therefore confirmed by the empirical results. This is an important finding since the analysis of Baskaran and da Fonseca (2016b) did not find an effect of varying implicit thresholds on the number of parties.

In section 3.2, I have explained the role of *implicit* thresholds, which are lower in larger municipalities due to larger councils. This suggests that the estimates hide a great deal of heterogeneity. I therefore test for heterogeneous effects by looking at small and large municipalities separately in a difference-in-differences setup (similar to equation (3.3)), dividing the sample into municipalities above and below 10 000 inhabitants.[23] This implies an average approximated implicit threshold of 4% in small municipalities and an average approximated implicit threshold of 1% in large municipalities. As can be seen from table 3.2, the effects indeed vary substantially by municipality size. Significant at the one percent level, the estimates show that the reform led to an increase in the number of parties represented of about 1.4 on average in large municipalities, compared with 0.08 in small municipalities. The effects are also increasing by municipality size when the outcome is the number of parties competing for seats. The latter does not seem to change at all in small municipalities but does increase by 0.6 in large municipalities. This means that hypothesis 3, stating that effects differ by municipality size, is confirmed for the outcomes analyzed so far.

As for difference-in-differences estimates, the overall effect of 0.25 for the number of represented parties is of the same magnitude as the first lag of the estimated event study. By contrast, after three elections, the event study yields a larger estimate. This can be explained as follows: the only state for which more than one post-reform election is observed is the state of North Rhine-Westphalia, whereas the other two reformed states in *sample 1*, Saarland and Thuringia, are only observed in the year in which the reform takes place and in one subsequent election.[24] This unbalancedness exacerbates the short-run bias of the difference-in-difference estimator discussed in Borusyak and Jaravel (2017) and described in the last section since the long-run effects receive an even lower weight due to the fact that the short-run observations in the sample outnumber the long-run observations.[25] Therefore the difference-in-differences estimates can be considered conservative. In addition, the two main conclusions – an increasing number of parties represented over time and heterogeneity of the effect by municipality size – still remain valid since the short-run bias of the difference-in-difference estimator equally applies to both small and large municipalities.

While the number of parties represented on a council is an important measure of concentration of power, it is imperfect since some of the parties may be very small. Hypothesis 2 states that fragmentation as measured by the number of effective parties increases by less than the actual number of parties represented. Fragmentation effects are presented in table C.2.

[23] The population means of *sample 1* and *sample 2* are approximately 15 000 and 7000 respectively. I explore the robustness with respect to the definition of small and large municipalities below. When a municipality's size classification varies over time, I only consider observations after the last change.

[24] Recall that the reform took place in North Rhine-Westphalia in the year 1999 but in the year 2009 in the other two states.

[25] As for the number of competing parties, the difference is even stronger due to the fact that it takes more time till the effect materializes.

Table 3.2: Difference-in-differences estimates: parties represented and competing

	(1) represented	(2) represented	(3) competing	(4) competing
reform	0.251***		0.065	
	(0.042)		(0.045)	
small × reform		0.082**		0.009
		(0.039)		(0.046)
large × reform		1.372***		0.587***
		(0.135)		(0.181)
Size-Year-FE	yes	yes	yes	yes
Observations	9689	9689	9638	9638
Adjusted R^2	0.284	0.300	0.130	0.132

Notes: Standard errors in parentheses (* $p < .1$, ** $p < .05$, *** $p < .01$). Sample: *sample 1*. Small municipalities: 10 000 inhabitants or fewer. Clustering of standard errors is at the municipality level.

In large municipalities, the Herfindahl index decreases by 0.03, indicating a decrease in concentration. The effective number of parties increases by about 0.2 on average in large municipalities. That is, fragmentation increases as if the number of equal sized parties increases by 0.2, well below the increase in parties represented (1.4), which suggests that entering parties indeed attain only a small number of seats. Therefore, hypothesis 3, as it applies to fragmentation is confirmed in that only large municipalities are affected. Notice that even if fragmentation increases only slightly, an effect on expenditure might still materialize in case of slim majorities of coalitions pre-reform.

I explore the robustness of the main result by leaving out one of the treated states at a time, similar to a jackknife estimation and reassuringly, the results do not change significantly, as can be seen from columns (3)-(5) of table 3.3. This suggests that the results are not driven by state-specific shocks to the number of represented parties that happen to occur in the reform year or afterward. Furthermore, I use different definitions for small and large municipalities (using 15 000 and 7000 inhabitants as cutoffs, corresponding to the population means of *sample 1* and *sample 2*, respectively), leaving the results unchanged. In addition, I look at three size groups instead of two to explore heterogeneity within the group of larger municipalities. Table 3.4 reveals that municipalities having 200 000 inhabitants or more are affected most severely. The number of parties represented increases by 2.8. However, this group of municipalities is very small.[26]

[26] There are only 98 observations of municipalities having 200 000 inhabitants or more in *sample 1*.

Table 3.3: Difference-in-differences estimates: parties represented (robustness checks)

	(1)	(2)	(3)	(4)	(5)
pop$<$ 7000 $\times$ reform	0.059				
	(0.042)				
pop $\geq$ 7000 $\times$ reform	1.268***				
	(0.122)				
pop $<$ 15000 $\times$ reform		0.106***			
		(0.037)			
pop $\geq$ 15000 $\times$ reform		1.355***			
		(0.142)			
small $\times$ reform			0.071*	0.069*	0.710***
			(0.041)	(0.039)	(0.115)
large $\times$ reform			1.568***	1.285***	1.571***
			(0.182)	(0.176)	(0.165)
Size-Year-FE	yes	yes	yes	yes	yes
Excluded state			NW	SL	TH
Observations	9689	9689	7751	9589	6772
Adjusted R^2	0.271	0.313	0.118	0.288	0.356

Notes: Standard errors in parentheses (* $p < .1$, ** $p < .05$, *** $p < .01$). Sample: *sample 1*. Small municipalities: 10 000 inhabitants or fewer. Clustering of standard errors is at the municipality level.

Table 3.4: Difference-in-differences estimates: parties represented (different size groups)

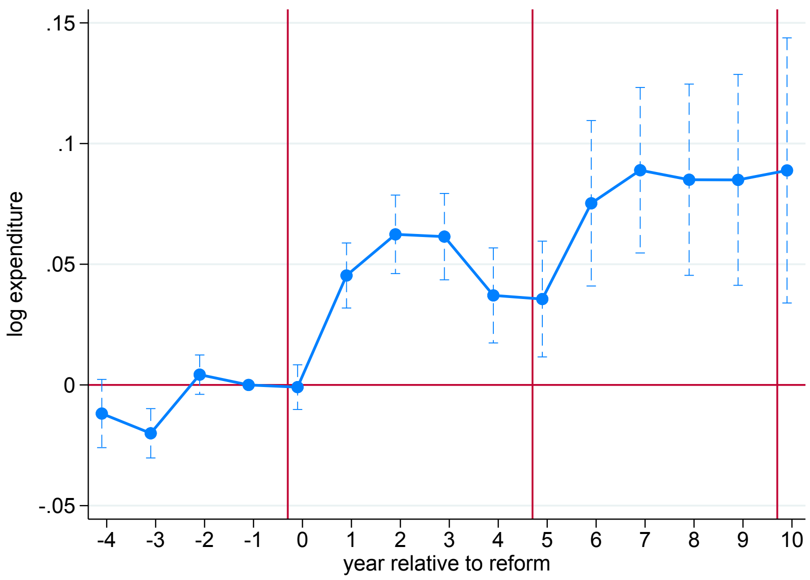
	(1)
pop$<$ 10000 $\times$ reform	0.082**
	(0.039)
10000 $\leq$pop$<$ 200000 $\times$ reform	1.325***
	(0.131)
pop $\geq$ 200000 $\times$ reform	2.826***
	(0.198)
Size-Year-FE	yes
Observations	9689
Adjusted R^2	0.348

Notes: Standard errors in parentheses (* $p < .1$, ** $p < .05$, *** $p < .01$). Sample: *sample 1*. Clustering of standard errors is at the municipality level.

3.6.2 Economic outcomes

Given that the abolishment of an electoral threshold has an effect on the number of parties represented on a council, particularly in larger municipalities, what are the consequences in terms of policy? Hypothesis 4 conjectures that expenditure increases, potentially due to a common-pool problem, if fragmentation is not too severe. Figure 3.6 presents the event study based on equation (3.2) where the outcome is the logarithm of municipal expenditure and time is measured in years relative to the removal of an electoral threshold.[27]

Figure 3.6: Event study estimates: effect of reforms on log expenditure



Notes: This figure shows event study estimates of equation (3.2) along with 95% confidence intervals. Size-year fixed effects and size-year specific linear trends are included. *Sample 2* is used for economic outcomes. Log municipal expenditure can be found on the vertical axis. Years are on the horizontal axis. Standard errors are clustered at the municipality level.

While differences in the development of expenditure pre-reform are small between the treatment and control groups, there is a marked increase one year after the reform and then again five years later with a period of relative constancy in between and subsequently. An increase in expenditure of about 9 percent obtains ten years or more after a reform. This result is similar to the finding by Egger and Koethenbuerger (2010) of an increase in expenditure by 11 percent when council size increases by 2.6 seats. Since elections take place every five years (indicated by red vertical lines), the pattern is consistent with the political effects found above which are increasing over elections following the reform. Furthermore, readjustment after elections with a lag of one year is consistent with the fact that many municipalities implement budgets extending over a period of two years. Before I turn to potential mechanisms, I explore the effect's heterogeneity with respect to municipality size. Table 3.5 presents difference-in-difference estimates based on equation (3.3) where the outcome is log expenditure for *sample 2* (columns 1 and 2) and separately for the municipalities of

[27] In what follows, I make use of the larger *sample 2* which covers more states (and years) than the sample used for the analysis of political outcomes.

sample 1 (columns 3 and 4).[28] The average effect is clearly higher for large municipalities at 6 percent compared to 2 percent for small municipalities in *sample 2*, consistent with the stronger response of the number of parties represented for large municipalities and confirming the second part of hypothesis 4. Again, there seems to be a severe short-run bias compared with the event-study estimates. The effects are smaller in the municipalities of *sample 1* but still, large municipalities see a relatively higher increase in expenditure. However, it is unclear whether political effects are stronger for the complete set of states featured in *sample 2* (for which the relevant outcomes are unavailable), compared with *sample 1*.

Table 3.5: Difference-in-differences estimates: log expenditure

	(1)	(2)	(3)	(4)
reform	0.027***		0.016*	
	(0.005)		(0.008)	
small × reform		0.022***		0.009
		(0.006)		(0.012)
large × reform		0.064***		0.030***
		(0.016)		(0.009)
Size-Year-FE	yes	yes	yes	yes
Linear state trends by size	yes	yes	yes	yes
Observations	173591	173591	41875	41875
Adjusted R^2	0.091	0.091	0.183	0.183

Notes: Standard errors in parentheses (* $p < .1$, ** $p < .05$, *** $p < .01$). Models (1) and (2): *sample 2*, models (3) and (4): municipalities of *sample 1*. Small municipalities: 10 000 inhabitants or fewer. Clustering of standard errors is at the municipality level.

So far, I have focused on the reduced form effect, i.e. the reform's effect on expenditure. Which of the political effects of the threshold removal is likely to be responsible for the increase in municipal expenditure? Only indirect evidence is available to answer this question since some potential channels cannot be excluded with the data available. For example, the threat of competition may lead incumbent parties to change their behavior (e.g. increase expenditure) in response to the reform even before other parties decide to run for election. Alternatively, the threat of entry could trigger a change in incumbent behavior only if additional parties actually compete by running for election due to the reform. However, looking again at figure 3.5, it is evident that it is unlikely that the number of competing parties is responsible for the initial increase in expenditure since it is not before the second post-reform election that significantly increases. The number of parties represented, on the other hand, increases already at the first post-reform election.

[28] In contrast to the analysis of political effects, I use annual observations since data on expenditure is available for each year. In *sample 2*, where clustering at the state-year level is feasible, doing so leaves the estimate for large municipalities significant at the 5% level while the estimate for small municipalities turns insignificant. Without linear trends, estimates remain significant and point estimates change only slightly. These results are available from the author upon request.

If one is willing to make the assumption that the exclusion restriction holds, i.e. that the reform affects expenditure only via the number of parties represented on a council, one can readily use the electoral reform as an instrument: the Wald (1940) estimator implies that *one* additional party represented on a municipal council leads to an increase in expenditure of $\frac{2.7}{0.25} = 10.8$ percent (using *sample 2* for the expenditure effect).[29] Using *sample 1* reduces the effect to 6.4 percent. Even if it is indeed the number of parties represented that is responsible for the increase in expenditure, one can still not be certain that the underlying cause is a common pool problem. Only then would it be possible to conclude that the threshold removal exacerbates inefficiency of the type found in such models.

For robustness, I explore the same alternative definitions for size groups as above (see columns 1 and 2 of table 3.6), which does not matter for the results. I also conduct the same analysis using only states that experience a reform at some point, which again does not alter the results (columns 3 of table 3.6). Further distinguishing between large and very large municipalities (having more than 200 000 inhabitants) reveals that the effects are particularly large at 15 % in the latter municipalities, mirroring the political results. However, significance is lost which is likely due to the fact that there are only few observations for this group, as mentioned above.

Table C.5 presents estimates when one of the treatment states is dropped from the sample. Throughout, the effect is higher for large municipalities and dropping states does not change the magnitude of estimates substantially, the only exception being Mecklenburg-Vorpommern (even then, the effect for large municipalities remains significant). Next, I look at log revenue (which includes deficits, to be explored separately below) for consistency and find effects that are nearly identical as those for expenditure (see table C.3). I then explore the effects on different categories of expenditure in table C.6. These results should be interpreted with caution, however: apart from investment spending (in column 3), increases in the other categories (spending on staff and material) seem to be larger in small municipalities.[30] This finding is however consistent with Jarman (2016), who finds that it is investment spending that reacts when one additional party enters a council.

If spending increases, an important question is which source of revenue increases accordingly. One possibility is that parties find it hard to agree on raising tax multipliers[31] and instead postpone consolidation by borrowing. This would imply that threshold removals contribute to higher overall government budget deficits.

In recent years, acquiring loans has become easier for municipalities (Deutsche Bundesbank, 2016). In principle, budgetary regulations allow loans only for investment expenditure and only if loans are small relative to expected future revenue. However, these regulations were

[29] In my application, the Wald estimator is simply the ratio of the expenditure effect and the number-of-parties effect.

[30] Investment spending and spending on staff each amount to 17% of overall expenditure on average, while spending on material accounts for 15%.

[31] As for the local business tax, tax competition among municipalities further complicates revenue raising.

Table 3.6: Difference-in-differences estimates: log expenditure (robustness checks)

	(1)	(2)	(3)
pop$<$ 7000 $\times$ reform	0.026***		
	(0.006)		
pop $\geq$ 7000 $\times$ reform	0.059***		
	(0.014)		
pop $<$ 15000 $\times$ reform		0.020***	
		(0.005)	
pop $\geq$ 15000 $\times$ reform		0.083***	
		(0.022)	
small $\times$ reform			0.027***
			(0.005)
large $\times$ reform			0.077***
			(0.023)
Size-Year-FE	yes	yes	yes
Linear state trends by size	yes	yes	yes
Never treated excluded			yes
Observations	173591	173591	91623
Adjusted R^2	0.089	0.096	0.087

Notes: Standard errors in parentheses (* $p < .1$, ** $p < .05$, *** $p < .01$). Sample: *sample 2*. Small municipalities: 10 000 inhabitants or fewer. Clustering of standard errors is at the municipality level.

Table 3.7: Difference-in-differences estimates: log expenditure (different size groups)

	(1)
pop$<$ 10000 $\times$ reform	0.022***
	(0.006)
10000 $\leq$pop$<$ 200000 $\times$ reform	0.061***
	(0.016)
pop $\geq$ 200000 $\times$ reform	0.153
	(0.171)
Size-Year-FE	yes
Linear state trends by size	yes
Observations	173591
Adjusted R^2	0.092

Notes: Standard errors in parentheses (* $p < .1$, ** $p < .05$, *** $p < .01$). Sample: *sample 2*. Small municipalities: 10 000 inhabitants or fewer. Clustering of standard errors is at the municipality level.

apparently not enforced in recent years such that municipalities were able to run budget deficits on a regular basis. Nevertheless, the majority of municipalities runs a balanced budget.[32]

The empirical evidence is only weakly in favor of higher borrowing. While table 3.8 reveals that debt financing increased in reformed municipalities and more strongly so in larger ones, three qualifications need to be made: the effect for large municipalities is insignificant and only slightly larger than the one for smaller municipalities even though the increase in expenditure is rather low in the latter group. Furthermore, the number of observations is drastically lower since there are many zero-entries in the data, reflecting the fact that many municipalities do not run deficits on a regular basis.

Table 3.8: Difference-in-differences estimates: log credit revenue

	(1)	(2)
reform	0.057**	
	(0.028)	
small × reform		0.051*
		(0.030)
large × reform		0.073
		(0.063)
Size-Year-FE	yes	yes
Linear state trends by size	yes	yes
Observations	75537	75537
Adjusted R^2	0.034	0.034

Notes: Standard errors in parentheses (* $p < .1$, ** $p < .05$, *** $p < .01$). Sample: *sample 2*. Small municipalities: 10 000 inhabitants or fewer. Clustering of standard errors is at the municipality level.

Looking at revenue sources other than borrowing, table C.4 confirms that the role of credit is limited. Revenue from other sources increases in line with expenditure. This result is another piece of evidence in support of no paralysation since councils need to reach agreement e.g. to raise tax multipliers or fees.

3.6.3 Survey evidence from the German Internet Panel

What do voters know about electoral thresholds and how would they behave if the threshold at the federal level were abolished? Only survey data can help to answer these questions. By finding an answer to the first question, one can obtain suggestive evidence on whether behavioral effects due to the reform on the part of voters are likely to be large. The second question relates to external validity of the results at other levels of government. I have asked a random sample of the German population about their voting behavior as part of the

[32] Deficits make up only 4 % of overall revenues on average.

German Internet Panel (GIP)'s wave 30 (Blom et al., 2018). The GIP generates longitudinal survey data on political and economic topics based on a probability sample of the German population (Blom et al., 2015). The current waves' sample size is varying but approximately includes 3000 participants.[33]

The topic of electoral thresholds had already been featured in wave 3 of the GIP (Blom et al., 2016), when participants were asked about the five percent threshold in place at federal elections in Germany ("What vote share of second votes does a party require overall in order to be able to obtain a seat in parliament?").[34] The results reveal that a substantial share of the population is unaware of this electoral institution: 53% of participants answered correctly, 40% answered "Don't know." and the remainder answered incorrectly.

Given this rather low number of correct answers, it seems even more questionable that many voters are informed about thresholds at the local level since municipal elections receive much less attention than federal elections. Figure 3.7 shows the fraction of survey respondents by state who believe that there is a threshold in place in their state of residence.[35]. In all states, more than 50% of respondents think that a threshold exists while in fact there is no threshold in place in any state (with the exception of city states). The case of North Rhine-Westphalia is quite interesting: the state saw an abolishment in 1999, but a 2.5% threshold was introduced in 2016 (Landtag Nordrhein-Westfalen, 2016). Consistent with this re-introduction, the percentage of the response "yes" is visibly higher than in other states.

What matters for the existence of behavioral effects on the part of voters is whether people *learned* about the change in thresholds in affected states. Since we do not know what respondents would have answered before the reforms took place, the only statement that can be made is that *less* than 50% of respondents could have learned about the change in thresholds in affected states (because the majority in each state still thinks a threshold is in place).

Can the behavioral effects on the part of voters be expected to vary by municipality size? This would require voter knowledge about implicit thresholds. In order to test whether voters are uninformed about implicit thresholds, a question about the 2014 election in Germany for the European Parliament was implemented. Respondents were asked to guess the implicit threshold for this election, which took place without an explicit threshold in place. Given that there were close to 100 seats available for Germany, the implicit threshold was approximately equal to 0.5% of all votes.[36] More precisely, the party "Pirates" obtained a seat with 0.6% of votes. Figure C.2 reveals that the vast majority did not answer correctly. Therefore, if

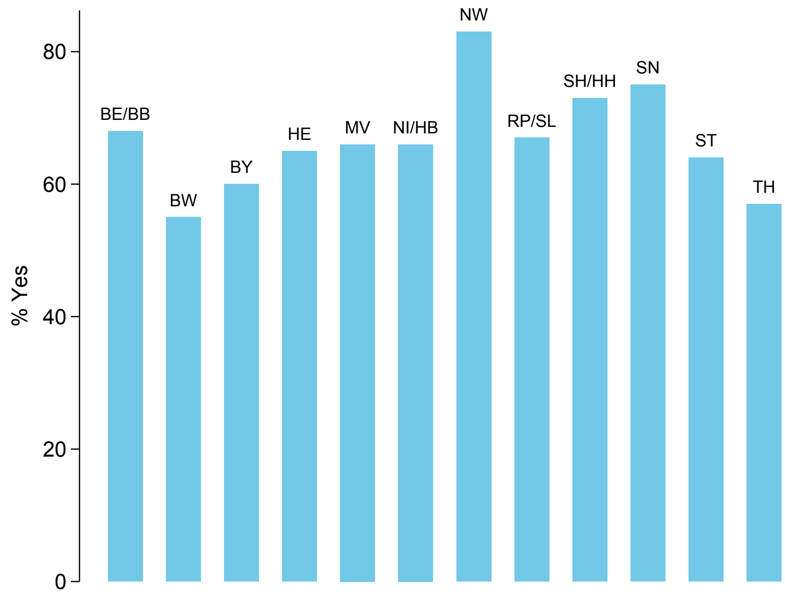
[33] See http://reforms.uni-mannheim.de/internet_panel/Methodology/, retrieved August 21, 2018.

[34] Note that each voter has the opportunity to vote both for a party (the "second vote") and, independently, for a candidate in the voter's district at federal elections in Germany.

[35] Results for small states are reported in conjunction with another state such that these figures are hard to interpret. This is true in particular for the city states Hamburg (HH), Bremen (HB) and Berlin (BE).

[36] Recall the formula from above, $\frac{1}{2 \times seats}$.

Figure 3.7: Is there a threshold at the municipal level in your state of residence?



Source: Wave 30 of the German Internet Panel (Blom et al., 2018)

behavioral effects vary by municipality size, as claimed by Baskaran and da Fonseca (2016b), this must be due to parties behaving differently in municipalities of different size.

The question of external validity is of course crucial since many national parliaments have more than 100 seats which means that implicit thresholds are quite low and therefore the number of parties may increase significantly if explicit thresholds were removed.[37] It is quite likely that salience at the federal level would be higher than at the local level such that behavioral effects could be larger. Stakes are higher at the federal level, such that parties might also react differently. On the other hand, voters might be less willing to experiment at the federal level, knowing that small parties entering a national parliament can have a larger impact than at the local level (e.g. in terms of legislation). Figure C.1 presents answers to the question of whether respondents would have behaved differently in the last federal election if it had taken place without an explicit threshold. Clearly, this is not the case for almost all respondents.

3.7 Conclusions

This paper estimates the causal effect of the removal of an electoral threshold on political and economic outcomes. The findings can be summarized as follows: both the number of parties competing and the number of parties represented on municipal councils significantly increase in response to an abolishment. The effect is significantly more pronounced in large municipalities, which can be explained by differing implicit thresholds. Municipal

[37] As for the effects on expenditure, it is quite likely that the common pool problem exists at the federal level, too, since the federal government is typically composed of more than one party.

expenditure increases, consistent with a common pool problem. Survey results reveal limited voter knowledge about thresholds in general and about municipal thresholds in particular, suggesting a limited role of behavioral effects.

Future research may clarify the exact size of behavioral effects, could analyze the role of changing party behavior versus voter behavior due to the reforms and could explore additional policy outcomes. As for the effect on spending, it would be interesting to know which exact channel is at play. In addition, the threshold removals used for identification could serve as an instrument for other political variables, e.g. to explore the impact of council representation of extremist parties on policy outcomes.

Appendix A

Appendix to chapter 1

A.1 Theoretical details

A.1.1 Derivation of efficient equalization payments

Planner's problem

Solving the planner's problem amounts to maximizing worker utility in a given location under the constraint of equal utility across locations (and resource, consumption, technology and non-negativity constraints). As in Albouy (2012), the planner's choice for each region j consists of the tuple $\left(N^j, N_X^j, N_Y^j, N_G^j, K_X^j, K_Y^j, K_G^j, G^j, x^j, y^j\right)$ to solve

$$\max \quad U\left(x^1, y^1, \frac{G^1}{(N^1)^\omega}, Q^1\right) \quad s.t. \quad U\left(x^j, y^j, \frac{G^j}{(N^j)^\omega}, Q^j\right) \geq U\left(x^1, y^1, \frac{G^1}{(N^1)^\omega}, Q^1\right) \quad \forall j$$

Defining $\psi^1 := 1 - \sum_{j \neq 1} \psi^j$, the corresponding Lagrangian L can be written as

$$\begin{aligned}
L = &\sum_j \psi^j U\left(x^j, y^j, \frac{G^j}{(N^j)^\omega}, Q^j\right) + \pi_X\left(\sum_j F_X\left(K_X^j, L_X^j, N_X^j, A_X\left(N_X^j\right)\right) - \sum_j N^j x^j\right) \\
&+ \sum_j \pi_Y^j\left(F_Y\left(K_Y^j, L_Y^j, N_Y^j, A_Y^j\right) - N^j y^j\right) + \sum_j \pi_G^j\left(F_G\left(K_G^j, L_G^j, N_G^j, A_G^j\right) - G^j\right) \\
&+ \sum_j \pi_K^j\left(K^j - K_X^j - K_Y^j - K_G^j\right) + \kappa\left(\bar{K} - \sum_j K^j\right) \\
&+ \sum_j \pi_N^j\left(N^j - N_X^j - N_Y^j - N_G^j\right) + \nu\left(\bar{N} - \sum_j N^j\right)
\end{aligned}$$

where the notation of production functions for housing and the publicly provided good (F_Y and F_G, respectively) is analogous to the tradable good except that productivity is exogenous.

$\bar{K}$ and $\bar{N}$ represent the fixed quantities of capital and labor at the national level and the non-negativity constraints have been omitted.

Substitution of Karush-Kuhn-Tucker necessary first order conditions implies that $\frac{\partial F_X^j}{\partial K_X} = \frac{\partial F_X^{j'}}{\partial K_X}$ must hold for any two regions j and j' at an interior solution.[1] Existence of an interior solution and sufficiency are discussed below for particular functional form assumptions. The focus of this paper is on the condition with respect to labor in region j, which reads as follows:

$$\frac{\partial F_X^j}{\partial N_X} + \frac{\partial F_X^j}{\partial A_X}\frac{\partial A_X^j}{\partial N_X} - x^j - MRT_{YX}^j y^j - \omega MRT_{GX}^j \frac{G^j}{N^j} = \nu \tag{A.1}$$

where ν is a Lagrange multiplier constant across regions. This means that the number of workers in a location should be increased if productivity (including the indirect effect via agglomeration) is high relative to the cost of consumption and congestion of the publicly provided good. Regions that offer higher quality of life (as reflected in amenities) exhibit lower resource cost as they require e.g. a lower level of housing consumption for a given level of utility equalized across locations.

Budget constraints

The workers' budget constraint reads as follows:

$$x^j + p_Y^j y^j + \tau_w^F w^j = w^j + F^j \tag{A.2}$$

where p_Y is the price of housing. Local governments finance the publicly provided good by taxing the returns to land and capital:

$$p_G^j G^j = \tau_L^j r^j L^j + \tau_K^j i K^j \tag{A.3}$$

The federal government pays for equalization payments using its wage income tax revenues:

$$\sum_j N^j F^j = \sum_j N^j \left(\tau_w^F w^j\right) \tag{A.4}$$

The market solution

Imposing market conditions, i.e. $w^j = \frac{\partial F_X^j}{\partial N_X}$ and $p_Y^j = MRT_{YX}^j$ on equation (A.1) and using the definition $p_G := MRT_{GX}^j$ yields

$$w^j + \frac{\partial F_X^j}{\partial A_X}\frac{\partial A_X^j}{\partial N_X} - x^j - p_Y^j y^j - \omega p_G^j \frac{G^j}{N^j} = \nu \tag{A.5}$$

[1] The FOC condition for capital calls for capital tax harmonization. If taxes on capital differ across regions, satisfying the condition with respect to labor does not lead to the second-best except if by chance.

which then gives equation (1.1) after making use of equations (A.2), (A.3) and (A.4) and solving for equalization payments. Uniqueness of the market solution is discussed below for particular functional form assumptions.

With the price i of capital determined by the fixed supply of capital at the level, Euler's equation implies that the local price of land used in the production of tradable goods r_X^j is

$$r_X^j = \frac{X^j - K_X^j i - N_X^j w^j}{L_X^j} \tag{A.6}$$

which makes land owners the residual claimants.

A.1.2 The model for a particular functional form assumption

For illustration, I assume that utility and production in each sector obey a Cobb-Douglas functional form. Doing so allows me to establish sufficiency of the KKT conditions, existence of an interior solution and uniqueness of the market outcome. Furthermore, functional form assumptions are required for the estimation of agglomeration elasticities.

Local labor demand

Production of tradables I assume production of the tradable good as $F_X(.) = A_X^j \left(K_X^j\right)^{\alpha} \left(L_X^j\right)^{\beta} \left(N_X^j\right)^{1-\alpha-\beta}$. When firms take regional population as given, first order conditions imply the log local labor demand curve

$$\ln w^j = \frac{1}{1-\alpha} \ln A_X^j - \frac{\alpha}{1-\alpha} \ln i^j + \frac{\beta}{1-\alpha} \ln L_X^j - \frac{\beta}{1-\alpha} \ln N_X^j + \frac{\alpha}{1-\alpha} \ln \alpha + \ln (1-\alpha-\beta) \tag{A.7}$$

where the first order condition for capital has been substituted.

Equation (1.4) follows directly from the definition of σ^j and the fact that the wage in a region is proportional to average output per worker:

$$\frac{dF_X^j}{dN_X} = \frac{F_X^j}{N_X}\left(1-\alpha-\beta+\sigma^j\right) = w^j + \frac{w^j}{1-\alpha-\beta}\sigma^j \tag{A.8}$$

Assumption on the agglomeration function If the agglomeration function is log-linear (as suggested by the results in Kline and Moretti (2014a)), i.e.

$$g(.) = \sigma \ln \frac{N_X}{R} + c \tag{A.9}$$

where σ is the constant agglomeration elasticity, we have $\frac{\partial \ln w^j}{\partial \ln N_X^j} = \frac{\sigma-\beta}{1-\alpha}$. If, in addition, $\sigma < \beta$ (also suggested by the results in Kline and Moretti (2014a)), the labor demand curve is downward-sloping. Intuitively, this assumption restricts the magnitude of agglomeration economies such that the crowding of the fixed factor land outweighs the productivity gain of adding an additional worker. Notice that when estimating agglomeration elasticities, I do not restrict the functional form or the elasticities. The empirical evidence reported in section 1.6 supports the assumption of log-linearity and $\sigma < \beta$.

Overall local labor demand Cobb-Douglas production of housing and the publicly provided good is analogous to the tradable good except that productivity is exogenous. This implies downward sloping labor demand as in equation (A.7) in both sectors. Summing labor demand across sectors yields overall downward-sloping labor demand.

Local labor supply

For $U\left(x, y, g^j, Q^j\right) = Q^j \left(g^j\right)^{\tilde{\gamma}} (y)^{\tilde{\alpha}} (x)^{\tilde{\beta}}$ with $\tilde{\alpha} + \tilde{\beta} + \tilde{\gamma} = 1$, agents solve (in a given location)

$$\max_{x,y} \quad \ln Q + \tilde{\gamma} \ln g + \tilde{\alpha} \ln y + \tilde{\beta} \ln x \quad s.t. \quad p_Y y + x = w(1 - \tau_w^F) + F =: \tilde{w},$$

where $\tilde{w}$ denotes income after taxes and transfers. This yields demand proportional to income: $x = \frac{\tilde{\beta}}{\tilde{\alpha}+\tilde{\beta}} \tilde{w}$ and $y = \frac{\tilde{\alpha}}{(\tilde{\alpha}+\tilde{\beta})} \frac{\tilde{w}}{r}$. Regional log housing demand then reads

$$\ln H^d = \ln N + \ln \tilde{w} - \ln p_Y + c_1$$

for some constant c_1.

Production of housing is analogous to production of the tradable good (with land being a fixed factor) but with exogenous productivity: $F_Y(.) = A_Y^j \left(K_Y^j\right)^{\alpha^y} \left(L_Y^j\right)^{\beta^y} \left(N_Y^j\right)^{1-\alpha^y-\beta^y}$. The implied log housing supply is a linear function of the log wage and price:

$$\ln H^s = \frac{1-\beta^y}{\beta^y} \ln p_Y + \frac{\alpha^y + \beta^y - 1}{\beta^y} \ln w + c_2$$

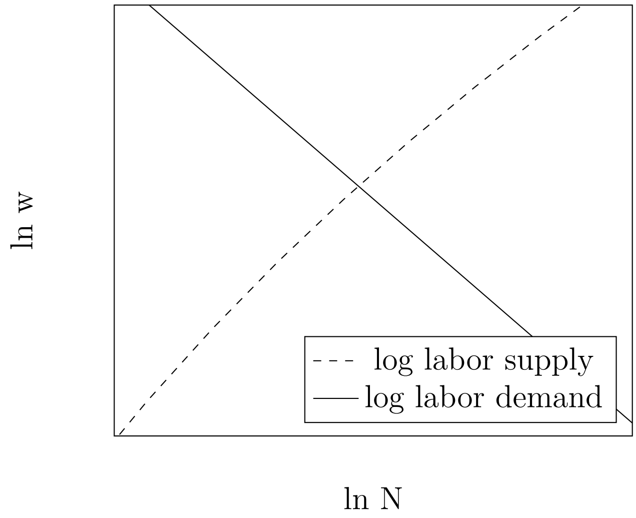
Solving for effective local labor supply amounts to solving for local housing market equilibria, i.e. imposing

$$\ln N + \ln \tilde{w} - \ln p_Y + c_1 = \frac{1-\beta^y}{\beta^y} \ln p_Y + \frac{\alpha^y + \beta^y - 1}{\beta^y} \ln w + c_2$$

which yields $\ln p_Y = (1 - \alpha^y - \beta^y) \ln w + \beta^y \ln \tilde{w} + \beta^y \ln N + c_3$

Equilibrium across locations requires $\ln \bar{U} = \ln U^j \quad \forall j$ such that, using $g = \frac{G}{N^\omega}$, local labor supply N^j is determined by

Figure A.1: Local labor market equilibrium



$$\ln \bar{U} = \ln Q^j + \tilde{\gamma} \ln G^j - (\omega\tilde{\gamma} + \tilde{\alpha}\beta^y) \ln N^j + (\tilde{\alpha} + \tilde{\beta} - \tilde{\alpha}\beta^y) \ln(w^j - T^j + F^j)$$
$$- (\tilde{\alpha}(1 - \alpha^y - \beta^y)) \ln w^j + c_4$$

Upward-sloping local labor supply is obtained if $(\tilde{\alpha}+\tilde{\beta}-\tilde{\alpha}\beta^y)w^j > \tilde{\alpha}(1-\alpha^y-\beta^y)(w^j-T^j+F^j)$. Intuitively, this requires that an increase in local wages makes local agents better off, i.e. the increase in the cost of housing due to higher local wages is relatively less important than the increase in consumption. Notice that this condition is satisfied in particular if $T^j \geq F^j$.[2] The descriptive statistics reveal that in Germany, wage income tax payments due to differing wage premia are considerably larger than per-capita equalization payments. Finally, the condition $\sum_j N^j = 1$ determines $\bar{U}$. A higher level of $\bar{U}$ causes an inward shift of local labor supply in all regions.

Uniqueness of the market solution

Downward-sloping local labor demand and upward-sloping local labor supply imply uniqueness of the local labor market equilibrium, as illustrated in figure A.1.

Existence of an interior solution

The first order condition necessary as written in equation (A.1) is valid only at an interior solution. It is not clear that Inada conditions can be assumed to hold for the production

[2] Also note that incorporating housing in the model is not essential for the main result of this paper. Without housing, upward-sloping labor supply obtains due to the congestion of the publicly provided good if $\omega > 0$.

function in the presence of agglomeration economies in general. Given that I assume log-linearity of the agglomeration function as in equation (A.9) and $\sigma < \beta$ however, an interior solution obtains.

First consider what happens when N_X^j approaches infinity. Then, the first two terms on the left hand side of equation (A.1), i.e. the entire marginal product of labor decrease. To see why this is the case, note that $\frac{\partial F_X^j}{\partial N_X} = w^j$ and that $\frac{\partial \ln w^j}{\partial \ln N_X^j} = \frac{\sigma-\beta}{1-\alpha} < 0$. The second term on the left hand side of equation (A.1) is a multiple of the first term according to equation (1.4). The resource cost of housing is increasing in N^j. I assume that it is not optimal for regions to be empty, i.e. that the last term of equation (A.1) relating to the congestion of the publicly provided good does not dominate as N^j approaches zero (N^j can approach zero if and only if N_X^j, N_Y^j and N_G^j all approach zero).

Sufficiency

With a strictly quasi-concave utility function such as the one chosen above, the KKT conditions are sufficient for a unique global maximum if the constraint functions satisfy quasi-concavity (Sundaram, 1996). Cobb-Douglas production of the housing good and the publicly provided good exhibiting decreasing returns (due to the fixed factor) directly yields concavity since productivity is exogenous. Concavity of the constraint function relating to production of tradables, $\left(\sum_j F_X\left(K_X^j, L_X^j, N_X^j, A_X\left(N_X^j\right)\right) - \sum_j N^j x^j\right)$, is less straightforward due to endogenous productivity. However, with Cobb-Douglas production, provided that the agglomeration function fulfills equation (A.9) and $\sigma < \beta$, concavity is immediate since the exponents of the variable factors then add up to less than one.

Production in the dynamic model

In the dynamic version of the model, production of tradable goods in decade t occurs according to $F_{X,t}(.) = A_{X,t}^j \left(K_{X,t}^j\right)^\alpha \left(L_{X,t}^j\right)^\beta \left(N_{X,t}^j\right)^{1-\alpha-\beta}$. Agglomeration is assumed to operate with a decadal lag, i.e.

$$\ln A_{X,t}^j = g\left(\frac{N_{X,t-1}^j}{R^j}\right) + \nu^j + \phi_t + \epsilon_t^j \tag{A.10}$$

where ϕ_t is a period fixed-effect (equal across locations) and ν_j represents time-constant region-specific productivity. The error ϵ_t^j is a shock to productivity in region j and decade t, e.g. due to changes in technology.

A.1.3 Efficient equalization payments

If equalization payments were re-directed, wages would change, affecting efficient equalization payments derived from equation (1.1).[3] In particular, if equalization payments were increased in high-wage regions (e.g. in West Germany), crowding of the fixed factor would cause wages to decline which in turn would decrease the portion of optimal equalization payments due to wage income tax payments. I approximate efficient equalization payments not conditional on wages by deriving a first-order Taylor series approximation to conditional efficient payments and then looking for a fixed point.

In a first step I simplify by assuming that $F^{j\star} = w^j$ based on the estimate that the agglomeration effect can be written as $1.02w^j$ and $\tau = 0.25$. Using observed wages and equalization payments $({w^j}^0, {F^j}^0)$, we then have that for alternative equalization payments F^j

$$F^{j\star} \approx {w^j}^0 + \frac{\partial w^j}{\partial F^j}({F^j}^0) \cdot (F^j - {F^j}^0) \tag{A.11}$$

where I then use labor demand in the tradable sector to approximate $\frac{\partial w^j}{\partial F^j}$. This gives $\frac{\partial w^j}{\partial F^j} = w^j \frac{\sigma-\beta}{1-\alpha}\frac{\epsilon}{\bar{m}}$. Actually, overall labor demand is the object of interest here. However, it is for labor demand in the tradable sector that I have obtained estimates above.[4]

Imposing $F^{j\star} = F^j$ and making use of calibrated and estimated parameters[5] of the estimate for ϵ as in section 1.7 and $\bar{m} = 22000$, I obtain

$$F^{j\star} \approx \frac{22000{w^j}^0 + 0.2{w^j}^0{F^j}^0}{22000 + 0.2{w^j}^0} \tag{A.12}$$

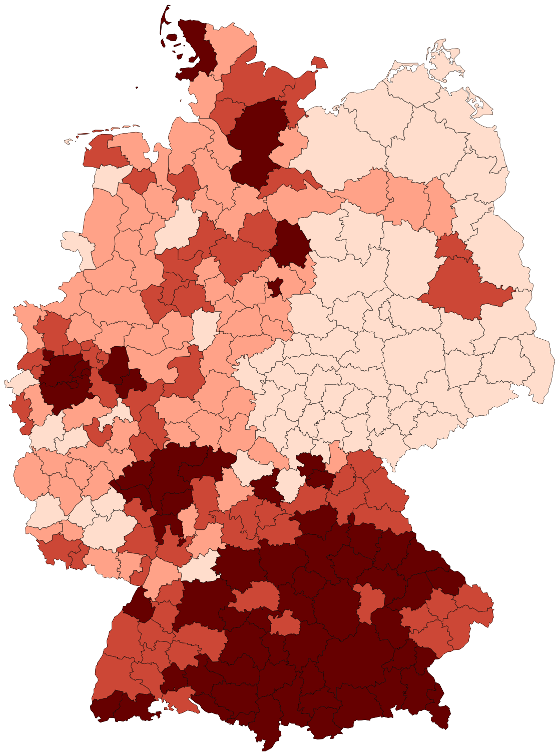
Approximated optimal payments are shown in figure (A.2). Compared to figure (1.4), the general pattern remains unchanged but there is a noticeable change in magnitudes of payments: As expected, the variance of optimal payments declines since an inflow of workers into high-wage regions leads to lower wages there and the opposite happens in low-wage regions (the approximation $F^{j\star} = w^j$ further lowers the variance of optimal payments).

[3] In addition, the portion of efficient equalization payments due to source-based taxation would change too, but I ignore this effect since deviations in source-based tax revenues are small compared to wage premia differentials as shown above.

[4] In a robustness check, I set $\sigma = 0$ to simulate the reaction of overall labor demand and the results change only marginally.

[5] $\hat{\sigma} = 0.24$, $\beta = 0.47$ and $\alpha = 0.3$.

Figure A.2: Efficient equalization payments taking into account wage adjustment (year 2005)



Notes: This map shows efficient equalization payments that take into account the wage adjustment in Euro per capita relative to the population weighted mean for each labor market region. The distribution is partitioned into four intervals containing an equal number of observations. Darker colors indicate higher values.

A.1.4 Equalization payments to local governments instead of workers

If $\omega = 1$, optimal payments in equation (1.1) remain unchanged if they flow to local governments instead of workers. This can be seen from the amended local government's budget constraint: $p_G^j G^j = \tau_L^j r^j L^j + \tau_K^j i K^j + N^j F^j$.

A.1.5 Tax-sharing as residence-based taxation

Above, I treat municipal revenues from tax-sharing as equalization payments and thereby implicitly assume that the sharing agreement can be controlled by the federal government. If the revenue-sharing system in place cannot be altered, the condition for efficient payments $\hat{F}^j$ is now

$$\hat{F}^{j\star} = \tau_w^F w^j - \omega \frac{S^j + \tau_L^j r^j L^j + \tau_K^j i K^j}{N^j} + \frac{\partial F_X^j}{\partial A_X} \frac{\partial A_X^j}{\partial N_X} + \hat{F} \qquad \text{(A.13)}$$

where S^j is the revenue sharing payment to region j, paid for by federal government revenue. If $\omega = 1$, the NFB doesn't change.

Shared revenues from wage income taxation, to the extent that they accrue to municipalities, can approximately be treated as residence-based taxation. The reason is that the fraction a municipality receives from the overall income tax revenue share that all municipalities receive is determined to a certain extent based upon total earnings in a municipality.[6] Then we have

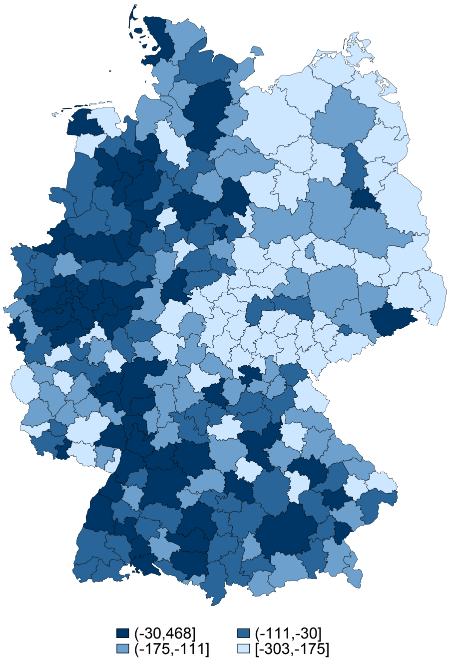
$$\hat{F^j}^{\star} = (\hat{\tau_w^F} + \tau_w^S) w^j - \omega \frac{\tau_w^S w^j N^j + \tau_L^j r^j L^j + \tau_K^j i K^j}{N^j} + \frac{\partial F_X^j}{\partial A_X} \frac{\partial A_X^j}{\partial N_X} + \hat{F} \tag{A.14}$$

where τ_w^S is the location-independent income tax rate that is implicitly due to tax sharing and $\hat{\tau_w^F} := \tau_w^F - \tau_w^S$. Note that the residence-based revenue term cancels if $\omega = 1$. Intuitively, residence-based taxes do not require correction in this case since the corresponding distortion is exactly offset by the congestion term that arises due to the contribution to local government expenditures.

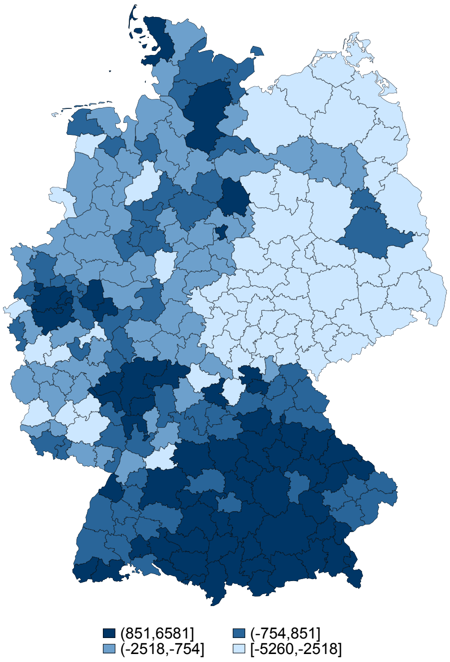
[6] In fact, revenue is first allocated to groups of municipalities that belong to the same state and then the income generated in a municipality is taken into account up to a threshold, which has an equalizing effect among municipalities in a given state. For details, see http://www.bundesfinanzministerium.de/Content/DE/Standardartikel/Themen/Oeffentliche_Finanzen/Foederale_Finanzbeziehungen/Kommunalfinanzen/der-gemeindeanteil-an-der-einkommensteuer-in-der-gemeindefinanzreform.html, retrieved February 1, 2018.

A.2 Additional empirical results

Figure A.3: Components of equation (1.2)



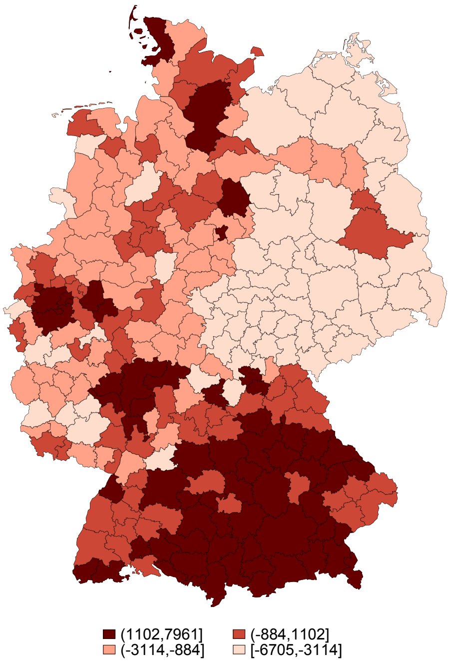
(a) Source based tax revenues (year 2005)



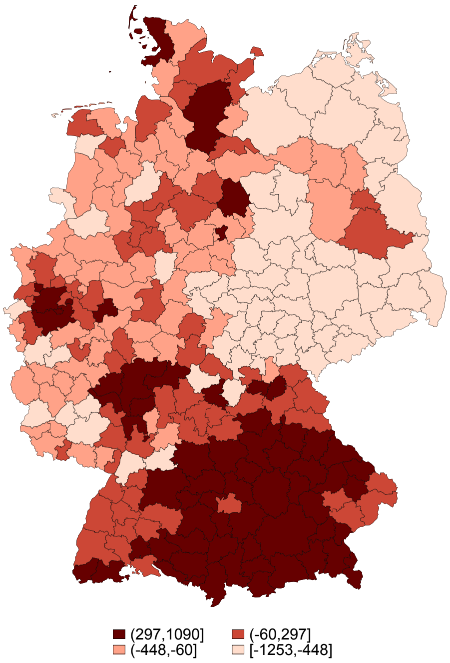
(b) Agglomeration effects

Notes: Map (a) shows source based tax revenues in Euro per capita relative to the population weighted mean for each labor market region. Map (b) shows agglomeration effects relative to the population weighted mean for each labor market region. The distributions are partitioned into four intervals containing an equal number of observations. Darker colors indicate higher values.

Figure A.4: Efficient equalization payments (year 2005)...



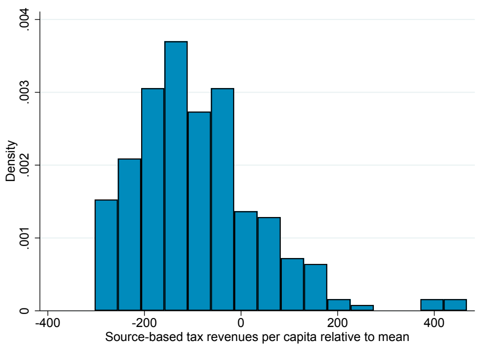
(a) ...when $\omega = 0$



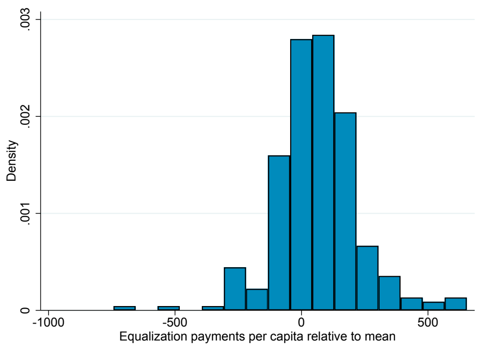
(b) ... when agglomeration is ignored

Notes: Map (a) shows efficient equalization payments when $\omega = 0$ in Euro per capita relative to the population weighted mean for each labor market region. Map (b) shows efficient equalization payments when agglomeration effects are ignored in Euro per capita relative to the population weighted mean for each labor market region. The distributions are partitioned into four intervals containing an equal number of observations. Darker colors indicate higher values.

Figure A.5: Histograms (year 2005)...



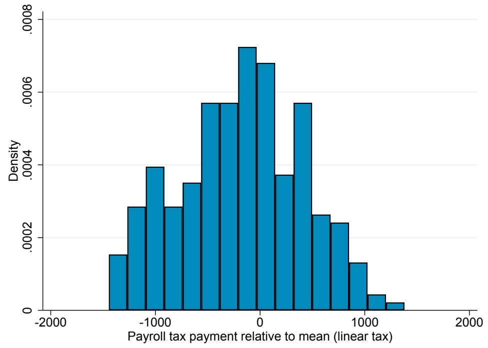
(a) ...of source-based tax revenues



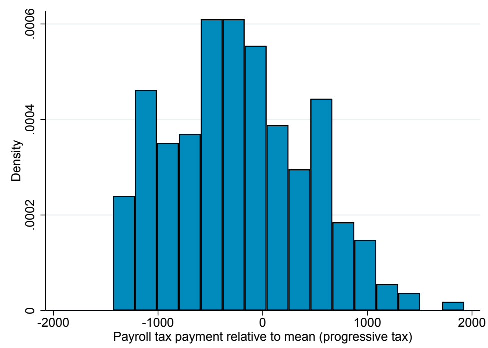
(b) ...of equalization payments

Notes: The amounts are in Euro per capita relative to the population weighted mean.

Figure A.6: Histograms of wage income tax payments (year 2005)



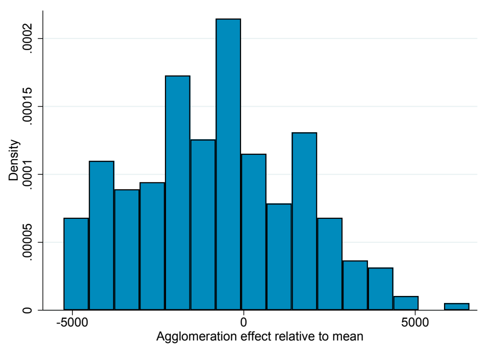
(a) Linear tax rate



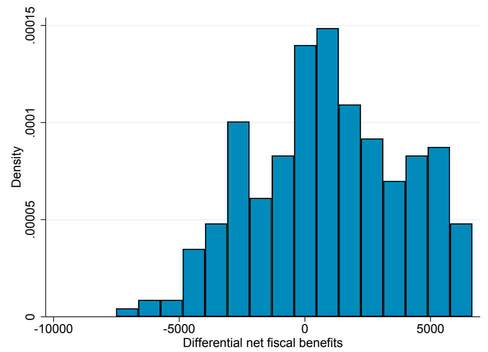
(b) Progressive tax

Notes: The amounts are in Euro relative to the population weighted mean.

Figure A.7: Histograms (year 2005)...



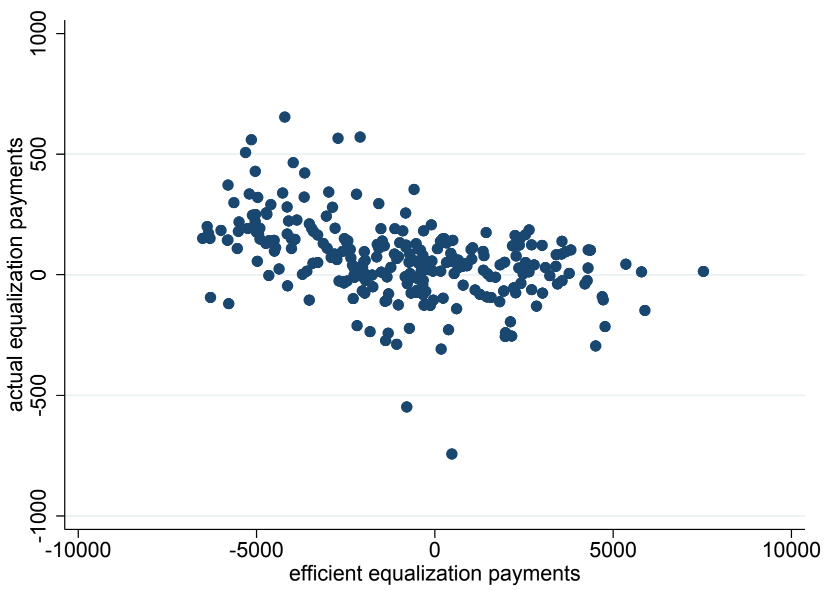
(a) ... of agglomeration effects



(b) ...of net fiscal benefits

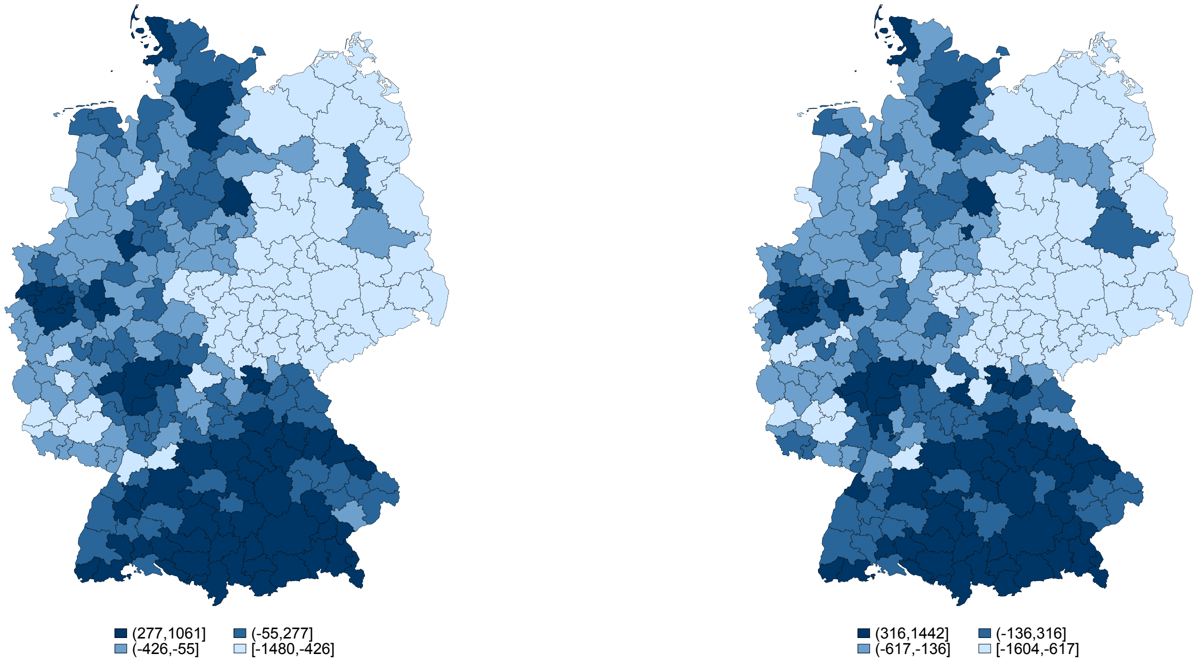
Notes: The amounts are in Euro relative to the population weighted mean.

Figure A.8: Efficient and actual equalization payments (year 2005), all observations



Notes: The amounts are in Euro per capita relative to the population weighted mean.

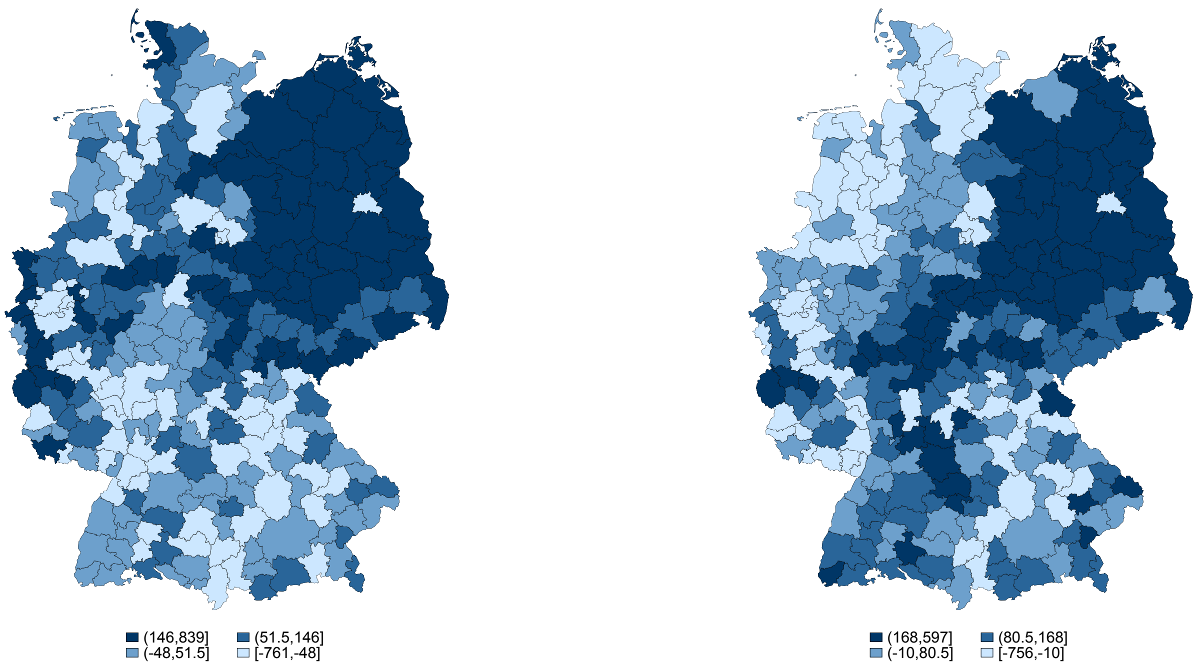
Figure A.9: Average wage income tax payments for different time periods



(a) Years 1993-2000 **(b)** Years 2000-2010

Notes: These maps shows wage income tax payments relative to the population weighted mean for each labor market region. The distributions are partitioned into four intervals containing an equal number of observations. Darker colors indicate higher values.

Figure A.10: Average equalization payments for different time periods



(a) Years 1995-2000 **(b)** Years 2000-2010

Notes: These maps show equalization payments in Euro per capita relative to the population weighted mean for each labor market region. The distributions are partitioned into four intervals containing an equal number of observations. Darker colors indicate higher values.

Table A.1: Estimates of log labor market region wage premia

Model	(1)	(2)	(3)	(4)
Heide	-0.242***	-0.146***	-0.119***	-0.155***
Itzehoe	-0.130***	-0.0228	-0.0544**	-0.0291
Flensburg	-0.188***	-0.0775***	-0.0868***	-0.0821***
Lübeck	-0.152***	-0.00515	-0.0568***	-0.00870
Kiel	-0.146***	-0.0386*	-0.0578***	-0.0403*
Ratzeburg	-0.202***	-0.0542**	-0.0688***	-0.0529*
Hamburg	0.0929***	0.0951***	0.0414**	0.100***
Braunschweig	-0.172***	-0.0358	-0.0894***	-0.0427*
Salzgitter	-0.0236	0.0665**	0.00595	0.0243
Wolfsburg	0.132***	0.0996***	0.0368*	0.0902***
Göttingen	-0.262***	-0.124***	-0.121***	-0.142***
Goslar	-0.251***	-0.0943***	-0.131***	-0.0981***
Helmstedt	-0.244***	-0.142***	-0.0757***	-0.148***
Einbeck	-0.260***	-0.122***	-0.107***	-0.160***
Osterode	-0.222***	-0.115***	-0.122***	-0.136***
Hannover	-0.0686**	0.0398*	-0.0269	0.0387*
Sulingen	-0.300***	-0.0735***	-0.0907***	-0.0803***
Hameln	-0.194***	-0.0299	-0.0874***	-0.0444
Hildesheim	-0.226***	-0.0308	-0.0762***	-0.0545**
Holzminden	-0.216***	-0.0673*	-0.121***	-0.107***
Nienburg	-0.147***	-0.0355	-0.0817***	-0.0489
Stadthagen	-0.260***	-0.109***	-0.0626**	-0.131***
Celle	-0.144***	-0.0574**	-0.0914***	-0.0617**
Lüneburg	-0.138***	-0.0256	-0.0865***	-0.0286
Zeven	-0.246***	-0.0740**	-0.0493**	-0.0837***
Soltau	-0.190***	-0.0451	-0.0579**	-0.0531*
Stade	-0.226***	-0.0472*	-0.0513**	-0.0456
Uelzen	-0.238***	-0.105***	-0.135***	-0.112***
Verden	-0.222***	-0.0434	-0.0614***	-0.0479
Emden	-0.136***	-0.0683**	-0.0756***	-0.0754***
Westerstede	-0.159***	-0.0282	-0.0585**	-0.0247
Oldenburg	-0.185***	-0.0359	-0.0735***	-0.0360
Osnabrück	-0.212***	-0.0750***	-0.0657***	-0.0824***
Wilhelmshaven	-0.195***	-0.0790***	-0.107***	-0.0822***
Cloppenburg	-0.228***	-0.134***	-0.145***	-0.162***
Lingen	-0.239***	-0.127***	-0.114***	-0.148***

Nordhorn	-0.435***	-0.244***	-0.133***	-0.255***
Leer	-0.343***	-0.119***	-0.128***	-0.114***
Vechta	-0.183***	-0.0885***	-0.0603**	-0.111***
Nordenham	-0.218***	-0.0392	-0.0626*	-0.0782**
Bremen	-0.152***	0.0370*	-0.0237	0.0368*
Bremerhaven	-0.180***	-0.0560**	-0.0744***	-0.0602**
Höxter	-0.294***	-0.135***	-0.113***	-0.160***
Düsseldorf	0.0841***	0.103***	0.0513***	0.104***
Duisburg	-0.128***	-0.0124	-0.0403**	-0.0126
Essen	-0.0839***	0.0297	-0.0143	0.0367*
Krefeld	-0.0249	0.0428*	-0.0181	0.0413*
Viersen	-0.112***	-0.00452	0.00316	-0.0167
Mönchengladbach	-0.104***	0.00551	-0.0132	0.00629
Heinsberg	-0.303***	-0.0803***	-0.0408*	-0.0879***
Wuppertal	-0.0231	0.0355	-0.00306	0.0274
Schwelm	-0.115***	0.0196	-0.00873	-0.00612
Remscheid	-0.0199	0.0201	0.00356	0.00783
Kleve	-0.221***	-0.0845***	-0.0639***	-0.0921***
Aachen	-0.160***	0.00434	-0.00801	0.00467
Köln	0.0143	0.0596***	0.0318*	0.0673***
Leverkusen	0.0889***	0.0977***	0.0334	0.0862***
Bonn	-0.166***	-0.108***	0.00311	-0.0954***
Düren	-0.166***	-0.0175	-0.0158	-0.0182
Euskirchen	-0.278***	-0.144***	-0.0575**	-0.165***
Gummersbach	-0.0955***	-0.00449	-0.0125	-0.0109
Gelsenkirchen	-0.200***	-0.0394*	-0.0604***	-0.0395*
Münster	-0.184***	-0.0576**	-0.0529***	-0.0626***
Borken	-0.229***	-0.113***	-0.0952***	-0.129***
Steinfurt	-0.265***	-0.0931***	-0.0724***	-0.103***
Bielefeld	-0.106***	0.0135	-0.0312*	0.00202
Gütersloh	-0.0858***	0.0263	-0.0158	0.00303
Detmold	-0.139***	-0.0285	-0.0431**	-0.0539**
Minden	-0.142***	-0.0197	-0.0435**	-0.0295
Paderborn	-0.201***	-0.0510**	-0.0707***	-0.0665***
Bochum	-0.127***	-0.00508	-0.0290	-0.00891
Dortmund	-0.192***	-0.0493**	-0.0452**	-0.0459**
Hagen	-0.126***	0.0133	-0.0481**	0.00787
Lüdenscheid	-0.0550*	0.0429*	-0.00163	0.00470
Meschede	-0.218***	-0.0240	-0.0387*	-0.0549**

Siegen	-0.161***	-0.0276	-0.0372*	-0.0426*
Olpe	0.0124	0.0164	-0.00832	-0.0153
Soest	-0.179***	-0.0559**	-0.0506**	-0.0773***
Korbach	-0.132***	-0.0522*	-0.0975***	-0.0833***
Kassel	-0.216***	-0.0453*	-0.0567***	-0.0512**
Eschwege	-0.337***	-0.185***	-0.161***	-0.214***
Schwalm-Eder	-0.258***	-0.108***	-0.100***	-0.123***
Hersfeld	-0.167***	-0.147***	-0.144***	-0.154***
Marburg	-0.178***	0.00148	-0.0210	-0.0103
Lauterbach	-0.247***	-0.0842**	-0.0257	-0.104***
Fulda	-0.175***	-0.113***	-0.101***	-0.111***
Wetzlar	-0.156***	-0.0627**	-0.0589***	-0.0771***
Gießen	-0.248***	-0.0389	-0.00562	-0.0450*
Limburg	-0.149***	-0.0454	-0.0555**	-0.0470*
Wiesbaden	0.0414	0.0688***	0.0309*	0.0716***
Frankfurt/Main	0.143***	0.118***	0.0804***	0.120***
Hanau	-0.00379	0.0509**	0.0275	0.0415*
Darmstadt	-0.0261	0.0658***	0.0316*	0.0631***
Erbach	-0.208***	-0.0567	-0.0782***	-0.0671*
Altenkirchen	-0.284***	-0.120***	-0.0184	-0.140***
Montabaur	-0.204***	-0.132***	-0.0528**	-0.151***
Neuwied	-0.131***	-0.0739***	-0.0817***	-0.0994***
Ahrweiler	-0.377***	-0.239***	-0.111***	-0.256***
Koblenz	-0.185***	-0.0815***	-0.0689***	-0.0850***
Bad Kreuznach	-0.286***	-0.0927***	-0.108***	-0.0983***
Idar-Oberstein	-0.295***	-0.157***	-0.142***	-0.170***
Cochem	-0.270***	-0.0975**	-0.0766**	-0.103***
Simmern	-0.199***	-0.135***	-0.119***	-0.150***
Trier	-0.469***	-0.0951***	-0.0583**	-0.100***
Bernkastel-Wittlich	-0.257***	-0.122***	-0.0741***	-0.150***
Daun	-0.236***	-0.180***	-0.0873**	-0.201***
Bitburg	-0.276***	-0.147***	-0.0672**	-0.168***
Kaiserslautern	-0.303***	-0.122***	-0.0669***	-0.125***
Landau	-0.237***	-0.0856***	-0.0455*	-0.0974***
Mainz	-0.0135	0.0114	-0.0183	0.0165
Alzey-Worms	-0.134***	-0.0456*	-0.0465**	-0.0501*
Pirmasens	-0.235***	-0.116***	-0.0914***	-0.136***
Ludwigshafen	-0.100***	-0.0161	-0.0268	-0.0198
Germersheim	-0.152***	-0.0209	0.0295	-0.0487

Merzig	-0.216***	-0.0689**	-0.0442	-0.0829**
St. Wendel	-0.233***	-0.147***	-0.110***	-0.163***
Saarbrücken	-0.162***	-0.0668***	-0.0579***	-0.0695***
Homburg/Saar	-0.0802**	-0.0579**	-0.0582**	-0.0780***
Stuttgart	0.0552**	0.107***	0.0786***	0.0909***
Göppingen	-0.00978	0.0439*	0.0210	0.0232
Heilbronn	-0.305***	-0.171***	0.0331*	-0.171***
Schwäbisch Hall	-0.0182	0.00351	0.00999	-0.0199
Tauberbischofsheim	-0.127***	-0.0427	-0.0171	-0.0789**
Heidenheim	-0.0150	0.0792**	0.0340	0.0444
Aalen	-0.0757**	0.00763	0.00942	-0.0237
Baden-Baden	-0.0480	0.0295	0.0324	-0.00269
Karlsruhe	-0.242***	0.0331	0.0260	0.0273
Heidelberg	-0.0971***	0.0199	0.0231	0.0104
Mannheim	-0.0457*	0.0322	0.0205	0.0270
Mosbach	-0.253***	-0.149***	-0.0643**	-0.168***
Pforzheim	-0.0694**	0.0430*	0.0289	0.0107
Calw	-0.118***	-0.0717**	-0.0182	-0.0890***
Freudenstadt	-0.0750*	-0.0453	-0.00679	-0.0713**
Freiburg	-0.115***	-0.0190	-0.0121	-0.0328
Offenburg	-0.0939***	0.0166	0.0187	-0.00340
Rottweil	-0.134***	-0.00149	-0.0172	-0.0393
Villingen-Schwenningen	-0.0988***	0.0168	0.00478	-0.0131
Tuttlingen	0.0727*	0.104***	0.0861***	0.0415
Konstanz	-0.0992***	0.0710***	0.0320	0.0522**
Lörrach	0.0361	0.0668**	0.0569**	0.0392
Waldshut	0.0357	0.0608*	0.0421*	0.0303
Reutlingen/Tübingen	-0.108***	-0.00108	0.0151	-0.0189
Balingen	-0.131***	-0.0129	-0.0245	-0.0389
Ulm	0.0262	0.0633***	0.0253	0.0529**
Biberach	0.0496	0.0428	0.0302	0.00827
Friedrichshafen	0.0663**	0.0730***	0.0577***	0.0379
Ravensburg	-0.0134	0.0310	0.0179	0.0113
Sigmaringen	-0.0602	0.0145	-0.000675	-0.0208
Bad Reichenhall	-0.00970	0.0173	-0.0265	0.000708
Traunstein	0.0136	0.00794	-0.0113	-0.0181
Burghausen	-0.0157	0.0362	0.00254	0.0173
Mühldorf	-0.0612	-0.0170	0.0139	-0.0399
Rosenheim	0.0365	0.0254	0.00927	0.0107

Bad Tölz	0.0261	0.0656**	0.0504**	0.0480*
Garmisch-Partenkirchen	0.0653	0.0487	0.0235	0.0374
Weilheim	0.0134	0.0383	0.0596**	-0.00234
Landsberg	0.0371	0.0274	0.0301	0.0119
München	0.209***	0.172***	0.0980***	0.169***
Ingolstadt	0.0821***	0.106***	0.0572***	0.0855***
Kelheim-Mainburg	-0.0900**	0.00147	0.0126	-0.0317
Landshut	-0.0351	0.0427	-0.0130	0.0303
Dingolfing	-0.0835*	-0.0350	0.0338	-0.0869**
Eggenfelden/Pfarrkirchen	-0.161***	-0.0452	-0.0105	-0.0705**
Passau	-0.148***	-0.0774***	-0.0552**	-0.0961***
Freyung	-0.159***	-0.0543	-0.0374	-0.0849**
Regen-Zwiesel	-0.0632	-0.0772**	-0.0886***	-0.106***
Deggendorf	-0.0981**	-0.0274	-0.0473**	-0.0493*
Straubing	-0.0545	0.0164	-0.0254	-0.00170
Cham	0.0255	0.00677	-0.0215	-0.0334
Regensburg	-0.0478	0.0655***	0.0190	0.0506**
Schwandorf	-0.0211	-0.0134	-0.0496**	-0.0385
Amberg	-0.0185	-0.0202	-0.0677***	-0.0478
Neumarkt	-0.0144	0.0139	-0.00735	-0.0122
Weiden	-0.158***	-0.00711	-0.0572**	-0.0347
Marktredwitz	-0.118***	-0.0303	-0.0657***	-0.0734**
Hof	-0.161***	-0.0591**	-0.0939***	-0.0853***
Bayreuth	-0.127***	-0.0278	-0.0648***	-0.0321
Bamberg	-0.137***	-0.0358	-0.0478**	-0.0561**
Kulmbach	-0.0791*	-0.0297	-0.0472*	-0.0660*
Kronach	-0.225***	-0.0962**	-0.0723**	-0.152***
Coburg	-0.0303	0.00468	-0.0651***	-0.0369
Lichtenfels	-0.00637	-0.0103	-0.0503*	-0.0426
Erlangen	0.0651**	0.0804***	0.0369*	0.0522**
Nürnberg	0.0104	0.0532**	0.00498	0.0439**
Weißenburg-Gunzenhausen	-0.0502	-0.0109	-0.0605**	-0.0366
Ansbach	-0.0157	0.0208	-0.0310	0.00109
Neustadt/Aisch	-0.157***	-0.0914**	-0.0452	-0.111***
Kitzingen	-0.145***	-0.0409	-0.0267	-0.0702**
Würzburg	-0.136***	-0.0375	-0.0374*	-0.0403
Schweinfurt	-0.0316	0.0394	-0.0407*	0.0188
Haßfurt	-0.312***	-0.145***	-0.0519**	-0.184***
Bad Neustadt/Saale	-0.180***	-0.0551	-0.0875***	-0.0839**

Bad Kissingen	-0.306***	-0.126***	-0.0500**	-0.151***
Lohr am Main	-0.163***	-0.0125	-0.0231	-0.0512*
Aschaffenburg	-0.142***	-0.0122	-0.00769	-0.0292
Donauwörth-Nördlingen	-0.0357	0.0483	0.00582	0.0238
Dillingen	-0.101**	-0.0149	-0.0550*	-0.0455
Günzburg	-0.0164	0.0217	0.00272	0.00296
Augsburg	-0.0627**	0.0692***	0.0211	0.0554**
Memmingen	0.0816**	0.0456*	-0.000761	0.0231
Kaufbeuren	0.0121	-0.0152	0.000285	-0.0368
Kempten	-0.0186	-0.0219	-0.00993	-0.0299
Lindau	0.106**	0.0430	0.0158	0.0110
Berlin	-0.0981***	-0.0325	-0.0888***	-0.0305
Potsdam-Brandenburg	-0.278***	-0.207***	-0.231***	-0.210***
Cottbus	-0.414***	-0.270***	-0.326***	-0.281***
Frankfurt/Oder	-0.319***	-0.204***	-0.256***	-0.212***
Eberswalde	-0.323***	-0.214***	-0.225***	-0.221***
Luckenwalde	-0.155***	-0.116***	-0.165***	-0.128***
Finsterwalde	-0.409***	-0.362***	-0.336***	-0.381***
Oranienburg	-0.200***	-0.156***	-0.181***	-0.167***
Neuruppin	-0.268***	-0.226***	-0.236***	-0.232***
Perleberg	-0.237***	-0.239***	-0.277***	-0.243***
Prenzlau	-0.353***	-0.234***	-0.250***	-0.229***
Rostock	-0.360***	-0.228***	-0.273***	-0.230***
Schwerin	-0.381***	-0.241***	-0.285***	-0.251***
Mecklenburgische Seenplatte	-0.440***	-0.325***	-0.351***	-0.330***
Nordvorpommern	-0.378***	-0.323***	-0.335***	-0.318***
Südvorpommern	-0.395***	-0.284***	-0.304***	-0.290***
Chemnitz	-0.434***	-0.227***	-0.272***	-0.217***
Erzgebirgskreis	-0.476***	-0.331***	-0.342***	-0.354***
Mittelsachsen	-0.407***	-0.297***	-0.303***	-0.316***
Vogtlandkreis	-0.364***	-0.281***	-0.296***	-0.301***
Zwickau	-0.486***	-0.359***	-0.366***	-0.376***
Dresden	-0.367***	-0.236***	-0.275***	-0.234***
Bautzen	-0.366***	-0.230***	-0.303***	-0.249***
Görlitz	-0.392***	-0.261***	-0.325***	-0.279***
Meißen	-0.393***	-0.288***	-0.286***	-0.308***
Leipzig	-0.472***	-0.328***	-0.265***	-0.340***
Dessau-Roßlau	-0.308***	-0.256***	-0.321***	-0.252***
Halle	-0.450***	-0.314***	-0.297***	-0.320***

Magdeburg	-0.340***	-0.208***	-0.249***	-0.216***
Salzwedel	-0.409***	-0.252***	-0.293***	-0.259***
Anhalt-Bitterfeld	-0.354***	-0.285***	-0.287***	-0.301***
Burgenlandkreis	-0.426***	-0.288***	-0.275***	-0.299***
Harz	-0.374***	-0.294***	-0.322***	-0.308***
Mansfeld-Südharz	-0.383***	-0.316***	-0.315***	-0.330***
Salzlandkreis	-0.397***	-0.281***	-0.321***	-0.298***
Stendal	-0.340***	-0.272***	-0.303***	-0.282***
Wittenberg	-0.398***	-0.303***	-0.319***	-0.319***
Erfurt	-0.419***	-0.231***	-0.277***	-0.224***
Gera	-0.427***	-0.315***	-0.324***	-0.315***
Jena	-0.395***	-0.280***	-0.311***	-0.290***
Suhl	-0.426***	-0.278***	-0.313***	-0.293***
Weimar	-0.368***	-0.247***	-0.278***	-0.244***
Eisenach	-0.277***	-0.237***	-0.271***	-0.259***
Eichsfeld	-0.483***	-0.337***	-0.343***	-0.368***
Nordhausen	-0.398***	-0.304***	-0.311***	-0.313***
Mühlhausen	-0.407***	-0.298***	-0.357***	-0.309***
Sondershausen	-0.402***	-0.317***	-0.350***	-0.323***
Meiningen	-0.393***	-0.288***	-0.323***	-0.309***
Gotha	-0.338***	-0.249***	-0.300***	-0.271***
Arnstadt	-0.342***	-0.197***	-0.257***	-0.236***
Sonneberg	-0.341***	-0.272***	-0.305***	-0.291***
Saalfeld	-0.348***	-0.300***	-0.339***	-0.321***
Pößneck	-0.332***	-0.287***	-0.329***	-0.322***
Altenburg	-0.448***	-0.376***	-0.313***	-0.404***
Husum	0	0	0	0
Constant	3.954***	3.705***	3.423***	3.555***
Individual FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Occupation controls	no	no	yes	no
Industry controls	no	no	no	yes
Education controls	no	yes	yes	yes
Incl. part-time empl.	yes	yes	no	yes
Observations	23175028	16555698	12178375	16553522

* $p < .1$, ** $p < .05$, *** $p < .01$

Notes: The dependent variable is the log wage. Clustering of standard errors is at the worker level.

Table A.2: Estimates of agglomeration elasticities (different controls and calibrations)

	Δ log employment OLS	Δ log employment IV	Δ log employment IV	Δ log employment IV
Δ log density spline low	0.506*** (0.072)	0.718*** (0.164)	0.522*** (0.144)	0.517*** (0.136)
Δ log density spline medium	0.394*** (0.099)	0.620*** (0.201)	0.507*** (0.138)	0.511*** (0.130)
Δ log density spline high	0.368*** (0.100)	0.696*** (0.227)	0.492*** (0.177)	0.486*** (0.167)
Δ log wage	-1.5	-1.5	-1.0	-0.75
N	410	205	205	205
State-Decade FE	no	no	yes	yes
Adj R-squared	0.69	0.87	0.87	0.85
Spline knots (percentiles)	50,75	50,75	50,75	50,75

Notes: Standard errors in parentheses (* $p < .1$, ** $p < .05$, *** $p < .01$). Clustering of standard errors is at the labor market region level.

Table A.3: Estimates of agglomeration elasticities (different spline knots)

	Δ log employment IV	Δ log employment IV
Δ log density spline low	0.460*** (0.130)	0.551*** (0.141)
Δ log density spline medium	0.548*** (0.166)	0.529*** (0.195)
Δ log density spline high	0.524*** (0.190)	0.738** (0.240)
Δ log wage	-1.5	-1.5
N	205	205
State-Decade FE	yes	yes
Adj R-squared	0.91	0.90
Spline knots (percentiles)	33,60	60,85

Notes: Standard errors in parentheses (* $p < .1$, ** $p < .05$, *** $p < .01$). Clustering of standard errors is at the labor market region level.

Table A.4: Estimates of agglomeration elasticities (including East Germany)

	Δ log employment OLS	Δ log employment OLS
Δ log density spline low	0.332***	0.467***
	(0.083)	(0.096)
Δ log density spline medium	0.301***	0.412***
	(0.095)	(0.105)
Δ log density spline high	0.257**	0.382***
	(0.109)	(0.122)
Δ log wage	-1.5	-1.5
N	258	205
State-Decade FE	yes	yes
Adj R-squared	0.23	0.26
Spline knots (percentiles)	50,75	50,75
Regions	All	West only

Notes: Standard errors in parentheses (* $p < .1$, ** $p < .05$, *** $p < .01$). Clustering of standard errors is at the labor market region level. In order to be able to include East Germany, I use the years 1993, 2000 and 2010 instead of decadal observations (re-unification took place in 1990). However, this leaves me unable to instrument for density since I miss a fourth observation for each region.

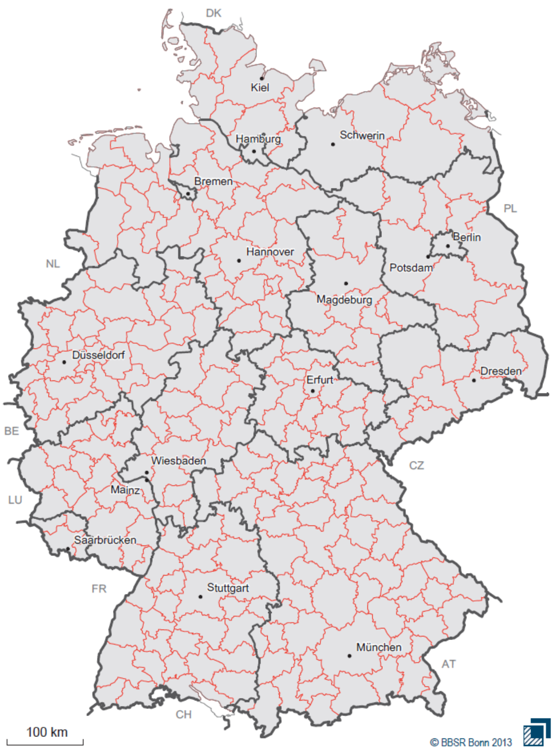
Table A.5: Estimates of agglomeration elasticities (linear splines)

	Δ log employment OLS	Δ log employment IV
Δ linear density spline low	0.0237***	0.0228***
	(0.00407)	(0.00884)
Δ linear density spline medium	0.00671***	0.0111***
	(0.00246)	(0.00417)
Δ linear density spline high	0.00208***	0.00178
	(0.000608)	(0.00117)
Δ log wage	-1.5	-1.5
N	410	205
State-Decade FE	yes	yes
Adj R-squared	0.70	0.90
Spline knots (percentiles)	50,75	50,75

Notes: Standard errors in parentheses (* $p < .1$, ** $p < .05$, *** $p < .01$). Clustering of standard errors is at the labor market region level.

A.3 Data Appendix

Figure A.11: Labor market regions (borders as defined in 2011)



A.3.1 List of equalization payments aggregated (in German)

General equalization payments include

- Schlüsselzuweisungen vom Land, Bedarfszuweisungen vom Land, Bedarfszuweisungen von Gemeinden/Gemeindeverbänden
- Sonstige allgemeine Zuweisungen von Bund, Land und Gemeinden/Gemeindeverbänden
- Ausgleichsleistungen nach dem Familienleistungsausgleich
- Leistungen des Landes für Dienstleistungen am Arbeitsmarkt
- Leistungen des Landes aus dem Ausgleich von Sonderlasten bei der Zusammenführung von Arbeitslosen- und Sozialhilfe

Investment subsidies include

- Zuweisungen und Zuschüsse für Investitionen und Investitionsförderungsmaßnahmen
  - von Bund, LAF, ERP-Sondervermögen, vom Land, von Gemeinden (Gemeindeverbänden), von Zweckverbänden und dergleichen, vom sonstigen öffentlichen Bereich
  - von kommunalen Sonderrechnungen, von sonstigen öffentlichen Sonderrechnungen
  - von öffentlichen, wirtschaftlichen Unternehmen, von privaten Unternehmen, von übrigen Bereichen

Appendix B

Appendix to chapter 2

B.1 Institutional details

Indicator formulas

The following formulas describe the indicator used to evaluate the economic performance of labor market region 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 regions's infrastructure[1] 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.1, B.2 and B.3 illustrate the indicator rankings and cutoffs for the years 1997, 2000 and 2011. If there is more than one county that has e.g. rank 13 below a cutoff, we use all counties that have that rank. When counties merge, we take the average of the individual counties' indicators. Table B.4 reports a county's rank for each reform along with treatment intensities. Due to missing data, three indicator values are unavailable.

[1] The infrastructure indicator is explained in section 2.2.

Table B.1: Counties around the cutoff (year 1997)

County	Indicator	Subsidy cut
...		
Saale-Orla-Kreis	99.713	no
Märkisch Oderland	99.714	yes
KS Frankfurt/Oder	99.714	no
Oder-Spree	99.714	yes
Mittelsachsen	99.725	no
Gotha	99.757	yes
Zwickau	99.767	no
Magdeburg	99.801	no
Jerichower Land	99.801	no
Boerde	99.801	no
Ludwigslust-Parchim	99.868	yes
Salzlandkreis	99.902	yes
Rostock	99.904	no
Chemnitz	99.914	no
Spree-Neiße	99.926	no
KS Cottbus	99.926	no
Dahme-Spreewald	99.926	yes
Halle (Saale)	100.003	yes
Leipzig Lkr.	100.069	yes
Nordsachsen	100.069	yes
Schwerin	100.096	yes
Weimarer Land	100.162	yes
KS Weimar	100.162	yes
Sömmerda	100.173	yes
KS Erfurt, Landeshauptstadt	100.173	yes
Meissen	100.326	yes
Saale-Holzland-Kreis	100.442	yes
KS Jena	100.442	yes
Leipzig	100.476	yes
Dresden	101.073	yes

Source: Federal Ministry for Economic Affairs. *Notes*: Only counties above indicator value 99.7 are shown.

Table B.2: Counties around the cutoff (year 2000)

County	Indicator	Subsidy cut
...		
Saalekreis	99.712	yes
Mittelsachsen	99.724	yes
Hildburghausen	99.724	no
KS Suhl	99.724	no
Eichsfeld	99.728	no
Gotha	99.742	yes
Vogtlandkreis	99.752	no
Jerichower Land	99.765	no
KS Cottbus	99.774	no
Spree-Neiße	99.774	no
Dahme-Spreewald	99.774	yes
Bautzen	99.813	yes
Saale-Orla-Kreis	99.854	no
Teltow-Fläming	99.856	yes
Zwickau	99.884	yes
Rostock	99.902	no
Nordwestmecklenburg	99.951	no
Chemnitz	100.008	yes
Ludwigslust-Parchim	100.034	yes
Boerde	100.070	yes
Magdeburg	100.070	yes
Nordsachsen	100.083	yes
KS Weimar	100.144	yes
Weimarer Land	100.144	yes
Wartburgkreis	100.151	yes
KS Eisenach	100.151	yes
Halle (Saale)	100.169	yes
Saechsische Schweiz-Osterzgebirge	100.177	yes
Sonneberg	100.181	yes
KS Erfurt, Landeshauptstadt	100.246	yes
Sömmerda	100.246	yes
KS Jena	100.256	yes
Saale-Holzland-Kreis	100.256	yes
Leipzig Lkr.	100.377	yes
Schwerin	100.388	yes
Meissen	100.444	yes
Potsdam-Mittelmark	100.496	yes
Leipzig	100.563	yes
Dresden	101.117	yes

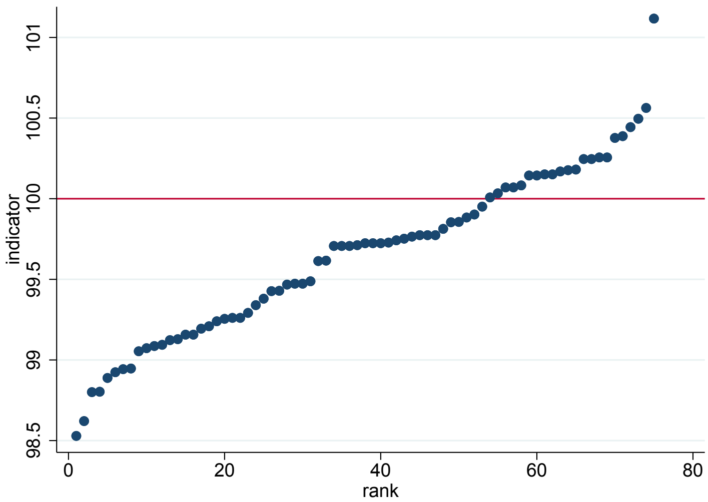
Source: Federal Ministry for Economic Affairs. *Notes*: Only counties above indicator value 99.7 are shown.

Table B.3: Counties around the cutoff (year 2011)

County	NUTSII-region	Subsidy cut	GDP per capita (€)
...			
Greiz	Thueringen	no	20662
Ilm-Kreis	Thueringen	no	20662
Hildburghausen	Thueringen	no	20662
Saalfeld-Rudolstadt	Thueringen	no	20662
Eisenach, Stadt	Thueringen	no	20662
Sonneberg	Thueringen	no	20662
Weimar, Stadt	Thueringen	no	20662
Erfurt, Stadt	Thueringen	no	20662
Kyffhaeuserkreis	Thueringen	no	20662
Schmalkalden-Meiningen	Thueringen	no	20662
Gera, Stadt	Thueringen	no	20662
Eichsfeld	Thueringen	no	20662
Saale-Holzland-Kreis	Thueringen	no	20662
Suhl, Stadt	Thueringen	no	20662
Nordhausen	Thueringen	no	20662
Jena, Stadt	Thueringen	no	20662
Gotha	Thueringen	no	20662
Unstrut-Hainich-Kreis	Thueringen	no	20662
Weimarer Land	Thueringen	no	20662
Altenburger Land	Thueringen	no	20662
Saale-Orla-Kreis	Thueringen	no	20662
Soemmerda	Thueringen	no	20662
Wartburgkreis	Thueringen	no	20662
Magdeburg, Stadt	Magdeburg	no	20822
Jerichower Land	Magdeburg	no	20822
Altmarkkreis Salzwedel	Magdeburg	no	20822
Boerde	Magdeburg	no	20822
Harz	Magdeburg	no	20822
Salzlandkreis	Magdeburg	no	20822
Stendal	Magdeburg	no	20822
Vogtlandkreis	Chemnitz	no	20914
Chemnitz, Stadt	Chemnitz	no	20914
Zwickau	Chemnitz	no	20914
Mittelsachsen	Chemnitz	no	20914
Erzgebirgskreis	Chemnitz	no	20914
Mansfeld-Suedharz	Halle	yes	21228
Burgenlandkreis	Halle	yes	21228
Halle (Saale), Stadt	Halle	yes	21228
Saalekreis	Halle	yes	21228
Elbe-Elster	Brandenburg-Suedwest	yes	22572
Cottbus, Stadt	Brandenburg-Suedwest	yes	22572
Teltow-Flaeming	Brandenburg-Suedwest	yes	22572
Dahme-Spreewald	Brandenburg-Suedwest	yes	22572
Havelland	Brandenburg-Suedwest	yes	22572
Brandenburg an der Havel, Stadt	Brandenburg-Suedwest	yes	22572
Potsdam-Mittelmark	Brandenburg-Suedwest	yes	22572
Oberspreewald-Lausitz	Brandenburg-Suedwest	yes	22572
Spree-Neisse	Brandenburg-Suedwest	yes	22572
Potsdam, Stadt	Brandenburg-Suedwest	yes	22572
Goerlitz	Dresden	no	22919
Meissen	Dresden	no	22919
Saechsische Schweiz-Osterzgebirge	Dresden	no	22919
Bautzen	Dresden	no	22919
Dresden, Stadt	Dresden	no	22919
Nordsachsen	Leipzig	yes	23028
Leipzig, Stadt	Leipzig	yes	23028
Leipzig	Leipzig	yes	23028

Source: Statistical Offices of German States (Laender), Deutscher Bundestag (2007). *Notes*: Only counties whose GDP per capita is above €20 660 are shown.

Figure B.1: Ranking of counties based on indicator (year 2000)



Source: Federal Ministry for Economic Affairs. *Notes*: This figure plots indicator values and the ranks of counties in the year 2000. The cutoff was formally at indicator value 100 (however some counties below the cutoff still lost eligibility for the highest assistance rates).

Table B.4: Cutoff ranks and maximum assistance rates by subsidy rate regime and firm size

County	*s97*	*m97*	*l97*	*r97*	*s00*	*m00*	*l00*	*r00*	*s07*	*m07*	*l07*	*r07*	*s11*	*m11*	*l11*	*r11*
Brandenburg an der Havel, Stadt	.5	.5	.35	B29	.5	.5	.35	B39	.5	.4	.3	All	.4	.3	.2	A5
Cottbus, Stadt	.5	.5	.35	B3	.5	.5	.35	B7	.5	.4	.3	All	.4	.3	.2	A5
Frankfurt (Oder), Stadt	.5	.5	.35	B14	.5	.5	.35	B18	.5	.4	.3	All	.5	.4	.3	B47
Potsdam, Stadt	.43	.43	.28		.43	.43	.28		.5	.4	.3	All	.4	.3	.2	A5
Barnim	.43	.43	.28	B45	.43	.43	.28	B35	.5	.4	.3	All	.5	.4	.3	B47
Dahme-Spreewald	.43	.43	.28	B1	.43	.43	.28	B9	.5	.4	.3	All	.4	.3	.2	A5
Elbe-Elster	.5	.5	.35	B47	.5	.5	.35	B42	.5	.4	.3	All	.4	.3	.2	A5
Havelland	.43	.43	.28	B27	.43	.43	.28	B38	.5	.4	.3	All	.4	.3	.2	A5
Maerkisch-Oderland	.43	.43	.28	B16	.43	.43	.28	B20	.5	.4	.3	All	.5	.4	.3	B47
Oberhavel	.43	.43	.28	B49	.43	.43	.28	B33	.5	.4	.3	All	.5	.4	.3	B47
Oberspreewald-Lausitz	.5	.5	.35	B22	.5	.5	.35	B45	.5	.4	.3	All	.4	.3	.2	A5
Oder-Spree	.43	.43	.28	B15	.43	.43	.28	B19	.5	.4	.3	All	.5	.4	.3	B47
Ostprignitz-Ruppin	.5	.5	.35	B50	.5	.5	.35	B32	.5	.4	.3	All	.5	.4	.3	B47
Potsdam-Mittelmark	.43	.43	.28	B28	.43	.43	.28	A20	.5	.4	.3	All	.4	.3	.2	A5
Prignitz	.5	.5	.35	B51	.5	.5	.35	B47	.5	.4	.3	All	.5	.4	.3	B47
Spree-Neisse	.5	.5	.35	B2	.5	.5	.35	B8	.5	.4	.3	All	.4	.3	.2	A5
Teltow-Flaeming	.43	.43	.28	B24	.43	.43	.28	B4	.5	.4	.3	All	.4	.3	.2	A5
Uckermark	.5	.5	.35	B46	.5	.5	.35	B50	.5	.4	.3	All	.5	.4	.3	B47
Rostock, Stadt	.5	.5	.35	B5	.5	.5	.35	B2	.5	.4	.3	All	.5	.4	.3	B39
Schwerin, Stadt	.43	.43	.28	A4	.43	.43	.28	A18	.5	.4	.3	All	.5	.4	.3	B39
Mecklenburgische Seenplatte	.5	.5	.35	B55	.5	.5	.35	B49	.5	.4	.3	All	.5	.4	.3	B39
Landkreis Rostock	.5	.5	.35	B35	.5	.5	.35	B22	.5	.4	.3	All	.5	.4	.3	B39

Vorpommern-Ruegen	.5	.5	.35	B60	.5	.5	.35	B43	.5	.4	.3	All	.5	.4	.3	B39
Nordwestmecklenburg	.5	.5	.35	B36	.5	.5	.35	B1	.5	.4	.3	All	.5	.4	.3	B39
Vorpommern-Greifswald	.5	.5	.35	B59	.5	.5	.35	B51	.5	.4	.3	All	.5	.4	.3	B39
Ludwigslust-Parchim	.43	.43	.28	B7	.43	.43	.28	A2	.5	.4	.3	All	.5	.4	.3	B39
Chemnitz, Stadt	.5	.5	.35	B4	.43	.43	.28	A1	.5	.4	.3	All	.5	.4	.3	B1
Erzgebirgskreis	.5	.5	.35	B37	.5	.5	.35	B26	.5	.4	.3	All	.5	.4	.3	B1
Mittelsachsen	.5	.5	.35	B13	.43	.43	.28	B16	.5	.4	.3	All	.4	.3	.2	B1
Vogtlandkreis	.5	.5	.35	B30	.5	.5	.35	B11	.5	.4	.3	All	.5	.4	.3	B1
Zwickau	.5	.5	.35	B11	.43	.43	.28	B3	.5	.4	.3	All	.5	.4	.3	B1
Dresden, Stadt	.43	.43	.28	A13	.43	.43	.28	A22	.5	.4	.3	All	.5	.4	.3	A15
Bautzen	.5	.5	.35	B34	.43	.43	.28	B6	.5	.4	.3	All	.5	.4	.3	A15
Goerlitz	.5	.5	.35	B43	.5	.5	.35	B36	.5	.4	.3	All	.5	.4	.3	A15
Meissen	.43	.43	.28	A9	.43	.43	.28	A19	.5	.4	.3	All	.5	.4	.3	A15
Saechsische Schweiz-Osterzgebirge	.5	.5	.35	B23	.43	.43	.28	A11	.5	.4	.3	All	.5	.4	.3	A15
Leipzig, Stadt	.43	.43	.28	A12	.43	.43	.28	A21	.5	.4	.3	All	.4	.3	.2	A20
Leipzig	.43	.43	.28	A2	.43	.43	.28	A17	.5	.4	.3	All	.4	.3	.2	A20
Nordsachsen	.43	.43	.28	A3	.43	.43	.28	A5	.5	.4	.3	All	.4	.3	.2	A20
Dessau-Rosslau, Stadt	.5	.5	.35	B38	.5	.5	.35	B30	.5	.4	.3	All	.5	.4	.3	B26
Halle (Saale), Stadt	.43	.43	.28	A1	.43	.43	.28	A10	.5	.4	.3	All	.4	.3	.2	A1
Magdeburg, Stadt	.5	.5	.35	B8	.43	.43	.28	A4	.5	.4	.3	All	.5	.4	.3	B6
Altmarkkreis Salzwedel	.5	.5	.35	B41	.5	.5	.35	B28	.5	.4	.3	All	.5	.4	.3	B6
Anhalt-Bitterfeld	.5	.5	.35	B32	.5	.5	.35	B44	.5	.4	.3	All	.5	.4	.3	B26
Boerde	.5	.5	.35	B9	.43	.43	.28	A3	.5	.4	.3	All	.5	.4	.3	B6
Burgenlandkreis	.5	.5	.35	B42	.5	.5	.35	B34	.5	.4	.3	All	.4	.3	.2	A1
Harz	.5	.5	.35	B44	.5	.5	.35	B27	.5	.4	.3	All	.5	.4	.3	B6
Jerichower Land	.5	.5	.35	B10	.5	.5	.35	B10	.5	.4	.3	All	.5	.4	.3	B6

Mansfeld-Suedharz	.5	.5	.35	B61	.5	.5	.35	B52	.5	.4	.3	All	.4	.3	.2	A1
Saalekreis	.43	.43	.28	B25	.43	.43	.28	B17	.5	.4	.3	All	.4	.3	.2	A1
Salzlandkreis	.43	.43	.28	B6	.5	.5	.35	B46	.5	.4	.3	All	.5	.4	.3	B6
Stendal	.5	.5	.35	B56	.5	.5	.35	B48	.5	.4	.3	All	.5	.4	.3	B6
Wittenberg	.5	.5	.35	B40	.5	.5	.35	B31	.5	.4	.3	All	.5	.4	.3	B26
Erfurt, Stadt	.43	.43	.28	A8	.43	.43	.28	A14	.5	.4	.3	All	.5	.4	.3	B13
Gera, Stadt	.5	.5	.35	B19	.5	.5	.35	B24	.5	.4	.3	All	.5	.4	.3	B13
Jena, Stadt	.43	.43	.28	A11	.43	.43	.28	A16	.5	.4	.3	All	.5	.4	.3	B13
Suhl, Stadt	.5	.5	.35	B20	.5	.5	.35	B15	.5	.4	.3	All	.5	.4	.3	B13
Weimar, Stadt	.43	.43	.28	A5	.43	.43	.28	A7	.5	.4	.3	All	.5	.4	.3	B13
Eisenach, Stadt	.43	.43	.28		.43	.43	.28	A9	.5	.4	.3	All	.5	.4	.3	B13
Eichsfeld	.5	.5	.35	B54	.5	.5	.35	B13	.5	.4	.3	All	.5	.4	.3	B13
Nordhausen	.5	.5	.35	B58	.5	.5	.35	B37	.5	.4	.3	All	.5	.4	.3	B13
Wartburgkreis	.5	.5	.35	B31	.43	.43	.28	A8	.5	.4	.3	All	.5	.4	.3	B13
Unstrut-Hainich-Kreis	.5	.5	.35	B48	.5	.5	.35	B41	.5	.4	.3	All	.5	.4	.3	B13
Kyffhaeuserkreis	.5	.5	.35	B57	.5	.5	.35	B53	.5	.4	.3	All	.5	.4	.3	B13
Schmalkalden-Meiningen	.5	.5	.35	B39	.5	.5	.35	B21	.5	.4	.3	All	.5	.4	.3	B13
Gotha	.43	.43	.28	B12	.43	.43	.28	B12	.5	.4	.3	All	.5	.4	.3	B13
Soemmerda	.43	.43	.28	A7	.43	.43	.28	A13	.5	.4	.3	All	.5	.4	.3	B13
Hildburghausen	.5	.5	.35	B21	.5	.5	.35	B14	.5	.4	.3	All	.5	.4	.3	B13
Ilm-Kreis	.5	.5	.35	B53	.5	.5	.35	B29	.5	.4	.3	All	.5	.4	.3	B13
Weimarer Land	.43	.43	.28	A6	.43	.43	.28	A6	.5	.4	.3	All	.5	.4	.3	B13
Sonneberg	.5	.5	.35	B26	.43	.43	.28	A12	.5	.4	.3	All	.5	.4	.3	B13
Saalfeld-Rudolstadt	.5	.5	.35	B33	.5	.5	.35	B23	.5	.4	.3	All	.5	.4	.3	B13
Saale-Holzland-Kreis	.43	.43	.28	A10	.43	.43	.28	A15	.5	.4	.3	All	.5	.4	.3	B13
Saale-Orla-Kreis	.5	.5	.35	B17	.5	.5	.35	B5	.5	.4	.3	All	.5	.4	.3	B13

Greiz	.5	.5	.35	B18	.5	.5	.35	B25	.5	.4	.3	All	.5	.4	.3	B13
Altenburger Land	.5	.5	.35	B52	.5	.5	.35	B40	.5	.4	.3	All	.5	.4	.3	B13

Source: Federal Ministry for Economic Affairs, Deutscher Bundestag (1997), Deutscher Bundestag (2000), Deutscher Bundestag (2007).

Notes: This table shows county-specific maximum assistance rate for each firm size group and subsidy regime (e.g. *s97* is the maximum rate for small firms in year 1997).

A county's rank below (B) or above (A) a cutoff is contained in *r97* for year 1997 and analogously for other years.

Table B.5: Eligible industries for GRW subsidies

Production, processing and preserving of meat and meat products
Processing and preserving of fish and fish products
Processing and preserving of fruit and vegetables
Manufacture of vegetable and animal oils and fats
Manufacture of dairy products
Manufacture of grain mill products, starches and starch products
Manufacture of prepared animal feeds
Manufacture of other food products
Manufacture of beverages
Manufacture of tobacco product
Preparation and spinning of textile fibres
Textile weaving
Finishing of textiles
Manufacture of made-up textile articles, except apparel
Manufacture of other textiles
Manufacture of knitted and crocheted fabrics
Manufacture of knitted and crocheted articles
Manufacture of leather clothes
Manufacture of other wearing apparel and accessories
Dressing and dyeing of fur; manufacture of articles of fur
Tanning and dressing of leather
Manufacture of luggage, handbags and the like, saddlery and harness
Manufacture of footwear
Sawmilling and planing of wood; impregnation of wood
Manufacture of veneer sheets; manufacture of plywood, laminboard, particle board, fibre board and other panels and boards
Manufacture of builders' carpentry and joinery
Manufacture of wooden containers
Manufacture of other products of wood; manufacture of articles of cork, straw and plaiting Materials
Manufacture of pulp, paper and paperboard
Manufacture of articles of paper and paperboard
Publishing
Printing and service activities related to printing
Reproduction of recorded media
Manufacture of coke oven products
Manufacture of refined petroleum products
Processing of nuclear fuel
Manufacture of basic chemicals
Manufacture of pesticides and other agro-chemical products
Manufacture of paints, varnishes and similar coatings, printing ink and mastics
Manufacture of pharmaceuticals, medicinal chemicals and botanical products
Manufacture of soap and detergents, cleaning and polishing preparations, perfumes and tollet preparations
Manufacture of other chemical products
Manufacture of rubber products
Manufacture of plastic products
Manufacture of glass and glass products
Manufacture of non-refractory ceramic goods other than for construction purposes; manufacture of refractory ceramic products
Manufacture of ceramic tiles and flags
Manufacture of bricks, tiles and construction products, in baked clay
Manufacture of cement, lime and plaster

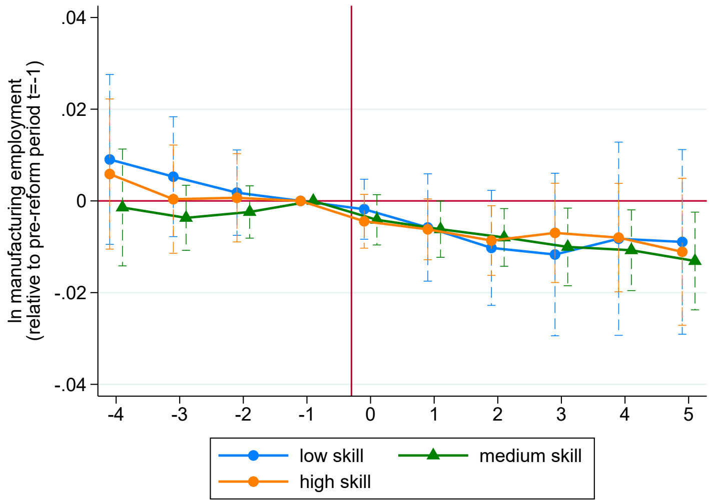
Manufacture of articles of concrete, plaster and cement
Cutting, shaping and finishing of stone
Manufacture of other non-metallic mineral products
Manufacture of basic iron and steel and of ferro-alloys (ECSC1)
Manufacture of tubes
Other first processing of iron and steel and production of non-ECSC1 ferro-alloys
Manufacture of basic precious and non-ferrous metals
Casting of metals
Manufacture of structural metal products
Manufacture of tanks, reservoirs and containers of metal; manufacture of central heating radiators and boilers
Manufacture of steam generators, except central heating hot water boilers
Forging, pressing, stamping and roll forming of metal; powder metallurgy
Treatment and coating of metals; general mechanical engineering
Manufacture of cutlery, tools and general hardware
Manufacture of other fabricated metal products
Manufacture of machinery for the production and use of mechanical power, except aircraft, vehicle and cycle engines
Manufacture of other general purpose machinery
Manufacture of agricultural and forestry machinery
Manufacture of machine-tools
Manufacture of other special purpose machinery
Manufacture of weapons and ammunition
Manufacture of domestic appliances n.e.c.
Manufacture of office machinery and computers
Manufacture of electric motors, generators and transformers
Manufacture of electricity distribution and control apparatus
Manufacture of insulated wire and cable
Manufacture of accumulators, primary cells and primary batteries
Manufacture of lighting equipment and electric lamps
Manufacture of electrical equipment n.e.c.
Manufacture of electronic valves and tubes and other electronic components
Manufacture of television and radio transmitters and apparatus for line telephony and line telegraphy
Manufacture of television and radio receivers, sound or video recording or reproducing apparatus and associated goods
Manufacture of medical and surgical equipment and orthopaedic appliances
Manufacture of instruments and appliances for measuring, checking, testing, navigating and other purposes, except industrial process control equipment
Manufacture of industrial process control equipment
Manufacture of optical instruments and photographic equipment
Manufacture of watches and clocks
Manufacture of motor vehicles
Manufacture of bodies (coachwork) for motor vehicles; manufacture of trailers and semitrailers
Manufacture of parts and accessories for motor vehicles and their engines
Building and repairing of ships and boats
Manufacture of railway and tramway locomotives and rolling stock
Manufacture of aircraft and spacecraft
Manufacture of motorcycles and bicycles
Manufacture of other transport equipment n.e.c.
Manufacture of furniture
Manufacture of jewellery and related articles
Manufacture of musical instruments
Manufacture of sports goods
Manufacture of games and toys
Miscellaneous manufacturing n.e.c.
Recycling of metal waste and scrap

Recycling of non-metal waste and scrap
Wholesale an a fee or contract basis
Wholesale of agricultural raw Materials and live animals
Wholesale of food, beverages and tobacco
Wholesale of household goods
Wholesale of non-agricultural intermediate products, waste and scrap
Wholesale of machinery, equipment and supplies
Other wholesale
Hotels
Camping sites and other provision of short-stay accommodation
Activities of other transport agencies
Hardware consultancy
Software consultancy and supply
Data processing
Database activities
Maintenance and repair of office, accounting and computing machinery
Other computer related activities
Research and experimental development an natural sciences and engineering
Research and experimental development an social sciences and humanities
Legal, accounting, book-keeping and auditing activities; tax consultancy; market research and public opinion polling; business and management consultancy
Architectural and engineering activities and related technical consultancy
Technical testing and analysis
Advertising
Radio and television activities

Source: Deutscher Bundestag (1997), Deutscher Bundestag (2000), Deutscher Bundestag (2007).
Notes: This table shows eligible industry branches according to the classification of economic activities in year 1993 (3-digit industries). For the shown industries, eligibility for GRW subsidies is guaranteed whereas other industries are eligible only if the conditions mentioned in section 2 are met.

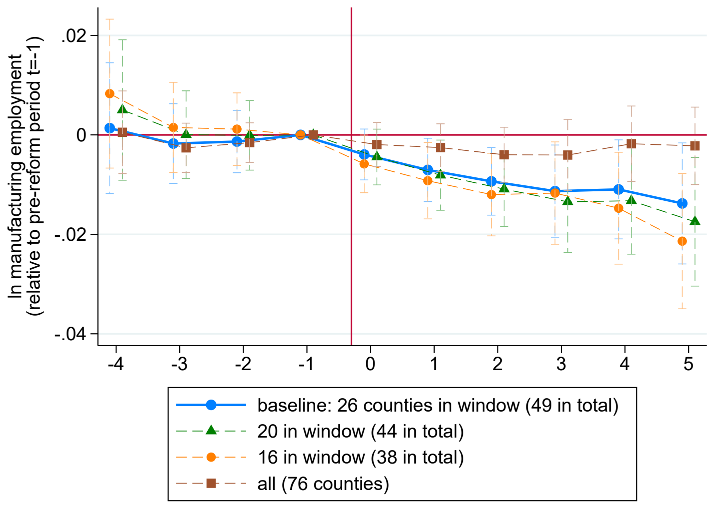
B.2 Additional figures

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



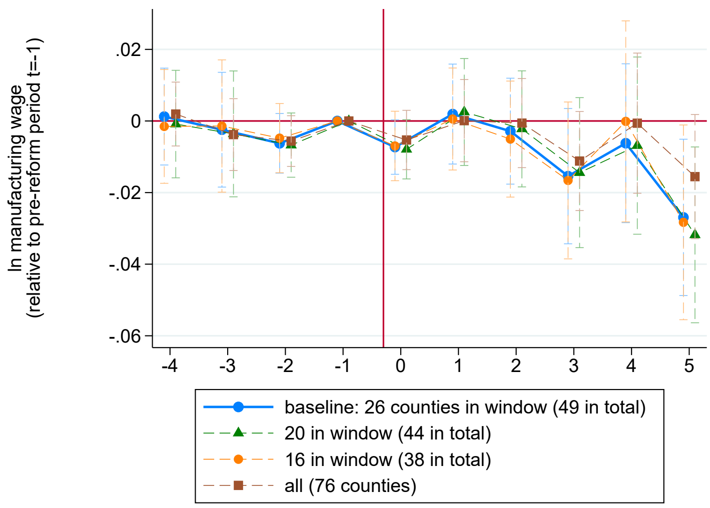
Source: BHP. *Notes*: This figure plots coefficients along with 95 % confidence intervals of a regression of log manufacturing employment by skill on leads and lags of a change in the maximum assistance rate as in equation (2.4). The sample includes the 49 counties closest to cutoffs. State-year fixed effects are included. Clustering of standard errors is at the county level.

Figure B.3: Event study estimates: manufacturing employment (different cutoff samples, refined treatment)



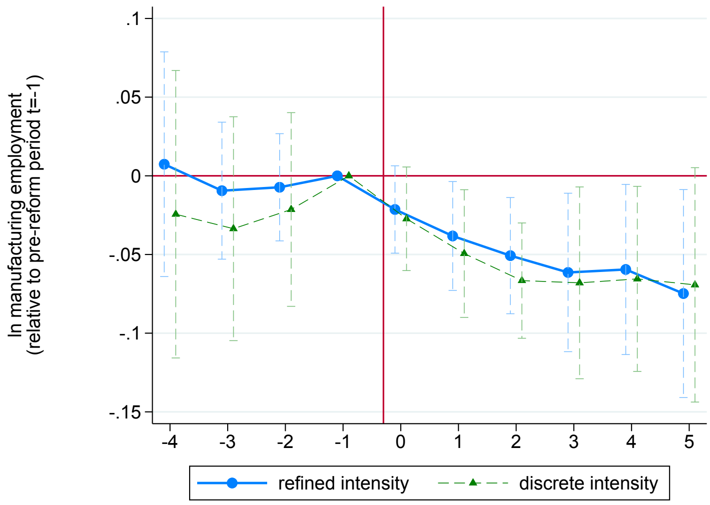
Source: BHP. *Notes*: This figure plots coefficients along with 95 % confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate as in equation (2.4). State-year fixed effects are included. Clustering of standard errors is at the county level.

Figure B.4: Event study estimates: low-skilled manufacturing wages (different cutoff samples, refined treatment)



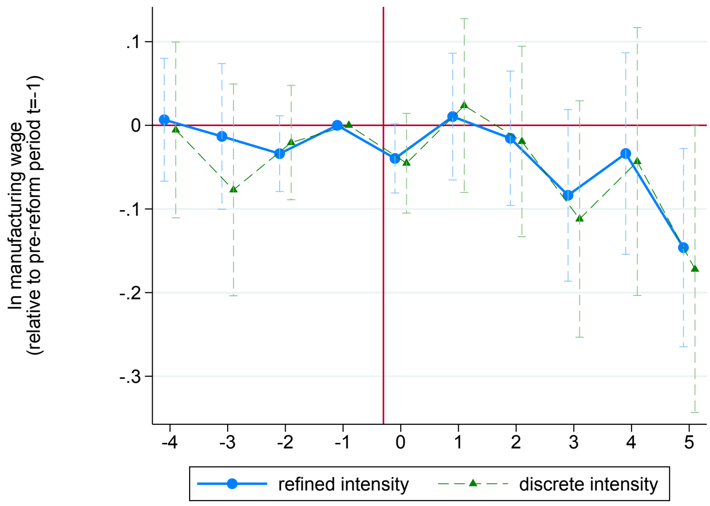
Source: SIAB. *Notes*: This figure plots coefficients along with 95 % confidence intervals of a regression of log low-skilled wages on leads and lags of a change in the maximum assistance rate as in equation (2.4). State-year fixed effects are included. Clustering of standard errors is at the county level.

Figure B.5: Event study estimates: manufacturing employment (different treatment intensities)



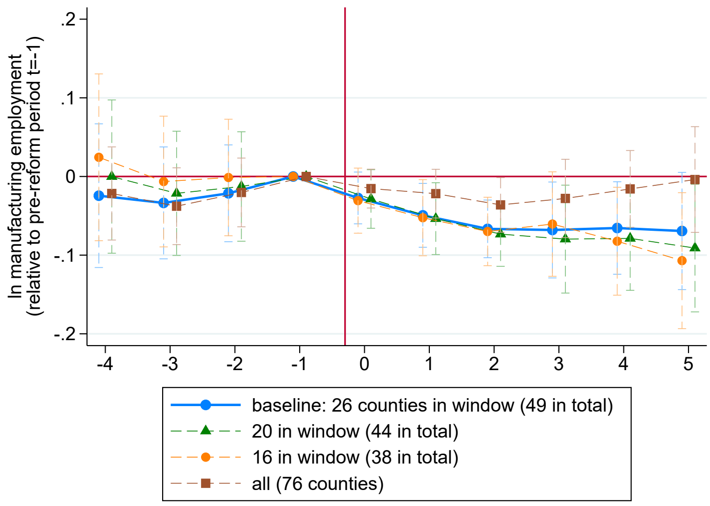
Source: BHP. *Notes*: This figure plots coefficients along with 95 % confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate. Treatment is discrete as in equation (2.1) or refined as in equation (2.4). The sample includes the 49 counties closest to cutoffs. State-year fixed effects are included. Clustering of standard errors is at the county level.

Figure B.6: Event study estimates: low-skilled manufacturing wages (different treatment intensities)



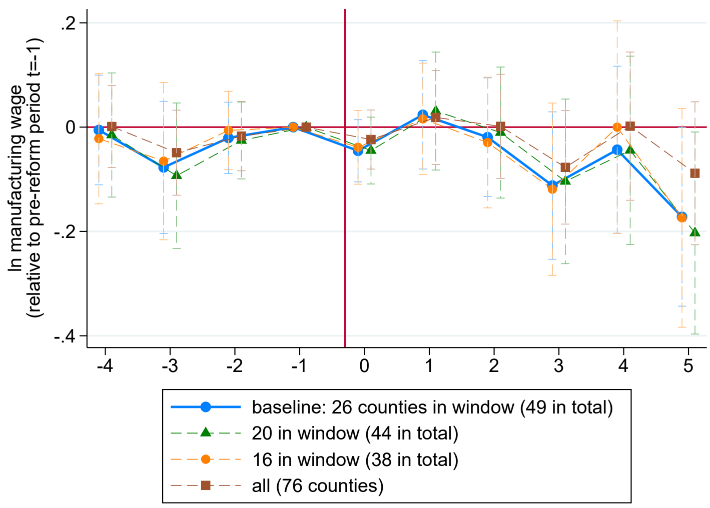
Source: SIAB. *Notes*: This figure plots coefficients along with 95 % confidence intervals of a regression of log low-skilled wages on leads and lags of a change in the maximum assistance rate. Treatment is discrete as in equation (2.1) or refined as in equation (2.4). The sample includes the 49 counties closest to cutoffs. State-year fixed effects are included. Clustering of standard errors is at the county level.

Figure B.7: Event study estimates: manufacturing employment (different cutoff samples, discrete treatment)



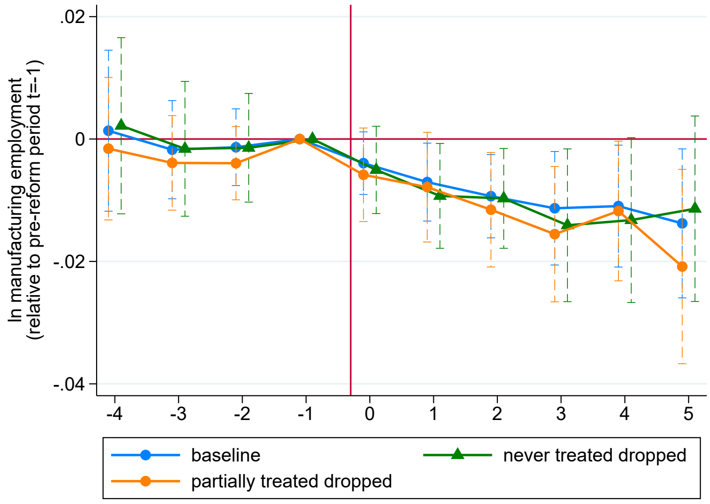
Source: BHP. *Notes*: This figure plots coefficients along with 95 % confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate as in equation (2.1). State-year fixed effects are included. Clustering of standard errors is at the county level.

Figure B.8: Event study estimates: low-skilled manufacturing wages (different cutoff samples, discrete treatment)



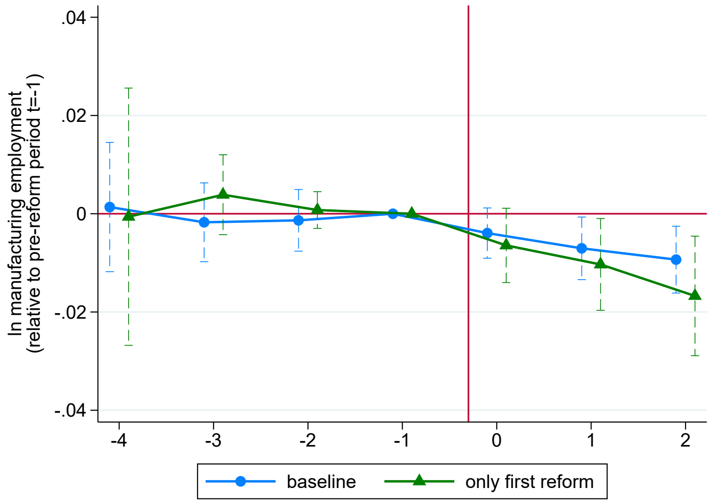
Source: SIAB. *Notes*: This figure plots coefficients along with 95 % confidence intervals of a regression of log low-skilled wages on leads and lags of a change in the maximum assistance rate as in equation (2.1). State-year fixed effects are included. Clustering of standard errors is at the county level.

Figure B.9: Event study estimates: manufacturing employment (further robustness checks)



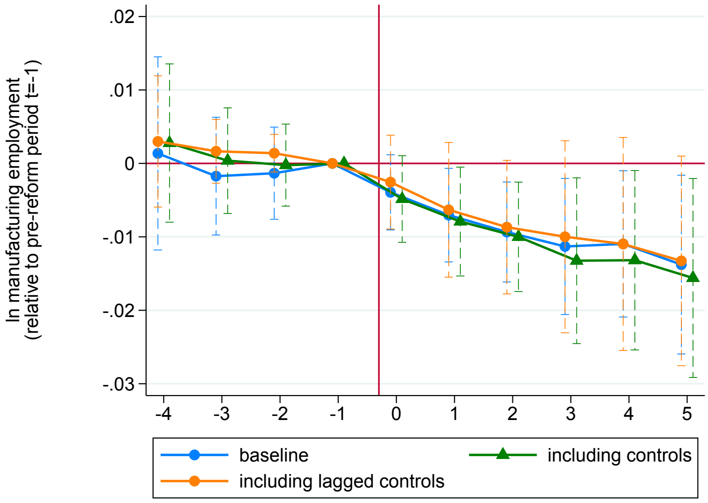
Source: BHP. *Notes*: This figure plots coefficients along with 95 % confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate as in equation (2.4). The sample includes the 49 counties closest to cutoffs. State-year fixed effects are included. Clustering of standard errors is at the county level.

Figure B.10: Event study estimates: manufacturing employment (only first reform)



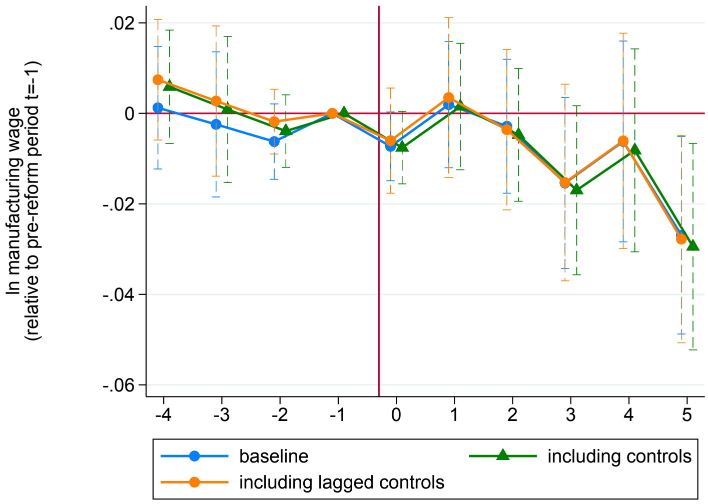
Source: BHP. *Notes*: This figure plots coefficients along with 95 % confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate as in equation (2.4). The sample includes all counties in East Germany. State-year fixed effects are included. Clustering of standard errors is at the county level.

Figure B.11: Event study estimates: manufacturing employment (including controls)



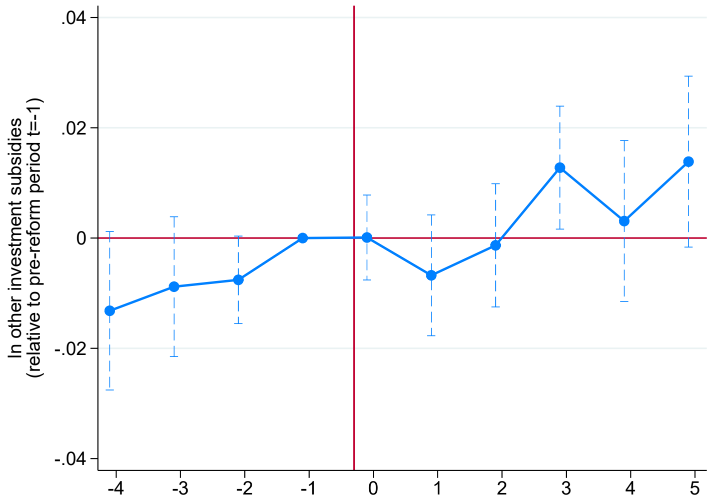
Source: BHP. *Notes*: This figure plots coefficients along with 95 % confidence intervals of a regression of log manufacturing employment on leads and lags of a change in the maximum assistance rate as in equation (2.4). Controls are either contemporaneous or lagged by one year. The sample includes the 49 counties closest to cutoffs. State-year fixed effects are included. Clustering of standard errors is at the county level.

Figure B.12: Event study estimates: low-skilled manufacturing wages (including controls)



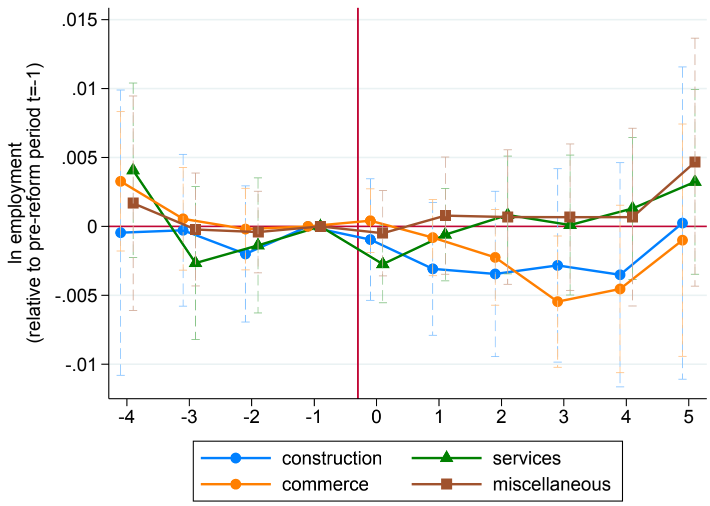
Source: SIAB. *Notes*: This figure plots coefficients along with 95 % confidence intervals of a regression of log low-skilled wages on leads and lags of a change in the maximum assistance rate as in equation (2.4). Controls are either contemporaneous or lagged by one year. The sample includes the 49 counties closest to cutoffs. State-year fixed effects are included. Clustering of standard errors is at the county level.

Figure B.13: Event study estimates: subsidies received by municipalities at county level



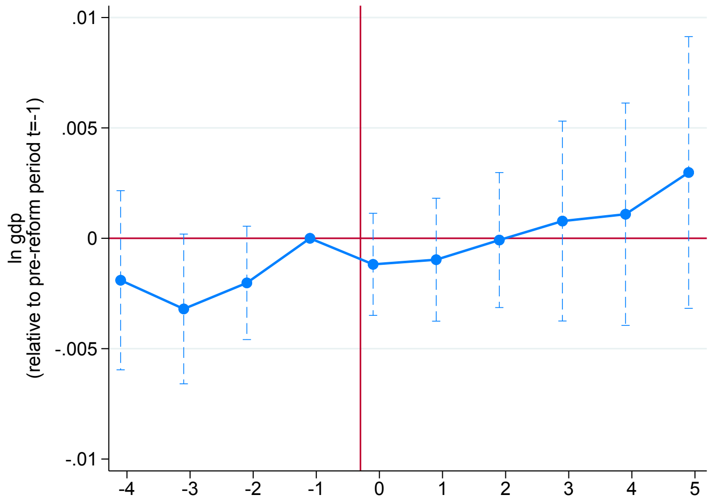
Source: Statistical Offices of German States (Laender). *Notes*: This figure plots coefficients along with 95 % confidence intervals of a regression of log municipal subsidies on leads and lags of a change in the maximum assistance rate as in equation (2.4). The sample includes the 49 counties closest to cutoffs. State-year fixed effects are included. Clustering of standard errors is at the county level.

Figure B.14: Event study estimates: employment by industry



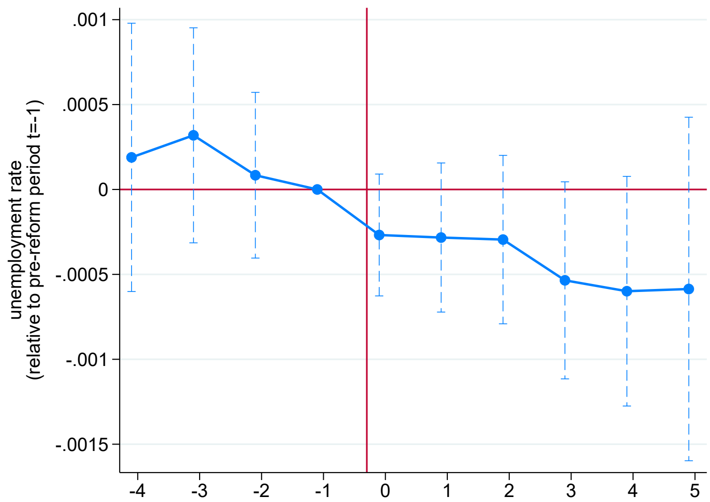
Source: BHP. *Notes*: This figure plots coefficients along with 95 % confidence intervals of a regression of log industry employment on leads and lags of a change in the maximum assistance rate as in equation (2.4). "Miscellaneous" represents remaining industries other than manufacturing (agriculture, energy and mining are excluded). The sample includes the 49 counties closest to cutoffs. State-year fixed effects are included. Clustering of standard errors is at the county level.

Figure B.15: Event study estimates: GDP at county level



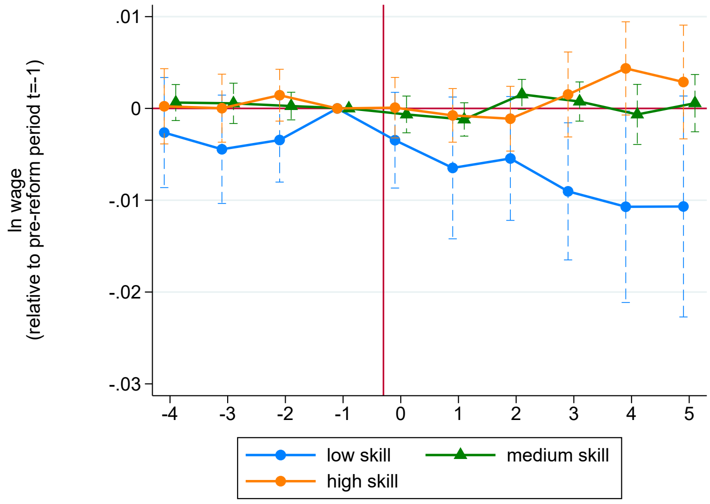
Source: Statistical Offices of German States (Laender). *Notes*: This figure plots coefficients along with 95 % confidence intervals of a regression of log GDP on leads and lags of a change in the maximum assistance rate as in equation (2.4), controlling for log population. The sample includes the 49 counties closest to cutoffs. State-year fixed effects are included. Clustering of standard errors is at the county level.

Figure B.16: Event study estimates: unemployment rate at county level



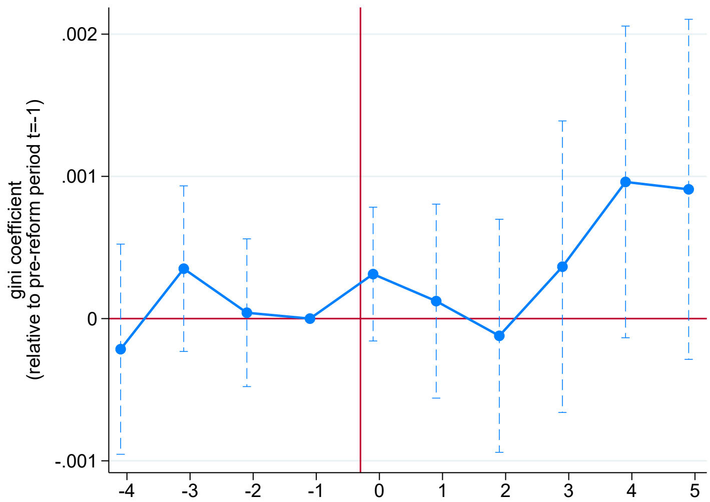
Source: Statistical Offices of German States (Laender). *Notes*: This figure plots coefficients along with 95 % confidence intervals of a regression of the unemployment rate (divided by 100) on leads and lags of a change in the maximum assistance rate as in equation (2.4). The sample includes the 49 counties closest to cutoffs. State-year fixed effects are included. Clustering of standard errors is at the county level.

Figure B.17: Event study estimates: wages by skill in sectors other than manufacturing



Source: SIAB. *Notes*: This figure plots coefficients along with 95 % confidence intervals of a regression of log wages by skill on leads and lags of a change in the maximum assistance rate as in equation (2.4). The sample includes the 49 counties closest to cutoffs. State-year fixed effects are included. Clustering of standard errors is at the county level.

Figure B.18: Event study estimates: Gini coefficient



Source: SIAB. *Notes*: This figure plots coefficients along with 95 % confidence intervals of a regression of the Gini coefficient on leads and lags of a change in the maximum assistance rate as in equation (2.4). The sample includes the 49 counties closest to cutoffs. State-year fixed effects are included. Clustering of standard errors is at the county level.

B.3 Additional tables

Table B.6: Event study estimates: first stage at county level

	(1) ln subsidized investment	(2) ln GRW subsidies
F4	-.0254 (.0169)	-.0177 (.0165)
F3	-.0079 (.0174)	-.004 (.0179)
F2	-.0251 (.0156)	-.0134 (.0155)
L0	-.0639 (.0217)	-.0772 (.0216)
L1	-.0657 (.0176)	-.0917 (.0178)
L2	-.0364 (.0221)	-.0659 (.0208)
L3	-.0731 (.0238)	-.0992 (.0224)
L4	-.0436 (.0239)	-.0679 (.0238)
L5	-.0628 (.0256)	-.0916 (.0247)
Obs	1351	1351

Notes: Standard errors in parentheses. See event study graph (figure 2.3) for detailed information.

Table B.7: Event study estimates: log employment by sector

	(1) manufacturing	(2) other	(3) overall
F4	.0014	.0046	.0038
	(.0067)	(.0025)	(.0024)
F3	-.0017	-.0003	-.0005
	(.004)	(.0016)	(.0016)
F2	-.0013	-.0001	-.0001
	(.0032)	(.0014)	(.0014)
L0	-.0039	-.0014	-.0018
	(.0026)	(.0011)	(.0009)
L1	-.007	-.0002	-.0015
	(.0032)	(.0014)	(.0012)
L2	-.0093	.0004	-.0016
	(.0034)	(.0016)	(.0014)
L3	-.0113	-.0005	-.002
	(.0047)	(.0017)	(.0017)
L4	-.011	-.0001	-.0019
	(.005)	(.002)	(.002)
L5	-.0138	.0044	.0012
	(.0062)	(.0026)	(.0023)
Obs	882	882	882

Notes: Standard errors in parentheses. See event study graph (figure 2.7) for detailed information.

Table B.8: Event study estimates: log manufacturing employment (long run)

	(1)
F4	.0029
	(.0058)
F3	-.0011
	(.0042)
F2	-.0013
	(.0032)
L0	-.0043
	(.0029)
L1	-.0079
	(.0034)
L2	-.0101
	(.004)
L3	-.0124
	(.0051)
L4	-.0124
	(.0065)
L5	-.0141
	(.0073)
L6	-.0146
	(.008)
L7	-.0153
	(.012)
L8	-.0182
	(.013)
L9	-.0175
	(.0135)
L10	-.0179
	(.0187)
Obs	882

Notes: Standard errors in parentheses. See event study graph (figure 2.4) for detailed information.

Table B.9: Event study estimates: log number of manufacturing establishments

	(1)
F4	.0019
	(.0021)
F3	-.0009
	(.0014)
F2	-.0004
	(.0012)
L0	-.0008
	(.001)
L1	-.0013
	(.0012)
L2	-.002
	(.0014)
L3	-.0021
	(.0017)
L4	-.003
	(.0019)
L5	-.0028
	(.0024)
Obs	882

Notes: Standard errors in parentheses. See event study graph (figure 2.5) for detailed information.

Table B.10: Event study estimates: log wages by skill in manufacturing

	(1) low skill	(2) medium skill	(3) high skill
F4	.0012	.001	.0038
	(.0069)	(.0023)	(.0036)
F3	-.0024	.0002	.0033
	(.0081)	(.0014)	(.0032)
F2	-.0062	.0024	.0001
	(.0042)	(.0012)	(.0028)
L0	-.0073	-.0004	-.0025
	(.0038)	(.0014)	(.0027)
L1	.0019	.0027	-.0032
	(.0071)	(.0012)	(.003)
L2	-.0028	.0026	.0021
	(.0075)	(.0013)	(.0035)
L3	-.0154	.0014	.0057
	(.0096)	(.002)	(.0038)
L4	-.0062	.0017	-.0043
	(.0113)	(.0019)	(.005)
L5	-.0269	.0031	.0012
	(.0111)	(.0023)	(.0048)
Obs	882	882	882

Notes: Standard errors in parentheses. See event study graph (figure 2.6) for detailed information.

Table B.11: Event study estimates: measures of inequality

	(1) gini coefficient	(2) p75p50	(3) p75p25
F4	-.0002	.0039	-.0025
	(.0003)	(.0019)	(.0153)
F3	.0004	.0022	.0193
	(.0002)	(.0017)	(.0155)
F2	0	.0027	.008
	(.0002)	(.0015)	(.0133)
L0	.0003	.0005	.0115
	(.0002)	(.0011)	(.0113)
L1	.0001	.0005	.0058
	(.0003)	(.0013)	(.0164)
L2	-.0001	-.0003	-.0018
	(.0004)	(.0015)	(.0185)
L3	.0004	.0027	.0155
	(.0005)	(.0021)	(.0287)
L4	.001	.0047	.0503
	(.0005)	(.0021)	(.0335)
L5	.0009	.0053	.0271
	(.0006)	(.0035)	(.0357)
Obs	882	882	882

Notes: Standard errors in parentheses. See event study graphs (figures 2.8 and B.18) for detailed information.

Table B.12: Event study estimates: log labor market region manufacturing employment

	(1)
F4	.0004
	(.006)
F3	-.0021
	(.0026)
F2	-.0004
	(.0021)
L0	-.0029
	(.0024)
L1	-.0039
	(.0028)
L2	-.0057
	(.0031)
L3	-.0059
	(.0039)
L4	-.0044
	(.0043)
L5	-.005
	(.0059)
Obs	558

Notes: Standard errors in parentheses. See event study graph (figure 2.9) for detailed information.

Table B.13: Event study estimates: log manufacturing employment by skill

	(1) low skill	(2) medium skill	(3) high skill
F4	.009	-.0014	.0059
	(.0094)	(.0065)	(.0083)
F3	.0053	-.0037	.0004
	(.0066)	(.0036)	(.006)
F2	.0018	-.0024	.0007
	(.0047)	(.0029)	(.0049)
L0	-.0018	-.0041	-.0044
	(.0033)	(.0028)	(.0029)
L1	-.0058	-.0062	-.0062
	(.0059)	(.0031)	(.0033)
L2	-.0102	-.008	-.0086
	(.0064)	(.0032)	(.0038)
L3	-.0117	-.01	-.007
	(.009)	(.0043)	(.0055)
L4	-.0082	-.0107	-.008
	(.0107)	(.0044)	(.006)
L5	-.0089	-.0131	-.0111
	(.0102)	(.0054)	(.0081)
Obs	882	882	882

Notes: Standard errors in parentheses. See event study graph (figure B.2) for detailed information.

Table B.14: Event study estimates: log manufacturing employment (different cutoff samples, refined treatment)

	(1) all counties	(2) 20 in window	(3) 16 in window
F4	.0005	.005	.0083
	(.0042)	(.0072)	(.0076)
F3	-.0026	0	.0015
	(.0025)	(.0045)	(.0046)
F2	-.0015	-.0001	.0012
	(.002)	(.0035)	(.0037)
L0	-.0019	-.0044	-.0058
	(.0022)	(.0028)	(.0029)
L1	-.0025	-.0081	-.0092
	(.0024)	(.0036)	(.0039)
L2	-.004	-.0109	-.012
	(.0028)	(.0038)	(.0042)
L3	-.004	-.0135	-.0117
	(.0036)	(.0052)	(.0052)
L4	-.0018	-.0132	-.0147
	(.0038)	(.0055)	(.0057)
L5	-.0022	-.0175	-.0214
	(.0039)	(.0066)	(.0069)
Obs	1368	792	684

Notes: Standard errors in parentheses. See event study graph (figure B.3) for detailed information.

Table B.15: Event study estimates: low-skilled log manufacturing wages (different cutoff samples, refined treatment)

	(1) all counties	(2) 20 in window	(3) 16 in window
F4	.0019	-.0009	-.0015
	(.0045)	(.0076)	(.0081)
F3	-.0038	-.0036	-.0014
	(.0051)	(.0089)	(.0094)
F2	-.0056	-.0067	-.0048
	(.0036)	(.0045)	(.0049)
L0	-.0053	-.0079	-.007
	(.0042)	(.0042)	(.0049)
L1	.0001	.0025	.0006
	(.0058)	(.0076)	(.0072)
L2	-.0006	-.0022	-.0051
	(.0063)	(.0082)	(.0082)
L3	-.0112	-.0144	-.0166
	(.007)	(.0106)	(.0111)
L4	-.0006	-.0069	-.0001
	(.0099)	(.0126)	(.0143)
L5	-.0155	-.0318	-.0283
	(.0088)	(.0125)	(.0138)
Obs	1368	792	684

Notes: Standard errors in parentheses. See event study graph (figure B.4) for detailed information.

Table B.16: Event study estimates: log manufacturing employment (different cutoff samples, discrete treatment)

	(1) baseline	(2) all counties	(3) 20 in window	(4) 16 in window
F4	-.0244	-.0216	-.0001	.0244
	(.046)	(.03)	(.049)	(.054)
F3	-.0336	-.0378	-.0214	-.0064
	(.036)	(.024)	(.04)	(.042)
F2	-.0214	-.0203	-.0127	-.0013
	(.031)	(.022)	(.035)	(.037)
L0	-.0273	-.0154	-.0286	-.0307
	(.016)	(.012)	(.019)	(.021)
L1	-.0494	-.0218	-.0536	-.0524
	(.02)	(.015)	(.023)	(.024)
L2	-.0666	-.0361	-.0733	-.0699
	(.018)	(.017)	(.02)	(.022)
L3	-.068	-.0279	-.0796	-.0605
	(.031)	(.025)	(.035)	(.033)
L4	-.0655	-.0158	-.0787	-.0823
	(.03)	(.024)	(.033)	(.035)
L5	-.0693	-.0039	-.091	-.107
	(.038)	(.034)	(.041)	(.044)
Obs	882	1368	792	684

Notes: Standard errors in parentheses. See event study graph (figure B.7) for detailed information.

Table B.17: Event study estimates: low-skilled log manufacturing wages (different cutoff samples, discrete treatment)

	(1) baseline	(2) all counties	(3) 20 in window	(4) 16 in window
F4	-.0055	.0012	-.0151	-.022
	(.053)	(.04)	(.06)	(.063)
F3	-.0772	-.049	-.0931	-.0652
	(.064)	(.041)	(.071)	(.076)
F2	-.0206	-.0172	-.0256	-.0064
	(.034)	(.033)	(.037)	(.038)
L0	-.0453	-.0238	-.0449	-.0387
	(.03)	(.028)	(.032)	(.036)
L1	.0236	.0186	.0308	.0158
	(.053)	(.046)	(.057)	(.054)
L2	-.0193	.0015	-.0104	-.0295
	(.058)	(.05)	(.064)	(.063)
L3	-.112	-.0771	-.104	-.119
	(.072)	(.055)	(.08)	(.084)
L4	-.0433	.0019	-.0445	-.0001
	(.081)	(.072)	(.092)	(.104)
L5	-.172	-.0884	-.203	-.174
	(.087)	(.069)	(.098)	(.107)
Obs	882	1368	792	684

Notes: Standard errors in parentheses. See event study graph (figure B.8) for detailed information.

Table B.18: Event study estimates: log manufacturing employment (further robustness checks)

	(1) never treated dropped	(2) partially treated dropped
F4	.0022	-.0016
	(.0073)	(.0059)
F3	-.0016	-.0039
	(.0056)	(.0039)
F2	-.0014	-.0039
	(.0045)	(.003)
L0	-.005	-.0058
	(.0036)	(.0039)
L1	-.0093	-.0079
	(.0043)	(.0045)
L2	-.0097	-.0115
	(.0041)	(.0047)
L3	-.0141	-.0155
	(.0063)	(.0056)
L4	-.0132	-.0118
	(.0068)	(.0058)
L5	-.0114	-.0208
	(.0077)	(.0081)
Obs	702	576

Notes: Standard errors in parentheses. See event study graph (figure B.9) for detailed information.

Table B.19: Event study estimates: log manufacturing employment (only first reform)

	(1)
F4	-.0006
	(.0133)
F3	.0039
	(.0041)
F2	.0008
	(.0019)
L0	-.0064
	(.0038)
L1	-.0103
	(.0047)
L2	-.0167
	(.0062)
Obs	456

Notes: Standard errors in parentheses. See event study graph (figure B.10) for detailed information.

Table B.20: Event study estimates: log manufacturing employment (including controls)

	(1) contemporaneous controls	(2) lagged controls
F4	.0028	.003
	(.0055)	(.0045)
F3	.0004	.0017
	(.0036)	(.0022)
F2	-.0002	.0014
	(.0028)	(.0013)
L0	-.0048	-.0025
	(.003)	(.0032)
L1	-.0079	-.0063
	(.0037)	(.0046)
L2	-.01	-.0087
	(.0038)	(.0046)
L3	-.0132	-.01
	(.0057)	(.0066)
L4	-.0132	-.011
	(.0062)	(.0074)
L5	-.0156	-.0133
	(.0069)	(.0072)
Obs	866	817

Notes: Standard errors in parentheses. See event study graph (figure B.11) for detailed information.

Table B.21: Event study estimates: low-skilled log manufacturing wages (including controls)

	(1) contemporaneous controls	(2) lagged controls
F4	.0059	.0074
	(.0063)	(.0068)
F3	.0009	.0027
	(.0082)	(.0084)
F2	-.0039	-.0018
	(.004)	(.0036)
L0	-.0076	-.006
	(.004)	(.0059)
L1	.0015	.0035
	(.0071)	(.009)
L2	-.0047	-.0036
	(.0074)	(.009)
L3	-.017	-.0153
	(.0095)	(.011)
L4	-.0082	-.0061
	(.0114)	(.0121)
L5	-.0294	-.0278
	(.0116)	(.0116)
Obs	866	817

Notes: Standard errors in parentheses. See event study graph (figure B.12) for detailed information.

Table B.22: Event study estimates: other outcomes

	(1) ln other subsidies	(2) unemployment rate	(3) ln gdp
F4	-.0132	.0002	-.0019
	(.0073)	(.0004)	(.002)
F3	-.0088	.0003	-.0032
	(.0064)	(.0003)	(.0017)
F2	-.0076	.0001	-.002
	(.004)	(.0002)	(.0013)
L0	.0001	-.0003	-.0012
	(.0039)	(.0001)	(.0011)
L1	-.0068	-.0003	-.001
	(.0055)	(.0002)	(.0014)
L2	-.0013	-.0003	-.0001
	(.0057)	(.0002)	(.0015)
L3	.0128	-.0005	.0008
	(.0056)	(.0002)	(.0023)
L4	.0031	-.0006	.0011
	(.0074)	(.0003)	(.0025)
L5	.0139	-.0006	.003
	(.0079)	(.0005)	(.0031)
Obs	795	882	882

Notes: Standard errors in parentheses. See event study graphs (figures B.13, B.15 and B.16) for detailed information.

Table B.23: Event study estimates: log employment by industry (different cutoff samples, discrete treatment)

	(1) construction	(2) commerce	(3) services	(4) miscellaneous
F4	-.0005	.0033	.0041	.0017
	(.0052)	(.0025)	(.0032)	(.0039)
F3	-.0003	.0006	-.0027	-.0002
	(.0028)	(.0019)	(.0028)	(.002)
F2	-.002	-.0002	-.0014	-.0004
	(.0025)	(.0015)	(.0025)	(.0015)
L0	-.001	.0004	-.0027	-.0005
	(.0022)	(.0011)	(.0014)	(.0015)
L1	-.0031	-.0008	-.0006	.0008
	(.0024)	(.0014)	(.0017)	(.0021)
L2	-.0034	-.0022	.0008	.0007
	(.003)	(.0017)	(.0021)	(.0024)
L3	-.0028	-.0055	.0001	.0007
	(.0035)	(.0024)	(.0025)	(.0027)
L4	-.0035	-.0045	.0013	.0007
	(.0041)	(.0031)	(.0026)	(.0032)
L5	.0002	-.001	.0032	.0047
	(.0057)	(.0043)	(.0034)	(.0045)
Obs	882	882	882	882

Notes: Standard errors in parentheses. See event study graph (figure B.14) for detailed information.

Table B.24: Event study estimates: log wages by skill in sectors other than manufacturing

	(1) low skill	(2) medium skill	(3) high skill
F4	-.0026	.0006	.0002
	(.003)	(.001)	(.002)
F3	-.0044	.0006	0
	(.003)	(.0011)	(.0018)
F2	-.0034	.0003	.0014
	(.0023)	(.0007)	(.0014)
L0	-.0035	-.0007	.0001
	(.0026)	(.001)	(.0016)
L1	-.0065	-.0012	-.0008
	(.0039)	(.0009)	(.0014)
L2	-.0054	.0015	-.0011
	(.0034)	(.0008)	(.0018)
L3	-.009	.0008	.0015
	(.0038)	(.001)	(.0023)
L4	-.0107	-.0007	.0044
	(.0053)	(.0016)	(.0025)
L5	-.0107	.0006	.0029
	(.0061)	(.0015)	(.0031)
Obs	882	882	882

Notes: Standard errors in parentheses. See event study graph (figure B.17) for detailed information.

Appendix C

Appendix to chapter 3

C.1 Additional results

Table C.1: Event study estimates: parties represented

	(1)	(2)	(3)
reform_F3	0.112	-0.011	-0.099
	(0.079)	(0.084)	(0.299)
reform_F2	0.021	-0.052	0.135
	(0.058)	(0.060)	(0.240)
reform_L0	0.261***	0.073**	1.525***
	(0.039)	(0.034)	(0.149)
reform_L1	0.310***	0.083*	1.566***
	(0.047)	(0.043)	(0.153)
reform_L2	0.611***	0.478***	1.947***
	(0.075)	(0.111)	(0.178)
reform_L3	0.684***	0.596***	2.065***
	(0.091)	(0.133)	(0.196)
Size-Year-FE	yes	yes	yes
Size of municipalities	all	small	large
Observations	9689	6825	2864
Adjusted R^2	0.290	0.018	0.490

Notes: Standard errors in parentheses (* $p < .1$, ** $p < .05$, *** $p < .01$). Sample: *sample 1*. Small municipalities: 10 000 inhabitants or fewer. Clustering of standard errors is at the municipality level.

Table C.2: Difference-in-differences estimates: effective number of parties and Herfindahl index

	(1) effective	(2) effective	(3) herfindahl	(4) herfindahl
reform	0.020		-0.003	
	(0.028)		(0.005)	
small × reform		-0.012		0.001
		(0.031)		(0.005)
large × reform		0.231***		-0.029***
		(0.066)		(0.006)
Size-Year-FE	yes	yes	yes	yes
Observations	9689	9689	9689	9689
Adjusted R^2	0.157	0.158	0.076	0.077

Notes: Standard errors in parentheses (* $p < .1$, ** $p < .05$, *** $p < .01$). Sample: *sample 1*. Small municipalities: 10 000 inhabitants or fewer. Clustering of standard errors is at the municipality level.

Table C.3: Difference-in-differences estimates: log revenue

	(1)	(2)
reform	0.024***	
	(0.005)	
small × reform		0.018***
		(0.005)
large × reform		0.060***
		(0.015)
Size-Year-FE	yes	yes
Linear state trends by size	yes	yes
Observations	172912	172912
Adjusted R^2	0.108	0.108

Notes: Standard errors in parentheses (* $p < .1$, ** $p < .05$, *** $p < .01$). Sample: *sample 2*. Small municipalities: 10 000 inhabitants or fewer. Clustering of standard errors is at the municipality level.

Table C.4: Difference-in-differences estimates: log revenue (excluding credit)

	(1)	(2)
reform	0.026***	
	(0.005)	
small × reform		0.020***
		(0.005)
large × reform		0.060***
		(0.015)
Size-Year-FE	yes	yes
Linear state trends by size	yes	yes
Observations	171342	171342
Adjusted R^2	0.112	0.112

Notes: Standard errors in parentheses (* $p < .1$, ** $p < .05$, *** $p < .01$). Sample: *sample 2*. Small municipalities: 10 000 inhabitants or fewer. Clustering of standard errors is at the municipality level.

Table C.5: Difference-in-differences estimates: log expenditure (further robustness checks)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
small × reform	0.021***	0.021***	0.022***	0.021***	0.125***	-0.006	-0.008
	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
large × reform	0.077***	0.095***	0.063***	0.063***	0.079***	0.019**	0.066***
	(0.025)	(0.030)	(0.016)	(0.017)	(0.016)	(0.008)	(0.017)
Size-Year-FE	yes	yes	yes	yes	yes	yes	yes
Linear state trends by size	yes	yes	yes	yes	yes	yes	yes
Excluded state	NW	HE	SL	TH	RP	MV	SH
Observations	168153	166469	173019	161147	134463	165474	154789
Adjusted R^2	0.092	0.092	0.091	0.089	0.126	0.087	0.081

Notes: Standard errors in parentheses (* $p < .1$, ** $p < .05$, *** $p < .01$). Sample: *sample 2*. Small municipalities: 10 000 inhabitants or fewer. Clustering of standard errors is at the municipality level.

Table C.6: Difference-in-differences estimates: composition of log expenditure

	(1) personell	(2) material	(3) investment
small × reform	0.111***	0.063***	-0.046**
	(0.007)	(0.006)	(0.022)
large × reform	0.063***	0.049***	0.135***
	(0.016)	(0.017)	(0.026)
Size-Year-FE	yes	yes	yes
Linear state trends by size	yes	yes	yes
Observations	173555	173574	169403
Adjusted R^2	0.274	0.171	0.103

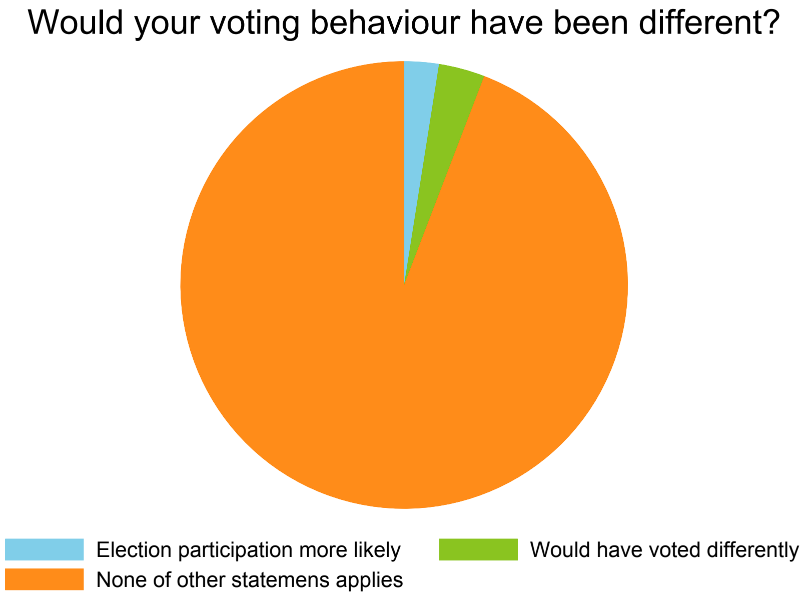
Notes: Standard errors in parentheses (* $p < .1$, ** $p < .05$, *** $p < .01$). Sample: *sample 2*. Small municipalities: 10 000 inhabitants or fewer. Clustering of standard errors is at the municipality level.

C.2 Survey evidence from the German Internet Panel

In wave 30 of the GIP (Blom et al., 2018) I have asked the following questions related to *municipal* elections. Results can be found below (see also figure 3.7 above).

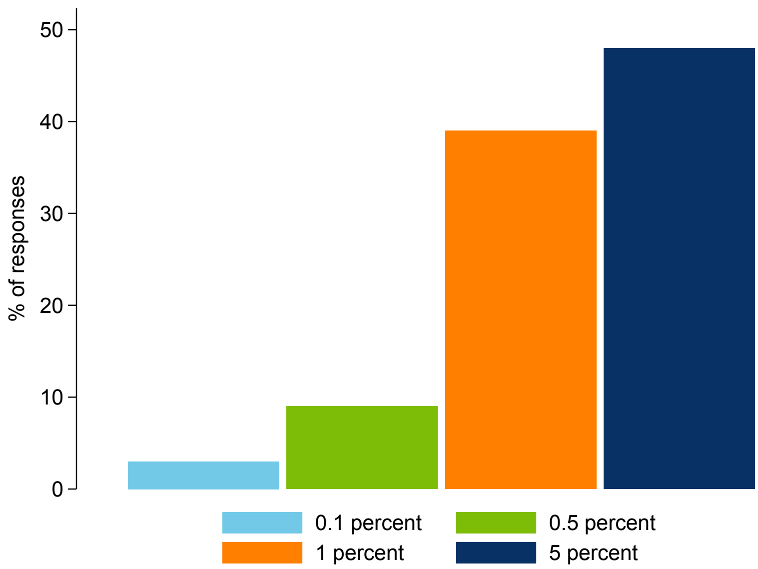
- At the last federal election in 2013, a five percent electoral threshold was in place. Suppose such a threshold hadn't existed: does one of the following statements apply to you?
  - I would have been more likely to cast a vote.
  - I would have voted for a different party.
  - None of the above statements applies.
- An electoral threshold is the minimum vote share that a party needs to obtain in order to be able to receive seats in a parliament or council. In your state of residence, does there exist an electoral threshold for municipal elections?
- At the European election in 2014, Germany was eligible for approximately 100 seats in the European Parliament and no electoral threshold applied. What vote share do you think a party had to achieve at a minimum in order to obtain a seat?
  - 0.1 percent
  - 0.5 percent
  - 1 percent
  - 5 percent

Figure C.1: Counterfactual behavior in federal elections without explicit threshold



Source: Wave 30 of the German Internet Panel (Blom et al., 2018)

Figure C.2: Presumed implicit threshold for Germany in European Parliament election of 2014



Source: Wave 30 of the German Internet Panel (Blom et al., 2018)

C.3 Data sources and election dates

C.3.1 Election data sources

Municipal election data used in sample 1 is available publicly via state statistical offices:

- Nordrhein-Westfalen: `http://www.it.nrw/`
- Baden-Württemberg: `http://www.statistik-bw.de/`

- Saarland: http://www.saarland.de/62919.htm
- Sachsen: http://www.statistik.sachsen.de/
- Thüringen: http://wahlen.thueringen.de/

C.3.2 Election dates

Table C.7: Election years after 1990

State	Years				
Schleswig-Holstein (SH)	**1994**	**1998**	**2003**	2008	2013
Niedersachsen (NI)	1991	1996	2001	2006	2011
Nordrhein-Westfalen (NW)	**1994**	1999	2004	2009	2014
Hessen (HE)	**1993**	**1997**	2001	2006	2011
Rheinland-Pfalz (RP)	**1994**	**1999**	**2004**	2009	2014
Baden-Württemberg (BW)	1994	1999	2004	2009	2014
Bayern (BY)		1996	2002	2008	2014
Saarland (SL)	**1994**	**1999**	**2004**	2009	2014
Brandenburg (BB)	1993	1998	2003	2008	2014
Mecklenburg-Vorpommern (MV)	**1994**	**1999**	2004	2009	2014
Sachsen (SN)	1994	1999	2004	2009	2014
Sachsen-Anhalt (ST)	1994	1999	2004	2009	2014
Thüringen (TH)	**1994**	**1999**	**2004**	2009	2014

Notes: Elections with threshold in place in bold.

Bibliography

Albouy, D. (2009): "The unequal geographic burden of federal taxation," *Journal of Political Economy*, 117, 635–667.

——— (2012): "Evaluating the efficiency and equity of federal fiscal equalization," *Journal of Public Economics*, 96, 824–839.

Alder, S., L. Shao, and F. Zilibotti (2016): "Economic reforms and industrial policy in a panel of Chinese cities," *Journal of Economic Growth*, 21, 305–349.

Angrist, J. D. and J.-S. Pischke (2008): *Mostly harmless econometrics: An empiricist's companion*, Princeton University Press.

Antoni, M., P. vom Berge, and A. Ganzer (2016): "Sample of Integrated Labour Market Biographies 1975-2014," Tech. rep., FDZ-Datenreport, 04/2016 (en), Nuremberg.

Ashworth, J., B. Geys, B. Heyndels, and F. Wille (2014): "Competition in the political arena and local government performance," *Applied Economics*, 46, 2264–2276.

Bakhshi, S., M. Shakeri, M. R. Olfert, M. D. Partridge, and S. Weseen (2009): "Do Local Residents Value Federal Transfers? Evidence from Interprovincial Migration in Canada," *Public Finance Review*, 37, 235–268.

Baskaran, T. and M. L. da Fonseca (2016a): "Electoral competition and endogenous political institutions: quasi-experimental evidence from Germany," *Journal of Economic Behavior & Organization*, 122, 43–61.

——— (2016b): "Electoral thresholds and political representation," *Public Choice*, 169, 117–136.

Batten, A. and A. Martina (2007): "Diseases dominate," in *Institutions and Market Economies*, Springer, 186–221.

Becker, S. O., P. H. Egger, and M. Von Ehrlich (2010): "Going NUTS: The effect of EU Structural Funds on regional performance," *Journal of Public Economics*, 94, 578–590.

——— (2012): "Too much of a good thing? On the growth effects of the EU's regional policy," *European Economic Review*, 56, 648–668.

——— (2013): "Absorptive capacity and the growth and investment effects of regional transfers: A regression discontinuity design with heterogeneous treatment effects," *American Economic Journal: Economic Policy*, 5, 29–77.

Bergstrom, T. C. and R. P. Goodman (1973): "Private demands for public goods," *The American Economic Review*, 63, 280–296.

Bertrand, M., E. Duflo, and S. Mullainathan (2004): "How much should we trust differences-in-differences estimates?" *The Quarterly Journal of Economics*, 119, 249–275.

Besley, T. and A. Case (2003): "Political Institutions and Policy Choices: Evidence from the United States," *Journal of Economic Literature*, 41, 7–73.

Birch, S. (2001): "Electoral systems and party systems in Europe East and West," *Perspectives on European Politics and Society*, 2, 355–377.

Blom, A. G., D. Bossert, F. Funke, F. Gebhard, A. Holthausen, and U. Krieger (2016): "SFB 884 Political Economy of Reforms, Universität Mannheim: German Internet Panel, Welle 3 (Januar 2013)," .

Blom, A. G., B. Felderer, J. Herzing, U. Krieger, and T. Rettig (2018): "SFB 884 Political Economy of Reforms, Universität Mannheim: German Internet Panel, Welle 30 (Juli 2017)," .

Blom, A. G., C. Gathmann, and U. Krieger (2015): "Setting Up an Online Panel Representative of the General Population," *Field Methods*, 27, 391–408.

Boadway, R. (2004): "The theory and practice of equalization," *CESifo Economic Studies*, 50, 211–254.

Boadway, R. and F. Flatters (1982): "Efficiency and equalization payments in a federal system of government: A synthesis and extension of recent results," *Canadian Journal of Economics*, 613–633.

Bogumil, J., D. H. Gehne, B. Garske, M. Seuberlich, and J. Hafner (2015): *Auswirkungen der Aufhebung der kommunalen Sperrklausel auf das kommunalpolitische Entscheidungssystem in Nordrhein-Westfalen*, Zentrum für Interdisziplinäre Regionalforschung (ZEFIR), Ruhr-Universität Bochum.

Borusyak, K. and X. Jaravel (2017): "Revisiting Event Study Designs," Tech. rep.

Brennan, G. and L. E. Lomasky (1993): "Democracy and Decision: The Pure Theory of Electoral Politics Cambridge," .

Brülhart, M. and N. A. Mathys (2008): "Sectoral agglomeration economies in a panel of European regions," *Regional Science and Urban Economics*, 38, 348 – 362.

Bronzini, R. and G. de Blasio (2006): "Evaluating the impact of investment incentives: The case of Italy's Law 488/1992," *Journal of Urban Economics*, 60, 327–349.

Bucovetsky, S. and M. Smart (2006): "The efficiency consequences of local revenue equalization: Tax competition and tax distortions," *Journal of Public Economic Theory*, 8, 119–144.

Buettner, T. (2006): "The incentive effect of fiscal equalization transfers on tax policy," *Journal of Public Economics*, 90, 477 – 497.

Buettner, T. and F. Holm-Hadulla (2008): "Fiscal Equalization: The Case of German Municipalities," *CESifo DICE Report*, 6, 16–20.

Bundesverfassungsgericht (2008): "Urteil des Zweiten Senats vom 13. Februar 2008," 2 BvK 1/07 - Rn. (1-149), Karlsruhe.

Burda, M. C. (2006): "Factor reallocation in Eastern Germany after reunification," *American Economic Review*, 96, 368–374.

Burda, M. C. and J. Hunt (2001): "From reunification to economic integration: productivity and the labor market in Eastern Germany," *Brookings Papers on Economic Activity*, 2001, 1–71.

Busso, M., J. Gregory, and P. Kline (2013): "Assessing the Incidence and Efficiency of a Prominent Place Based Policy," *The American Economic Review*, 103, 897–947.

Ciccone, A. and R. E. Hall (1996): "Productivity and the Density of Economic Activity," *The American Economic Review*, 54–70.

Dettmann, E., M. Brachert, and M. Titze (2016): "Identifying the Effects of Place-Based Policies-Causal Evidence from Germany," CESifo WP No. 5901.

Deutsche Bundesbank (2016): "Gemeindefinanzen: Entwicklung und ausgewählte Aspekte," Monatsbericht Oktober 2016.

Deutscher Bundestag (1996): "Fuenfundzwanzigster Rahmenplan der Gemeinschaftsaufgabe 'Verbesserung der regionalen Wirtschaftsstruktur' für den Zeitraum 1996 bis 1999 (2000)," Drucksache 13/4291, Bonn.

——— (1997): "Sechsundzwanzigster Rahmenplan der Gemeinschaftsaufgabe 'Verbesserung der regionalen Wirtschaftsstruktur' für den Zeitraum 1997 bis 2000 (2001)," Drucksache 13/7205, Bonn.

——— (2000): "Neunundzwanzigster Rahmenplan der Gemeinschaftsaufgabe 'Verbesserung der regionalen Wirtschaftsstruktur' für den Zeitraum 2000 bis 2003 (2004)," Drucksache 14/3250, Berlin.

——— (2007): “Sechsunddreißigster Rahmenplan der Gemeinschaftsaufgabe ‘Verbesserung der regionalen Wirtschaftsstruktur’ für den Zeitraum 2007 bis 2010,” Drucksache 16/5215, Berlin.

Diamond, R. (2016): “The determinants and welfare implications of US workers’ diverging location choices by skill: 1980-2000,” *American Economic Review*, 106, 479–524.

Eeckhout, J. (2004): “Gibrat’s law for (all) cities,” *American Economic Review*, 94, 1429–1451.

Egger, P. and M. Koethenbuerger (2010): “Government spending and legislative organization: Quasi-experimental evidence from Germany,” *American Economic Journal: Applied Economics*, 2, 200–212.

Ehrlich, M. V. and T. Seidel (2018): “The Persistent Effects of Place-Based Policy: Evidence from the West-German Zonenrandgebie,” *American Economic Journal: Economic Policy (forthcoming).*

Flatters, F., V. Henderson, and P. Mieszkowski (1974): “Public goods, efficiency, and regional fiscal equalization,” *Journal of Public Economics*, 3, 99–112.

Fuest, C., A. Peichl, and S. Siegloch (2018): “Do higher corporate taxes reduce wages? Micro evidence from Germany,” *American Economic Review*, 108, 393–418.

Glaeser, E. and D. Maré (2001): “Cities and Skills,” *Journal of Labor Economics*, 19, 316–42.

Glaeser, E. L. (2008): *Cities, Agglomeration economies and Spatial Equilibrium. The Lindahl Lectures*, Oxford University Press.

Glaeser, E. L. and J. D. Gottlieb (2008): “The Economics of Place-Making Policies,” *Brookings Papers on Economic Activity*, 155–239.

Harberger, A. C. (1964): “The measurement of waste,” *The American Economic Review*, 54, 58–76.

Heinemann, F. (2017): “Was Katalonien lehrt,” *Sueddeutsche Zeittung*, October 5.

Henkel, M., T. Seidel, and J. Suedekum (2018): “Fiscal Transfers in the Spatial Economy,” *London, Centre for Economic Policy Research.*

Hentze, T. (2017): “Die Abschaffung des Länderfinanzausgleichs,” *IW policy paper 16/2017.*

Hirsch, B., E. J. Jahn, and M. Oberfichtner (2016): “The urban wage premium in imperfect labour markets,” Tech. rep.

Janeba, E. and W. Peters (2000): "Implikationen des Kommunalen Finanzausgleichs auf den Standort-und Steuerwettbewerb (Implications of Intergovernmental Revenue Sharing on Tax Competition)," *Beihefte der Konjunkturpolitik (Applied Economics Quarterly)*, 50, 35–53.

Jarman, F. (2016): "Essays in Collective Decision Making," Dissertation (University of Mannheim).

Kawka, R. (2010): "Regionale Preisunterschiede in den alten und neuen Ländern," *ifo Dresden berichtet*, 17, 5–16.

Kline, P. and E. Moretti (2014a): "Local Economic Development, Agglomeration Economies, and the Big Push: 100 Years of Evidence from the Tennessee Valley Authority," *The Quarterly Journal of Economics*, 129, 275–331.

——— (2014b): "People, Places, and Public Policy: Some Simple Welfare Economics of Local Economic Development Programs," *Annual Review of Economics*, 6, 629–662.

Kopfermann, K. (1991): *Mathematische Aspekte der Wahlverfahren: Mandatsverteilung bei Abstimmungen*, Mannheim: BI-Wissenschaftsverlag.

Köthenbürger, M. (2002): "Tax competition and fiscal equalization," *International Tax and Public Finance*, 9, 391–408.

——— (2007): "Ex-post redistribution in a federation: Implications for corrective policy," *Journal of Public Economics*, 91, 481–496.

Laakso, M. and R. Taagepera (1979): ""Effective" number of parties: a measure with application to West Europe," *Comparative Political Studies*, 12, 3–27.

Land Hessen (2000): "Gesetz zur Stärkung der Bürgerbeteiligung und kommunalen Selbstverwaltung vom 23.12.1999," Gesetz- und Verordnungsblatt für das Land Hessen I 2000 (Nr. 1, S. 2 - 12).

Land Rheinland-Pfalz (2008): "Fünfzehntes Landesgesetz zur Änderung des Kommunalwahlgesetzes vom 28.05.2008," Gesetz- und Verordnungsblatt für das Land Rheinland-Pfalz 2008 (Nr. 7, S. 79 - 81).

Landesbetrieb Information und Technik Nordrhein-Westfalen (2003-2011): "Statistik lokal - Daten für die Gemeinden und Kreise Deutschlands," CD-/DVD-Reihe, Düsseldorf.

Landesverfassungsgericht Mecklenburg-Vorpommern (2000): "Urteil vom 14. Dezember 2000," Az LVerfG 4/99.

Landtag des Saarlandes (2008): "Plenarprotokoll der 56. Sitzung des Landtages des Saarlandes vom 20. August 2008," Drucksache 13/1997, Saarbrücken.

Landtag Nordrhein-Westfalen (2016): "Gesetz zur Änderung der Verfassung für das Land Nordrhein-Westfalen und wahlrechtlicher Vorschriften (Kommunalvertretungsstärkungsgesetz)," Drucksache 16/12134, Düsseldorf.

Lichter, A., A. Peichl, and S. Siegloch (2015): "The own-wage elasticity of labor demand: A meta-regression analysis," *European Economic Review*, 80, 94–119.

Lijphart, A. and D. Aitkin (1994): *Electoral systems and party systems: A study of twenty-seven democracies, 1945-1990*, Oxford University Press.

Lijphart, A. and R. W. Gibberd (1977): "Thresholds and payoffs in list systems of proportional representation," *European Journal of Political Research*, 5, 219–244.

Lizzeri, A. and N. Persico (2005): "A drawback of electoral competition," *Journal of the European Economic Association*, 3, 1318–1348.

Marshall, A. (1920): *Principles of Economics: An Introductory Volume, 8th ed.*, London: The Macmillan Press.

McCrary, J. (2007): "The effect of court-ordered hiring quotas on the composition and quality of police," *American Economic Review*, 97, 318–353.

Moretti, E. (2011): "Local labor markets," *Handbook of Labor Economics*, 4, 1237–1313.

Moser, R. G. (1999): "Electoral systems and the number of parties in postcommunist states," *World Politics*, 51, 359–384.

Neumark, D. and J. Kolko (2010): "Do enterprise zones create jobs? Evidence from California's enterprise zone program," *Journal of Urban Economics*, 68, 1–19.

Neumark, D. and H. Simpson (2015): "Place-based policies," in *Handbook of Regional and Urban Economics*, Elsevier, vol. 5, 1197–1287.

Overman, H. G. (2018): "Some causal effects of an industrial policy," *American Economic Review (forthcoming).*

Pellicer, M. and E. Wegner (2014): "The mechanical and psychological effects of legal thresholds," *Electoral Studies*, 33, 258–266.

Pettersson-Lidbom, P. (2012): "Does the size of the legislature affect the size of government? Evidence from two natural experiments," *Journal of Public Economics*, 96, 269–278.

Roback, J. (1982): "Wages, rents, and the quality of life," *The Journal of Political Economy*, 90, 1257–1278.

Rosen, S. (1979): "Wage-based indexes of urban quality of life," in *Current Issues in Urban Economics*, ed. by P. N. Miezkowski and M. R. Straszheim, Johns Hopkins University Press, Baltimore, MD, 74–104.

Rosenthal, S. S. and W. C. Strange (2004): "Chapter 49 - Evidence on the Nature and Sources of Agglomeration Economies," in *Cities and Geography*, ed. by J. V. Henderson and J.-F. Thisse, Elsevier, vol. 4 of *Handbook of Regional and Urban Economics*, 2119 – 2171.

Sander, N. (2014): "Internal migration in Germany, 1995-2010," *Comparative Population Studies*, 39, 217–246.

Schaltegger, C. A. and L. P. Feld (2009): "Do large cabinets favor large governments? Evidence on the fiscal commons problem for Swiss Cantons," *Journal of Public Economics*, 93, 35–47.

Schmucker, A., S. Seth, J. Ludsteck, J. Eberle, and A. Ganzer (2016): "Establishment History Panel 1975-2014," Tech. rep., FDZ-Datenreport, 03/2016 (en), Nuremberg.

Stock, J. H. and M. Yogo (2002): "Testing for weak instruments in linear IV regression," Tech. rep.

Suarez Serrato, J. C. and O. Zidar (2016): "Who benefits from state corporate tax cuts? A local labor markets approach with heterogeneous firms," *American Economic Review*, 106, 2582–2624.

Sundaram, R. K. (1996): *A first course in optimization theory*, Cambridge University Press.

Tsebelis, G. (1995): "Decision making in political systems: Veto players in presidentialism, parliamentarism, multicameralism and multipartyism," *British Journal of Political Science*, 25, 289–325.

Uhlig, H. (2006): "Regional labor markets, network externalities and migration: The case of German reunification," *American Economic Review*, 96, 383–387.

Verfassungsgerichtshof Nordrhein-Westfalen (1999): "Urteil vom 6. Juli 1999," VerfGH NRW 14/98, 15/98.

Verfassungsgerichtshof Thüringen (2008): "Urteil vom 10. April 2008," VerfGH TH 22/05.

Wald, A. (1940): "The fitting of straight lines if both variables are subject to error," *The Annals of Mathematical Statistics*, 11, 284–300.

Weingast, B. R., K. A. Shepsle, and C. Johnsen (1981): “The political economy of benefits and costs: A neoclassical approach to distributive politics,” *Journal of Political Economy*, 89, 642–664.

Wilson, L. S. (2003): “Equalization, efficiency and migration: Watson revisited,” *Canadian Public Policy/Analyse de Politiques*, 385–396.

Eidesstattliche Erklärung

Hiermit erkläre ich, die vorliegende Dissertation selbstständig angefertigt und mich keiner anderen als der in ihr angegebenen Hilfsmittel bedient zu haben. Insbesondere sind sämtliche Zitate aus anderen Quellen als solche gekennzeichnet und mit Quellenangaben versehen.

Mannheim, 3.10.2018

Curriculum Vitae, Tobias Etzel

2013 – 2018	PhD Studies in Economics, Center for Doctoral Studies in Economics, University of Mannheim. Employee at the Chair for Economic Policy, Prof. Hans Peter Grüner, University of Mannheim.
2013 – 2015	M.Sc. in Economic Research, University of Mannheim.
2012 – 2013	M.Phil. in Economic Research, University of Cambridge, United Kingdom.
2008 – 2012	B.Sc. in Economics, University of Vienna, Austria.
2007 – 2008	Freiwilliges Soziales Jahr im Ausland, Skokie, USA.
2007	Abitur, HAP Grieshaber Gymnasium, Reutlingen.