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How Political, Spatial, and Administrative Changes Shape the Local Level

Evidence from Municipalities

Luisa Dörr

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Herausgeber der Reihe: Clemens Fuest
Schriftleitung: Chang Woon Nam

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Preface

Luisa Dörr prepared this study while she was working at the ifo Center for Public Finance and Political Economy. The study was completed in February 2022 and accepted as doctoral thesis by the Department of Economics at the University of Munich. It consists of three distinct empirical essays investigating various aspects of regional development using data from Germany and Austria. Chapter 2 investigates how far-right populist mayors influence local political culture and economic outcomes. Chapter 3 examines how highway access affects commuting and employment in connected municipalities. Chapter 4 investigates the consequences of municipal mergers on fiscal equalization transfers. The chapters employ regression discontinuity, difference-in-differences, and event study estimations to answer the respective research questions.

Keywords: Local government, municipalities, populism, far-right politics, partisan politics, polarization, immigration, economic policy, budget transparency, highway, infrastructure, accessibility, commuting, employment, municipal mergers, fiscal equalization, expenditures

JEL-No: D72, P16, H72, Z18, H54, O18, H11, H77, R53

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	Prof. Dr. Dr. h.c. Clemens Fuest
	Prof. Dr. Andreas Haufler

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1 Introduction

1.1 Motivation: Regional economic disparities

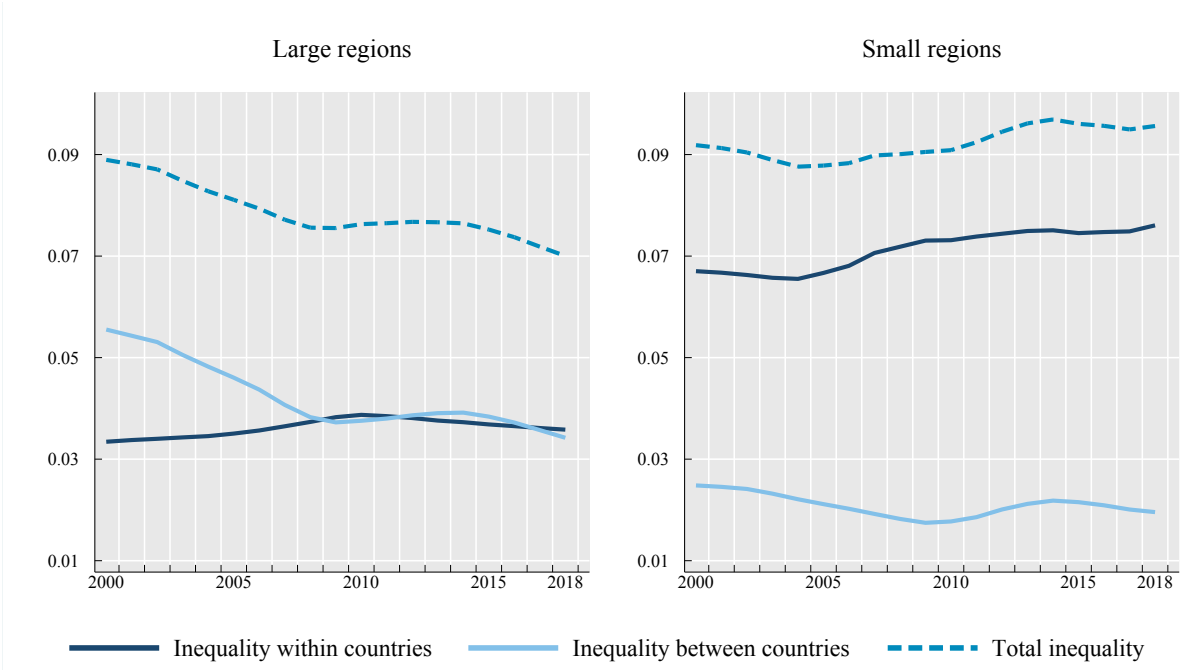
Economic disparities across regions constitute the main reason behind unequal access to public services across a country (OECD, 2013). The COVID-19-pandemic, which has hit the world with unprecedented force, is likely to exacerbate existing within-country disparities (Irlacher and Koch, 2020; Capello and Caragliu, 2021), posing a momentous challenge for policymakers worldwide. Low-growth and low-income regions have been at the top of the regional policy agenda for decades. In order to support job creation, business competitiveness, and economic growth, the European Union for instance spent almost a third of its total budget on its Cohesion Policy 2014-2020.[1] The need for regional redistribution is increasingly emphasized by policymakers, also to avoid social divisions (Rodríguez-Pose, 2018; Iammarino *et al.*, 2019). Vivid examples such as the rising support for populist parties, the Brexit vote, or the election of Donald Trump demonstrate that regional differences in prosperity can have serious consequences for society as a whole (Gordon, 2018). Understanding disparities across sub-national jurisdictions is thus key for designing effective regional policies.

Depending on the size of regions, sub-national economic development has followed different patterns over the past two decades. Figure 1.1 shows the development of an index measuring inequality in GDP per capita across large and small sub-national regions in OECD countries. The index decomposes total inequality into inequality due to differences within and between countries. Large regions are defined as the first administrative tier of sub-national government. Small regions correspond to administrative units that are contained in the large regions.[2]

Large regions across OECD countries exhibit greater economic convergence, with disparities in GDP per capita being at their lowest level since 2000 (left graph in figure 1.1; OECD, 2020). This convergence is, however, mainly driven by a decreasing gap in inequality between countries, while disparities within countries increased slightly over the considered period. When assessing economic disparities across a more fine-grained geographical level, the trend in total inequality is reversed: the gap in GDP per capita across small OECD regions has almost constantly widened since the beginning of the 21^{st} century (right graph in figure 1.1). Rather than between countries, the drift in economic development between small regions manifests itself within national borders.

[1] `https://ec.europa.eu/regional_policy/en/policy/what/investment-policy/`.
[2] In the German context, for example, small regions correspond to districts, while large regions refer to the state level.

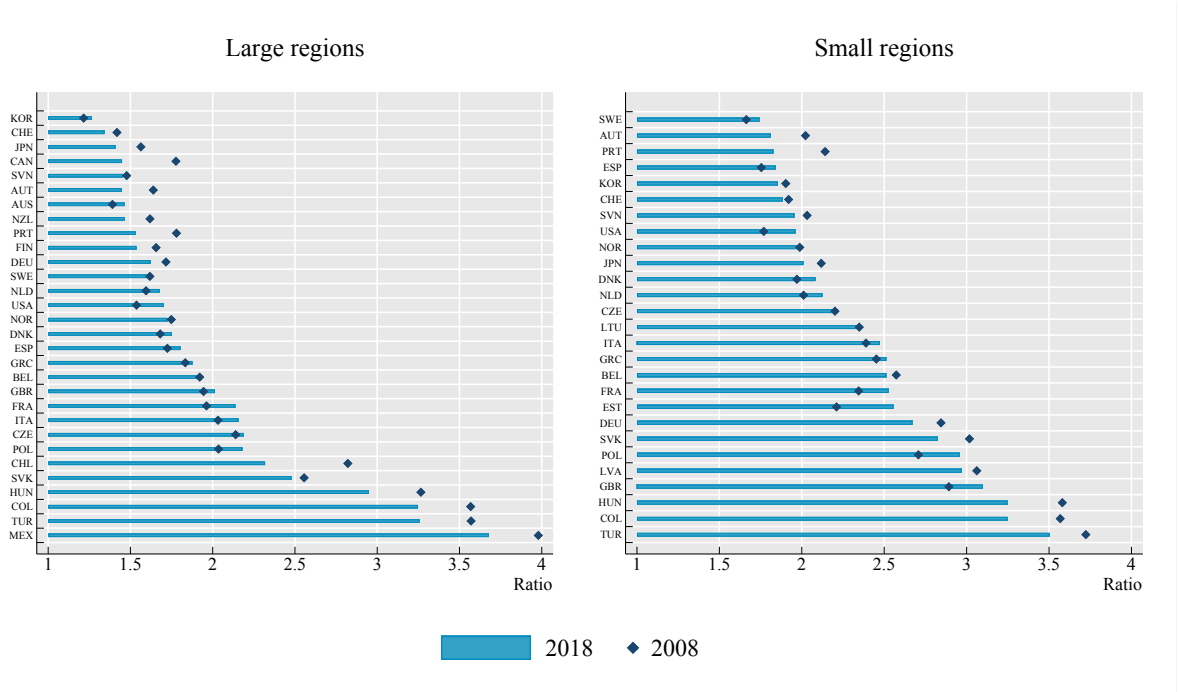
Figure 1.1: Trends in regional economic disparities in OECD countries



Source: OECD (2020).
Notes: This figure shows the trend in the Theil inequality index of GDP per capita based on large and small regions. The index breaks down the overall inequality into inequality due to differences within countries and inequality due to discrepancies across countries. Values vary between zero (equal distribution of GDP per capita) to infinity (greatest possible inequality in GDP per capita) and are presented over 3-year averages, except for 2000 and 2001.

Complementing the aggregate view on economic disparities across small and large OECD regions, figure 1.2 illustrates the substantial heterogeneity in regional economic development within countries. From 2008 to 2018, almost half of all OECD countries saw an increase in the GDP per capita gap between their top and bottom 20% of regions. This increase was particularly striking in France, Italy, Poland, and the United States. Consistent with the descriptive evidence in figure 1.1, economic polarization across space is more pronounced between small regions: in 2018, the level of GDP per capita in the top 20% of small regions exceeded the level in the bottom 20% by a factor of 2.4. This is not only a phenomenon of upper middle income countries. High income countries, such as Germany or the United Kingdom, rank among the places with the largest regional disparities in GDP per capita.

Figure 1.2: Index of regional disparity in GDP per capita



Source: OECD (2020).
Notes: This figure shows the ratio of GDP in the top 20% richest regions over GDP in the bottom 20% poorest regions for the years 2008 and 2018. The top and bottom 20% regions are defined as those with highest/lowest GDP per capita until the equivalent of 20% of national population is reached. GDP per capita values are expressed at constant 2015 prices. Countries are ranked in ascending order of the 2018 ratio.

Studying the causes and consequences of an increasingly polarized geography of prosperity is important to offer policy solutions. While many studies have made headway in exploring how regional inequality translates e.g. into greater polarization of political attitudes or voting behavior (Gordon, 2018; Spicer, 2018), many questions remain unanswered. In this thesis, I investigate causes and consequences of unequal economic development across small regions in Germany and Austria. The analysis focuses on the municipality level. In both countries, municipalities represent the lowest administrative units at the sub-national level and are

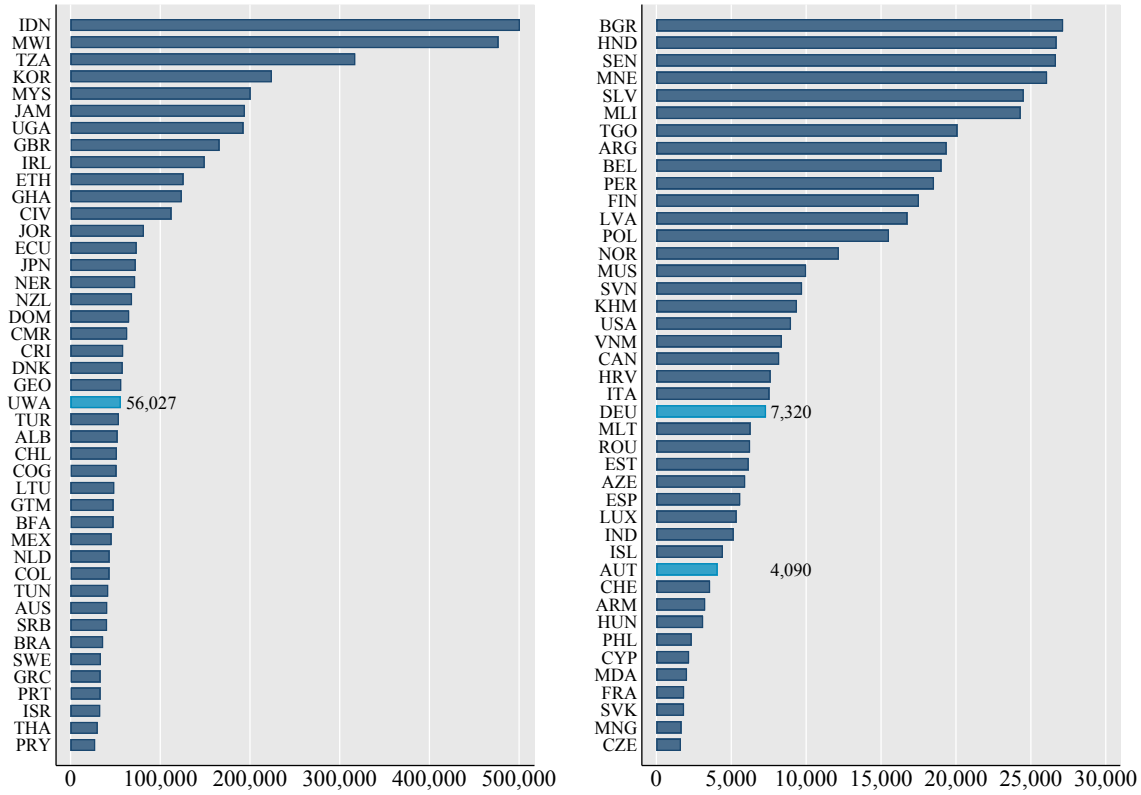
cornerstone of local self-government. Across the world, municipalities enjoy a wide diversity of responsibilities, often defined by state constitutions or laws. Generally, they deliver key services to the local population in the areas of education, social affairs, urban planning, local utility networks, local economic development, local roads, and public transport. The user group of local services and thus municipal size varies greatly on a global scale (see figure 1.3). Municipalities in countries like Indonesia, Korea, Malawi, or Malaysia are positioned at the upper end of the size distribution and resemble large metropolitan areas with up to 500,000 inhabitants.[3] In contrast, Austrian and German municipalities rank among the smallest regions in international comparison, providing services for 4,090 and 7,320 inhabitants on average (OECD/UCLG, 2016). As the descriptive evidence in figures 1.1 and 1.2 has underlined, small regions – even within countries – exhibit a high degree of economic polarization and are therefore particularly suitable to examine questions of economic disparities.

Regional disparities within countries have many different facets. To measure regional development, I use established economic indicators, such as local public finances (chapters 2 and 4), population, and employment outcomes (chapters 2 and 3). I also devise novel measures of the social divide within local societies, like political polarization or the diversity on local sports teams (chapter 2). Since many public goods are provided at the sub-national level, local public finances are a key prerequisite for the overall functioning of government. Differences in tax raising capacities make it difficult for some jurisdictions to provide adequate levels of public services (Gadenne, 2017). At the same time, local demography and the composition of the population vary across jurisdictions and the costs of public services targeted at special groups – like the elderly or socially weak – are higher (Borck *et al.*, 2015). GDP per capita is usually a good summary indicator of regional economic development, as it describes how demography, labor force participation, and labor productivity interact (Iammarino *et al.*, 2017). However, comparisons across this indicator may be distorted when people live and work in different jurisdictions. This is why I explicitly focus on commuting patterns and local employment at the place of work and the place of residence in chapter 3 of this thesis. Finally, economic history studies have shown that economic and socio-demographic factors alone are not sufficient to explain the growing socio-spatial differentiation. Social cohesion is equally important for regional economic development. As Rodrik (1997, p.7) notes: “The deepening of social fissures can harm all.” Chapter 2 thus uses novel measures of political polarization and polarization within civil society as “soft” factors that are decisive for regional prosperity.

Not only are the measures to gauge local economic development diverse, but so are the underlying determinants shaping the local level. This thesis focuses on three determinants of local development relating to the political economy and local economic geography of municipalities: far-right populist rule (chapter 2), infrastructure access (chapter 3), and jurisdiction size (chapter 4).

[3] These countries usually have a well-developed network of sub-municipal administrative units that ensure proximity to citizens.

Figure 1.3: Average municipal size



Source: OECD/UCLG (2016).
Notes: This figure shows average municipality size (number of inhabitants) in 85 selected countries in 2015. The highlighted bars refer to the unweighted average (UWA), Germany (DEU), and Austria (AUT). Countries are ranked in descending order of municipal size.

1.2 Chapter overview

It has long been acknowledged that political actors play a role in shaping economic outcomes. According to the traditional view of partisan theories developed in the 1970s, left-wing parties prefer more expansionary economic policies and a larger size and scope of government than right-wing parties (Hibbs, 1977; Chappell and Keech, 1986; Alesina, 1987). However, partisan theories offer little guidance in explaining the policy platforms of far-right populist parties, which have mobilized a growing share of voters since the early 2000s. While the literature on the drivers of populist success has stressed factors such as rising concerns over and exposure to immigration (Facchini and Mayda, 2009; Barone *et al.*, 2016; Halla *et al.*, 2017), structural change and trade shocks (Becker *et al.*, 2017; Dippel *et al.*, 2018; Autor *et al.*, 2020), economic crises (Funke *et al.*, 2016), and socio-economic inequalities (Solt, 2011), there is hardly any evidence on the effects when populists *themselves* enter government (Bracco *et al.*, 2018; Funke *et al.*, 2020).

In joint work with Niklas Potrafke and Felix Roesel, chapter 2 examines the consequences of far-right populists in power on political culture and economic outcomes. To establish causality, we exploit close mayoral elections in the Austrian state of Carinthia, where candidates of the far-right FPÖ won or were defeated by a close margin. Austrian FPÖ mayors are an ideal case to investigate far-right populist rule for mainly two reasons. First, directly elected Austrian mayors rank among the most influential local leaders in Europe with great political power over local affairs. Second, far-right populist mayors use the same populist rhetoric as national FPÖ politicians. Investigating the local level in the Austrian state of Carinthia therefore allows for drawing conclusions about far-right populism in general.

An innovation of our study is using both a new index of political polarization and a diversity measure of local football teams as dependent variables to measure the social divide within local communities. Based on their strong anti-establishment rhetoric and scapegoating mechanisms ("us" vs. "them"), we expect that far-right populist mayors fuel polarization, both along partisan lines and within civil society. In contrast, regarding economic policies, far-right populists offer no clear policy agenda, since they try to offer platforms gratifying the economic needs of both left-wing and right-wing voters. The results confirm our expectations. Under far-right populist mayors, political polarization increases, i.e. the local electorate is divided between more ideologically distant parties representing irreconcilable positions. This political discord also translates to the local civil society: the composition of local football teams becomes less diverse, reflecting a bias towards Austrian players. Economic outcomes, however, did not differ under far-right mayors and mayors from other parties.

Infrastructure access is generally viewed as a catalyst for regional economic growth. The provision of new roads, railways, or airport infrastructure is shown to have largely beneficial effects for connected regions (Hornung, 2015; Donaldson and Hornbeck, 2016; Ahlfeldt and Feddersen, 2018; Banerjee *et al.*, 2020; Doerr *et al.*, 2020). However, new transportation infrastructure also comes with an ambiguity, dubbed as the "two way roads problem" (Cheshire *et al.*, 2014, p.209). On the one hand, enhanced integration increases the effective size of local economies, giving rise to agglomeration benefits. On the other hand, new infrastructure can also pose "threats" to home producers arising from greater competition, especially when poorer regions are connected. In new economic geography models (see e.g., Krugman, 1991), lower transport costs alter the balance between agglomeration and congestion forces and trigger a shift of economic activity from the periphery to the core.

Against the background of the theoretical ambiguity of new transportation infrastructure, Stefanie Gaebler and I empirically examine the effects of highway access on local employment outcomes in chapter 3 of this thesis. As a testing ground, we use the stage-wise expansion of the "Baltic Sea Highway" (BAB 20) in the East German State of Mecklenburg-Western Pomerania, which provides us with variation in the timing of infrastructure access. Furthermore, as largest contiguous highway construction project in Germany since 1945, the BAB 20 had a considerable impact on municipalities' accessibility. To reduce concerns of endogeneity, we follow the inconsequential units approach and exclude large and economically strong cities

that shape the route of the highway (Chandra and Thompson, 2000; Faber, 2014; Möller and Zierer, 2018). For non-agglomeration municipalities that lie on a convenient route between two larger cities, access to the new highway can be regarded as close to random.

We focus on local labor market outcomes, such as the number of employees and firms, but also commuting flows, which have not received much attention in the literature so far (exceptions are Baum-Snow, 2010; Heuermann and Schmieder, 2019). Especially at a small-scaled geographical level – municipalities in Mecklenburg-Western Pomerania have on average 1,563 inhabitants –, the use of GDP to measure regional disparities can be misleading when people live and work in different jurisdictions. Our results estimated with difference-in-differences and event study methods suggest that inbound commuting is negatively affected by highway access. This effect is driven by peripheral municipalities, i.e. rather remote municipalities located further away from economically strong regional centers. We observe a shift of commuting flows in line with predictions from the core-periphery model: central locations seem to benefit from an increase in commuters and employment at the expense of the periphery, where commuting volumes decline.

Access to public services depends crucially on the geography and the number and size of sub-national jurisdictions (Saarimaa and Tukiainen, 2014). Larger local jurisdictions are generally considered more efficient in providing services at lower unit costs. Such arguments are often brought forward by national governments to justify large-scale territorial reforms at the local level. At the same time, larger jurisdictions are often more successful in raising tax revenues and benefit from above average tax capacity (Blöchliger *et al.*, 2007). To avoid an increasing gap between "superstar" regions and economically weaker localities, almost all OECD countries rely on fiscal equalization schemes to correct imbalances between sub-national governments. Chapter 4 of this thesis examines how municipal mergers affect fiscal equalization transfers in Germany.

Many countries have equalization arrangements in place that account for the industrial organization of public services by adjusting their fiscal need indicators to jurisdiction size or population density. In their municipal fiscal equalization schemes, the 13 German territorial states[4] artificially inflate population counts for larger local jurisdictions to account for their higher living costs – a practice that runs counter to the argument of economies of scale and scope pushing small jurisdictions to merge. In the East German state of Saxony, the relationship between the fiscal need indicator and the population count is especially progressive. At the same time, Saxony's local government landscape was subject to extensive merger waves during the 1990s, both voluntary and compulsory, offering an interesting laboratory to investigate how mergers influence transfers.

[4] In the three city states – Berlin, Hamburg, and Bremen – the state and municipal levels are integrated.

First, comparing realized to potential mergers, I test whether fiscal equalization gains potentially incentivize voluntary mergers. Although it has been acknowledged that the institutional design of fiscal equalization schemes can provide significant (dis-)incentives to merge, my empirical results do not corroborate this hypothesis. While coalition size matters for merger decisions, municipalities do not choose their partners in order to maximize transfer levels. However, large-scale territorial reforms give rise to a significant and permanent reallocation of per capita transfers within the fiscal equalization scheme: merging municipalities benefit from an 11% increase in their per capita transfer levels. Against this finding, it is surprising that expenditures hardly change. While total administrative cost savings from mergers amount to a moderate 3%, higher transfer levels seem to be spent on municipal staff.

1.3 Contribution to the literature

This thesis adds to several areas of the economic literature. Chapter 2 contributes to the field of political economy and the growing literature on the causes and consequences of populist success. There is a lack of evidence on policies of populist parties, mainly because far-right populists have not yet made it to senior coalition partner in many national governments. Portraying policies of far-right populist parties is important, because they often criticize established parties for not offering polarized policies and promise drastic policy changes when being in office. Chapter 3 contributes to the field of urban and regional economics, in particular transportation analysis. New infrastructure is often viewed as a panacea for structurally weak regions. Outlining theoretical and also empirical ambiguities of improved accessibility is thus vital to inform the policy debate. Finally, chapter 4 deals with key questions in public economics and fiscal federalism, in particular how municipal mergers affect fiscal equalization transfers. Policymakers should consider whether the documented large reallocation of transfers in the wake of territorial reform is intended. The following chapters are self-contained and can be read independently. Chapter 5 provides concluding remarks.

2 Populists in power[1]

Abstract

Abstract: We examine how populist politicians influence political culture and economic outcomes. We exploit close mayoral elections in Austria where candidates of the FPÖ won or were defeated by only a few votes. The FPÖ is among Europe's leading far-right populist parties, with close connections to the German AfD, the French RN, and the Italian Lega. FPÖ mayors use the populist rhetoric of national politicians and have great power over local affairs. An innovation of our study is using an index of political polarization and the diversity in local football teams to measure polarization within civil society. Far-right populist mayors increase political polarization and decrease diversity among players of local football teams, reflecting a bias towards Austrian players. This result corroborates that net foreign migration decreases under far-right populist mayors. By contrast, we do not observe that far-right populist mayors influence wealth, unemployment, or public debt. We conclude that populist politicians aim at shaping the cultural rather than the economic landscape.

[1] This chapter is joint work with Niklas Potrafke and Felix Roesel.

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2.1 Introduction

Far-right populists enjoy great electoral success in many industrialized countries. They use a strong anti-establishment rhetoric, often combined with nationalist, xenophobic, or anti-Muslim sentiments. Well known examples include the French Rassemblement National (RN, formerly: Front National), the Italian Lega, the Austrian Freiheitliche Partei Österreichs (FPÖ), the German Alternative für Deutschland (AfD), or the UK independence party (UKIP). Scholars describe the rise of far-right populism as a cultural backlash to unintended globalization outcomes (Norris and Inglehart, 2019) and investigate drivers of populism. Prominent examples are rising concerns over and exposure to immigration (Facchini and Mayda, 2009; Card *et al.*, 2009; Otto and Steinhardt, 2014; Hainmueller *et al.*, 2015; Barone *et al.*, 2016; Halla *et al.*, 2017; Dustmann *et al.*, 2018; Edo *et al.*, 2019), structural change and trade shocks (Becker *et al.*, 2017; Dippel *et al.*, 2018; Autor *et al.*, 2020), economic crises (Funke *et al.*, 2016), and socio-economic inequalities (Solt, 2011).[2]

Against the background of how both populist parties criticize the established parties and opponents of populist parties fear their emergence, an intriguing question is whether populist parties would, in fact, implement different policies if they were to gain office. The effects when populists *themselves* enter government have hardly been explored to date. The main reason for this is a lack of incidences at a global scale. Since the early 2000s, far-right populist movements have mobilized a sizable and growing share of voters, but usually missed absolute majorities. Prominent examples are the US president Donald Trump, the Brazilian president Jair Bolsonaro, and the Brexit vote. Some populist parties have participated in coalition governments in Europe, for example in Austria, Finland, Greece, and Italy, or supporting minority governments in Denmark and the Netherlands. A recent study by Funke *et al.* (2020) suggests that per capita GDP declines when populist leaders, such as presidents or prime ministers, are in power; after 15 years, it is lowered by around 10% as compared to an estimated counterfactual. In Italian municipalities, the share of foreigners decreased when far-right populist mayors were in office (Bracco *et al.*, 2018).[3]

It is conceivable that far-right populists influence cultural policies, such as migration flows, because those policies have been their front-line issue (List and Sturm, 2006). Our study also shows that the share of foreigners decreased under far-right populist mayors.

We examine how far-right populist politicians influence both political culture and economic outcomes. We exploit close mayoral elections in Austria where candidates of the far-right FPÖ won or were defeated by only a few votes. The FPÖ is among Europe's leading far-right populist parties, with close connections to the German AfD, the French RN, and the Italian Lega and has a long tradition dating back to the 1920s. The FPÖ already enjoyed great electoral success in the late 1980s, long before the recent renaissance of populism. From 2000 to 2006 and from

[2] For a survey on the determinants of far-right populist voting, see Arzheimer (2018).

[3] In a similar vein, migration to German cities decreased when far-right protests took place (Brox and Krieger, 2021).

2017 to 2019, the FPÖ joined coalition governments with the conservative ÖVP. The FPÖ also has strong local roots, in the Austrian federal state of Carinthia in particular. Far-right populists won around 14% of all mayoral elections in Carinthia between 1991 and 2017. Austrian mayors rank among the most powerful local leaders in Europe (Heinelt and Hlepas, 2006, see also table A2.1 in the appendix). Newspapers call Austrian mayors "village pashas", having executive power over staff, public finance, and local public goods even Austrian chancellors, ministers, or national and state MPs can only dream of.[4]

Moreover, far-right populist mayors use the same populist rhetoric as national FPÖ politicians. For example, one FPÖ mayor in Carinthia explicitly admired Nazism:

> *"I dissociate myself only from their [the Nazis] actions, but not from Nazism."* (Siegfried Kampl, FPÖ mayor of Gurk, 2014).[5]

Another FPÖ mayor describes his local policies towards migrants as follows:

> *"Turks do not get social housing. Muslim children are not allowed to go to school here. If Turks want to buy property, then we buy it first."* (Peter Suntinger, FPÖ mayor of Großkirchheim, 2009).[6]

The FPÖ also cultivates strong anti-Semitic roots. In the Austrian state of Vorarlberg, for example, the mayor of Hohenems called the German director of the local Jewish Museum, Hanno Loewy, an

> *"exiled Jew from America in his highly subsidized museum."* (Dieter Egger, FPÖ mayor of Hohenems).[7]

Against the background of great political power over local affairs and the far-right rhetoric even at the local level, we propose that Austrian FPÖ mayors are an excellent case in point to investigate how far-right populist politicians influence the political and economic landscape. We exploit the narrow victories and defeats of far-right populist candidates to establish causality. If political races are very close, electoral outcomes can be considered as quasi-exogenous (e.g., Ferreira and Gyourko, 2009). In our sample, in the 1997 mayoral election in the municipality of Stockenboi the far-right populist candidate defeated the social democratic candidate by

[4] "Die wahre Macht der Bürgermeister", Salzburger Nachrichen, 25.11.2017.

[5] Original in German language: "Nur von dem, was sie gemacht haben, distanziere ich mich, nicht vom Nationalsozialismus." (Kleine Zeitung, 17.09.2014). Kampl was excluded from the FPÖ.

[6] Original in German language: "Türken bekommen von uns keine Wohnung. Muslimische Kinder dürfen hier nicht zur Schule. Wenn Türken hier Grund kaufen wollen, dann kaufen wir den vorher weg." (SZ Magazin, 08.04.2009).

[7] Original in German language: "Exil-Jude aus Amerika in seinem hochsubventionierten Museum." (ORF Online, 18.04.2015, http://vorarlberg.orf.at/news/stories/2705950/).

565 to 560 votes. By contrast, a far-right populist candidate lost the 2015 mayoral election in the municipality of Albeck by 362 to 363 votes. We implement RD estimations to exploit close races involving a far-right candidate.

We examine dependent variables measuring the local political culture and economic outcomes. An innovation of our study is using a new measure of political polarization and diversity on local football teams as dependent variables to measure the divide within civil society. Based on anecdotal evidence, we expect that polarization increases under far-right populist mayors, both at the political level and within civil society. In contrast, regarding economic policies, far-right populists are caught between the devil and the deep blue sea and may hardly differ from the political center. The results show that under far-right populist mayors, political polarization increases. This political discord is also translated to the local civil society: the composition of local football teams becomes less diverse, reflecting a bias towards Austrian players. Moreover, net foreign migration decreases. The results do not show that far-right populist mayors influence economic outcomes such as the unemployment rate. However, budget transparency decreases under far-right populists mayors. We conclude that populist governments influence cultural outcomes to a much larger extent than economic outcomes.

It has long been acknowledged that political actors influence local economic development. The partisan theories developed in the 1970s describe how traditional left-wing and right-wing governments implement economic policies to gratify their constituencies (Hibbs 1977, Chappell and Keech 1986 and Alesina 1987; see Potrafke 2017, 2018 for surveys). However, party systems have changed in many industrialized countries and partisan theories offer little guidance in explaining the policy platforms of (far-right) populist parties. Many studies deal with the determinants of populists' electoral success (Facchini and Mayda, 2009; Card *et al.*, 2009; Solt, 2011; Otto and Steinhardt, 2014; Hainmueller *et al.*, 2015; Autor *et al.*, 2020; Barone *et al.*, 2016; Funke *et al.*, 2016; Becker *et al.*, 2017; Halla *et al.*, 2017; Dippel *et al.*, 2018; Dustmann *et al.*, 2018; Edo *et al.*, 2019; Norris and Inglehart, 2019), while we investigate the consequences of far-right populist politicians. Our study is related to the recent literature examining how populist politicians and governments influence economic outcomes (Funke *et al.*, 2020). We use fine-grained data at the local level and estimate causal effects of populist mayors on economic outcomes. A major innovation of our study is introducing new measures of polarization, both along partisan lines and within civil society, as outcome variables.

2.2 Institutional background

2.2.1 Austrian state of Carinthia

Austria runs a three-tier government consisting of the national level, nine federal states (*Bundesländer*), and around 2,100 municipalities (*Gemeinden*). Carinthia is the most southern state bordering on Italy and Slovenia. Large parts of Carinthia are alpine and rural, the densely populated valley around the capital of Klagenfurt being the sole exception. By 2017, Carinthia

had a population of around 560,000 inhabitants living in 132 municipalities. Around 10% of the population is foreign (Austrian average: 15%). The Carinthian GDP per capita is at around 85% of the national average; growth rates between 2000 and 2016 roughly correspond with the national average. Most Carinthians are German speaking (96%) and Catholic (77%). There are, however, two substantial minorities of 10% Protestants and 2–3% ethnic Slovenians (which hardly overlap, see figure A2.1 in the appendix). Conflicts over bilingual village signs have dominated Carinthian politics for over 40 years. The powerful far-right populist camp refused to implement bilingual signs. The conflict was not resolved until the constitutional court and national government intervened in the 2000s.

Traditionally, the social democrats (SPÖ) are the leading political force in Carinthia. Over the period from 1989 to 2013, however, the far-right populist FPÖ (2008 to 2013: BZÖ) played a major role in Carinthian politics. In 16 out of 24 years (1989 to 1991 and 1999 to 2013), the state government was led by a far-right populist prime minister. The conservative party ÖVP, by contrast, does not enjoy pronounced electoral support and does not play an important role in present day Carinthia. There are some less important small parties in Carinthia such as the Greens (figure 2.1 and table A2.5 later on). There is no other Austrian state with a comparable density of far-right populist mayors. In the three states neighbouring Carinthia (Styria, Tyrol, and Salzburg), FPÖ mayors account for less than 0.3% of all mayors. We describe the role of mayors in Austrian politics in greater detail below.

2.2.2 Far-right populism in Austria

Austria has a longstanding tradition in far-right parties beyond moderate conservative party ideology. The right-wing camp has made it to the Austrian national parliament in all democratic elections since 1919.[8] In the democratic interwar period from 1919 to 1934, pan-German parties (*Deutschnationale*) proposing to annex Austria to Germany won some 20% to 25% of all votes in national elections. After WWII, the right-wing camp was soon reestablished (*Verband der Unabhängigen*, VdU) and enjoyed substantial support from former pan-German party supporters, including the Nazis (Ignazi, 2003; Ochsner and Roesel, 2020). In 1955, main parts of the VdU were transformed into the newly-founded *Freiheitliche Partei Österreich* (FPÖ), which is the main right-wing party in Austria to the present day. Electoral support, however, declined in the early 1980s. In 1986, a right-wing faction led by the Carinthian politician Jörg Haider took control and started to run far-right populist campaigns (Luther, 1997). Vote shares skyrocketed and Haider became prime minister of the state of Carinthia. In 2000, the FPÖ formed a government with the conservative party at the national level. The radical wing of the FPÖ rebelled. In 2005, Haider and his more moderate followers left the FPÖ after internal disputes and formed a new far-right populist party (BZÖ), which had its stronghold in Carinthia where the BZÖ almost completely absorbed the former FPÖ. The FPÖ however remained strong outside Carinthia and became even more populist right-wing. When Haider died in a

[8] The sole exception is 1945, when the right-wing camp was banned and did not participate in the national election.

car accident in 2008, BZÖ support steadily declined. Today, after one decade of further party splits and consolidation rounds, the FPÖ is in Carinthia the only remaining large far-right populist party, attracting 20% of all votes in the 2019 national election in Carinthia.

The FPÖ has an anti-elite and anti-immigration platform and is one of the leading far-right populist parties in Europe. Haider campaigned against immigration and foreigners in general and suppressed the Slovenian minority in Carinthia. Today, the FPÖ relies more on xenophobia and islamophobia, but also cultivates its neo-fascist roots. Party members from the local level, MP backbenchers, and leading party members are often involved in political scandals. In the Carinthian municipality of Gurk, for example, the FPÖ mayor Siegfried Kampl declared in 2014 that he dissociates himself from the actions by the Nazis, but not from Nazism.[9] Kampl was reelected as mayor in 2015. In the state election campaign of 2009, the BZÖ proclaimed a "Chechens free" ("*tschetschenenfreies*") Carinthia. The BZÖ gathered around 45% of all votes. There are many more examples of this nature.[10] Far-right rhetoric dominates across all FPÖ party layers from the very local to the national level. Carinthia, in particular, is a hotbed of political scandals involving far-right politicians. Investigating the local level in the Austrian state of Carinthia is therefore suitable to draw conclusions about far-right populism in general because local FPÖ politicians are hardly less radical than politicians at the state or national level.

2.2.3 Mayors in Austrian local politics

Mayors are the most powerful players in Austrian local politics (Hämmerle, 2000; Fallend *et al.*, 2001). They represent the municipality, lead the local governments as "managing directors", and generally enjoy great autonomy in deciding on local affairs. For example, mayors decide on public safety and order, staff, and spending (within the limits of the local budget).[11] Given their far-reaching power, mayors also have the ability to polarize local communities – particularly regarding cultural policies. Although immigration and refugee policy is decided at the national level, anecdotal evidence suggests that mayors can – beyond their rhetoric towards immigrants – exert indirect influence on immigration policies. During the "2015 refugee crisis", for example, Carinthia met only 90% of its admission quota of 2,500 refugees.[12] Many local leaders in whose communities there were no refugees argued that

[9] See section 2.1.
[10] For a recent compilation see, for example, Mauthausen Komitee Österreich (2017).
[11] The national level sets the main legislative framework and the federal states are mainly responsible for individual regional issues, such as teacher salaries, social care, and hospital care.
[12] "Bezirk Spittal: Viele sind ohne Flüchtlinge", meinbezirk.at, 23.06.2015, `https://www.meinbezirk.at/spittal/c-lokales/bezirk-spittal-viele-sind-ohne-fluechtlinge_a1385101`.

they do not have any premises. The FPÖ mayor of Ossiach initially even ordered a halt to the construction of the property in which the Carinthian initial reception center was to be set up.[13]

Being mayor is a full time job. Carinthian mayors earn some 3,000 to 7,000 Euros per month, depending on municipality size. There is no limit to re-elections. Mayors also prepare a draft of the local budget, which needs to be approved by the local council. De jure, the local council decides on the budget. De facto, however, the mayor determines almost the entire budget; in most cases, local councils hardly change budget drafts at all. This is particularly true of small municipalities where honorary local councilors face a mayor backed by the professional local government administration (see Klammer, 2000; Aigner *et al.*, 2001). Newspapers therefore call Austrian mayors "village pashas", which are sometimes barely controlled by the local council.[14] Mayors are also often board members of local public enterprises, allocating locally provided goods, such as water supply, waste disposal, or cemeteries. Carinthian mayors are therefore key players in Austrian local politics and good examples of politicians with large agenda-setting power (see Wastl-Walter, 2000; Pleschberger, 2003).

Most Austrian states have a presidential-style mayor-council system. Carinthia was the first Austrian state to introduce direct mayoral elections in 1991. Voters separately elect a local council (with 11 to 45 councilors, depending on population size), and a mayor. There are two rounds of mayoral elections. If no candidate achieves more than 50% of all votes in the first ballot, a run-off ballot is held between the two candidates with the most votes. Council elections and the first round of mayoral elections are held simultaneously every six years. If necessary, the run-off ballot takes place 14 days after the first ballot. All mayors served until the end of the term; there were no irregular elections.[15]

Austrian mayors are among the most powerful local leaders in Europe (Heinelt and Hlepas, 2006). Table A2.1 in the appendix shows that even council-elected Austrian mayors (e.g., in the federal states of Styria and Lower Austria) rank well above the European average in terms of strength. However, directly elected Austrian mayors such as those in the federal state of Carinthia, are among the top of the strongest local leaders in Europe. Thus, the Carinthian case is of particular interest and is suitable to draw conclusions beyond Austrian politics.

[13] "Stopp für Asylquartier", Die Presse, 05.08.2015. Ultimately, however, the initial reception center was completed after the federal government used the so-called right of passage, rendering the construction freeze ineffective.
[14] See reference in footnote 4.
[15] In 1991 and 1992, three municipalities were split into six new ones. Elections were held in 1992 instead of 1991.

2.3 FPÖ mayors

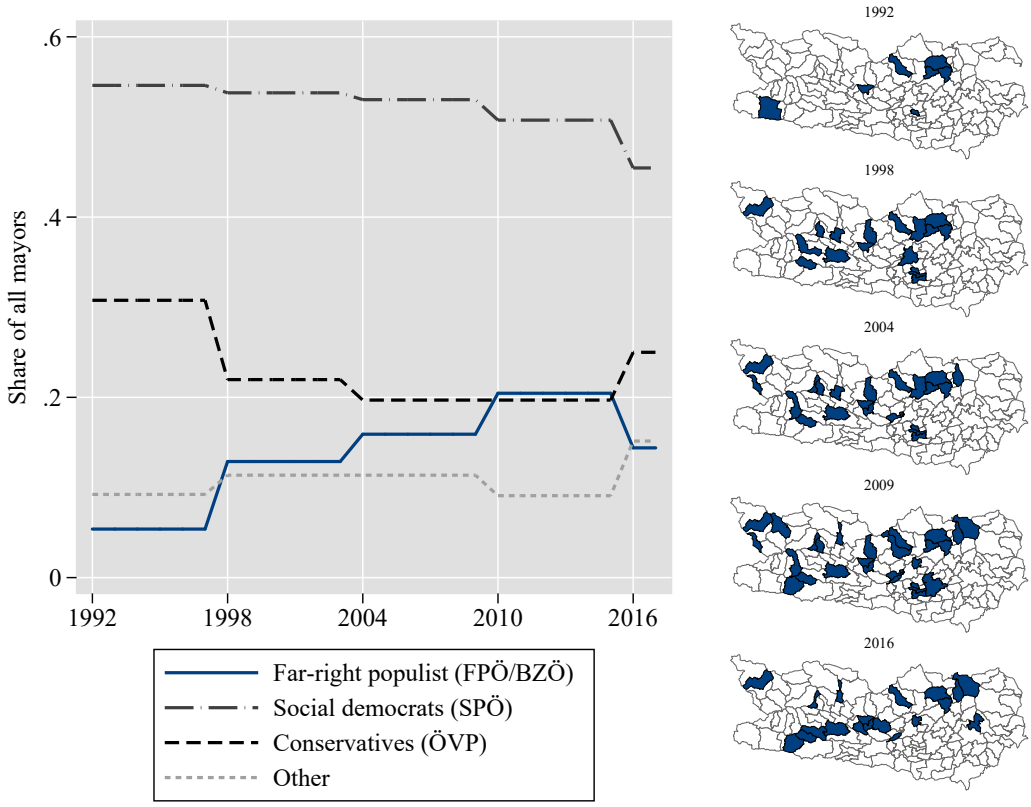
We collect data on mayor's party membership. Our sample period covers five local elections, which are held every six years. The FPÖ and the BZÖ are defined as far-right populist. The Carinthian FPÖ was the only far-right party until 2005 when the BZÖ was founded and absorbed major parts of the FPÖ.[16] Many members of the BZÖ, however, re-joined the FPÖ in 2013 when the BZÖ collapsed. Because the BZÖ almost entirely replaced the FPÖ in Carinthia between 2005 and 2013, but protagonists and platforms hardly changed, we label both parties as far-right populist. Figure 2.1 shows how mayors in the 132 Carinthian municipalities allocate across parties. Almost all mayors in Carinthia are affiliated with one of the three main parties ÖVP, SPÖ, or FPÖ, which nominate the candidates running in mayoral elections. The FPÖ continuously gained electoral support in mayoral elections reaching a peak in 2009, when 27 out of 132 (around 20%) of Carinthian municipalities were governed by a far-right populist mayor. At this point, the FPÖ held even more positions than the conservative ÖVP, whose share of mayors has been steadily declining from its initial level of 30% in 1991. In the most recent elections, both SPÖ and FPÖ suffered comparable losses, while the conservative ÖVP succeeded in reversing the downward trend.

Figure 2.1 also shows the regional pattern of far-right populist mayors for the five municipal elections included in our sample. Far-right populist victories only cluster to a small extent and are less pronounced in southeastern Carinthia. However, there are hardly any geographic patterns (for comparison, maps in figure A2.2 in the appendix show the socio-economic and geographic characteristics of Carinthia).

We also examine whether observable biographical characteristics differ between far-right populist mayors and other mayors. Table A2.2 shows that far-right populist winners do not significantly differ from their colleagues from other parties in terms of vote shares, education, gender, or total electoral terms. On average, far-right populist mayors receive a vote-share of 63% and remain three periods in office. Mayors elected in run-off elections differ from those elected in the first round in that their vote shares and periods in office are slightly lower. The share of women and university graduates among all mayors varies between 2-6% and 13-22%. There is also substantial variation within municipalities over time. Table A2.3 in the appendix shows the transition of mayor's parties. In 22% of all cases, the mayor's party changed after a local election. In 29 out of 526 cases, a far-right populist followed a non-populist mayor. In 17 cases, a mayor of another party replaced a far-right populist. Table A2.4 in the appendix reports that the mayor's party never changed between 1991 and 2017 in 60 municipalities; four municipalities had a far-right populist mayor over the entire period. The remaining 72 municipalities were swing municipalities. We conclude that there is substantial within-municipality variation to exploit in cultural, as well as in economic outcomes. Table A2.5 provides descriptive statistics for the variables used in the empirical analysis.

[16] In 1993, some liberal politicians left the FPÖ and found a liberal party LIF. The LIF, however, did only compete in few municipalities and receives very few votes in local elections.

Figure 2.1: Far-right populist mayors in the Austrian state of Carinthia



Notes: The left graph shows the share of mayors by parties in the Austrian state of Carinthia. There are 132 municipalities in total. The maps on the right show the spatial distribution of far-right populist mayors shaded in blue. We consider the election term to start with the first full year in office. Mayoral elections were held in 1991, 1997, 2003, 2008, and 2015.

2.4 Political culture under far-right populists

2.4.1 Political polarization

Background

Far-right populism includes a strong anti-establishment rhetoric, often combined with nationalist, xenophobic, or anti-Muslim sentiments. Well-known examples of European far-right populist parties include the PVV (Netherlands), Lega (Italy), Rassemblement National (France), FPÖ (Austria), or the AfD (Germany). Scholars describe far-right populism as a cultural backlash to unintended globalization outcomes and migration in particular (Norris and Inglehart, 2019). Accordingly, the main campaigning issue of far-right populists is to restrict immigration, including its related effects on unemployment and economic insecurity (e.g., Mudde, 2013; Guiso *et al.*, 2017; Algan *et al.*, 2018; Norris and Inglehart, 2019; for a survey see also Arzheimer,

2018). Far-right populist parties shift the political competition to issues of identity, as they position themselves as defending "the people". This dynamic gives rise to the polarization of party systems especially along the social-cultural dimension (Vachudova, 2019).

Political polarization – also termed the "voiding of the middle" – under far-right populist parties is empirically documented for the Netherlands (Castanho Silva, 2018; Bischof and Wagner, 2019). Two hypotheses describe how far-right populists contribute to ideological polarization of the public: first, the electoral success of far-right populist parties legitimizes more extreme views in the electorate. Citizens identifying with the right-wing populist policy platforms now feel more free to declare their pre-existing radical positions (Bischof and Wagner, 2019). Second, as a backlash reaction, voters sympathizing with the opposite side of the political spectrum strengthen their opposition to far-right views.

Data

An innovation of our study is to use an index that measures political polarization. Political polarization increases the more voters are divided between ideologically distant parties representing irreconcilable positions. We derive ideological distances between parties from voting advice application data – provided by Jäckle and König (2019) –, covering policy stances across a wide range of topics. (Dis-)similarities between parties are reduced to a two-dimensional policy space by multidimensional scaling. The polarization index is calculated as the sum of parties' (Euclidean) distances to the ideological center, weighted by their vote share. We use national (and not local) election outcomes to avoid picking up location-specific political features in the polarization index. Party platforms at national elections should be more comparable across municipalities. A description of how we compute the polarization index is provided in the appendix.

We compile a panel dataset covering all 132 municipalities of the Austrian state of Carinthia over a period of some 25 years (1991 to 2017).[17] The sample period covers eight national elections, which are held every five years. Voting advice application data is available for the four national elections since 2006. For previous elections, we use the party configuration as of 2006, i.e. for the time period 1991-2005, variation in the polarization index stems from the respective party vote shares at the municipality level.[18] Polarization in our sample varies between 16.69 at the national elections 2006 in the municipality Zell and 32.27 at the national elections 2008 in the municipality Freistritz ob Bleiburg. Figure 2.2 illustrates political polarization for the national elections 2006, 2008, 2013, and 2017.[19] It shows the corresponding party configurations with the areas of the circles representing the vote shares in the municipalities which were the least (graphs on the left) or the most (graphs on the right) politically polarized. With minimum polarization, a large share of the votes goes to parties

[17] Table A2.12 reports our data sources, which are mainly the Statistical Offices of Austria and Carinthia.
[18] Baseline results remain unchanged when we restrict the sample to the period from 2006 onward.
[19] Note that we multiply the original values of the polarization index by 100.

that are very close or moderately close to the ideological center in the policy space. In the 2017 election for example, a large share of the local electorate in Mörtschach supported the FPÖ and ÖVP, two parties clustering around the ideological center. The position of parties within the policy space is determined by their election-specific programmes. Polarization is at its maximum when votes are distributed relatively evenly between all parties in the policy space. The difference between minimum and maximum polarization is particularly striking for the 2008 national elections (polarization of 18.04 vs. 32.27): while in Lesachtal, the ÖVP and BZÖ, the two parties closest to the ideological center, gained by far the largest support, vote shares in Freistritz were more evenly distributed between the BZÖ and parties more distant from the ideological center (SPÖ, LIF/NEOS, and GRÜNE).

Empirical strategy

Endogeneity of economic and election outcomes is very likely to bias OLS dummy estimates. For example, citizens may vote for populists when their local governments perform badly (reverse causality). Omitted variables are another issue: election and economic outcomes may follow joint underlying trends like culture or mentality. To establish causality, we exploit narrow election outcomes to identify the causal effects of far-right populists in office using different regression discontinuity (RD) specifications. RD allows to focus on close elections with tight vote margins as a quasi-experimental setting (see Lee, 2008; Ferreira and Gyourko, 2009; Lee and Lemieux, 2010). In tight political races, majorities depend on a few pivotal ballots and arguably on exogenous circumstances like weather conditions (Arnold and Freier, 2016). In many municipalities, far-right populist candidates won or were defeated by only a few votes. For example, in the municipality of Stockenboi, the far-right populist candidate defeated the social democratic candidate in the 1997 mayoral election by 565 to 560 votes. By contrast, a far-right populist candidate lost the 2015 mayoral election in the municipality of Albeck by 362 to 363 votes. Weather conditions or the sickness of individual voters may have changed the voting outcome.[20] Table A2.6 in the appendix shows that the observable characteristics of elected far-right populist candidates do not differ from those of their defeated party fellows. Table A2.7 also shows that geographic and socio-economic variables are continuous at the threshold of 50% votes for a far-right populist candidate. Finally, we perform bunching tests proposed by McCrary (2008) (see also Cattaneo *et al.*, 2020): we do not find evidence for a manipulative distribution of vote shares at either side of the 50% threshold (figure A2.3 in the appendix). We are therefore confident that election outcomes quasi-exogenously sort into narrow defeats and victories.

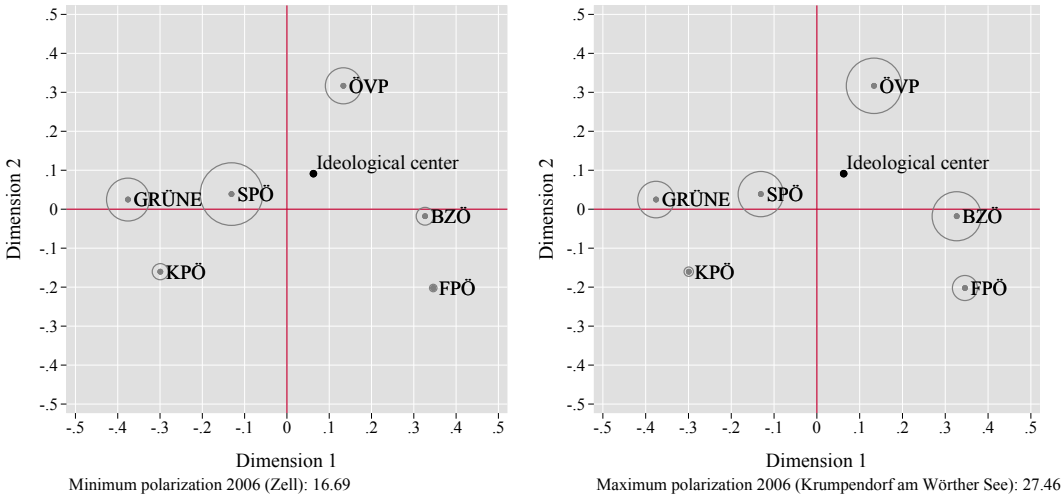
[20] Compared to population thresholds (Eggers *et al.*, 2018), compound treatment and sorting should not play a major role. See Hyytinen *et al.* (2018).

We use a linear interacted RD specification as our baseline. RD results represent the local average treatment effect of a marginally elected far-right populist mayor compared to marginally defeated far-right populist candidates. Our baseline RD specification takes the following form

$$Y_{it} = f(Populist_{it}) + \epsilon_{it} \quad \text{with } i = 1, ..., 132; t = 1, ..., 26, \tag{2.1}$$

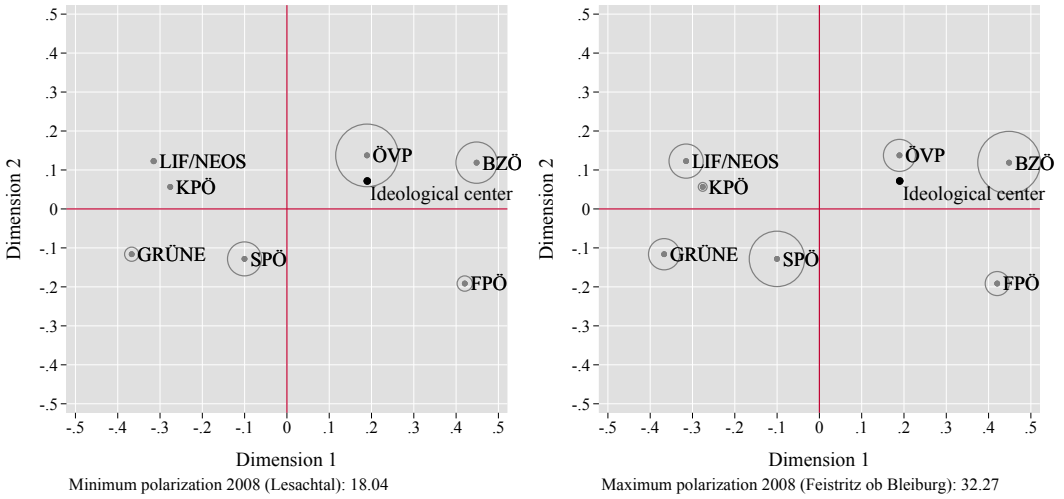
Figure 2.2: Political polarization

(a) 2006 national elections



Minimum polarization 2006 (Zell): 16.69 | Maximum polarization 2006 (Krumpendorf am Wörther See): 27.46

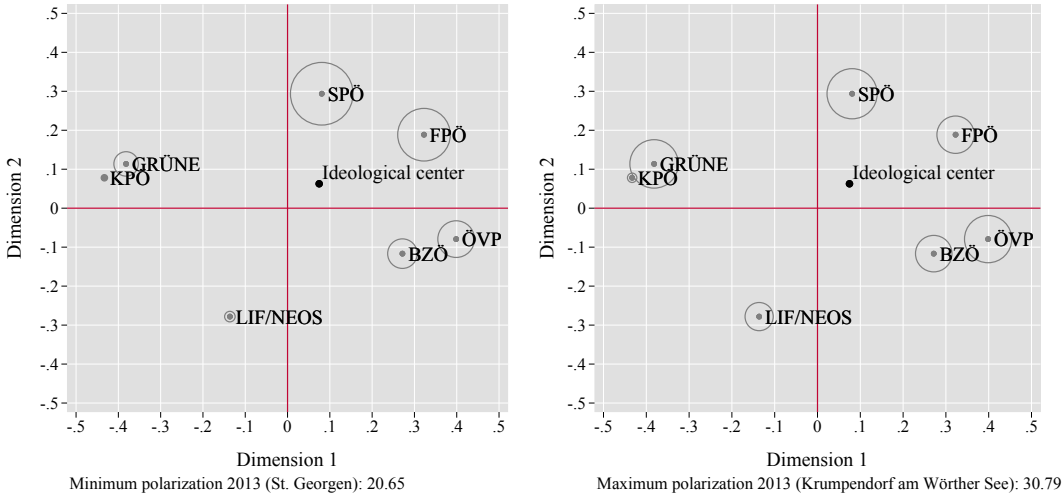
(b) 2008 national elections



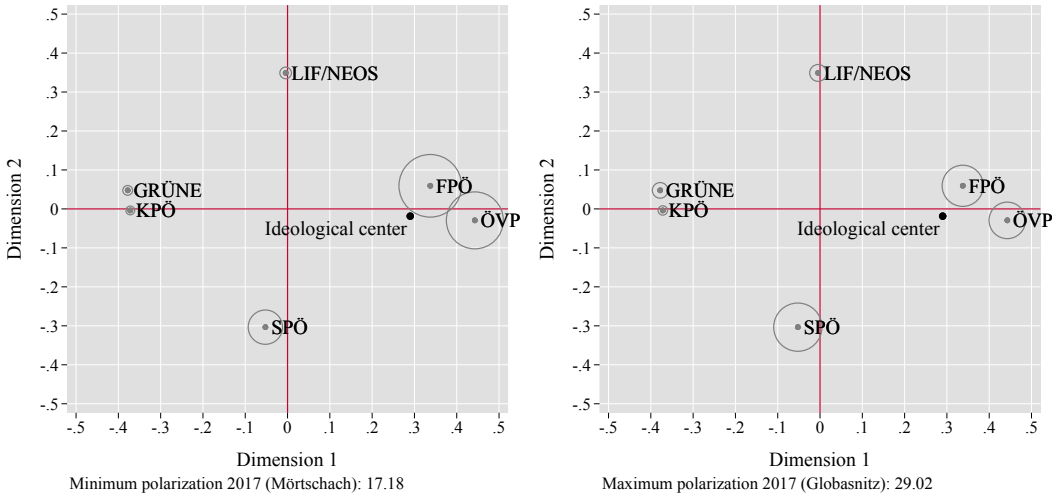
Minimum polarization 2008 (Lesachtal): 18.04 | Maximum polarization 2008 (Feistritz ob Bleiburg): 32.27

Figure 2.2: Political polarization, continued

(c) 2013 national elections



(d) 2017 national elections



Notes: The figure shows the party configurations and municipal vote shares (represented by the area of circles) underlying the minimum (graphs on the left) and maximum (graphs on the right) values of our polarization index for the 2006, 2008, 2013, and 2017 national elections. To locate Austrian parties in a policy space, we follow Jäckle and König (2019) by applying multidimensional scaling to data from the Austrian voting advice application *wahlkabine.at*. More details on the calculation of the polarization index are provided in the appendix.

where Y_{it} describes our measure of political polarization. ϵ_{it} is the error term. We do not include control variables because all socio-economic and fiscal variables may be determined by a local mayor as well and are therefore "bad controls". We cluster standard errors at the

level of municipalities. We use a bandwidth of 30% around the 50% threshold. We also run estimations where we weight the sample by matching weights derived from propensity score matching. Table A2.8 in the appendix shows the matching variables used and their sample mean under right-wing populist mayors before and after the matching procedure. We match over a cross-section and several population and geographical characteristics, namely the share of Slovenian speaking and Protestant population in 2001 and the minimum altitude above sea level, the altitude slope, and the share of settlement area. A comparison of the mean values between municipalities with and without far-right populist mayors reveals that the propensity score matching balances the two samples quite well.

We also apply the nonparametric RD technique developed by Calonico *et al.* (2014, 2018), including the optimal polynomial and bandwidth selection procedure as the most flexible specification. Hyytinen *et al.* (2018) show that the nonparametric technique by Calonico *et al.* (2014, 2018) may well be equivalent to a randomized experiment in the context of close elections when using robust standard errors. We also allow for flexible bandwidths and RD polynomials.

Results

Table 2.1 shows our baseline results for political polarization: linear polynomial RD estimations (columns 1 to 3) and local-linear RD estimations including the optimal polynomial bandwidth selection procedure (column 4). Coefficient estimates correspond to the local average treatment effect of a marginally elected far-right populist mayor compared to marginally defeated far-right populist candidates. We use annual data in column (1) and election term averages in all other specifications. The results in column (1) to (4) show that far-right mayors increased political polarization: the index of polarization was around 4.17 points (almost two standard deviations) higher under far-right mayors than mayors of other parties (column 4). The estimates are smaller when we use linear polynomial RD compared to local-linear RD (columns 1 to 3). We believe that increasing political polarization under far-right mayors conveys how far-right policymakers influence social coexistence in many industrialized countries.

We submitted our results to six robustness tests for which we use election term averages and the local-linear RD specification. Table A2.9 shows point estimates of the far-right populist dummy variable for our preferred local-linear RD specification (corresponding to column 4 in table 2.1). As a first robustness test, we exclude all municipalities that never had a far-right populist mayor over the sample period. SPÖ-strongholds in particular can differ considerably from municipalities where the FPÖ/BZÖ receives large vote shares (column 1). Secondly, we exclude municipalities with a Slovenian population share above 5% (column 2). The indigenous Slovene-speaking population group in southern Carinthia constitutes an ethnic minority whose guaranteed rights have been continuously called into question, especially under the Carinthian governor Jörg Haider. As a third robustness test, we exclude municipalities with a substantial Protestant population (population share above 50%), since

Table 2.1: Political polarization under populists

	Political polarization			
	(1)	(2)	(3)	(4)
Far-right populist	0.96*	1.27*	1.34	4.17**
	(0.58)	(0.70)	(0.84)	(2.12)
RD method	Linear	Linear	Linear	Local-lin.
Term average	No	Yes	Yes	Yes
Matching	No	No	Yes	No
Mean dep. var.	26.76	26.10	26.17	26.32
RD Bandwidth	30	30	30	9.32
Municipalities	86	86	79	45
Observations	340	221	194	76
Adjusted R^2	0.05	0.06	0.08	–

Notes: The table shows the results of linear polynomial RD estimations (columns 1 to 3) and local-linear RD estimations including the optimal polynomial and bandwidth selection procedure, see Calonico *et al.* (2014, 2018) (column 4). The dependent variable is an index of political polarization at national elections, measured at the level of 132 Austrian municipalities. The running variable is the vote share for a far-right populist candidate in mayoral elections. For linear specifications, a bandwidth of ± 30 around the victory threshold of 50% applies. Significance levels (standard errors clustered at the municipality level in linear RD, robust RD standard errors in local-linear RD): *** 0.01, ** 0.05, * 0.10.

religion might shape voting preferences in such areas (column 3). Fourthly, we use only mayors elected in a run-off ballot. When candidates fail to gain an absolute majority in the first electoral round, the electoral race is more hotly contested and we would also expect a sharpened far-right populist profile when mayors elected under such circumstances take office (column 4). We also compare far-right populist mayors to social democratic (SPÖ) mayors in column (5) and conservative (ÖVP) mayors in column (6) only to investigate whether there exist counteracting effects regarding the FPÖ/BZÖ's main political contestants.

The robustness tests corroborate that far-right populist mayors increased political polarization. Most importantly, the polarizing effect of FPÖ mayors turns out equally strong, irrespective of whether we compare them to their social democratic or conservative counterparts only.

2.4.2 Civil society

The scapegoating mechanisms employed by far-right populists are directed towards ethnic or religious minorities ("them below") and, when expedient, target socialists, capitalists, the European Union, the media, ruling parties ("them above"), and more (Wodak, 2019). Constructing a seemingly self-evident "us" and "them" plays an outsized role in far-right populist rhetoric and lays the foundation for social divide.

So far, however, there is no empirical evidence on polarization within the civil society under far-right populist politicians. We propose to investigate local football teams, since sports clubs are an important means for integrative achievements and social capital within society. Far-right activities are perceptible in parts of organized civil society, the AfD for instance declares the "*march through the organizations*"[21], i.e. the deliberate infiltration of associations and clubs, as a strategic goal. Football in particular offers a central point of reference for far-right ideologies due to its cultural significance and the aspect of national identity (Geisler and Gerster, 2009). Accordingly, far-right populists often polemicize against a supposedly foreign infiltration of (national) football teams. In 2010, the FPÖ mayor of Klagenfurt tied a loan for the Bundesliga soccer club Austria Kärnten to the following condition:

> "*If we are going to put together an aid package, it is imperative that we fix the number of Carinthians in the squad. No foreigners should be signed up, but the academy's kickers should be pushed.*" (Christian Schneider, FPÖ mayor of Klagenfurt, 2010).[22]

We measure polarization within civil society via the diversity on local football teams. Football (soccer) is the most famous sport in Europe. We self-compile individual match data on Carinthian football teams over the period 2006-2017 from the website *kfv-fussball.at*. The final dataset is constructed at the team-match level, i.e. there are two observations per match, one for each competing team. In total, our football data contains 7,923 matches of 138 Carinthian teams from 88 municipalities. For each individual match, we have information about the team that competed, more specifically the names of the players. Using an API (*Application Programming Interface*), we classify the 7,206 distinct names by their most likely country of origin. We aim to measure a home bias on local Carinthian football teams under far-right populist rule. Due to the similar sociolinguistics of Austrian, German, and Swiss names, we define players of these countries of origin as natives and all other players as foreign.[23] For each match, we calculate the share and number of competing foreign players as indicators of cultural diversity on the team.

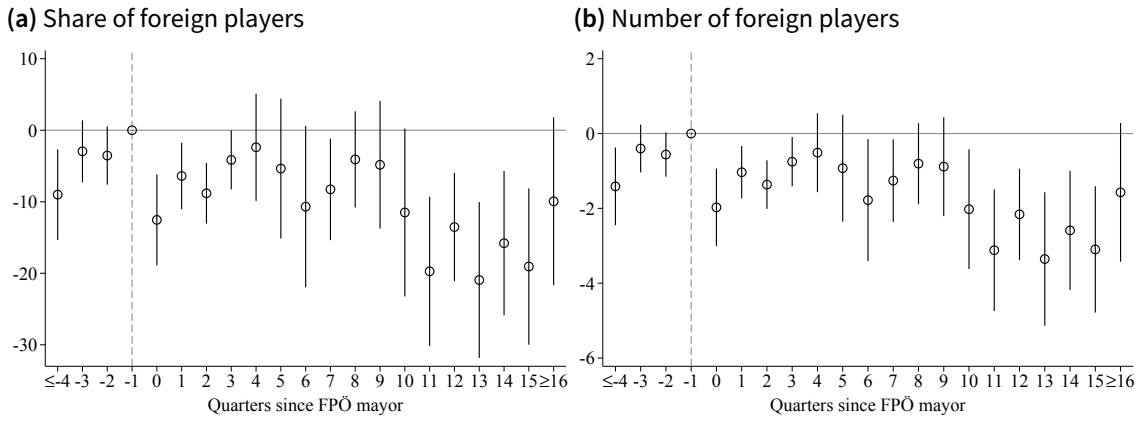
The overwhelming majority of names has a European background (96.53%), while players with Asian (2.15%) or African names (1.32%) are largely underrepresented. The top five countries of origin among foreign football players are Croatia, the Republic of Serbia, Bosnia and Herzegovina, Italy, and Slovenia, their shares ranging between 1% and 3%. These countries

[21] "Wie Rechtspopulisten Vereine und Verbände infiltrieren", Deutschlandfunk, 02.03.2020, `https://www.deutschlandfunk.de/zivilgesellschaft-wie-rechtspopulisten-vereine-und.724.de.html?dram:article_id=471987`.

[22] Original in German language: "Wenn wir ein Hilfspaket schnüren, ist ein Festschreiben der Anzahl der Kärntner im Kader unabdingbar. Es dürfen keine Ausländer verpflichtet, sondern die Kicker der Akademie forciert werden." (Kleine Zeitung, 07.04.2010).

[23] For the ease of readability, we differentiate between native and foreign players in the following, although strictly speaking, we should refer to players with names of native and foreign origin.

Figure 2.3: Event studies – Diversity on football teams



Notes: The figure plots quarter-specific coefficient estimates for diversity on local football teams (share and number of foreign players) from event study regressions with their 90% confidence intervals. We focus on the year before and four years after close mayoral elections (vote margin $\leq 10\%$), where an FPÖ candidate gained the first or second place. Effects are relative to the first quarter of the election year, indicated by the vertical dashed line. Data are at the team-match level and cover 7,923 matches over the period 2006-2017. All specifications include match, year, and league fixed effects and control for the diversity of the opposing team. Standard errors are clustered at the municipality level.

also rank among the top ten of foreign countries of origin in the overall Carinthian population. In terms of origin, local football teams thus seem to provide a good representation of the civil society as a whole. On average, there are three foreign players on every team.

Figure 2.3 and table 2.2 show results for our measures of diversity in local football teams. Figure 2.3 presents coefficients from event study specifications where we regress the share and number of foreign players on quarter-specific indicators of far-right populist rule. Our event window comprises the year before and the four years after a new mayor takes office and effect sizes are relative to the pre-electoral quarter.[24] To match the RD approach, we focus on closely elected or defeated far-right populist candidates. Our estimates suggest a significant negative effect of far-right populist mayors on the diversity of local football teams. This effect is immediate during the first year of office and gets even more pronounced in the third year. Quantitatively, under far-right populist mayors, the foreign share decreases by around 10 percentage points or two players on the local football team.

Regression results presented in table 2.2 confirm the event study estimates. The estimation sample is restricted to football teams from municipalities with a first- or second-placed far-right candidate at mayoral elections. All estimated coefficients for the diversity measures are negative, although they do not turn out to be statistically significant in columns (1) to (3), where we use all vote margins. As soon as we limit the sample to closely elected candidates,

[24] Mayoral elections take place in March, so the first quarter of the election year serves as base category.

Table 2.2: Regressions – Diversity on football teams

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Share of foreign players									
Far-right populist	-1.20	-0.95	-1.43	-8.94**	-9.23**	-2.93	-8.82**	-9.21**	-8.73**
	(3.16)	(3.09)	(3.26)	(3.96)	(4.02)	(3.35)	(3.92)	(3.97)	(4.07)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Club FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Home match		Yes	No		Yes	No		Yes	No
Opponent FE							Yes	Yes	Yes
Mayor margin				≤ 10	≤ 10	≤ 10	≤ 10	≤ 10	≤ 10
Observations	5,826	2,916	2,910	3,133	1,574	2,041	3,048	1,530	1,518
Panel B: Number of foreign players									
Far-right populist	-0.25	-0.15	-0.34	-1.39**	-1.45**	-0.47	-1.37**	-1.45**	-1.35**
	(0.46)	(0.49)	(0.44)	(0.58)	(0.60)	(0.50)	(0.58)	(0.59)	(0.59)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Club FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Home match		Yes	No		Yes	No		Yes	No
Opponent FE							Yes	Yes	Yes
Mayor margin				≤ 10	≤ 10	≤ 10	≤ 10	≤ 10	≤ 10
Observations	5,826	2,916	2,910	3,133	1,574	2,041	3,048	1,530	1,518

Notes: The table shows the results of linear regression estimations. The unit of observation are football matches of 106 Carinthian clubs (of 72 municipalities) between 2006-2013. We focus on football teams in municipalities where a far-right populist candidate gained the first or second place in mayoral elections. The dependent variables are measures for diversity on local football teams (the share and the number of foreign players). In columns (4) to (9), we restrict the sample to municipalities with a closely elected or closely defeated far-right populist candidate (margin of victory $\leq 10\%$). In columns (2) to (3), (5) to (6) and (8) to (9), we further differentiate between home and away matches. We add opponent fixed effects in columns (7) to (9). Significance levels (standard errors clustered at the municipality level): *** 0.01, ** 0.05, * 0.10.

in the spirit of RD, coefficient estimates reach statistical significance at the 5% level and also increase numerically. Even the most rigorous specifications (in columns 8 and 9), where we control for year, team, and opponent fixed effects, show a significant divide in terms of cultural diversity between teams from FPÖ-dominated and other municipalities: when the local civil society is exposed to a far-right populist mayor, the number of foreign players engaged in the local football team decreases by one player (or 9 percentage points in relative terms).

2.5 Economic outcomes under far-right populists

2.5.1 Background

As far as economic policies are concerned, far-right populist parties are stuck between the devil and the deep blue sea. Far-right populist parties wish to attract disenchanted voters from both established left-wing and right-wing parties. Disenchantment with the policies pursued by established parties, however, often has manifold reasons and motivations to support left-wing and right-wing parties used to differ (partisan theories – Hibbs 1977, Chappell and Keech 1986, Alesina 1987; see Potrafke 2017, 2018 for surveys). Offering a platform that gratifies the economic needs of both groups of voters is difficult. The struggle of far-right populist parties to attract voters from both established left-wing and right-wing parties translates into barely coherent party manifestos, or manifestos that merely exclude the policy fields in which party members do not agree – consistent anti-immigration proposals notwithstanding.

Norris and Inglehart (2019) show that far-right populist parties differ a great deal in their economic policies. On the one hand, some far-right populist parties promote expansionary economic policies (similar to those of established left-wing parties). This seems to be a necessary precondition for attracting blue-collar workers and may well be one of the instruments that far-right populist parties use strategically in the public discourse. Examples of far-right populist parties promoting left-wing economic policies are the German NPD, the Greek Golden Dawn, or the Hungarian Jobbik. On the other hand, white-collar or self-employed supporters of far-right populist parties prefer less expansionary economic policies – giving rise to a conflict of policy platforms. Some far-right populist parties like the German AfD or Donald Trump's supporters are quite market-oriented (similar to established right-wing parties).

Far-right populists also adjust their economic policy proposals according to the zeitgeist. Austrian far-right populists are an excellent case in point. In a coalition with the conservative ÖVP, the FPÖ implemented some market-oriented reforms, such as increasing the retirement age, in the early 2000s (Afonso, 2015). In 2005, the populist far-right camp broke up into the FPÖ, which proposed state interventions and called themselves "the social home party" (*"Die soziale Heimatpartei"*), and the somewhat more market-oriented BZÖ. However, the BZÖ did not implement consistent policies either. For example, the BZÖ prime minister of Carinthia, Jörg Haider, introduced state-run gas stations offering cheap diesel, but also proposed to cut bureaucracy, to introduce a "flat tax", and to oppose the *Basel II* recommendations on banking laws and regulation at the same time. It remains an empirical question what kind of economic policies far-right populists would implement if they were to enter office.

Evidence on the economic policies under far-right populists is scarce. Anecdotal evidence of far-right populist parties in Dutch governments does not suggest that these governments implement very different policies (Afonso, 2015). In Austria, the FPÖ basically fulfilled as many pledges as the conservative ÖVP when the ÖVP and the FPÖ were forming a coalition at the national level in 2002/2003 (Schermann and Ennser-Jedenastik, 2014).

2.5.2 Conventional economic outcomes

We investigate population, tax revenues, the unemployment rate, and public debt as economic outcomes. Mayors have large agenda-setting power in proposing the annual budget and allocating spending within expenditure types. Prioritizing spending on housing or childcare may stimulate migration and births, health expenditures can reduce mortality. We therefore investigate how total population changes under populist mayors. Mayors also influence economic growth; for example, by providing suitable public infrastructure or a business-friendly administration. To measure economic outcomes, we use total local tax revenues which are a fixed share of local wages and the unemployment rate.[25] Finally, we use public debt as a dependent variable. All monetary outcomes are measured in per capita values and 2017 prices. We employ an RD approach as described in section 2.4.1.

The results in table 2.3 do not suggest that economic outcomes were different under far-right populist mayors and mayors from other parties. The coefficients of the far-right populist lack statistical significance when we use population, tax revenues, the unemployment rate, and public debt as the dependent variables. These results are in line with the programmatic flexibility of far-right populist parties regarding economic policies. The FPÖ favors an arbitrary mix of redistribution and deregulation policies, possibly influenced by the wide range of voters it aims to cater to.

We have also examined public expenditure and migration outcomes, see tables A2.10 and 2.4. Total expenditures do not differ between far-right populist mayors and mayors from other parties. Social and health expenditures decrease under far-right populist mayors. Table 2.4 shows that net foreign migration decreased under far-right mayors, an effect which corroborates the results of Bracco *et al.* (2018) who use Italian data.

2.5.3 Budget transparency

We investigate whether far-right populists influence the transparency of economic policymaking. From a theoretical point of view, the effects of populists in office on transparency policies are ambiguous. On the one hand, populists claim to represent "the people" and to be in direct contact with their voters. Donald Trump, for example, uses Twitter to signal rapid actions and a direct connection to a broad audience. On the other hand, populists might be interested in more opaque policymaking.

We measure whether far-right populists adopt budget transparency to a different extent than other politicians using data from the project *offenerhaushalt.at*. The project was launched in 2013 and collects figures on all 2,100 Austrian municipality budgets. The project increased local budget transparency in Austria a great deal. Before 2013, comparative data on local public finance were mainly published in hardcover copies or databases of the statistical office

[25] We define the unemployment rate as the share of unemployed in the population between 20 and 65.

Table 2.3: Economic outcomes under populists

	Population				*Tax revenues*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Far-right populist	-2.05	-3.81	-5.86	0.40	57.26	44.33	53.34	78.68
	(2.02)	(3.64)	(5.67)	(2.42)	(41.66)	(44.19)	(42.23)	(59.38)
RD method	Linear	Linear	Linear	Local-lin.	Linear	Linear	Linear	Local-lin.
Term average	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Matching	No	No	Yes	No	No	No	Yes	No
Mean dep. var.	4.29	4.57	4.61	4.20	151.17	150.43	147.37	146.26
RD Bandwidth	30.00	30.00	30.00	9.22	30.00	30.00	30.00	11.37
Municipalities	85	85	78	44	83	83	76	47
Observations	806	186	166	62	776	146	132	57
Adjusted R^2	0.01	0.01	0.01	–	0.03	0.02	0.02	–
	Unemployment				*Public debt*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Far-right populist	-0.00	-0.00	-0.01	-0.01	-122.52	-29.19	3.59	-712.10
	(0.00)	(0.00)	(0.00)	(0.01)	(338.46)	(295.33)	(302.97)	(542.39)
RD method	Linear	Linear	Linear	Local-lin.	Linear	Linear	Linear	Local-lin.
Term average	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Matching	No	No	Yes	No	No	No	Yes	No
Mean dep. var.	0.06	0.06	0.06	0.06	1,427.56	1,390.00	1,435.33	1,440.36
RD Bandwidth	30.00	30.00	30.00	12.21	30.00	30.00	30.00	11.95
Municipalities	86	86	79	47	85	85	78	47
Observations	1,166	221	194	96	816	186	166	80
Adjusted R^2	0.00	0.01	0.01	–	0.00	0.00	0.00	–

Notes: The table shows the results of linear polynomial RD estimations (columns 1 to 3 and 5 to 7) and local-linear RD estimations including the optimal polynomial and bandwidth selection procedure, see Calonico *et al.* (2014, 2018) (columns 4 and 8). The dependent variables are economic outcomes (total population in 1,000, tax revenues per capita, the unemployment rate, and public debt per capita), all measured at the level of 132 Austrian municipalities. The running variable is the vote share for a far-right populist candidate in mayoral elections. For linear specifications, a bandwidth of ± 30 around the victory threshold of 50% applies. Significance levels (standard errors clustered at the municipality level in linear RD, robust RD standard errors in local-linear RD): *** 0.01, ** 0.05, * 0.10.

and municipality associations which are not easily accessible. State governments monitor and supervise local governments but also hardly publish any comparative data. The project *offenerhaushalt.at* lowered the barriers to access local budget information substantially. Data exists for all municipalities, but is only publicly accessible for municipalities that explicitly agree to publication. All municipalities were invited by letter to make their data public at zero costs: participation is free and municipalities do not have to deliver any data. By early 2018, around 1,000 municipalities have joined the project. In Carinthia, 7, 14, 18, 5, and 4 out of the

Table 2.4: Migration under populists

	Net foreign migration			
	(1)	(2)	(3)	(4)
Far-right populist	-4.76	-4.00	-5.02*	-5.46**
	(4.41)	(2.49)	(2.64)	(2.63)
RD method	Linear	Linear	Linear	Local-lin.
Term average	No	Yes	Yes	Yes
Matching	No	No	Yes	No
Mean dep. var.	4.12	3.90	4.15	3.64
RD Bandwidth	30	30	30	6.25
Municipalities	85	85	78	35
Observations	716	186	166	44
Adjusted R^2	0.01	0.06	0.05	–

Notes: The table shows the results of linear polynomial RD estimations (columns 1 to 3) and local-linear RD estimations including the optimal polynomial and bandwidth selection procedure (column 4), see Calonico *et al.* (2014, 2018). The dependent variable is net foreign migration (per 1,000 capita), measured at the level of 132 Austrian municipalities. The running variable is the vote share for a far-right populist candidate in mayoral elections. For linear specifications, a bandwidth of ± 30 around the victory threshold of 50% applies. Significance levels (standard errors clustered at the municipality level in linear RD, robust RD standard errors in local-linear RD): *** 0.01, ** 0.05, * 0.10.

total of 132 municipalities joined the project in the years 2013, 2014, 2015, 2016, and 2017. 84 municipalities refused to participate by the end of 2017. We collect information on cases where Carinthian municipalities decided to make their budget figures publicly available.

We apply probit and Cox (proportional hazard) regressions when we investigate budget transparency. The decision to join the project *offenerhaushalt.at* is binary, so RD is not suitable in this setting. In the probit estimations, the dependent variable is a dummy variable that takes on the value of one only in the year when a municipality made their budget figures publicly available on *offenerhaushalt.at*, and equals zero in years before and after the decision. However, adoption decisions are not independent over time. Once adopted, no Carinthian municipality left the project. Therefore, we model the survival time until joining the project using Cox regressions.

Table 2.5 shows the results of these regressions. Column (1) shows that the probability of adopting budget transparency was by around 4 percentage points lower under far-right populist mayors than under mayors of other parties. To account for the fact that adoption decisions are not independent over time, we report Cox regression results in columns (2) to (4) in table 2.5. They corroborate our probit estimations: the probability to adopt budget transparency decreases under far-right populist mayors, even when we include year fixed effects and restrict the sample to narrow election outcomes. Populists therefore seem to be less interested in transparency than their colleagues from more moderate parties.

Table 2.5: Budget transparency

	Budget transparency			
	(1)	(2)	(3)	(4)
Far-right populist	-0.43*	-1.05**	-1.08**	-0.96*
	(0.22)	(0.52)	(0.52)	(0.57)
Estimation	Probit	Cox	Cox	Cox
Year fixed effects	No	No	Yes	Yes
Bandwidth	–	–	–	30.00
Observations	660	549	549	240
Pseudo R^2	0.01	–	–	–

Notes: The table reports probit (column 1) and Cox regression outputs (columns 2 to 4). We report coefficients for Cox regressions. The unit of observation are the years 2013 to 2017 in the 132 municipalities of Carinthia. The dependent variable is a dummy variable that takes on the value of one if a municipality shares their budget figures online (`www.offenerhaushalt.at`), and equals zero otherwise. The project *offenerhaushalt.at* was launched in 2013. 7, 14, 18, 5, and 4 out of the total of 132 municipalities joined the project in the years 2013, 2014, 2015, 2016, and 2017. We add year fixed effects in column (3) and limit the sample to a bandwidth of ± 30 around the victory threshold of 50% in column (4). Significance levels (standard errors in brackets; probit: clustered at the municipality level): *** 0.01, ** 0.05, * 0.10.

2.6 Conclusion

There has been a lack of evidence on political and economic outcomes under far-right populist governments. An important reason is that far-right populist parties have not yet become senior coalition partners in many national governments. We examine how far-right populist mayors influence cultural and economic outcomes. The sample includes 132 Austrian municipalities over the period 1991-2017, of which 14% were led by a far-right populist mayor. Our results show that FPÖ mayors increased political polarization and polarization within civil society, as reflected in a home bias towards Austrian players on local football teams. This result is corroborated by decreased net foreign migration into municipalities under FPÖ rule. By contrast, the results do not suggest that far-right populists influenced economic outcomes such as unemployment and public debt in a different manner than mayors from other parties. Budget transparency decreased, however, under FPÖ mayors suggesting that far-right populists are less interested in the transparency of policymaking than politicians of established political parties.

Our results provide novel insights on the question of how far-right populists influence the political culture in local communities. While the anti-establishment and anti-immigrant rhetoric employed by far-right politicians is well-documented, we show evidence that far-right populists divide the civil society. These findings can be rationalized by changes in the social

acceptability of holding particular views. Electoral success of radical right parties legitimizes their views in the eyes of the electorate and might encourage people sharing those views to support them more openly (Bischof and Wagner, 2019; Bursztyn *et al.*, 2020).

Our results on political polarization under far-right populist rule align well with this intuition: when FPÖ mayors are elected into office, their extreme views gain social acceptance. Ultimately, voters on both sides will take up more radical positions, giving rise to a "voiding of the middle" when vote shares are relatively evenly split between ideologically distant parties. Moreover, the legitimization effect should be particularly strong for far-right populists' most salient policy issues regarding the openness to foreigners. Anecdotal evidence on the FPÖ's strong nationalist and xenophobic views even at the local level suggests that upon their election, the stigma associated with holding these previously-extreme views is reduced. In places where anti-foreign sentiments become acceptable, foreign residents withdraw from civil society by engaging less in local football teams. At the same time, net foreign migration decreases. These findings do not bode well for the social cohesion in communities governed by an FPÖ mayor.

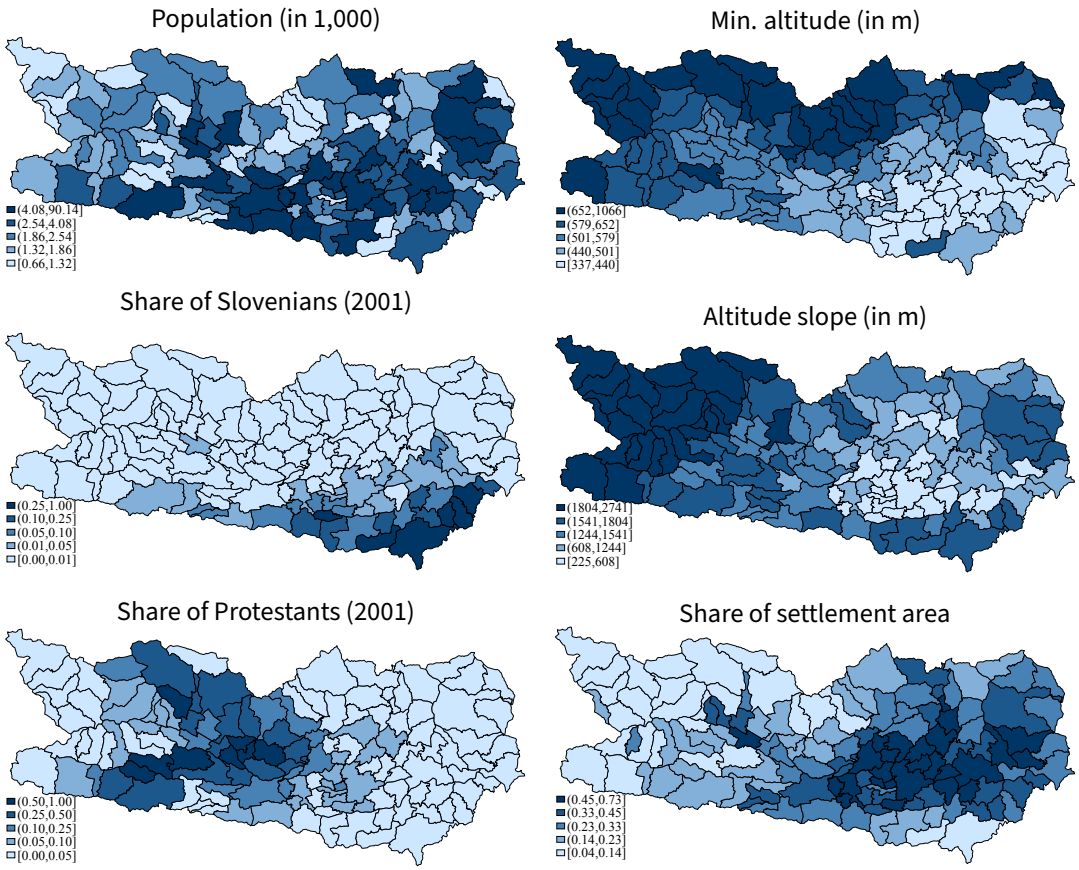
We propose changing social norms regarding xenophobic or more extreme views in general as a channel to explain our findings. We cannot, however, directly measure more common public expressions of anti-foreign sentiments at the local level. Future research should examine in more detail the polarizing effect of far-right populists and propose further measures of their divisive power.

Appendix

Figure A2.1: Geography of Carinthia

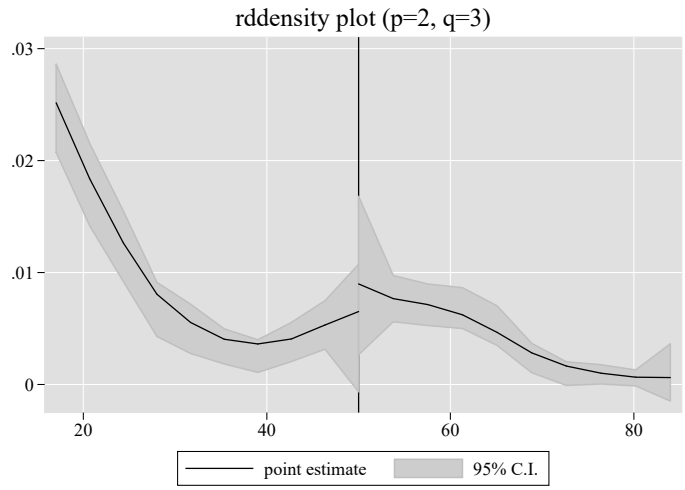
Notes: The map plots the location of the 132 Carinthian municipalities (blue shaded). Gray lines represent national boundaries, the black line denotes the country of Austria.

Figure A2.2: Socio-economic characteristics of Carinthia



Notes: The maps plot socio-economic and geographic characteristics of the Austrian state of Carinthia. The maps showing the Slovenian and Protestant minorities use class breaks as denoted in the legend. The other maps are organized in five quantiles: the darker a shaded municipality, the larger the respective variable.

Figure A2.3: McCrary bunching test



Notes: The figure reports the test for suspicious bunching at the 50% vote share threshold for a far-right populist victory. We use a local-polynomial density estimation (Cattaneo *et al.*, 2020, McCrary, 2008). The unit of observation are the 132 municipalities of Carinthia at five local elections between 1991 and 2017 (1991, 1997, 2003, 2009, 2015). The corresponding p-values are 0.42 (conventional) and 0.32 (robust).

Table A2.1: Strength of mayors in Europe

Country	*Strength score*
France	12
Spain	11
Italy	10
Greece	10
Austria (directly elected)	**9**
Germany	9
England (mayor and cabinet)	8.5
Belgium	8
Hungary	8
Austria (not directly elected)	7
Germany (state of Hesse)	7
Poland	6
Denmark	6
Czech Republic	5.5
Portugal	5
England (leader and cabinet)	5
England (alternative)	5
Ireland	5
Netherlands	5
Switzerland	4
Sweden	3

Notes: The table reproduces table 3 by Heinelt and Hlepas (2006, p.38), comparing the power of mayors in European countries. Mayors in in the federal state of Carinthia are directly elected (highlighted in bold).

Table A2.2: Characteristics of elected mayors

	Elected in first round			*Elected in run-off*		
	Far-right populist	*Other mayor*	*Mean diff.*	*Far-right populist*	*Other mayor*	*Mean diff.*
	(1)	(2)	(3)	(4)	(5)	(6)
Vote share	63.02	64.24	-1.21	56.70	56.41	0.29
University degree	0.18	0.13	0.05	0.17	0.22	-0.05
Female	0.02	0.02	0.00	0.02	0.06	-0.03
Periods in office	3.02	2.77	0.25	2.39	2.21	0.18
Observations	50	431	481	41	136	177

Notes: The table reports mean characteristics of far-right populist mayors (columns 1 and 4) compared to all other mayors (columns 2 and 5). The unit of observation are the 132 municipalities of Carinthia at five local elections between 1991 and 2017 (1991, 1997, 2003, 2009, 2015). We distinguish mayors elected in the first round of the election (left-hand side), and mayors elected in the run-off ballot (right-hand side). Columns (3) and (6) report mean differences. Significance levels: *** 0.01, ** 0.05, * 0.10 (no statistically significant difference to report).

Table A2.3: Transitions of mayors

	Next mayor			
	Far-right populist	*Social democratic*	*Conser-vative*	*Other party*
	(1)	(2)	(3)	(4)
Mayor				
Far-right populist (FPÖ/BZÖ)	**55**	7	6	4
Social democratic (SPÖ)	15	**233**	21	10
Conservative (ÖVP)	11	21	**85**	4
Other party	3	6	2	**43**

Notes: The table reports how mayors in the 132 municipalities of the Austrian state of Carinthia change over time. The table reads as follows: we plot mayors in office (rows) against the mayor winning the following election (columns). The diagonal represents cases with no change in office after a local election (bold). All other cells denote changes in mayor's party. In 11 cases, for example, a municipality with a conservative mayor switched to a far-right populist mayor after the local election (column 1, third row).

Table A2.4: Municipalities without changes in mayors or in mayor's party

	Far-right populist	*Social democratic*	*Conser-vative*	*Other party*	*Total*
	(1)	(2)	(3)	(4)	(5)
Mayor					
No change 1991–2017	0	4	2	4	10
Change 1991–2017	–	–	–	–	122
Mayor's party					
No change 1991–2017	4	39	11	6	60
Change 1991–2017	–	–	–	–	72

Notes: The table reports the number of municipalities without changes in the mayor (upper panel) or mayor's party (lower panel). 10 out of 314 mayors were in office over the entire period of observation (1991 to 2017). In 60 out of 132 municipalities, mayor's party did not change between 1991 and 2017. By contrast, there was a swing in 72 out of 132 municipalities.

Table A2.5: Descriptive statistics

	Obs.	*Mean*	*Std. Dev.*	*Min*	*Max*
	(1)	(2)	(3)	(4)	(5)
Political culture					
Political polarization (index)	1,052	26.70	2.33	16.69	32.27
Share of foreign players	15,080	17.67	11.97	0	92.86
Number of foreign players	15,080	2.66	1.81	0	13
Economic outcomes					
Population (in 1,000)	2,244	4.23	9.77	0.60	99.79
Tax revenues (per capita)	2,112	153.69	135.89	0.00	1,521.77
Unemployment (per working age pop.)	3,420	0.06	0.02	0	0.20
Public debt (per capita)	2,244	1,486.39	1,382.19	0	9,814.13
Budget transparency (0/1)	660	0.24	0.43	0	1
Mayor					
Far-right populist (FPÖ/BZÖ)	3,420	0.14	0.34	0	1
Social democratic (SPÖ)	3,420	0.52	0.50	0	1
Conservative (ÖVP)	3,420	0.23	0.42	0	1
Other party	3,420	0.11	0.31	0	1
Matching variables					
Share of Slovenians (2001)	3,420	0.04	0.10	0	0.89
Share of Protestants (2001)	3,420	0.11	0.16	0	0.74
Min. altitude (in m)	3,420	564.27	154.47	336.64	1,066.32
Altitude slope (in m)	3,420	1,302.80	627.69	225.36	2,740.96
Share of settlement area	3,420	0.31	0.18	0.04	0.73

Notes: The table reports the descriptive statistics of our dataset, including information on political culture, economic outcomes, the political landscape, and further geographical and socio-economic variables. We observe 132 municipalities in the Austrian state of Carinthia over the period 1991 to 2017 (1991: 127 municipalities, 1992 to 1997: 130 municipalities). Fiscal variables are in 2017 prices and per capita. The working age population is between 20 and 65 years old.

Table A2.6: Elected and defeated far-right populist candidates

	Vote margin ±30%			*Vote margin ±15%*		
	Elected	*Defeated*	*Mean diff.*	*Elected*	*Defeated*	*Mean diff.*
	(1)	(2)	(3)	(4)	(5)	(6)
Vote share	30.86	59.65	-28.78***	43.97	57.23	-13.25***
University degree	0.19	0.18	0.01	0.28	0.19	0.08
Female	0.03	0.02	0.00	0.06	0.03	0.03
Observations	89	113	202	73	34	107

Notes: The table compares characteristics of elected far-right populist mayors (columns 1 and 4) to defeated far-right populist candidates (columns 2 and 5). The unit of observation are the 132 municipalities of Carinthia at five local elections between 1991 and 2017 (1991, 1997, 2003, 2009, 2015). We use vote share margins of ±30% and ±15% around the 50% victory threshold. Columns (3) and (6) report the significance of mean differences. Significance levels: *** 0.01, ** 0.05, * 0.10.

Table A2.7: Balancing test for geographic and socio-economic variables

	RD estim.	*p-value*
	(1)	(2)
Population (in 1,000)	-6.14	0.33
Share of Slovenians (2001)	-0.01	0.35
Share of Protestants (2001)	-0.01	0.93
Min. altitude (in m)	34.85	0.55
Altitude slope (in m)	364.33	0.31
Share of settlement area	-0.02	0.76

Notes: The table reports the results of local-linear RD estimations including the optimal polynomial and bandwidth selection procedure, see Calonico *et al.* (2014, 2018). The dependent variables are several geographic and socio-economic outcomes as plotted in figure A2.2. The unit of observation are the 132 municipalities of Carinthia at five local elections between 1991 and 2017 (1991, 1997, 2003, 2009, 2015). The running variable is the vote share for a far-right populist candidate in mayoral elections. Column (2) reports the p-values.

Table A2.8: Matching procedure

	Before matching		*After matching*	
	Far-right populist	*Other mayor*	*Far-right populist*	*Other mayor*
	(1)	(2)	(3)	(4)
Share of Slovenians (2001)	0.04	0.01	0.01	0.01
Share of Protestants (2001)	0.10	0.19	0.13	0.13
Min. altitude (in m)	555.25	620.90	617.50	611.96
Altitude slope (in m)	1,308.10	1,269.55	1,359.12	1,256.56
Share of settlement area	0.31	0.27	0.28	0.28

Notes: The table reports means of different variables for far-right populist mayors before (columns 1 and 2) and after running a propensity score matching procedure on these variables (columns 3 and 4). We match over a cross-section; the matching variable is a dummy which equals one for municipalities that had at least one far-right populist mayor in the period 1991 to 2017.

Table A2.9: Robustness tests

	Political polarization					
	(1)	(2)	(3)	(4)	(5)	(6)
Far-right populist	5.97***	3.90*	3.80	1.33	4.79***	5.48*
	(2.06)	(2.27)	(2.54)	(3.55)	(1.76)	(2.95)
Mean dep. var.	26.35	26.35	26.30	26.67	26.60	25.58
RD Bandwidth	8.43	9.27	10.27	5.36	7.37	12.88
Municipalities	44	42	39	31	26	20
Observations	71	71	67	38	35	36
	Net foreign migration					
Far-right populist	-6.62**	-4.91**	-8.35**	-4.28**	-1.61	-10.64*
	(3.19)	(2.50)	(3.31)	(1.95)	(2.19)	(5.95)
Mean dep. var.	3.80	3.53	3.78	3.68	3.86	4.03
RD Bandwidth	7.87	7.47	5.86	9.16	11.04	6.47
Municipalities	41	38	28	40	31	15
Observations	51	47	33	41	39	17
	Population					
Far-right populist	-6.46	-4.16	7.01*	9.34	-16.49	4.99
	(6.55)	(5.66)	(3.86)	(6.32)	(11.08)	(4.82)
Mean dep. var.	4.03	4.53	6.12	5.54	5.97	2.56
RD Bandwidth	23.78	14.45	5.96	7.69	25.96	10.41
Municipalities	65	48	29	38	45	19
Observations	135	90	34	37	79	26
	Tax revenues					
Far-right populist	156.18***	66.74	57.51	169.71*	135.93	35.70
	(46.14)	(64.30)	(70.61)	(87.00)	(134.43)	(104.58)
Mean dep. var.	148.54	152.18	158.11	146.11	152.01	135.52
RD Bandwidth	10.02	11.49	16.44	7.50	8.18	10.18
Municipalities	45	44	46	38	28	19
Observations	50	53	65	30	22	20
	Unemployment					
Far-right populist	-0.01	-0.01	-0.01	0.01	-0.02	-0.00
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Mean dep. var.	0.06	0.06	0.06	0.06	0.06	0.06
RD Bandwidth	13.82	11.93	12.18	5.47	7.83	8.07
Municipalities	51	44	41	32	26	17
Observations	106	89	81	39	35	23
	Public debt					
Far-right populist	-643.88	-1069.72*	-376.37	-718.70	-1083.78	-558.01
	(551.71)	(573.56)	(560.20)	(879.62)	(874.53)	(722.79)
Mean dep. var.	1386.55	1448.44	1428.15	1323.97	1498.92	1318.43
RD Bandwidth	15.00	10.87	11.89	9.09	11.16	13.75
Municipalities	51	44	41	40	31	21
Observations	96	68	68	41	39	35
RD method	Local-lin.	Local-lin.	Local-lin.	Local-lin.	Local-lin.	Local-lin.
Term average	Yes	Yes	Yes	Yes	Yes	Yes
Robustness	Within var.	Slovenian	Protestant	Run-off	SPÖ	ÖVP

Notes: We conduct six robustness tests for our preferred local-linear RD specification (table 2.1, column 4). In the first robustness tests (column 1), we include municipalities only with at least one far-right populist mayor in the period 1991 to 2017 (with within variation). Second, we exclude municipalities with a Slovenian population share $\geq 5\%$ (column 2). Third, we exclude municipalities with a Protestant population share $\geq 50\%$ (column 3). Fourth, we use only mayors elected in a run-off ballot (column 4). Fifth, we compare far-right populist mayors to social democratic (SPÖ) contestants (column 5) and conservative (ÖVP) contestants (column 6) only.

Table A2.10: Public expenditures under populists

	Total expenditures				*Social and health expenditures*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Far-right populist	297.99	332.07	108.28	-669.75	-22.13	-26.75	-42.32*	-75.57*
	(328.17)	(303.78)	(326.01)	(482.43)	(18.03)	(20.73)	(21.53)	(42.46)
RD method	Linear	Linear	Linear	Local-lin.	Linear	Linear	Linear	Local-lin.
Term average	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Matching	No	No	Yes	No	No	No	Yes	No
Mean dep. var.	2,646.17	2,635.05	2,673.80	2,871.79	364.10	363.83	361.90	366.63
RD Bandwidth	30.00	30.00	30.00	5.07	30.00	30.00	30.00	8.75
Municipalities	85	85	78	33	85	85	78	44
Observations	816	186	166	39	816	186	166	61
Adjusted R^2	0.03	0.03	0.01	–	0.02	0.02	0.07	–

Notes: The table shows the results of linear polynomial RD estimations (columns 1 to 3 and 5 to 7) and local-linear RD estimations including the optimal polynomial and bandwidth selection procedure, see Calonico *et al.* (2014, 2018) (columns 4 and 8). The dependent variables are total expenditures per capita and social and health expenditures per capita by local governments, measured at the level of 132 Austrian municipalities. The running variable is the vote share for a far-right populist candidate in mayoral elections. For linear specifications, a bandwidth of ± 30 around the victory threshold of 50% applies. Significance levels (standard errors clustered at the municipality level in linear RD, robust RD standard error in local-linear RD): *** 0.01, ** 0.05, * 0.10.

Appendix: Data description and sources

Polarization index

We measure political polarization in each municipality i in electoral term t based on municipalities' voting behavior at national elections and a two-dimensional policy space. To locate the Austrian parties in a policy space, we follow Jäckle and König (2019) by applying multidimensional scaling to data from the Austrian voting advice application *wahlkabine.at*. The data by Jäckle and König (2019) combines information about the salience of specific policy issues and the party positions for the four national elections in 2006, 2008, 2013, and 2017. The multidimensional scaling approach uses a dissimilarity matrix between party positions as input to determine a map or configuration in a small number of dimensions such that the distances between parties on the map reproduce approximately the original distances from the distance matrix. For the 2006 national election, this approach delivers a party configuration as shown in figure A2.4a.

We follow Schmitt (2016) and measure political polarization for a given party configuration according to equations (A2.1)-(A2.3),

$$Polarization_{it} = \sum_{j=1}^{N} \omega_{ijt} \times D(p_{jt}, \bar{p}_{it}) \tag{A2.1}$$

$$D(p_{jt}, \bar{p}_{it}) = \sqrt{\sum_{d=1}^{D} (p_{djt} - \bar{p}_{dit})^2} \tag{A2.2}$$

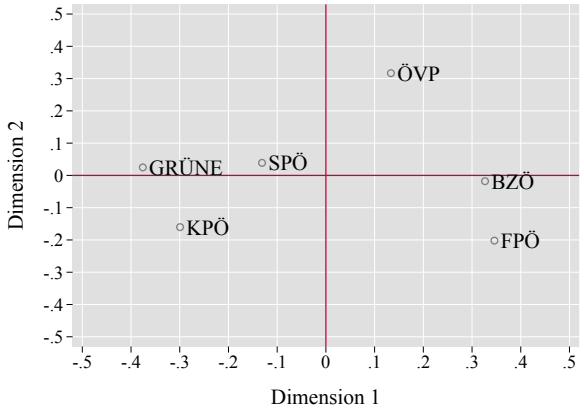
$$\bar{p}_{dit} = \sum_{j=1}^{N} \omega_{ijt} \times p_{djt}, \tag{A2.3}$$

where ω_{ijt} refers to the vote share gained by party j in municipality i in election year t and $D(p_{jt}, \bar{p}_{it})$ measures the Euclidean distance between each party's position (p_{jt}) and the ideological center ($\bar{p}_{it}$). The values of the polarization index are determined by the relative party positions in the two-dimensional space ($D = 2$) and the party vote shares in each municipality.

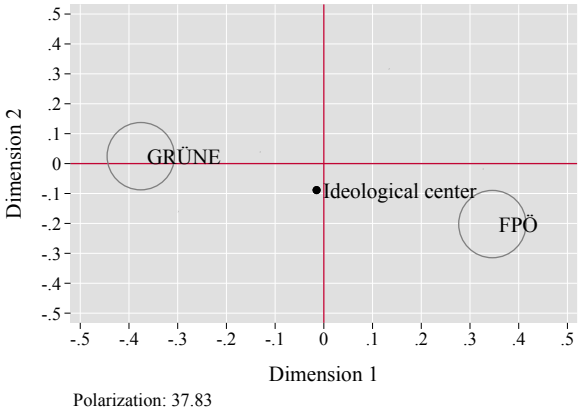
We use two- and three-party policy spaces – with the far-right FPÖ as constant political player – to show how the polarization index changes with its key parameters. All hypothetical scenarios are based on the party positions in the 2006 configuration. Figures A2.4b and A2.4c show extreme situations with a 50:50 vote split between the FPÖ and its most ideologically distant (close) political competitor GRÜNE (BZÖ). The corresponding values of the polarization index are 37.83 and 9.23. On the contrary, figure A2.4d portrays moderate polarization, where both FPÖ and SPÖ gain 50% of the votes ($Polarization_{it} = 26.72$). Referring to equation (A2.3), note that the ideological center in these stylized examples is always positioned halfway on

Figure A2.4: Measuring political polarization

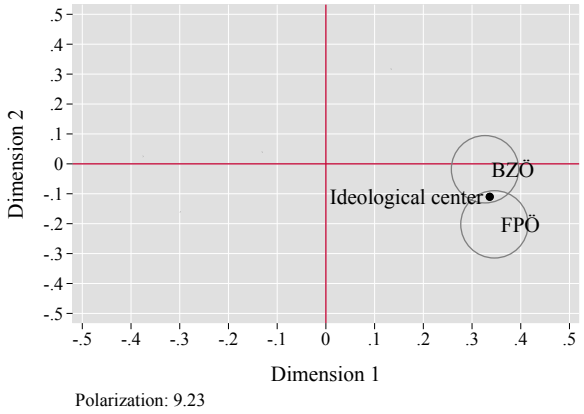
(a) Party configuration 2006



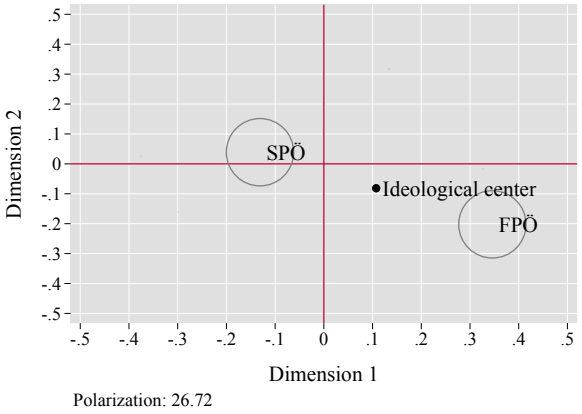
(b) Equal vote shares between FPÖ and GRÜNE



(c) Equal vote shares between FPÖ and BZÖ



(d) Equal vote shares between FPÖ and SPÖ



Notes: The figures show the two-dimensional party configuration derived from applying multidimensional scaling – Stata routine *mds* – to data on party positions at the Austrian national election 2006. The area of the circles represents (hypothetical) party vote shares.

the connecting line between the two parties in the policy space. If one party gets 100% of the votes, its position in the policy space coincides with the ideological center and the polarization index will be zero.

Table A2.11 lists the polarization index values for the above two-party policy spaces (figures A2.4b-A2.4d) with skewed voting outcomes: instead of a 50:50 vote split, the election outcome is now more skewed towards the FPÖ whose vote share is fixed at 70%. Polarization is now always lower than in the corresponding 50:50 vote split scenarios. Also, intuitively, polarization should be lower, since the FPÖ's vote margin is quite large.

Table A2.11: Polarization in a two-party policy space with skewed vote shares

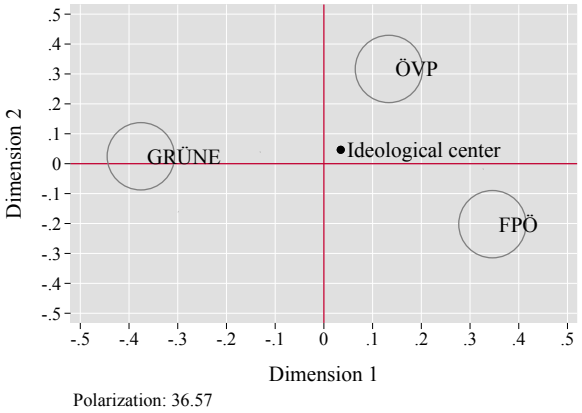
Party	FPÖ	GRÜNE	BZÖ	SPÖ	Polarization index
	70	30			31.78
Vote share (%)	70		30		7.78
	70			30	22.45

Notes: The table shows the values of the polarization index for different vote share configurations when fixing the FPÖ vote share at 70%.

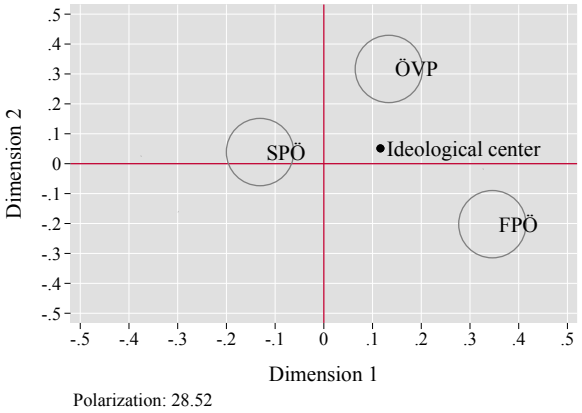
Finally, in figure A2.5 we consider three-party policy spaces with FPÖ, ÖVP, and GRÜNE (figures A2.5a and A2.5c), and FPÖ, ÖVP, and SPÖ (figures A2.5b and A2.5d). The corresponding values of the polarization index are denoted below the figures. Polarization is high ($Polarization_{it} = 36.57$) in the scenario shown in figure A2.5a, where FPÖ, ÖVP, and GRÜNE each gain one third of the votes. If the votes are distributed more unequally (FPÖ 50%, ÖVP 33% and GRÜNE 17%), the polarization index falls to a value of 33.37. In the more compact policy space with FPÖ, ÖVP, and SPÖ (figure A2.5b) polarization is lower. Moreover, the index value of 29 is relatively insensitive to the chosen vote share distribution.

Figure A2.5: Polarization in three-party policy spaces

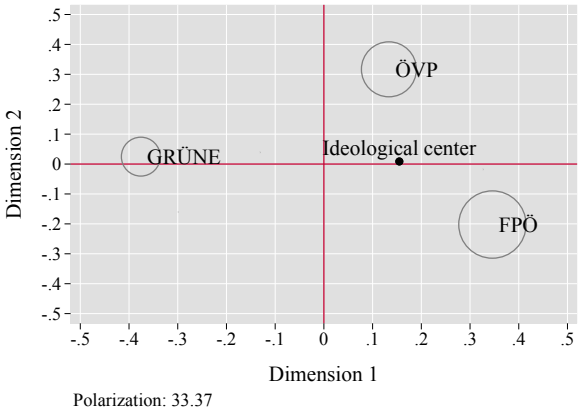
(a) Equal vote shares between FPÖ, ÖVP, and GRÜNE



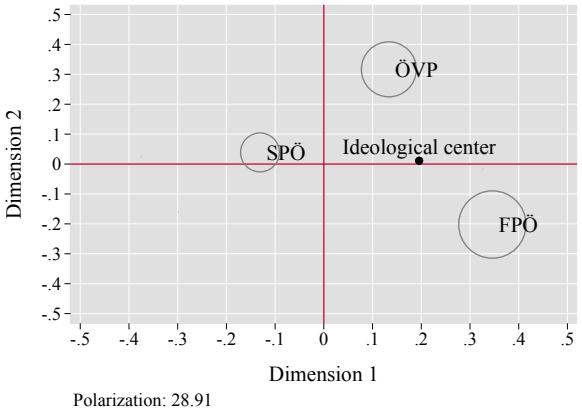
(b) Equal vote shares between FPÖ, ÖVP, and SPÖ



(c) Vote share split between FPÖ, ÖVP, and GRÜNE (50%,33%,17%)



(d) Vote share split between FPÖ, ÖVP, and SPÖ (50%,33%,17%)



Notes: The figures show the two-dimensional party configuration derived from applying multidimensional scaling – Stata routine *mds* – to data on party positions at the Austrian national election 2006. The area of the circles represents (hypothetical) party vote shares.

Table A2.12: Data sources

Data	*Source*
Local election data	Carinthian State Government (Statistical Office, series "Gemeinderatswahlen und Buergermeisterwahlen 2015 in Kaernten" and previous volumes)
National election data	Austrian Ministry of the Interior
Football data	Self-compiled from the website http://www.kfv-fussball.at
Expenditures, taxes, debt	Statistical Office of Austria (StatCube database)
Population and migration	Statistical Office of Austria (StatCube database)
Unemployment	Unemployment Agency of Austria (AMS)
Data on transparency	Self-compiled from the website http://www.offenerhaushalt.at
Geodata	Carinthian State Government (http://www.geoland.at)

Notes: The table reports our data sources.

3 Does highway access influence local employment? Evidence from German municipalities[1]

Abstract

We examine how highway access influences local employment outcomes. We exploit the stage-wise expansion of the "Baltic Sea highway", the largest contiguous highway construction project in Germany since 1945. Results from difference-in-differences estimations and an event study approach show that highway access influences local employment outcomes in peripheral municipalities within 10 km road distance; improved accessibility decreases employment by 9%. These effects are driven by reduced commuter flows within the periphery, while we find opposing effects on core municipalities. Improved accessibility also gives rise to a shift of population and economic activity from the periphery to the core, weakening the periphery as a place of work.

[1] This chapter is joint work with Stefanie Gaebler. A previous version of this paper is published as "Does Highway Access Influence Local Tax Factors? Evidence from German Municipalities", *ifo Working Paper 321*, 2020.
We thank Nathaniel Baum-Snow, Richard Franke, Ian Herzog, Guiseppe di Liddo, Simon Melch, Florian Neumeier, Niklas Potrafke, Marta Santamaria, Dirk Schindler, Kaspar Wüthrich, and the participants of the ifo-LMU Public Economics Workshop (2018), the Annual Meeting of the American Public Choice Society in Charleston (2018), the IIPF in Tampere (2018), the Annual Meeting of the European Economic Association in Cologne (2018), the Annual Meeting of the German Economic Association in Freiburg (2018), the European Public Choice Society in Jerusalem (2019), the Annual Meeting of the Canadian Economic Association in Banff (2019), the European Meeting of the Urban Economics Association in Amsterdam (2019), the 9^{th} ifo Dresden Workshop on Regional Economics (2019), and the Urban Economics Association Summer School in London (2019) for helpful comments and Kristin Fischer for valuable research assistance.

3.1 Introduction

New infrastructure projects are often viewed as catalysts for regional economic growth. However, there is an ambiguity of new transportation infrastructure known as the "two way roads problem" (Cheshire *et al.*, 2014). On the one hand, infrastructure acts as an agglomeration force because it improves a region's access *to* other regions. This taps additional market potential as (new) markets become accessible at reduced cost (Donaldson, 2018). On the other hand, investment in infrastructure triggers deagglomeration forces. Reachability of a region *from* other regions is broadened, increasing competitive pressure. Against this background, individuals might react to changing local economic conditions and adapt their employment choices.

The theoretical prediction that market access plays a major role in explaining the spatial distribution of economic activity has been widely established by the new economic geography literature (see e.g., Krugman, 1991; Hanson, 1996; Davis and Weinstein, 2002). While empirical contributions have shown that extensions of the transportation network are generally beneficial for the average region, recent work – especially in the context of developing countries – identified differential effects within the "regional hierarchy" (Baum-Snow *et al.*, 2020). We contribute to this literature by investigating how infrastructure influences the regional distribution of economic activity along the lines of a core-periphery model. More specifically, we focus on changes in employment locations and commuting patterns, two outcomes that have received little attention in the study of transport-induced effects so far (Baum-Snow, 2010; Heuermann and Schmieder, 2019). To examine the relationship between these variables and market access empirically, we exploit a particularly fast and extensive expansion of the East German highway network in the aftermath of reunification. As a proxy for municipalities' market access, we use road-distance measures to the next highway access point.

Many studies examine how infrastructure development affects economic outcomes. Large infrastructure investments in developing countries, such as China or India, offer a widely-used testing ground for these questions. Evidence on the general positive effect of transportation infrastructure on regional[2] economic development (Hornung, 2015; Donaldson and Hornbeck, 2016; Ahlfeldt and Feddersen, 2018; Banerjee *et al.*, 2020), however, has been complemented by findings that confirm substantial heterogeneity at the local level (Chandra and Thompson, 2000; Faber, 2014; Berger and Enflo, 2017). In China, better regional highways increase production and population in "regional primates" at the expense of peripheral areas (Baum-Snow *et al.*, 2020). Highways also have distributional consequences. For Switzerland, Fretz *et al.* (2017) show that in non-urban municipalities, the advent of a highway access point within 10 km increases the share of top-income taxpayers.

[2] At the firm level, there exists evidence that new transportation infrastructure influences production optimization (Datta, 2012) as well as, ultimately, productivity (Holl, 2016; Wan and Zhang, 2018; Gibbons *et al.*, 2019).

Our empirical study relates to the new economic geography literature predicting the emergence of a core-periphery pattern and the literature evaluating the effects of new transportation infrastructure. We investigate how infrastructure development influences employment outcomes, in particular commuting patterns. In a new economic geography framework, infrastructure development, i.e. a reduction in trade costs, gives rise to a shift of population and thus economic activity from the periphery to the core. In the context of these models, high transport costs protect producers in the periphery from trade competition. When transport costs fall, however, residents in the periphery substitute locally produced goods with imported ones from the core. This mechanism leads to an economic strengthening of the core at the expense of the periphery. When examining possible channels that drive the employment effect, we investigate variables that have been shown to react to new transportation infrastructure, such as population (Baum-Snow, 2007; Garcia-López *et al.*, 2015) and house prices (Mikelbank, 2004).

Our sample covers the period 1995-2015 in the German state of Mecklenburg-Western Pomerania (*Mecklenburg-Vorpommern*, MV). We consider the opening of the highway number 20 (BAB 20 or "Baltic Sea highway") in MV, which constitutes an ideal setting for two reasons. First, the opening of the BAB 20 in MV took place in several stages, providing us with variation in the timing of infrastructure access. Second, as the largest contiguous highway construction project since 1945 in Germany, the BAB 20 had a considerable impact on municipalities' accessibility. During our sample period, a municipality's average distance to the next highway access was more than halved. The location of new highways is likely endogenous to rural regional fundamentals because highways are built to connect economic units. To reduce concerns of endogeneity, we follow the inconsequential units approach and exclude large and economically strong cities that shape the route of the highway (Chandra and Thompson, 2000; Faber, 2014; Möller and Zierer, 2018; Banerjee *et al.*, 2020). Non-agglomeration regions often receive access to a new highway because they are located on a convenient route between two larger cities that are connected. The exact opening year for these municipalities is close to random. Using difference-in-differences and event study estimations, we find that highway access decreased inbound commuting. This effect is driven by peripheral or very peripheral municipalities that, due to the spatial structure of MV, make up the majority of our sample. We observe a shift of commuting flows very much in line with predictions from the core-periphery model: the volume of commuting between peripheral places of residence and work decreases, while central locations benefit from an increase in commuters and employment. When examining possible channels, we provide supportive evidence for these counteracting effects between central and peripheral municipalities. We propose that central localities benefit from highway accessibility in terms of population and employment, but do so at the expense of the periphery.

3.2 Theoretical considerations and hypothesis

The new economic geography literature shows that improved accessibility has spatially ambiguous effects on employment (Krugman, 1991; Fujita *et al.*, 2001). New infrastructure acts both as an agglomeration force or a deagglomeration force (the "two way roads problem", Cheshire *et al.* 2014).

Improved access *to* other regions taps new market potential as transportation costs to (new) markets decline. This in turn might attract new business activity and new residents (market potential or agglomeration effect). In contrast, being more easily and cheaply accessible *from* other regions might increase competitive pressure. High transportation costs are equivalent to tariffs and protect local producers. As transportation costs decline, inter-regional competition increases. When it becomes more profitable for consumers to import products rather than purchase from local producers, the connected region risks losing economic activity (competition or deagglomeration effect). Whether the market potential or the competition effect prevails depends on the location of a region. Following the core-periphery model by Krugman (1991), peripheral producers are protected by high transportation costs. As transport costs decline, the periphery is delivered from the core at a reduced rate, while core producers exploit agglomeration benefits. Baum-Snow *et al.* (2020), for instance, show that the construction of the Chinese national highway system increased population and economic output – not overall, but mainly in core regions – at the expense of peripheral regions.

When regions get connected to the highway network, we expect that the competition or deagglomeration effect outweighs the market potential or agglomeration effect in peripheral regions. Our hypothesis to be tested empirically is: highway access decreases (increases) local employment measures in peripheral (core) regions.

3.3 Institutional background

3.3.1 The federal system of Germany

The federal system of Germany distinguishes between the federal and state level as two layers of government. Local governments with counties (*Landkreise*) and municipalities (*Gemeinden*) are part of the state level. The German Constitution guarantees municipalities the right of self-government (Art.28 Basic Law). Responsibilities regarding their expenditures involve transferred compulsory tasks that are assigned by the federal government (*übertragene Selbstverwaltungsaufgaben*), compulsory responsibilities (*pflichtige Selbstverwaltungsaufgaben)*, and voluntary self-government responsibilities (*freiwillige Selbstverwaltungsaufgaben*). For voluntary tasks, municipalities possess full autonomy of decision. They decide on whether they will engage in these tasks and determine how much they want to invest or what quality they want to provide. The voluntary responsibilities of municipalities comprise economic, cultural, and social issues, like public transport, industry settlements, libraries, theater, sport

facilities, and elderly care. Compulsory tasks, like energy and water supply or land-use planning, must be executed by the municipalities, but they decide on how to do so. This is different for transferred compulsory responsibilities (for instance public administration and building supervision), where municipalities have no discretionary power at all. Municipalities also have revenue autonomy by setting user charges and taxes. Within the scope of their self-government responsibilities, they determine tax factors for business tax (*Gewerbesteuer*), general property tax (*Grundsteuer B*), and agricultural property tax (*Grundsteuer A*) independently.

3.3.2 Highway expansion in Mecklenburg-Western Pomerania

After reunification, as part of the German Unity Transport Project (*Verkehrsprojekt Deutsche Einheit*), the highway number 20 (*Bundesautobahn 20* – BAB 20) was built through Lower Saxony, Schleswig-Holstein, Mecklenburg-Western Pomerania, and Brandenburg to better connect regions in MV to the West German and European transportation network. The BAB 20 is the longest contiguous highway construction project in Germany since 1945.

Plans for long-distance roads passing through MV have existed since the 1930s. In construction plans from 1926, two roads connect Lübeck with Stettin[3], one in the interior of the country running through Neubrandenburg and one following the coastline passing by Stralsund. In 1934, the west-east connections disappeared in the construction plans and were replaced by north-south routes, connecting Hamburg, Stettin, Rostock, and Stralsund directly with Berlin. This route was given up in 1935 in favor of a new route in eastern direction. After an extensive examination of traffic conditions and requirements in Mecklenburg and Western Pomerania, the precursor of the BAB 20 was incorporated into the network of the *Reichsautobahnen* in 1937. Even though construction started in 1938, it was stopped in 1939 because of WWII. The construction of the BAB 20 was still planned by the German Democratic Republic, but the regional road development in north-eastern Germany was – due to the division of Germany – aligned for decades in a north-south direction (BMVBW et al. 2007).

After reunification, the construction of the BAB 20 started in 1992; in 1997 its first 26 km were opened for the public in MV. Another 311 km, spread on 18 subsections (16 in MV), opened between 2000 and 2009. The total length of the BAB 20 amounts to 345 km;[4] 280 km are located in MV. Starting in Lübeck in Schleswig-Holstein, the BAB 20 runs in an eastern direction through the cities of Wismar, Rostock, and Greifswald. In Greifswald, the BAB 20 turns south to connect the city of Neubrandenburg, where it turns south-east to the highway intersection of Uckermark in the state of Brandenburg (see figure A3.1). There, the BAB 20 merges with the BAB 11, which leads to Berlin. Parts of the BAB 14 were also open for the public and connect Wismar and Schwerin with Saxony-Anhalt and Saxony in the south of MV.[5] Other highways that run through MV and have already been open in 1992 are the BAB 24, connecting

[3] Szczecin, Poland.
[4] 196 km in Lower Saxony are still in planning.
[5] In 2006, the BAB 241 was renamed and became part of the BAB 14.

Hamburg and Berlin, the BAB 19, connecting Rostock and Berlin, and a small segment of the BAB 11, connecting the Polish border with Berlin. Figure 3.1a shows the highway network in MV in the years 1995 (gray) and 2015 (black). Further, it shows the variation in distances that we are going to exploit in the empirical analysis. We group municipalities in three 5 km distance bands between 0 and 15 km. Darker shaded areas mark those municipalities within the closest distance bands. Distance is measured as the road distance (in km) of each municipality's centroid in MV to the nearest highway access in each year. In 1995, the average road distance between a municipality centroid in MV and the nearest highway access was 47 km. With the expansion of the highway network, the road distance was more than halved; in 2015 the nearest highway access was on average within a distance of 22 km. Especially the north-east of MV with the regional centers Stralsund, Greifswald, and Neubrandenburg gained access to the highway network via the BAB 20. Figure 3.1b shows the spatial structure of MV which is largely classified as peripheral.

3.4 Empirical analysis

3.4.1 Data and sample

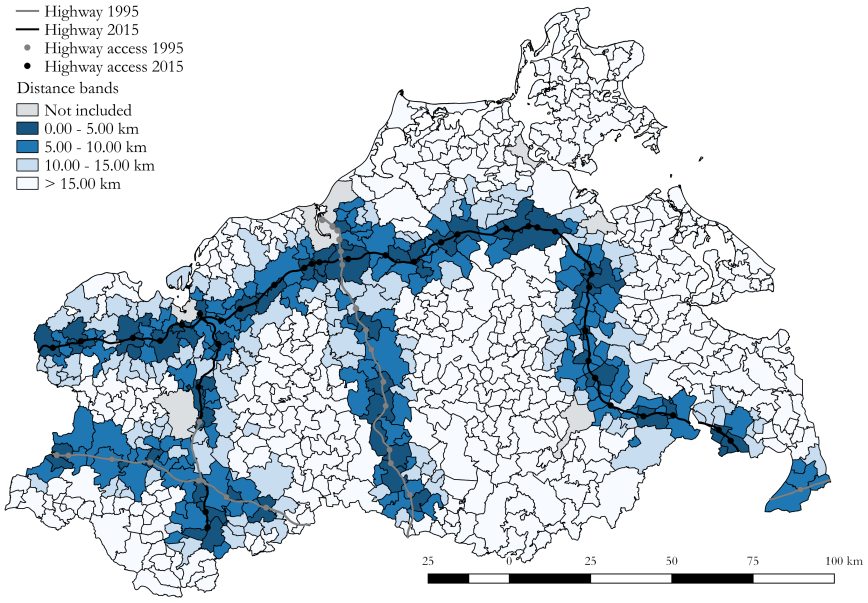
We use a panel of yearly employment and geographical data at the municipal level in Mecklenburg Western Pomerania. As dependent variables, we use labor market outcomes. To see how highways affect the spatial distribution of economic activity, we distinguish between whether a municipality is registered as an employee's place of work or residence. We also investigate aggregate and disaggregate commuting flows (inbound and outbound commuters) between municipalities and the number of firms as supply-side element of local labor markets. To calculate the road distance to the closest highway access, we use geographic data from the Federal Office for Cartography and Geodesy and Geofabrik. Using GIS software, OpenStreetMap Data, and the Open Source Routing Machine (OSRM), we compute the distance measure as the road distance in km of a municipality's centroid to the nearest highway access in each year. For the main analysis, we transform the distance measure to a dummy variable indicating whether the next highway access lies within 10 km road distance (*access*=1) or not (*access*=0).

We focus on the time period between 1998 (1995 for some analyses) and 2015.[6] After 1995, the length of the road network of national primary, state, and county roads in MV stayed constant and the only change in the road network was due to the construction of the highway (see table A3.1 in the appendix). The largest waves of connections to the highway occurred in 1998, 2001, and 2003, where 15, 22, and 32 municipalities gained access to the BAB 20 (see table A3.2 in the appendix).

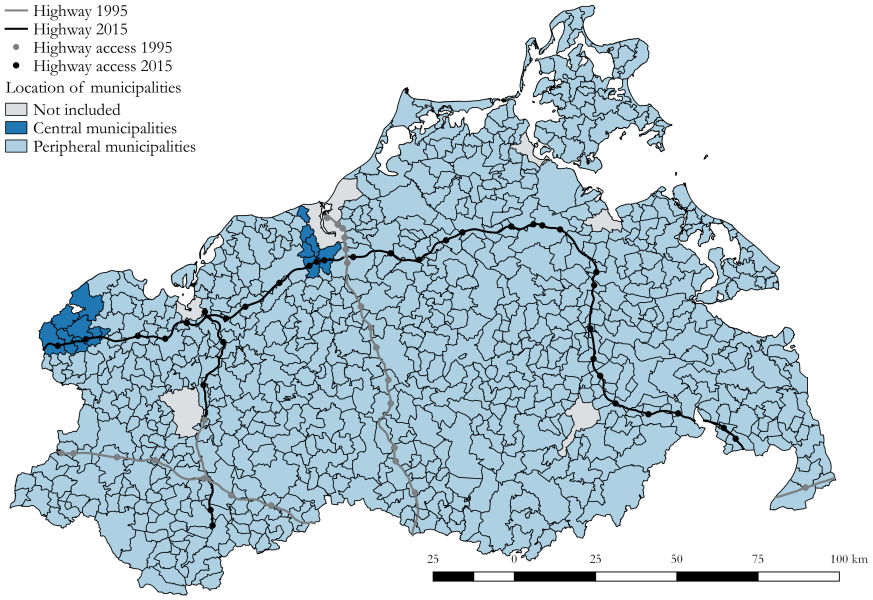
[6] The main dependent variables used in the analysis are only available from 1998 onward, whereas data on socio-demographic or political variables at the local level start in 1995.

Figure 3.1: Highway network in Mecklenburg-Western Pomerania

(a) Distance bands



(b) Peripheral and central municipalities



Notes: The maps show municipalities in the German state of Mecklenburg-Western Pomerania (black borders). The highway network (access points) of 1995 is depicted in dark gray lines (points); black lines (points) represent the highway network (access points) of 2015. Figure 3.1a groups municipalities in different distance bands to the next highway access points in 2015, figure 3.1b groups municipalities according to their location in central and peripheral. Gray municipalities are independent cities and not included.

There have been several local boundary reforms during our sample period. We adjust the data to the territorial status of 2015.[7] We exclude the cities Schwerin, Rostock, Wismar, Stralsund, Greifswald, and Neubrandenburg for two reasons. First, they have been district-free cities until 2011; after 2011, only Schwerin and Rostock remained district-free cities. District-free cities exercise functions of counties and municipalities at once and are therefore not comparable to municipalities. Second, the highways in MV are mainly built to better connect these cities and we exclude them to reduce endogeneity concerns. Our final panel dataset includes 745 municipalities over the period 1998-2015.

We control for lagged demographic and electoral variables. Demographic variables include population size (in log), population by age groups (age under 15, age between 15 and 25, age between 25 and 40, and age between 40 and 65), and population density. We include four age variables to map the age structure of the population to proxy for different levels of human capital. As electoral controls, we use the share of left-wing votes[8] in the last municipal election, as well as those of elections for the county assembly (*Kreistag*), and state assembly (*Landtag*). We include the individual party vote shares to control for potentially different local economic policies of left-wing governments. Mecklenburg-Western Pomerania shares an eastern border with Poland. To control for the dynamic economic growth accompanying Poland's transition to a market-based economy during the 1990s, which might have had an impact on economic development in Mecklenburg-Western Pomerania, we include in a robustness test GDP in Poland multiplied with the inverse linear distance of each municipality to the Polish border. At the same time, this variable proxies for the number of cross-border commuters.

Table 3.1 shows the descriptive statistics. We use data in levels which are later transformed to logarithms in the empirical analysis. Municipalities as local labor markets in MV are very small-scaled. Around 457 employees live and 551 employees work in an average municipality. However, these mean values are driven by few large regional centers, since the respective median values are 254 and 91. The number of inbound commuters was on average 296, ranging between 0 and 7,625. The average number of outbound commuters (412) was slightly higher and varied between 3 and 4,426. In the raw data, we observe some anonymized values for our dependent variables, see table A3.4 in the appendix.[9] Due to increased data protection regulations, the share of anonymized values – especially for employees at the place of work and inbound commuters – increased sharply from 2010 onward.[10] Although it is conceivable that some of these anonymized values truly reflect low numerical values, we code them all as

[7] Our baseline results are unchanged when we exclude all merged municipalities from our sample; see table A3.3 in the appendix.

[8] Left-wing votes combine votes for the social democratic party *SPD*, the green party *Grüne*, and the left party *Die Linke/PDS*.

[9] For reasons of data protection and statistical confidentiality, numerical values of 1 or 2 and data from which such a numerical value can be mathematically inferred are anonymized. The same applies if a region or an industry has 1 or 2 establishments or if one of the establishments has such a high share of employees that the number of employees is practically a single figure for this establishment (dominance case).

[10] We make sure that our baseline results remain unchanged when we focus on the period before 2010 only (table A3.5 in the appendix).

missing for our baseline analysis. The estimated effects can thus be understood as a lower bound. 21% of all observations lie within a road distance of 10 km to the next highway access point.

Table 3.1: Descriptive statistics

	Observations	Mean	SD	Min	Max
Dependent variables					
Employees (place of residence)	12,273	457.11	1,128.08	0	14842
Employees (place of work)	13,410	551.45	915.41	23	11591
Inbound commuters	12,092	296.09	658.33	0	7625
Outbound commuters	12,786	412.27	495.30	3	4426
Firms	12,664	44.84	93.88	0	1124
Access dummy (yes = 1)					
Access (<10 km)	13,410	0.21	0.41	0	1
Control variables					
Population	13,410	1,562.70	2,697.51	102	33014
Age: < 15, share	13,410	12.93	2.80	3.33	31.3
Age: between 15 and < 25, share	13,410	11.25	3.60	0.98	40.2
Age: between 25 and < 40, share	13,410	18.14	3.63	5.15	41.9
Age: between 40 and < 65, share	13,410	39.63	5.47	12.4	62.1
Population density	13,410	52.05	63.98	4.91	630.6
Election county assembly, share left	13,410	45.45	6.90	32.5	61.8
Election state assembly, share left	13,410	57.93	4.93	50.4	62.7
Election mayor, share left	13,410	16.27	18.93	0	100
Further control variables					
GDP Poland × distance	13,410	7,605.82	27,089.70	1032.4	683812

Notes: The 745 municipalities of the German state of Mecklenburg-Western Pomerania are our observation units. We use yearly data between 1998 and 2015. The dummy variable *Access* equals one when a municipality is within a 10 km road distance to the next highway access point, and zero otherwise. *GDP Poland* × *distance* is GDP in Poland multiplied by the inverse linear distance of each municipality to the Polish border.

Municipalities in Mecklenburg-Western Pomerania are relatively small in terms of their area with an average size of 30 km^2. Compared to studies that use counties as observational units, we pursue a more detailed geographical analysis. The geographical level plays an important role in case of relocation effects. An analysis at the aggregate (county) level is unable to uncover possibly large between-municipality movements of residents or firms.

3.4.2 Identification and regression specifications

To estimate how highway accessibility influences employment outcomes, we exploit variation across space and variation in time, since the highway was opened in different segments throughout our period of study (see figure A3.2 in the appendix). We estimate the following

difference-in-differences model:

$$y_{it} = \delta_i + \theta_t + \beta Access_{it} + X'_{it}\lambda + \epsilon_{it}, \tag{3.1}$$

with y_{it} as our dependent variables, employees, commuters or firms of municipality i in year t. The dummy $Access_{it}$ denotes a measure of transportation infrastructure external to municipality i. It takes the value of one when a municipality is within a road distance of 10 km to the next highway access point, and zero otherwise.[11] X'_{it} is a vector of location and time-specific covariates (see section 3.4.1). δ_i denotes location-specific time-invariant factors (like distance to large cities, airports, harbors), θ_t denotes common time effects for all locations and ϵ_{it} is the time-varying location-specific error. Our coefficient of interest is β, which measures the effect of access to the highway network on municipalities' labor market outcomes.

Identification relies on the main assumption that municipalities with a highway access would have evolved similarly to municipalities without a highway access in the hypothetical case without a new highway. To estimate a causal effect, two conditions have to be met. First, treatment and control municipalities should follow a common trend before the opening of the highway. To show that this condition is fulfilled, we extend equation (3.1) and estimate an event study of the following form:

$$y_{it} = \delta_i + \theta_t + \sum_{j=c}^{C} \beta_j Access^j_{it} + X'_{it}\lambda + \epsilon_{it} \tag{3.2}$$

Compared to equation (3.1), we replace the dummy $Access_{it}$ by a vector of dummies measuring the years before and after a municipality gained access to the highway. $\sum_{j=c}^{C} \beta_j$ describes our coefficients of interest. $Access^j_{it}$ takes on the value of one when a municipality i is within a road distance of 10 km to the next highway access point in $(t + j)$ years and zero otherwise. We include five dummies measuring the years before a municipality gains access (-5 *and less* to -1) and five dummies measuring the years after a municipality gains access (1 to 5 *and more*). The year before the highway opened serves as our base category. Therefore, j ranges from $c = -5$ *and less* to $C = +5$ *and more*, excluding -1 (base category). Event studies enable us to test the common trend assumption equation (3.1) rests on and also give a more detailed picture of the highway effects over time.

The second assumption for a causal interpretation of our results is an exogenous source of variation. The location of highways is likely endogenous to regional patterns because they are built to connect economic units. Location-specific factors, like productivity or amenity, which are generally unobserved, may influence both the location of infrastructure and employment outcomes (Redding and Turner, 2015). To reduce concerns of endogeneity, we follow the inconsequential units approach and focus on non-agglomeration areas (Chandra and Thompson, 2000; Faber, 2014; Möller and Zierer, 2018; Banerjee *et al.*, 2020). Non-agglomeration

[11] Additionally, we estimate specifications for $Access_{it}$ with road distances of 5 km, 15 km, 0-5 km, 5-10 km, and 10-15 km, see table 3.5.

regions often receive access to a new highway because they lie on a convenient route between two larger cities that are connected. Moreover, for these rather rural municipalities, the exact opening year can be regarded as close to random and exogenous to their development (Fretz *et al.*, 2017). While Chandra and Thompson (2000) and Möller and Zierer (2018) focus only on peripheral regions and assume exogeneity[12], Banerjee *et al.* (2020) draw straight lines to connect nearest neighbor pairs of historical cities and ports. Faber (2014) uses an IV approach and constructs a hypothetical least cost path spanning tree network. Figure 3.1 shows that the highways in MV connect the large centers Rostock, Wismar, Schwerin, Greifswald, and Neubrandenburg with Berlin, Hamburg, Lübeck, and Magdeburg in nearly straight lines. We follow Chandra and Thompson (2000) and Möller and Zierer (2018) and concentrate only on non-agglomeration municipalities, while excluding the larger cities connected by the highway.

The main planning and investments in high level transportation infrastructure in Germany are made at the federal level, not the local level. The planning of the course of the BAB 20 followed environmental, economic, spatial, and traffic concerns. First, a southern course was excluded and a broader environmentally sustainable corridor in the north was defined to connect the coastline. Second, an environmental impact study was conducted, covering 6,300 km^2 or a quarter of the area in MV. Several variants were worked out and compared before the course of the highway was determined (BMVBW et al. 2007). With the special environmental territory in MV (MV has more national parks than any other German state), the course of the highway, and hence which municipalities got connected, was not predominantly determined by economic reasons. To further strengthen the inconsequential units approach, we estimate equation (3.1) with two sub-samples where we first exclude municipalities whose location is classified as "central" and second exclude municipalities whose structure is classified as "predominantly urban". Moreover, to examine a potential difference between central and peripheral municipalities we conduct a heterogeneity analysis considering the location of each municipality in our baseline regression.

One may argue that municipalities that lie between two larger cities are not comparable to municipalities that are located in the hinterland. They could, even without the construction of a new transportation network, follow a different growth path, because municipalities located between two larger cities may be more accessible in the first place. Table 3.2 shows the mean of population (log), share of population between age 15 and 65, and population density for different clusters of municipalities, depending on their road distance to the next access in 2015 and their location before the first highway segment opened.[13] The upper part of table 3.2 shows that municipalities located less than 10 km and more than 10 km from the next highway access in 2015 had a similar demographic structure in 1995 and 1996. This indicates that municipalities are comparable, regardless of whether or not they are located close to the

[12] Möller and Zierer (2018) use the inconsequential units approach as a robustness test. Their main specification relies on historical instrumental variables. With both strategies they find "remarkably similar results" for Germany (p.19).

[13] Economic proxies, like unemployment, number of firms, or number of employed workers and commuters, are not available for 1995 and 1996.

future highway and therefore between two larger cities. Furthermore, the lower part of table 3.2 shows that central and peripheral municipalities also did not differ in these demographic outcomes.

Table 3.2: Sorting into treatment – T-tests

Panel A: Distance to access	Mean road distance to next access in 2015 $<$ 10km	Mean road distance to next access in 2015 $>$ 10km	Diff	SD	Obs
Population (log)	6.79	6.70	0.10	0.08	745
Population age 15-65 (log)	6.40	6.31	0.09	0.08	745
Population density	53.43	47.88	5.55	5.33	745
Panel B: Location of municipalities	Mean peripheral municipalities	Mean central municipalities	Diff	SD	Obs
Population (log)	6.77	6.81	-0.05	0.24	745
Population age 15-65 (log)	6.38	6.46	-0.08	0.24	745
Population density	51.64	68.50	-16.86	15.52	745

Notes: The 745 municipalities of the German state of Mecklenburg-Western Pomerania are our observation units. We use yearly data before the first highway segment was opened – 1995 and 1996.

Table 3.3 shows that demographic, economic, and political outcomes are not correlated with receiving highway access. We estimate survival models with getting a highway access within 10 km road distance as the failure event using Cox regressions. Demographic factors, employment variables, the number of firms, commuter patterns, fiscal variables, and election outcomes do not turn out to significantly alter the hazard rate.[14] We conclude that pre-reform characteristics do not predict sorting into treatment.

Figure 3.2 shows that our panel is well balanced. Between 1998 and 2007 the share of municipalities within a road distance of 10 km to the next highway access increased steadily and somewhat proportionally over time. Temporal clustering, therefore, should not be a problem.

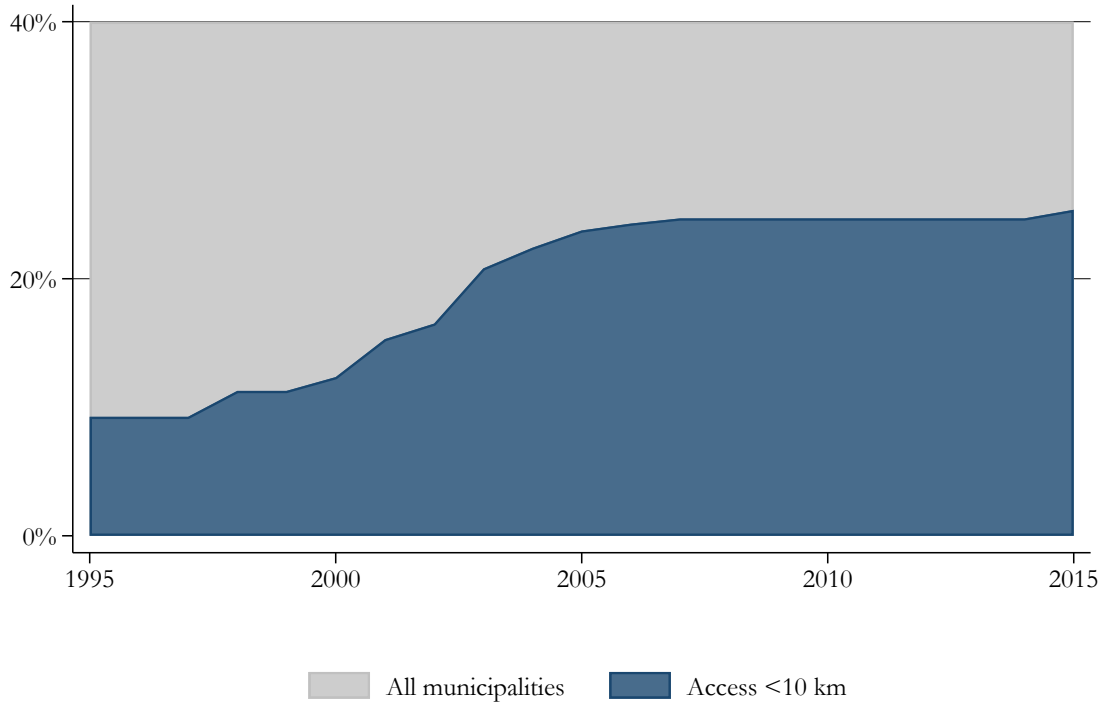
3.5 Results

3.5.1 Difference-in-differences

Panel A of table 3.4 shows regression results for the five labor market outcomes considered. All specifications include municipality fixed effects to account for variation in average employment, commuter or firm levels between municipalities and year fixed effects to address temporary shocks that are common to all municipalities. Differences in local labor market outcomes could be a result of different local preferences of employees or firms. Our estimation strategy accounts for these preference-related differences between municipalities, but changing preferences over time within municipalities could be a confounding factor. We control for

[14] The only exception are tourism destinations which are negatively correlated with highway access. We conduct a robustness check excluding these localities to make sure that they are not driving our results.

Figure 3.2: Sample balancedness



Notes: The figure shows the cumulative share of municipalities within a road distance of 10 km to the next highway access point between 1995 and 2015.

population, population age categories, population density, and the share of left-wing votes at local and state elections. Since these variables could at the same time be influenced by highway accessibility, we include them as lags.

On average, as the results in panel A show, we do not find statistically significant differences in employment outcomes between accessible and less accessible municipalities. Although the estimated coefficients for employees at their place of work, inbound commuters, and firms turn out negative and are also numerically larger than estimates for employees at their place of residence and outbound commuters in columns (1) and (4), they are too imprecisely estimated to reach conventional levels of statistical significance.

Strengthening the argument of the inconsequential units approach, we in turn exclude central (panel B of table 3.4) and predominantly urban (panel C of table 3.4) municipalities. Excluding central municipalities constitutes thereby our baseline specification, where we test our hypothesis that in peripheral regions the deagglomeration effect prevails. Central and peripheral municipalities are classified based on the accessibility of concentrations of population and employment, while the structural categories urban and rural are determined by population density and settlement area measures.[15] Due to the spatial structure of MV, only few municipalities, namely those surrounding the cities Rostock and Wismar, are defined as central. Against this background, the increase in the coefficient for the 10 km dummy in columns (2) and (3) (panel B) of table 3.4 when excluding these municipalities is sizable. Municipalities whose road distance to the next highway access does not exceed 10 km, experience a 9% decrease in their employment level (column 2), which seems to be entirely driven by a reduced inbound commuter flow (column 3). The remaining coefficients do not turn out statistically significant. When excluding predominantly urban municipalities in panel C, estimates closely resemble those in panel A. This first set of results already hints at some heterogeneity of the highway effect within our sample. Overall, we estimate zero effects of highway access on local labor market outcomes. For the periphery, i.e. localities without immediate access to populous and economically active urban centers, however, improved infrastructure seems to impact employment and inbound commuting detrimentally. Structural factors, i.e. whether the municipality itself is of (predominantly) urban or rural type, however, do not play a crucial role.

In an attempt to corroborate the baseline findings of table 3.4 (panel B) for peripheral municipalities, we conduct several robustness checks, which are presented in figure 3.3. The first bullet in each graph represents the baseline estimate for comparability. First, we include municipality-specific linear time trends to rule out the possibility that accessible municipalities – defined as falling under the 10 km distance band – and less accessible localities were already on differential growth paths in their outcome variables. In this case, we would find an effect on local labor market outcomes even in the absence of the construction of the BAB

[15] The spatial categories are defined by the Federal Institute for Building, Urban Affairs, and Spatial Research (*BBSR*) at one point in time, so they remain constant over our sample period and are not affected by highway accessibility.

20. However, these concerns are not supported by the results as the coefficient estimates in figures 3.3b and 3.3c demonstrate. For employees (place of work) and inbound commuters, the negative differential between accessible and less accessible municipalities persists. Coefficient estimates are quite comparable to our baseline estimates, so the treatment effect does not seem to be absorbed by the time trends.

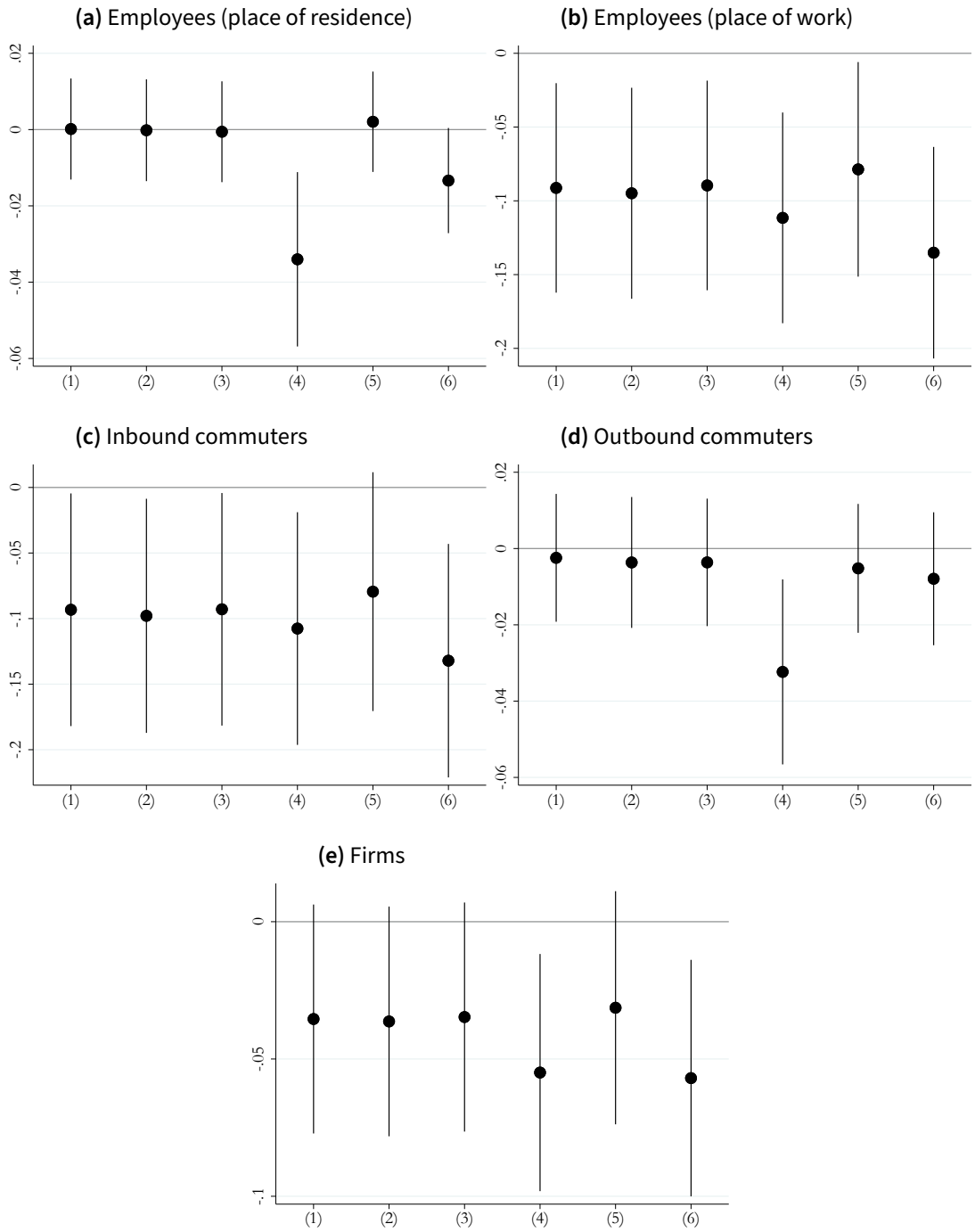
Second, we extend the set of control variables and control for the market access to Poland. As the third bullets in the respective graphs show, results remain unchanged. Third, we exclude all control variables (figure 3.3, fourth bullet). The results fairly reproduce our baseline findings for employees at their place of work and inbound commuters. Estimates for employees at their place of residence and outbound commuters are sensitive to this exclusion, however, suggesting that part of the effect of accessibility on these variables is mediated by the population channels.

Fourth, with its many national parks and its location at the Baltic Sea, MV is a popular tourism destination. As touristic municipalities might benefit particularly from better accessibility, we exclude them in a further robustness test. Our baseline results remain unchanged. Fifth, we follow Bertrand *et al.* (2004) and estimate a pooled OLS. Ignoring the time dimension accounts for a possible inconsistency of the standard errors. Again, figure 3.3 (sixth bullet) shows that our baseline results hold. Coefficient estimates even increase in magnitude.

Moreover, table 3.5 repeats our baseline analysis for peripheral municipalities when using alternative specifications of the distance variable to ease concerns about arbitrary cut-off-values. In panels A and B, we specify the treatment dummy to equal one for municipalities within a road distance of 5 km and 15 km. In panel C, we differentiate the effect for those distance bands by simultaneously including all 5 km sub-categories (as dummies). The results yield some interesting insights. Namely, our baseline finding of a negative employment and commuter differential is driven by peripheral municipalities within a distance band of 5 to 10 km from the next highway access (columns 2 and 3, panel C). In this distance band, also the number of firms decreases. Peripheral municipalities located in even closer proximity to the highway – less than 5 km – also have negative, albeit statistically insignificant, coefficient estimates for employees at their place of work and inbound commuters (columns 2 and 3, panel C). Coefficients for the maximum distance band (10-15 km) are statistically significant and negative, but their magnitude decreases by around 40% compared to municipalities within the medium distance band. Therefore, the detrimental effect of accessibility on employees working in and commuting to peripheral municipalities decays with distance to the highway access.

Is it really improved accessibility that matters for employment in peripheral locations? To answer this question, we conduct a final robustness check that differentiates between treatment intensity effects. In table 3.6, we re-estimate our baseline specification for peripheral municipalities including an interaction term between the access dummy and a dummy variable indicating high or low initial distance from the next highway access in 1995. The cut-off value between high and low initial distances is defined at the median. Results confirm

Figure 3.3: Robustness



Notes: We compare five different robustness checks to our baseline specification for peripheral municipalities (table 3.4, panel B). Each bullet represents the coefficient for the access dummy; bars represent the 90% confidence interval. The first bullet represents the baseline estimate for comparability. The dependent variables are labor market outcomes (in logs) at the level of 728 peripheral municipalities in Mecklenburg-Western Pomerania. In the first robustness check (second bullet), we include municipality-specific linear time trends. Second, we control for market access to Poland. Third, we exclude all control variables. Fourth, we exclude all municipalities classified as tourism destinations. Tourism destinations are municipalities listed as resorts, health resorts, spas, coastal resorts, and coastal health resorts by the Statistical Office of Mecklenburg-Western Pomerania in 2015. Fifth, we estimate a pooled OLS.

that the degree of infrastructure improvement matters. We observe statistically significant negative effects for employees, inbound commuters, and firms in peripheral municipalities that benefited the most from increased accessibility. In comparison, employment effects in peripheral municipalities that have always been moderately close to the next highway access do not turn out to be statistically significant.

3.5.2 Event Studies

Our baseline effects in table 3.4 measure averages over all municipality-year observations characterized by improved access to the highway following equation (3.1). One could, however, argue that the effect on local labor market outcomes is just a result of increased local labor demand during the construction phase of the BAB 20 that recedes back to normal levels when the highway segment and access points are completed and opened for the public. To discern whether these temporary patterns exist, we normalize the year of improved accessibility for all peripheral municipalities and build a categorical variable taking on different values for a time window of 5 years around the opening of the highway segment (equation 3.2). Figure 3.4 displays the coefficient estimates and their 90% confidence intervals graphically. All effects are relative to the year before the opening of the highway segment (indicated by the dashed line).

The observed patterns for the employees at their place of work and inbound commuters are relatively similar, although confidence bands for the estimates are large, which explains why we do not find any significant average effects in our baseline regressions. Nevertheless, for both variables, effect sizes increase with each year of highway access from roughly -0.03 to -0.08 (see figures 3.4b and 3.4c). Four years after the opening of the highway segment – and thus after falling under the 10 km distance band – the estimate for the negative employee differential turns statistically significant, suggesting negative long-term effects for employment in peripheral municipalities with highway access. Reassuringly, the development in both labor market outcomes does not differ significantly between treatment and control group before the highway was opened (90% confidence intervals always include the zero) and therefore the common trend assumption holds. In any event, there seem to be some positive labor demand effects in the years leading up to the opening of the highway, peaking in $t = -3$, which are potentially related to the construction process. For employees at their place of residence and outbound commuters the post-treatment patterns are less clear. If at all, there is some suggestive evidence for a negative effect on firms in peripheral municipalities within compared to outside of the 10 km distance band to the highway access.

To also submit our event study results to a robustness check, we use a more balanced window around highway access by excluding municipalities with less than three pre- or post-treatment years. Results for the remaining municipalities, as shown in figure A3.3,

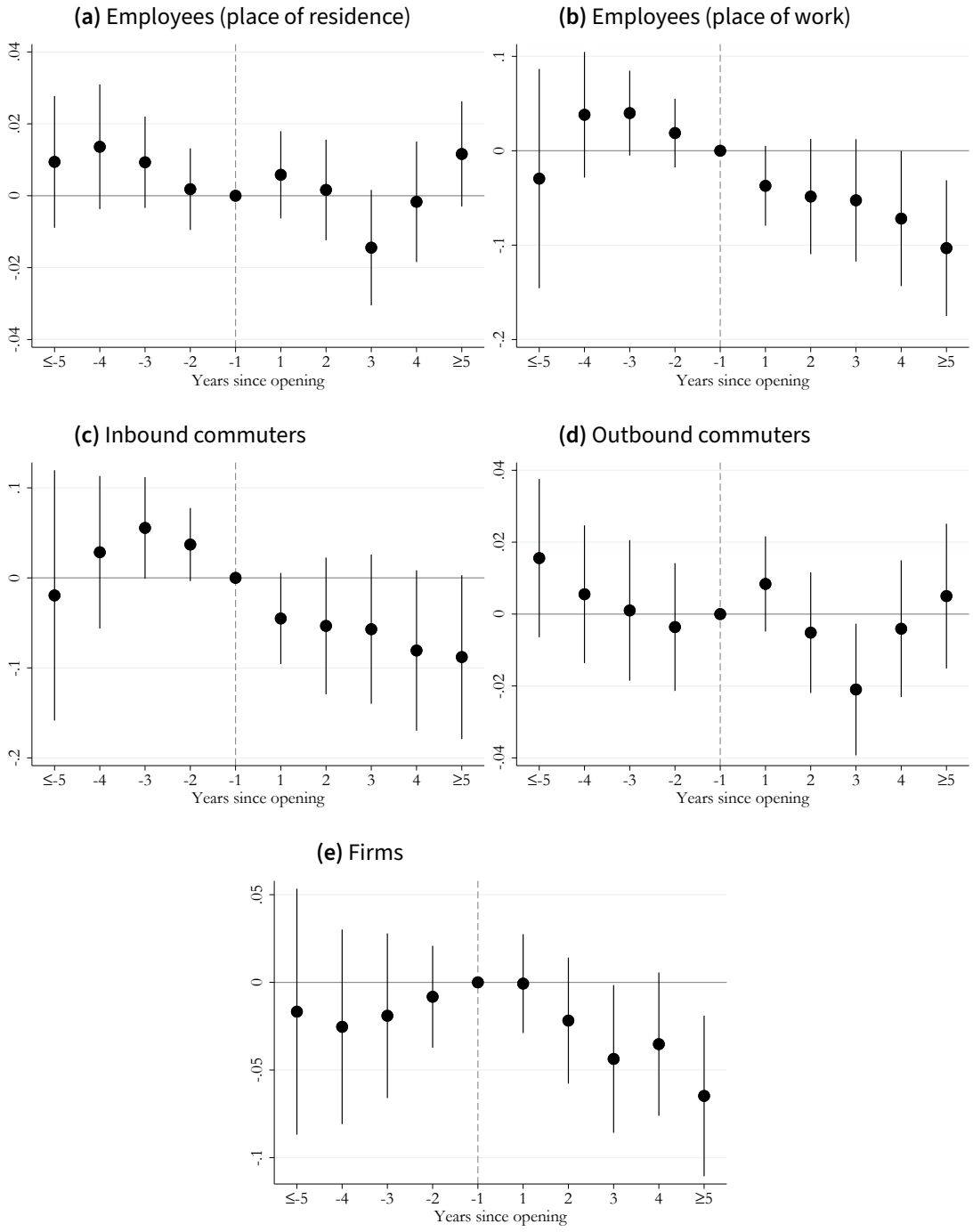
confirm the observed patterns in figure 3.4. From the year of improved highway access onward, peripheral municipalities experience a continuous decline in the number of inbound commuters, which is reflected in a corresponding decrease in local employment levels.

3.5.3 Heterogeneity

The average estimated effects of the baseline specifications might mask substantial heterogeneities in municipal labor market outcomes. Conditional on the spatial characteristics of a municipality, for example, firms might have more or less scope to adjust their employment levels in the first place, and more importantly, might be affected differently by increased accessibility. For this reason, we re-estimate the difference-in-differences specification with all municipalities by adding multiplicative interactions between the treatment indicator and dummy variables for central and peripheral municipalities. Although the small number of central municipalities in our sample warrants caution in interpreting the corresponding interaction coefficient, results in table 3.7 provide suggestive evidence for heterogeneity of the highway effect along the spatial dimension. For all labor market outcomes coefficient estimates for peripheral and central municipalities display opposing signs. Highway access makes central municipalities a more attractive place to live (+6% of employees) and peripheral municipalities a less attractive place to work (-9% of employees), while commuting flows adapt accordingly. Without knowing the origin or destination of inbound and outbound commuters, it remains unclear, however, how commuting volumes shift between the periphery and the core. This question will be examined in section 3.5.4.

We investigate possible mechanisms to explain the negative employment differential for accessible municipalities. Effects could run through population or area-based channels, which we investigate in turn in table 3.8. Panel A shows how these variables are affected by accessibility. Most of the coefficients are very imprecisely estimated, but overall population seems to matter. Receiving a highway access within 10 km is associated with a population decline of 2% in the connected municipality. Similar to table 3.7, panel B repeats the analysis when including an interaction term between the treatment indicator and the central/periphery-dummy. While effect sizes in peripheral locations – relative to central municipalities – are moderate, they nevertheless reveal a striking pattern: for all variables, we observe negative effects when peripheral municipalities fall within 10 km road distance to the next highway access. The respective municipalities lose around 4% of their overall and working-age population, rendering them less densely populated. In line with these downward trends in population, the supply of buildings with residential areas decreases. For central municipalities, we observe exactly the opposite pattern. Effect sizes are large and positive throughout, i.e. central municipalities seem to benefit in terms of population and residential buildings with increased highway accessibility. The small number of municipalities classified as central, however, raises questions about sample size and how reliable the estimate for

Figure 3.4: Event study results



Notes: The figure shows the results of five event study estimations. Vertical dashed lines represent the year before a municipality falls within a road distance of 10 km to the next highway access point. The 728 peripheral municipalities of the German state of Mecklenburg-Western Pomerania are our observation units. We use yearly data in levels (log) during the period 1998 to 2015. Bullets are point estimates, and black lines represent the 90% confidence interval. We include year and municipality fixed effects and control variables (see notes to table 3.4). $Year = -1$ is the base category. We use standard errors robust to heteroskedasticity.

central municipalities actually is. We view results as supporting counteracting effects between central and peripheral municipalities without taking coefficient sizes for central municipalities at face value.

The effect of highway accessibility on local labor market outcomes can also run via property values. If accessible municipalities become more attractive places to work or live, this should be reflected in the development of real estate prices. We test this channel with real estate advertisement data from 2005 to 2015 containing a large battery of object characteristics and the offer price.[16] Table A3.6 in the appendix presents summary statistics for the variables used in our analysis covering sales of detached houses, apartment buildings, condominiums, and commercial properties between 2005 to 2015 in MV. The data coverage, however, does not overlap with the main construction window of the BAB 20 such that variation in the access variable is low.[17] Therefore, the results presented in table 3.9 should be viewed as suggestive. We run pooled OLS estimations at the object level, controlling for municipality and year fixed effects, our baseline political and population variables, and object characteristics. The dependent variable is the object price per m^2. The results show a clear pattern of reduced offer prices across all property types. Detached houses within 10 km road distance to the next highway access are offered at around €165 less per m^2 than detached houses further away. Results in panel B show that the negative price effects are largest in the second distance band (5-10 km). Once we move further away from the highway access, prices for apartment buildings, condominiums, and commercial property are insensitive to highway accessibility.

3.5.4 Analysis of commuting flows

To give a more detailed picture on commuting flows in the wake of highway access, we use disaggregated commuter numbers by municipality of origin (for inbound commuters) and by municipality of destination (for outbound commuters). We have data on between-municipality commuting flows for the years 2002, 2008, and 2013. At these three points in time, we can link the place of residence and the place of work for all employees in Mecklenburg-Western Pomerania. We use the share of inbound and outbound commuters by spatial categories of origin and destination as dependent variables.[18] Due to the cross-sectional nature of the data, regression specifications are more parsimonious. We run simple difference-in-differences estimations including our access dummy and its interaction with a post-dummy, indicating whether a municipality gained highway access until 2008 or 2013. Furthermore, we control for district and year fixed effects as well as spatial, socio-demographic, and political characteristics of the municipality (see table A3.7).

[16] The data was collected by F+B, a commercial real estate consultancy firm, and covers roughly 18 million properties that were offered for sale in Germany during the period from January 2005 until December 2018.
[17] Because of the low variation in the shorter time window, we cannot conduct a heterogeneity analysis for peripheral and central municipalities.
[18] We further divide the “peripheral”-category from the *BBSR* into very peripheral and peripheral municipalities and additionally consider independent cities in the commuter flow analysis.

Table 3.10 shows results for the estimations with the share of inbound commuters from very peripheral, peripheral, central municipalities, and cities as the dependent variables. If an employee's place of work receives access to the new highway network, inbound commuting flows from peripheral municipalities and cities decrease by 6 and 5 percentage points (panel A). In line with previous results, it is the (very) peripheral places of work that drive these average effects (panel B). When remote localities get connected to the highway, they lose inbound commuters from both the periphery and the closest city, whereas the effect on the periphery is numerically larger than for the closest city.

Results for outbound commuters (see table 3.11) mirror those for inbound commuters. Within the different panels of table 3.11, we keep employees' places of residence constant and estimate differential effects by the location of the commuting destination. On average, if employees' municipality of residence falls under the 10 km distance band to the next highway access, they tend to commute less to very peripheral places of work. Results in panels B and C show that commuting flows within the periphery decrease substantially. At least for very peripheral places of work, we observe a shift of commuting flows to the nearest city.

3.6 Discussion

On average, our results suggest that peripheral municipalities in MV that gained immediate access to the newly constructed highway BAB 20 in the aftermath of German reunification experienced a decrease in local employment. Despite improved accessibility, inbound commuter flows to these locations were reduced. In general, the BAB 20 weakened the periphery especially as a place to work. With the advent of the new highway system, less city or peripheral residents choose to commute to (very) peripheral places to work. This effect proves to be persistent and very robust across specifications, conditioning factors, and estimation methods. We examine population, area-related, and property price channels for the employment effect. On average, improved highway accessibility is associated with a decrease in the overall population of a municipality. Furthermore, house prices seem to decline in municipalities close to a highway access. Since our findings are driven by peripheral municipalities, we distinguish between central and peripheral municipalities to unmask possible heterogeneous effects as discussed in section 3.2. Again, the average effects mask substantial spatial heterogeneity. Negative effects for the examined channels can be traced back to peripheral municipalities, while their central counterparts benefit on all margins. This suggests that in peripheral municipalities deagglomeration forces outweigh agglomeration forces and vice versa for core municipalities. In line with theoretical predictions by the core-periphery model, improved accessibility, i.e. a decline in trade costs, gives rise to a shift of population and employment to central municipalities at the expense of the periphery. Against the background of relatively equal populations between central and peripheral municipalities

preceding the construction of the BAB 20 (see table 3.2, panel B), this finding is especially striking. However, effect sizes for the regional centers should be interpreted with caution, since central municipalities make up only 2% of our sample.

Our findings can be rationalized by new economic geography models that state a home market effect amplified by population mobility. Upon construction of the BAB 20, falling transport costs reduce the degree of trade protection in the periphery and there might be substitution away from local production. Population and the number of firms are reduced in peripheral municipalities. In the long term, reducing transportation costs gives rise to concentration, i.e. to an agglomeration-periphery structure rather than to a uniform distribution between regions.

3.7 Conclusion

We focus on an episode of extensive highway construction in East Germany following re-unification and examine how access to the highway network influences municipal labor market outcomes. Getting access to a highway reduces transportation costs and increases attractiveness of municipalities as residential and firm locations. This may be an asset in the local competition for capital and labor. We consider the opening of highway number 20 that runs through the German state of Mecklenburg Western-Pomerania. The construction of the BAB 20 is the largest contiguous highway construction project in Germany since 1945. With its opening, the average distance of municipalities in MV to the next highway access was more than halved. In the baseline estimation, we use the difference-in-differences approach. The stage-wise opening of the highway also allows us to exploit variation in the timing of access in event study estimations. Our sample includes 745 municipalities over the 1998 to 2015 period. We follow the inconsequential units approach and exclude large cities that shape the route of the BAB 20. Highways are likely built to connect economic units, but peripheral municipalities often receive access to the highway network because they “accidentally” lie on a convenient route between two larger cities. Therefore, the connection to the highway network as well as the exact timing of access is close to random in peripheral municipalities.

Our results suggest that local employment levels in peripheral municipalities within 5 to 10 km road distance to a new highway access decrease by 14%. Using event studies, we show that there is no adjustment in local employment and inbound commuter flows in the four years prior to the highway opening, but both labor market outcomes start to fall immediately afterwards. In line with the baseline findings, more accessible municipalities experience a persistent decrease in employees who used to commute to work in the periphery. Finally, we reconcile our findings with the literature by examining the role of economic outcomes as possible drivers of the labor market effect. While the benefits of central municipalities in terms of population and employment seem implausibly large to be solely attributable to the

highway construction, the negative pattern for peripheral municipalities that gain close access is striking. Based on our analysis, we conclude that the BAB 20 cemented the core-periphery structure in MV by weakening the periphery as a place to work.

Table 3.3: Sorting into treatment – Cox regression

	(1)	(2)	(3)	(4)
Population (log)	-1.50	1.60	1.83	1.86
	(1.42)	(1.98)	(1.94)	(1.95)
Population age 15-65 (log)	1.43	-1.81	-2.37	-2.30
	(1.40)	(2.13)	(2.15)	(2.16)
Population density	-0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Unemployment rate		0.03	0.04	0.04
		(0.03)	(0.03)	(0.03)
Employees (place of work, log)		-0.15	-0.16	-0.07
		(0.45)	(0.45)	(0.43)
Employees (place of residence, log)		0.57	0.06	-0.19
		(0.76)	(0.83)	(0.82)
Firms (log)		-0.35	-0.41	-0.16
		(0.27)	(0.27)	(0.30)
Inbound commuter (log)		0.19	0.18	0.10
		(0.38)	(0.39)	(0.38)
Outbound commuter (log)		-0.12	-0.12	-0.13
		(0.13)	(0.14)	(0.14)
Income tax revenues (log)			0.85*	0.69
			(0.47)	(0.48)
Business tax revenues (log)			0.00	0.00
			(0.10)	(0.10)
Election mayor, share left				0.00
				(0.01)
Election county assembly, share left				0.02
				(0.02)
Tourism destination				-2.43**
				(1.17)
Pseudo R^2	0.00	0.00	0.01	0.02
Observations	12789	9505	8746	8746

Notes: The 745 municipalities of the German state of Mecklenburg-Western Pomerania are our observation units. The Cox regressions estimate a survival model with receiving a highway access within 10 km road distance as the failure event. Tourism destinations are municipalities listed in 2015 as resorts, health resorts, spas, coastal resorts, and coastal health resorts by the Statistical Office of Mecklenburg-Western Pomerania.

Table 3.4: Baseline results

	Employees		Commuters		Firms
	(1) place of residence	(2) place of work	(3) inbound	(4) outbound	(5) firms
Panel A: All municipalities					
Access (<10 km)	0.00	-0.05	-0.05	0.00	-0.03
	(0.01)	(0.04)	(0.05)	(0.01)	(0.02)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Within R^2	0.47	0.13	0.07	0.23	0.08
Number of mun.	745	737	732	744	737
Observations	13,410	12,237	12,038	12,786	11,933
Panel B: Peripheral municipalities - Baseline					
Access (<10 km)	0.00	-0.09**	-0.09*	-0.00	-0.04
	(0.01)	(0.04)	(0.05)	(0.01)	(0.03)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Within R^2	0.47	0.14	0.08	0.22	0.08
Number of mun.	728	720	715	727	720
Observations	13,104	11,961	11,784	12,522	11,653
Panel C: Rural municipalities					
Access (<10 km)	0.01	-0.05	-0.05	0.00	-0.03
	(0.01)	(0.04)	(0.05)	(0.01)	(0.02)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Within R^2	0.46	0.13	0.07	0.22	0.08
Number of mun.	733	725	720	732	725
Observations	13,194	12,022	11,823	12,570	11,730

Notes: Each panel shows the results of five difference-in-differences estimations. The 745 municipalities of the German state of Mecklenburg-Western Pomerania are our observation units (panel A). We use data in levels (logs) over the period 1998 to 2015. Our variable of interest (Access) equals one for municipalities within a road distance of 10 km to the next highway access point, and zero otherwise. Panel B shows our baseline regression results for the sub-sample where municipalities classified as being "central" (location) are excluded. In panel C, we exclude municipalities that are "predominantly urban" (structure). Control variables are lagged demographic and political variables, see table 3.1. Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

Table 3.5: Different distance bands

	Employees		Commuters		Firms
	(1) place of residence	(2) place of work	(3) inbound	(4) outbound	(5) firms
Panel A: Access <5 km					
Access (<5 km)	-0.01	-0.01	0.01	-0.01	0.03
	(0.01)	(0.08)	(0.09)	(0.02)	(0.06)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Within R^2	0.47	0.13	0.08	0.22	0.08
Number of mun.	728	720	715	727	720
Observations	13,104	11,961	11,784	12,522	11,653
Panel B: Access <15 km					
Access (<15 km)	-0.00	-0.10***	-0.10***	0.01	-0.01
	(0.01)	(0.03)	(0.04)	(0.01)	(0.02)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Within R^2	0.47	0.14	0.08	0.22	0.08
Number of mun.	728	720	715	727	720
Observations	13,104	11,961	11,784	12,522	11,653
Panel C: Distance bands together					
Access (<5 km)	-0.01	-0.04	-0.03	-0.00	0.03
	(0.01)	(0.08)	(0.09)	(0.02)	(0.06)
Access (5-10 km)	0.00	-0.14***	-0.15**	0.00	-0.06**
	(0.01)	(0.05)	(0.07)	(0.01)	(0.03)
Access (10-15 km)	-0.01	-0.08**	-0.09**	0.01	0.02
	(0.01)	(0.04)	(0.04)	(0.01)	(0.02)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Within R^2	0.47	0.14	0.08	0.22	0.08
Number of mun.	728	720	715	727	720
Observations	13,104	11,961	11,784	12,522	11,653

Notes: Each panel shows the results of five difference-in-differences estimations. The 728 peripheral municipalities of the German state of Mecklenburg-Western Pomerania are our observation units. We use data in levels (logs) during the period 1998 to 2015. Our variable of interest (Access) equals one for municipalities within a certain road distance to the next highway access point, and zero otherwise. Control variables are lagged demographic and political variables, see table 3.1. Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

Table 3.6: Treatment intensity

	Employees		Commuters		Firms
	(1) place of residence	(2) place of work	(3) inbound	(4) outbound	(5) firms
Access ($<$10 km) $\times$ high dist_{1995}	-0.00	-0.11***	-0.12**	-0.01	-0.06**
	(0.01)	(0.04)	(0.06)	(0.01)	(0.03)
Access ($<$10 km) $\times$ low dist_{1995}	0.00	-0.05	-0.04	0.01	0.01
	(0.01)	(0.10)	(0.11)	(0.01)	(0.05)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Within R^2	0.47	0.14	0.08	0.22	0.08
Number of mun.	728	720	715	727	720
Observations	13,104	11,961	11,784	12,522	11,653

Notes: The table shows the results of five difference-in-differences estimations. The 728 peripheral municipalities of the German state of Mecklenburg-Western Pomerania are our observation units. We use data in levels (logs) during the period 1998 to 2015. We interact our Access dummy with a dummy indicating high and low baseline distance from the highway in 1995. High/low initial distances are defined as values above/below the 1995 median value of 39 km. Control variables are lagged demographic and political variables, see table 3.1. Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

Table 3.7: Location heterogeneity

	Employees		Commuters		Firms
	(1) place of residence	(2) place of work	(3) inbound	(4) outbound	(5) firms
Access ($<$10 km) $\times$ periphery	-0.00	-0.09**	-0.09*	-0.00	-0.04
	(0.01)	(0.04)	(0.05)	(0.01)	(0.03)
Access ($<$10 km) $\times$ central	0.06***	0.32**	0.39**	0.04**	0.06
	(0.02)	(0.15)	(0.19)	(0.02)	(0.05)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Within R^2	0.48	0.13	0.08	0.23	0.08
Number of mun.	745	737	732	744	737
Observations	13,410	12,237	12,038	12,786	11,933

Notes: The table shows the results of five difference-in-differences estimations. The 745 municipalities of the German state of Mecklenburg-Western Pomerania are our observation units. We use data in levels (logs) during the period 1998 to 2015. We interact our Access dummy with a dummy for central (peripheral) municipalities. Control variables are lagged demographic and political variables, see table 3.1. Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

Table 3.8: Channels

	Population			Buildings
	(1) all	(2) age 15-65	(3) density	(4) with residential areas
Panel A: Baseline				
Access (<10 km)	-0.02**	-0.02	-1.28	-0.01
	(0.01)	(0.01)	(1.13)	(0.01)
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Within R^2	0.48	0.58	0.17	0.51
Number of mun.	745	745	745	745
Observations	13,410	13,410	13,410	2,980
Panel B: Heterogeneity				
Access (<10 km) × periphery	-0.04***	-0.03**	-2.90**	-0.03***
	(0.01)	(0.01)	(1.33)	(0.01)
Access (<10 km) × central	0.19***	0.20***	12.56***	0.12***
	(0.04)	(0.03)	(3.63)	(0.04)
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Within R^2	0.39	0.48	0.12	0.52
Number of mun.	745	745	745	745
Observations	14,900	14,900	14,900	2,980

Notes: Each panel shows the results of four difference-in-differences estimations. The 745 municipalities of the German state of Mecklenburg-Western Pomerania are our observation units. We use data in levels (logs), except for population density, which is measured in inhabitants per ha, during the period 1998 to 2015. Data on buildings with residential areas are aggregated for the periods 1996-2000, 2001-2004, 2005-2008, 2009-2011. We interact our Access dummy with a dummy for central (peripheral) municipalities. Control variables are lagged demographic and political variables, see table 3.1. Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

Table 3.9: Channels – Real estate offers

	Detached house	Apartment building	Condominium	Commercial property
	(1)	(2)	(3)	(4)
Panel A: Baseline				
Access (<10 km)	-165.45***	-205.92***	-345.72***	-248.47***
	(23.54)	(50.57)	(50.94)	(81.38)
Panel B: Different distance bands				
Access (<5 km)	-174.65***	-152.98***	-185.91*	-165.47*
	(40.31)	(55.63)	(99.44)	(85.95)
Access (5-10 km)	-192.64***	-236.36***	-427.67***	-291.64***
	(29.03)	(70.39)	(57.98)	(110.63)
Access (10-15 km)	-75.18***	102.18	-73.57	-321.94
	(18.92)	(74.18)	(47.95)	(298.85)
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Housing controls	Yes	Yes	Yes	Yes
Municipality controls	Yes	Yes	Yes	Yes
Within R^2	0.44	0.45	0.52	0.32
Observations	150,187	9,708	73,723	16,472

Notes: Each panel shows the results of four pooled OLS estimations. The observation units are the real estate properties offered for sale in the municipalities of the German state of Mecklenburg-Western Pomerania. We use data in levels during the period 2005 to 2015. All specifications include municipality and year fixed effects, housing controls, and municipality controls. Municipality controls are demographic and political variables, see table 3.1. Housing controls are the number of rooms, total area, year of construction, object type, and postal code, see table A3.6. Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

Table 3.10: Inbound commuters

	Inbound commuters by place of residence			
	(1) very peripheral	(2) peripheral	(3) central	(4) city
Panel A: All places of work				
Access (<10 km)	-0.01	0.01	0.02	-0.01
	(0.04)	(0.03)	(0.02)	(0.02)
Access (<10 km) × post	0.01	-0.06***	-0.03	-0.05***
	(0.03)	(0.02)	(0.03)	(0.02)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
R^2	0.53	0.41	0.86	0.65
Observations	1,019	772	258	1,290
Panel B: Very peripheral place of work				
Access (<10 km)	0.00	-0.03	0.03	-0.04*
	(0.05)	(0.02)	(0.02)	(0.02)
Access (<10 km) × post	0.00	-0.08**	-0.02	-0.04**
	(0.03)	(0.03)	(0.03)	(0.02)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
R^2	0.41	0.49	0.67	0.48
Observations	872	467	119	969
Panel C: Peripheral place of work				
Access (<10 km)	-0.05	0.06	0.06*	0.04
	(0.05)	(0.06)	(0.04)	(0.03)
Access (<10 km) × post	0.06	-0.07	0.00	-0.01
	(0.08)	(0.05)	(0.05)	(0.04)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
R^2	0.72	0.41	0.82	0.81
Observations	130	276	106	288
Panel D: Central place of work				
Access (<10 km)	0.00	-0.16	-0.04	0.26***
	(.)	(0.13)	(0.06)	(0.06)
Access (<10 km) × post	-0.18	-0.11	0.01	0.31***
	(0.33)	(0.13)	(0.07)	(0.08)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
R^2	0.96	0.95	0.99	0.99
Observations	17	29	33	33

Notes: Each panel shows the results of four difference-in-differences estimations. The 745 municipalities of the German state of Mecklenburg-Western Pomerania are our observation units. We use data on inbound commuters by place of residence in the years 2002, 2008, and 2013. Panel A shows results for all commuting destinations, while panels B, C, and D differentiate between very peripheral, peripheral, and central places of work. All specifications include district and year fixed effects. Control variables are spatial, demographic, and political variables, see table A3.7. Significance levels (standard errors clustered at the municipality of residence): *** 0.01, ** 0.05, * 0.10.

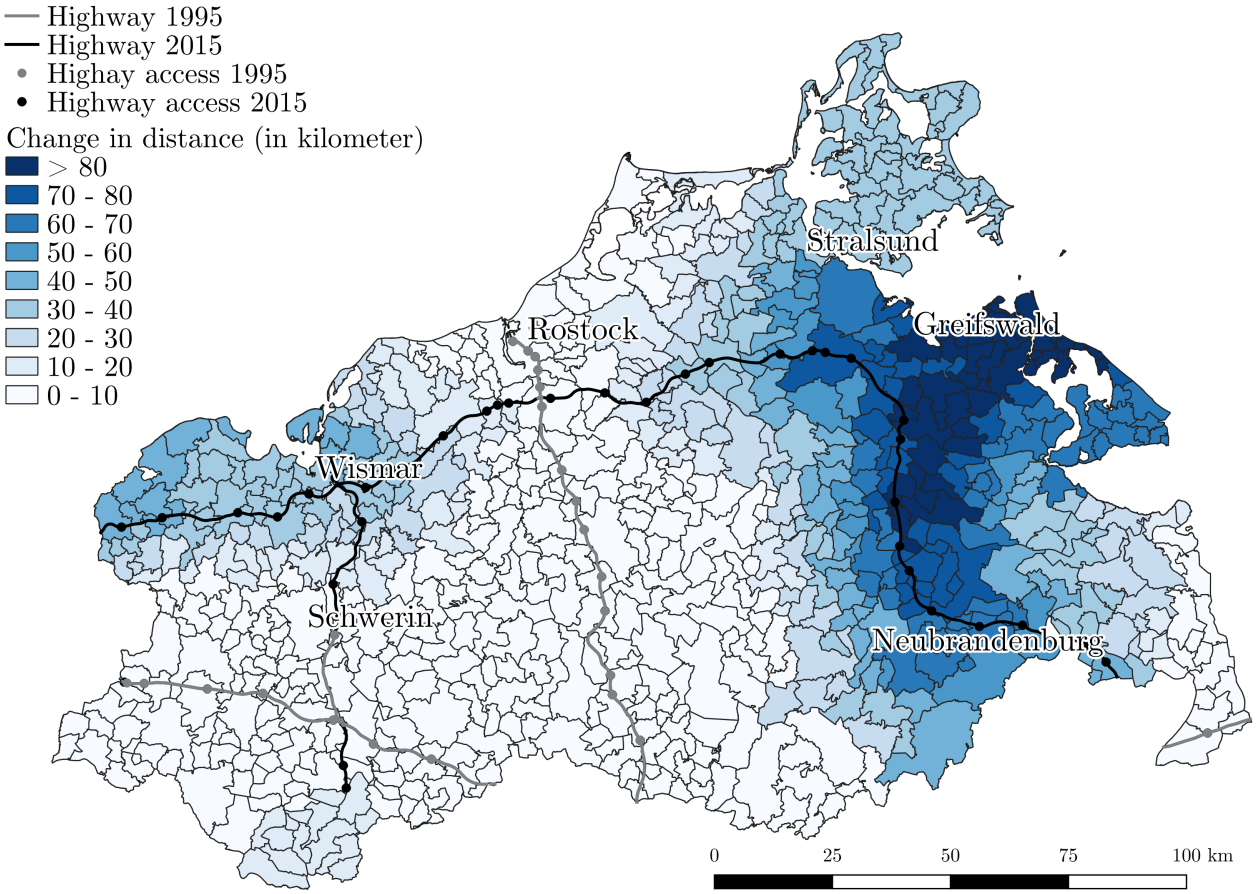
Table 3.11: Outbound commuters

	Outbound commuters by place of work			
	(1) very peripheral	(2) peripheral	(3) central	(4) city
Panel A: All places of residence				
Access (<10 km)	-0.07***	0.02	-0.02	0.01
	(0.02)	(0.02)	(0.01)	(0.02)
Access (<10 km) × post	-0.08***	0.03	-0.03***	0.02
	(0.02)	(0.02)	(0.01)	(0.02)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
R^2	0.51	0.43	0.86	0.74
Observations	1,709	1,782	678	2,187
Panel B: Very peripheral place of residence				
Access (<10 km)	-0.05**	0.03	-0.00	0.02
	(0.02)	(0.02)	(0.02)	(0.02)
Access (<10 km) × post	-0.07***	0.03	-0.01	0.04**
	(0.02)	(0.02)	(0.01)	(0.02)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
R^2	0.44	0.48	0.69	0.65
Observations	1,538	1,302	419	1,686
Panel C: Peripheral place of residence				
Access (<10 km)	-0.02	-0.08**	0.02	-0.01
	(0.02)	(0.04)	(0.02)	(0.02)
Access (<10 km) × post	-0.01	-0.11***	-0.02	-0.01
	(0.02)	(0.03)	(0.03)	(0.02)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
R^2	0.66	0.70	0.89	0.86
Observations	165	441	211	452
Panel D: Central place of residence				
Access (<10 km)		-0.11**	-0.01	0.00
		(0.04)	(0.02)	(0.02)
Access (<10 km) × post		-0.13***	-0.01	0.03
		(0.04)	(0.02)	(0.02)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
R^2	–	0.88	0.99	1.00
Observations	6	39	48	49

Notes: Each panel shows the results of four difference-in-differences estimations. The 745 municipalities of the German state of Mecklenburg-Western Pomerania are our observation units. We use data on outbound commuters by place of work in the years 2002, 2008, and 2013. Panel A shows results for all commuting origins, while panels B, C, and D differentiate between very peripheral, peripheral, and central places of residence. All specifications include district and year fixed effects. Control variables are spatial, demographic, and political variables, see table A3.7. Significance levels (standard errors clustered at the municipality of work): *** 0.01, ** 0.05, * 0.10.

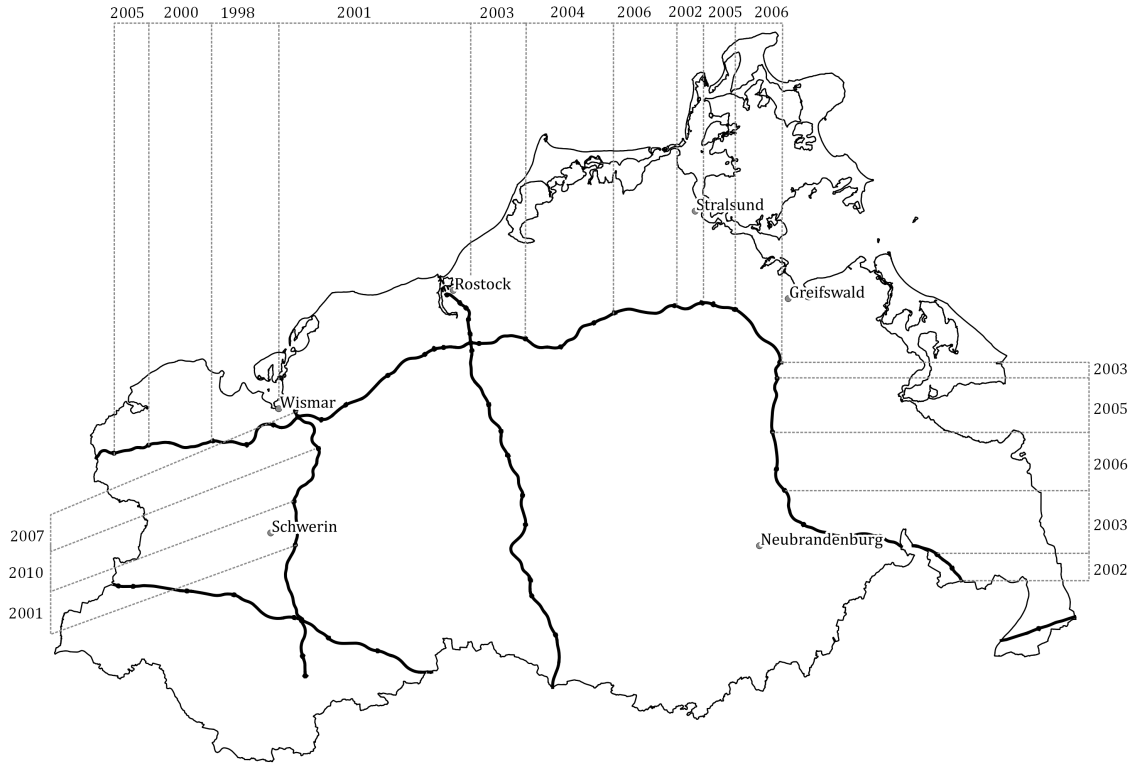
Appendix

Figure A3.1: Highway network in 1995 and 2015



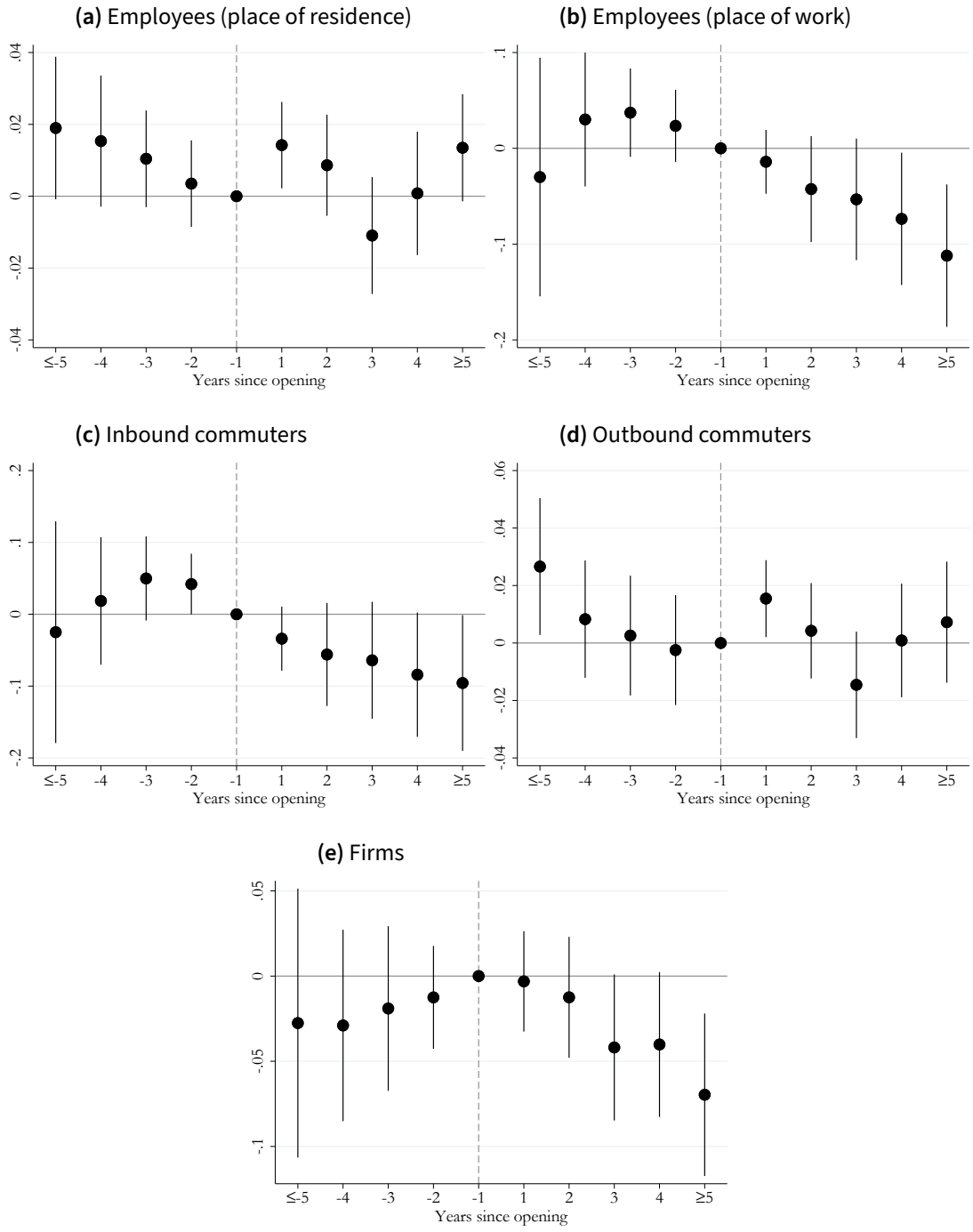
Notes: The map shows the municipalities of the German state of Mecklenburg-Western Pomerania (light gray borders). The highway network (access points) of 1995 is depicted in dark gray lines (points); black lines (points) represent the highway network (access points) of 2015. Municipalities are shaded according to the change in road distance to the next highway access point between 1995 and 2015.

Figure A3.2: Opening of highway segments in Mecklenburg-Western Pomerania, 1995-2015



Notes: The map shows the highway network in the German state of Mecklenburg-Western Pomerania. Parts of the highway that were open 2002, 2003, and 2005 were not immediately connected to larger cities until 2006. These parts without continuous routing to a larger city amounted to 7.4 km in 2002, 14.2 km in 2003 and 2004, and 33.4 km in 2005.

Figure A3.3: Event study results – Balanced event window



Notes: The figure shows the results of five event study estimations. Vertical dashed lines represent the year before a municipality falls within a road distance of 10 km to the next highway access point. We exclude municipalities with less than three pre- or post-treatment years, i.e. we end up with 717 peripheral municipalities of the German state of Mecklenburg-Western Pomerania as our observation units. We use yearly data in levels (logs) during the period 1998 to 2015. Bullets are point estimates and black lines represent the 90% confidence interval. We include year and municipality fixed effects and control variables (see notes to table 3.4). $Year = -1$ is the base category. We use standard errors robust to heteroskedasticity.

Table A3.1: Road network in Mecklenburg-Western Pomerania

Year	National highway (in km)	National primary, state, and county roads (in km)
1995	237	9,475
2015	554	9,434
Δ	317	-41

Notes: The table shows the length of the road network in Mecklenburg-Western Pomerania in 1995 and 2015. The decrease in the length of national primary, state, and county roads is due to a re-classification into municipal roads.

Table A3.2: Timing of highway access

	Freq.	Pct.
1998	15	12.50
2000	8	6.67
2001	22	18.33
2002	9	7.50
2003	32	26.67
2004	12	10.00
2005	10	8.33
2006	4	3.33
2007	3	2.50
2015	5	4.17
Total	120	100.00

Notes: The table displays the absolute and relative frequency of municipalities falling under 10 km road distance to the next highway access by year. The 745 municipalities of the German state of Mecklenburg-Western Pomerania are our observation units. We use yearly data between 1998 and 2015.

Table A3.3: Baseline without merged municipalities

	Employees		Commuters		Firms
	(1) place of residence	(2) place of work	(3) inbound	(4) outbound	(5) firms
Access (<10 km)	-0.01	-0.12**	-0.13*	-0.00	-0.04
	(0.01)	(0.06)	(0.07)	(0.01)	(0.03)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Within R^2	0.48	0.15	0.08	0.41	0.08
Number of mun.	548	540	535	547	540
Observations	9,864	8,772	8,599	9,289	8,621

Notes: This table reproduces panel B of table 3.4 without merged municipalities. Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

Table A3.4: Anonymized values

	Employees		Commuters		Firms
	place of residence	place of work	inbound	outbound	firms
1998	0.00	0.03	0.04	0.03	0.00
1999	0.00	0.05	0.05	0.05	0.00
2000	0.00	0.06	0.06	0.06	0.00
2001	0.00	0.03	0.06	0.06	0.00
2002	0.00	0.07	0.07	0.06	0.00
2003	0.00	0.07	0.07	0.05	0.00
2004	0.00	0.07	0.07	0.06	0.00
2005	0.00	0.05	0.07	0.04	0.00
2006	0.00	0.05	0.09	0.05	0.00
2007	0.00	0.04	0.07	0.05	0.00
2008	0.00	0.05	0.08	0.05	0.00
2009	0.00	0.04	0.08	0.05	0.00
2010	0.00	0.15	0.17	0.05	0.00
2011	0.00	0.15	0.16	0.04	0.00
2012	0.00	0.14	0.15	0.03	0.00
2013	0.00	0.15	0.16	0.04	0.00
2014	0.00	0.15	0.16	0.04	0.00
2015	0.00	0.16	0.17	0.04	0.00
Total	0.00	0.08	0.10	0.05	0.00

Notes: The table displays the yearly share (mean) of anonymized values for our main dependent variables. The 745 municipalities of the German state of Mecklenburg-Western Pomerania are our observation units. We use yearly data between 1998 and 2015.

Table A3.5: Baseline – Sample until 2009

	Employees		Commuters		Firms
	(1) place of residence	(2) place of work	(3) inbound	(4) outbound	(5) firms
Access (<10 km)	0.00	-0.08*	-0.11**	-0.00	-0.03
	(0.01)	(0.04)	(0.05)	(0.01)	(0.02)
Municipality FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Within R^2	0.51	0.18	0.11	0.18	0.10
Number of mun.	728	720	714	727	719
Observations	8,736	8,262	8,124	8,325	7,691

Notes: This table reproduces panel B of table 3.4 excluding the years 2010 to 2015. Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

Table A3.6: Descriptive statistics – Real estate offers

	Observations	Mean	SD
Detached house			
Price per m^2	150,187	1,112.68	695.35
Number of rooms	150,187	4.52	2.05
Area in m^2	150,187	140.03	65.95
Apartment building			
Price per m^2	9,708	714.31	578.39
Number of rooms	9,708	8.46	8.38
Area in m^2	9,708	357.28	369.10
Condominium			
Price per m^2	73,723	1,882.80	1,220.50
Number of rooms	73,723	2.93	1.75
Area in m^2	73,723	87.03	60.28
Commercial property			
Price per m^2	16,472	1,096.77	945.71
Number of rooms	16,472	4.00	6.35
Area in m^2	16,472	554.50	1,716.45

Notes: The observation units are the real estate properties offered for sale in the municipalities of the German state of Mecklenburg-Western Pomerania. We use data in levels during the period 2005 to 2015. For data protection reasons, we cannot show the minimum and maximum values of the variables.

Table A3.7: Descriptive statistics – Commuter data

	Observations	Mean	SD	Min	Max
Dependent variables					
Share inbound by place of residence:					
very peripheral	1,037	0.37	0.21	0	0.88
peripheral	790	0.24	0.16	0	0.73
central	276	0.17	0.18	0	0.69
city	1,755	0.08	0.14	0	0.65
Share outbound by place of work:					
very peripheral	1,710	0.33	0.19	0.0046	0.79
peripheral	1,783	0.30	0.20	0.020	0.77
central	679	0.16	0.19	0.0049	0.77
city	2,230	0.18	0.20	0	0.75
Control variables					
Distance to highway access (1995), in km	2,241	46.96	33.54	0.24	143.8
Location (categorical)	2,241	1.26	0.49	1	3
Distance to closest city, in km	2,241	29.93	15.18	1.04	77.4
Distance to Poland, in km	2,241	115.32	60.90	0.63	242.6
Population	2,241	2,232.31	9,367.86	110	203431
Dependency ratio	2,241	0.30	0.09	0.064	0.72
Population density	2,241	59.13	104.89	5.02	1122.3
Election mayor, share left	2,223	0.16	0.18	0	1
Election county assembly, share left	2,238	0.44	0.12	0.12	0.80
Election state assembly, share left	2,241	0.54	0.10	0.16	0.79
Share inbound (place of residence: access $<$10 km)	1,308	0.13	0.17	0	0.85
Share outbound (place of work: access $<$10 km)	2,188	0.18	0.21	0	0.78

Notes: The 745 municipalities of the German state of Mecklenburg-Western Pomerania are our observation units. We use data for the years 2002, 2008, and 2013. The location variable represents a categorical variable which takes on the value one for very peripheral municipalities, two for peripheral municipalities, and three for central municipalities. Distance to the closest highway access is measured as road distance in km, while the other distance variables measure linear distance (to the closest city/to the Polish border).

4 Mergers and transfers[1]

Abstract

The overarching goal of municipal territorial reform is to realize economies of scale. At the same time, fiscal equalization schemes often grant disproportionately higher grants to large municipalities. Little is known about the consequences of encompassing territorial reforms on fiscal equalization transfers. I examine how territorial reforms influence fiscal equalization transfers using the example of the German state of Saxony, where the number of municipalities decreased by 74% from 1,614 in 1992 to 419 in 2019. I use generalized difference-in-differences and event study strategies to estimate how mergers affect fiscal transfers and the local budget. The results suggest that municipalities do not choose merging partners to maximize transfer levels, i.e. there is no evidence for selection into treatment. However, territorial reforms give rise to a persistent redistribution of fiscal transfers: reform municipalities gain up to €34 per capita in yearly transfers, an effect that amounts to 11% of the mean transfer in the sample. With the exception of administrative expenditures, municipal spending does not change systematically after mergers. The findings raise doubts about the adequacy of including population-based indicators in equalization formulas.

[1] I thank Thomas Brosy, Clemens Fuest, Andreas Haufler, Marko Koethenbuerger, Niklas Potrafke, Yaniv Reingewertz, Felix Roesel, and participants of the Annual Meeting of the European Public Choice Society (2021), the NIPFP-IIPF conference (2021), the ifo-LMU Public Economics Workshop (2021), the 31^{st} BGPE Research Workshop, and the Annual Meeting of the IIPF (2021) for helpful comments and Theresa Berz for valuable research assistance.

4.1 Introduction

Mergers constitute a shock to jurisdiction size lending themselves to empirical investigations of the long-standing question about the optimal size of local jurisdictions: is bigger always better? The prime motivation for municipal mergers is the realization of economies of scale. Larger jurisdictions should professionalize their administration and be able to offer local services at lower unit costs. Although scholars have investigated territorial reforms taking place at different points in time, across a range of countries, and with distinct (legal) set-ups, the evidence does not suggest that mergers decrease total expenditures (Lüchinger and Stutzer, 2002; Allers and Geertsema, 2016; Blesse and Baskaran, 2016; Blom-Hansen *et al.*, 2016; Roesel, 2017).[2] Exceptions are the works by Reingewertz (2012) and Miyazaki (2018) who show that merged Israeli and Japanese municipalities reduced total expenditures on average. With the main focus of the literature on the consequences of mergers being whether and when economies of scale realize, the revenue side of local budgets has received relatively little attention so far (Studerus, 2016).

I investigate how mergers influence fiscal equalization transfers. Providing evidence on the relation between mergers and transfers is new and is useful for policy debates worldwide. Almost all OECD countries operate fiscal equalization arrangements to reduce fiscal disparities across sub-national jurisdictions (Blöchliger *et al.*, 2007). Some of these arrangements take the different production costs of public services into account to determine fiscal entitlements. Australia, Denmark, the Netherlands, Norway, Sweden, and the United Kingdom compensate municipalities for the cost of density or dispersion. Austria and the Czech Republic operate cost equalization systems that favor smaller municipalities. In Korea, the local tax share formula takes the number of administrative districts and government officials into account, causing the public sector to grow. Until 2006, the Portuguese equalization arrangement relied on the number of municipal sub-units to calculate a municipality's entitlements. In Germany, population figures are artificially inflated to grant disproportionately higher grants to larger municipalities. Thus, large-scale merger reforms might give rise to a substantial and permanent redistribution of fiscal equalization transfers across jurisdictions which in turn influence spending patterns at the local level (Hailemariam and Dzhumashev, 2019). The results of this paper raise doubts about the adequacy of including population-based indicators in the equalization formula. Systems that "reward" larger or more densely populated jurisdictions are hardly reconcilable with the efficiency argument underlying merger reforms (Birke and Lenk, 2000).

I study how mergers influence fiscal equalization transfers in the East German State of Saxony which is an ideal testing ground for several reasons. First, there were compulsory and voluntary mergers taking place in Saxony over the sample period 1992-2010. That means, even if municipalities strategically choose merging partners based on considerations about fiscal

[2] Some studies even find significant increases in total spending, such as Fritz (2011) or Moisio and Uusitalo (2013).

equalization transfers, this should not be the driving rationale for territorial reforms enacted by the state. Second, the Saxon merger waves were encompassing. Thus, redistribution effects within the equalization scheme affect a large number of municipalities. Third, Saxony's population weighting scheme within the fiscal equalization system is particularly progressive so that revenue effects are potentially large.

Generalized difference-in-differences regressions confirm the redistributive effects of merger reforms. The merger waves in Saxony gave rise to a significant and economically sizable increase in per capita fiscal transfers for reform municipalities. Moreover, event study estimations show that this revenue gain is permanent for merging municipalities. The positive revenue effects for fiscal transfers, however, are overcompensated by shortfalls in other revenue categories. Despite sizable revenue effects, expenditures hardly change. Although the parameter estimates often have negative signs across expenditure categories, they do not turn out to be statistically significant. The exception are administrative expenditures where some cost saving effects seem to be realized – especially with regard to material expenses.

The findings of this paper on the redistributive effects of merger reforms within the equalization arrangement add to the rich literature on the effects of fiscal equalization payments in a federation. Scholarly interest has revolved in particular around the flypaper effect – the extent to which additional grant receipts translate into higher government spending levels (Hines and Thaler, 1995; Inman, 2008) –, an empirical regularity that is at odds with standard predictions from collective choice theory (Bradford and Oates, 1971). Several studies exploit population or other thresholds in grant allocation formulas to identify the causal effects of grant receipts on local spending and taxes (Dahlberg *et al.*, 2008; Gennari and Messina, 2014; Liu and Ma, 2016). Most closely related to my paper is the work by Helm and Stuhler (2021) and Mense *et al.* (2019) who exploit quasi-experimental variation within Germany's fiscal equalization scheme triggered by Census revisions of official population counts. Their results show increased investment levels (Helm and Stuhler, 2021) and reduced debt (Mense *et al.*, 2019) as a result of transfer gains. The response of local government expenditures to intergovernmental grants is, however, not necessarily unequivocal: it has been shown that equalization payments can reduce the share of productive spending of sub-national governments (Cyrenne and Pandey, 2015; Hailemariam and Dzhumashev, 2019).

4.2 Theoretical considerations

Fiscal equalization systems are often rule-based, comparing a jurisdiction's tax capacity with its fiscal need to determine entitlements. How mergers influence fiscal transfers is theoretically ambiguous. On the one hand, larger municipalities receive more transfers than smaller municipalities. On the other hand, mergers are intended to give rise to economies of scale: in merged municipalities, fiscal capacity is expected to increase which, in turn, will ceteris paribus decrease fiscal transfers. I discuss the theoretical considerations based on the institutions in Saxony.

In Saxony, the majority of fiscal equalization transfers, the so-called “key allocations", are distributed via a lump-sum formula:

$$transfer_{it} = max(\zeta \times (G_{it} - C_{it}), 0) \tag{4.1}$$

The standardized fiscal need G_{it} of a municipality is compared with its tax capacity C_{it}. If this comparison results in a shortfall, 75% (equalization ratio ζ) of this shortfall is made up by key allocations.[3] In the Saxon municipal equalization scheme, a municipality's fiscal need depends mainly on the population count, while tax capacity is calculated from its tax revenues and state-wide average tax rates. To gauge the potential effects of mergers on transfers, I discuss how both quantities are expected to change with territorial reform.

A municipality's fiscal need is determined as follows:

$$G_{it} = \underbrace{(\eta_{it} \cdot pop_{it-2} + \omega_t \sum_{s=1}^{S} \theta_{st} \cdot pupil_{ist})}_{\text{total amount (}\textit{Gesamtansatz}\text{)}} \cdot B_t \tag{4.2}$$

The key element in calculating the fiscal need is a jurisdiction's population count (pop_{it-2}) which is weighted by a municipality-specific scaling factor η_{it}.[4] Note that although the relevant population count enters the formula as a two-year lag, the respective numbers are converted to the territorial status of January 1 of the equalization year. This means that, when the effective date of a municipal merger falls on January 1, equalization transfers are adjusted immediately. The scaling factors are a progressive function of the local population count and their range is stipulated in the Fiscal Equalization Act. Figure 4.1 shows the relationship between scaling factors and population count at the beginning (1993), in the middle (1999), and at the end of the sample period (2009).[5] Until 1998, the scaling factor followed a step function with respect to the population count, before Saxony switched to the more common linear progressive system, where scaling factors within the population brackets are determined by means of linear interpolation. The weighting of inhabitants is justified from a normative point of view as compensation for external effects from services primarily provided by large municipalities: they offer cultural, health, educational, or university facilities without the surrounding municipalities sharing in the resulting costs (Sächsisches Staatsministerium der Finanzen, 1999).

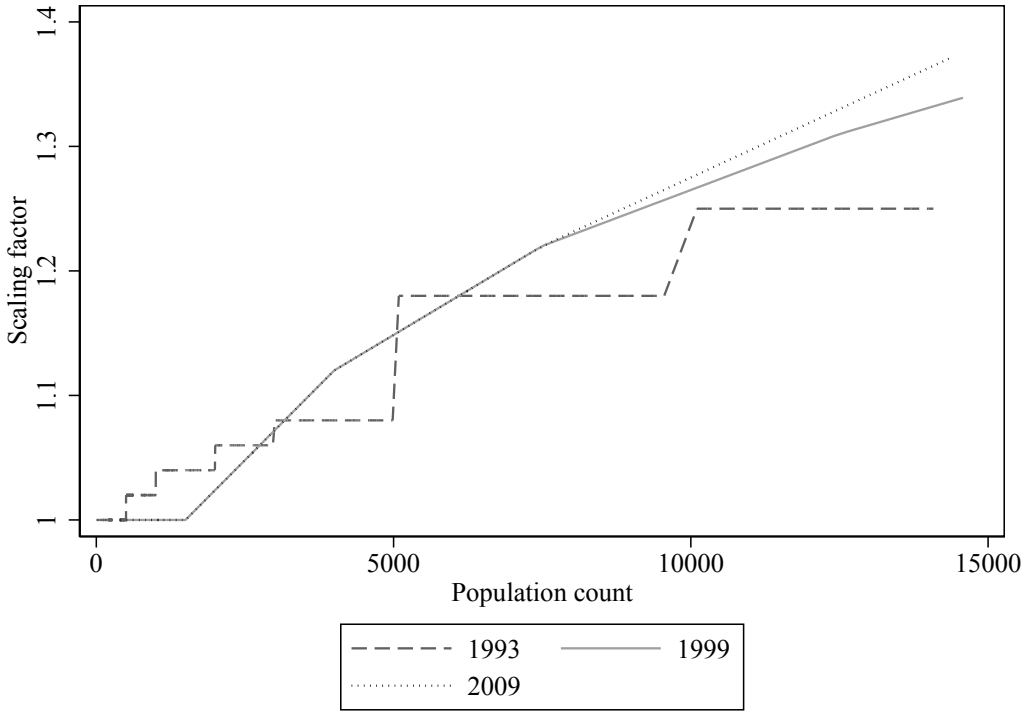
Finally, the total amount (*Gesamtansatz*) is multiplied by a basic allowance B_t to determine the fiscal need. The basic allowance is an endogenous quantity and calculated such that the available funds by the state are fully utilized.

[3] Until 1994, the equalization ratio was 70%.

[4] Since 1995, the specific financial requirements of municipalities that run schools are taken into account in the fiscal need calculation. Pupil counts are weighted by a percentage θ_{st} due to differences in per pupil cost intensity across school types s. This weighted sum is then multiplied by a factor ω_t which depends on a jurisdiction's status as district municipality or district-free city.

[5] For district-free cities constant scaling factors apply.

Figure 4.1: The scaling factor



Notes: The figure shows the functional relationship between the scaling factor and a municipality's population count as stipulated in the Saxon Fiscal Equalization Act. The dashed line represents the status quo as of 1993, the solid line refers to 1999, and the dotted line to 2009.

Since the Saxon equalization scheme grants disproportionately higher grants to larger municipalities, one part of the response of transfers on territorial reforms will be mechanical and generally positive. In a ceteris paribus-framework, higher population counts give rise to higher per capita fiscal transfers for the merging coalition. Ultimately, however, the availability of state funds limits the mechanical increase, since transfers within the system are calculated such that the overall transfer volume is fully utilized. Mergers create a sudden, possibly large, and permanent jump in the weighted population counts of reform municipalities. As a result, the endogenous basic allowance decreases ceteris paribus for all – merging and non-merging – municipalities, generating redistributive effects within the fiscal equalization scheme. Accordingly, transfers to a municipality ultimately depend on the funds available. Even though the choice of indicators used to derive the fiscal need is intended to reflect, at least in part, the burden situation of a municipality, the measure is not a function of the actual financial resources required by the individual municipality to cover its expenditure needs.

In theory, the effect of mergers on a municipality's tax capacity is expected to be positive. The widespread reform of local government in a number of developed countries overwhelmingly reflects the view of public policymakers that larger local jurisdictions are better able to capture economies of scale (Dollery and Robotti, 2008). Reformers of local government structures

argue that larger jurisdictions would enhance efficiency in the provision of local services and foster economic development. Less bureaucracy and more efficient governments will be more successful in attracting businesses, encourage the expansion of existing businesses, and increase local employment (Faulk and Schansberg, 2009). Empirical evidence supporting the theory of increased economic activity post-reform has been provided for China (Tang and Hewings, 2017) and recently for municipal mergers in the German context (Egger *et al.*, 2021). Moreover, agglomeration rents in the new administrative units can be taxed (Brülhart *et al.*, 2012). In Switzerland, municipal tax rates are positively related to jurisdiction size within urban areas (Luthi and Schmidheiny, 2014).

A priori, the individual consequences of mergers on transfers are unclear. On the one hand, they depend on the parameters of the equalization system, like the scaling factor and the volume of overall funds provided by the Saxon state. Assuming a fixed volume of funds, a merged municipality gains if the decrease in the basic allowance is overcompensated by the increase in weighted inhabitants. On the other hand, they depend on economic fundamentals of the newly formed jurisdictions. A merger can raise the tax capacity of a municipality which is inversely related to the individual transfer volume.

4.3 Background

4.3.1 Fiscal equalization

Local self-government must take place on the basis of secure financial resources. According to Article 87 (1) of the Saxon Constitution, the Free State of Saxony must ensure that the local authorities are able to perform their duties. For this purpose, it operates – as do all other federal states[6] – a municipal fiscal equalization system to redistribute funds from the state to the local level (vertical equalization). Total equalization payments are limited to a percentage of state tax revenues. The horizontal equalization between the municipalities is brought about by taking into account the differences in fiscal capacity and needs existing between the municipalities when assessing the state allocations (Sächsisches Staatsministerium der Finanzen, 1999).[7]

[6] Exceptions are the three city states Berlin, Hamburg, and Bremen. There, the municipal and state levels are integrated.

[7] While some institutional details vary across states, this basic structure is common to all municipal fiscal equalization schemes in Germany.

Saxony provides the majority of the equalization funds to its municipalities via general and investment key allocations as general, unrestricted funds (1999: 90.2%). They are at the free disposal of the municipalities and thus secure their freedom of self-government. The key allocations (*Schlüsselzuweisungen*) are therefore the most important income of the municipalities from the fiscal equalization system.[8]

4.3.2 The merger reform in Saxony

Goals

Saxony's local government landscape at the beginning of the 1990s – after German reunification – was very fragmented and small-scaled. In 1992, the median municipality had 753 inhabitants. The fundamental goal of the comprehensive reform of the municipal territory was to create administrative structures that promoted effectiveness and efficiency. By strengthening their performance and administrative capacity, municipalities were to be enabled to improve their services to citizens in line with their needs. Sufficiently large local administrative units were also expected to prevent competitive investment in the social and cultural sector. In addition, the reform aimed at a better utilization and more economic operation of cost-intensive public institutions.

Population size was seen as an important indicator of the performance of local self-governing bodies. In its proposals for the municipal territory reform laws, the Saxon state government had committed itself to a minimum population size of 5,000 inhabitants for reasons of viability.[9] Regional-specific needs made it possible to fall below a limit of 3,000 inhabitants in rural areas, while a minimum population of about 8,000 was aimed for in densely populated areas directly bordering on the regional centers.

Voluntary phase 1990-1998

Deliberations on regional and administrative reform at the municipal level – in addition to the district area reform – began very early on. Initially, in the interest of the recently restored local self-government[10], the reform of municipal territorial and administrative structures was to take place exclusively on a voluntary basis. In the first legislative period, only the principles and benchmarks for municipal reform should be developed, but no legislative regulation was intended. These "Principles for the municipal target planning in the Free State of Saxony"

[8] They compensate for the still relatively low level of taxation compared with the municipalities in the old federal states.

[9] The member municipalities of an administrative community or an administrative association should have a minimum size of at least 1,000 inhabitants.

[10] The municipalities in the German Democratic Republic had virtually no significance as independent political entities in the system of state socialism (Neckel, 1992).

were published in January 1994 by the Saxon State Ministry of the Interior (Sächsisches Staatsministerium des Inneren, 1994). The aim was to leave it to the municipalities to design the reform steps based on the Saxon municipal code and Saxon law on municipal cooperation.

In order to support this process, voluntary municipal associations and cross-municipal cooperation were granted one-time special needs allocations to strengthen administrative and performance capacity. The basic outlines of the communal target planning and the ideas of the Saxon State Ministry of the Interior for their practical spatial implementation were discussed with all districts. This was made possible in particular by the county tours conducted by the Saxon State Ministry of the Interior in the fall of 1993, 1994, and 1995. In addition, numerous discussions were held with affected municipalities. Between October 3 1990 and October 27 1998, the number of Saxon municipalities had been reduced from 1,626 to 773. Since the legal regulation provided for a reduction to 537 municipalities, over 75% of the reform need was met on a voluntary basis.

Compulsory phase 1999

Despite clear progress in the formation of larger local administrative units, it was already apparent during the voluntary phase that not all of the reform needs could be met voluntarily. Moreover, the reform process had progressed at different rates in the individual administrative districts of Saxony. The municipal reform was therefore also to be brought to an end by means of a legal regulation for those municipalities whose administrative structure did not yet correspond to the model of the municipal territory reform in the Free State of Saxony.

On March 14 1995, the Saxon state government therefore decided to prepare the necessary legislative decisions on regional and administrative reform at the municipal level in separate regulations and to give priority to the regulatory area of the urban-rural problem over the regulatory area in the rest of the state. In the meantime, the state government had commissioned an expert report to a research team at the “Technische Universität Dresden”, which was to investigate the necessary legislative measures for incorporation in view of the strong conflicts of interest that were to be expected, particularly in the surrounding area of the independent cities. With the help of the knowledge gained from this report, the draft bills for the integration laws were prepared and handed over to the affected residents, communities, districts, and public authorities for consultation between September 1996 and January 1997. In parallel, since mid-1996 the state government has been preparing legal regulations for the rest of the state.

Hearings on the draft bills of the new laws were essentially completed by the end of November 1997. As a result of the evaluation of the extensive comments on these speaker drafts, the drafts were revised and introduced into the Saxon state parliament by resolution of the state government. A process of extensive parliamentary deliberations, including a hearing of experts, began.

On October 27 1998, the Saxon state parliament passed the laws on municipal territory reform in the five planning regions of Upper Lusatia-Lower Silesia, Upper Elbe Valley/Eastern Ore Mountains, Southwest Saxony, Chemnitz-Ore Mountains and West Saxony, after having already passed the incorporation laws for the surroundings of the district-free cities on July 23 the same year. These laws entered into force on January 1 1999.

4.3.3 Local government in Germany

In the federal state structure of Germany, the municipalities are constituent parts of the German states. Article 28 (2) of the Basic Law secures municipalities the right "[...] to regulate on their own responsibility all the affairs of the local community within the framework of the law." The tasks assumed by the municipalities are therefore determined by the respective local needs and can change as a result of social developments. A distinction is made between municipal tasks within the sphere of action of the municipality itself and tasks within the sphere of action transferred to the municipality.

First of all, voluntary self-government tasks belong to the tasks within the municipality's own sphere of action. The municipality is free to decide *whether* and *how* to take on such tasks, provided it complies with the applicable laws and can finance them. Voluntary self-government tasks are for example business development, support to sports clubs and associations, and the establishment and maintenance of playgrounds, museums, theaters, green spaces etc. In addition, there are tasks that *must* be assumed by the municipalities because this is prescribed by federal or state laws. The only thing municipalities can decide for themselves is *how* they do it. These tasks are called obligatory self-government tasks and include the administration of schools of all types, kindergartens, urban land use planning, promotion of housing, waste disposal, social and youth welfare.[11] Finally, the tasks from the transferred sphere of action are tasks transferred by federal or state laws. In most cases, the municipalities no longer have any leeway in the way they carry out these tasks. This relates to tasks of building supervision, registration law, regulatory law, matters concerning foreigners, and civil defense.

4.4 Empirical framework and results

4.4.1 Determinants of mergers

Data

Voluntary mergers do not form randomly. To check whether municipalities strategically sort into treatment based on potential transfer gains, I first examine the determinants of mergers, before turning to the consequences. Data preparation thus follows a twofold approach. The

[11] It should be noted that municipal tasks can also be transferred to private companies. In many cities, for example, waste disposal or electricity supply is taken over by private companies.

inherent problem when studying local mergers is that municipalities constitute the lowest level at which official statistical data is provided. When boundaries change this statistical unit ceases to exist, rendering comparisons between pre-merger administrative units infeasible.

The analysis of merger determinants requires data at the pre-merger level of individual municipalities. I use self-compiled data from the Saxon municipal statistics which reflect yearly administrative boundaries and are available from 1996 onward. Therefore, the analysis of merger determinants is restricted to years after 1996. I analyze voluntary merger decisions in 1997 and 1998, i.e. the two years preceding the compulsory merger wave in 1999. I compare realized to potential mergers so the unit of observation is a group of municipalities that decides to merge or not. Individual municipal characteristics reflect the territorial status as of December 31 1996.

To create the dataset of potential mergers, I use the original map of Saxon municipalities at the end of 1996 and determine the set of neighbors for each municipality. The list of neighboring municipality pairs is then used as input for the ISMAGS algorithm (Houbraken *et al.*, 2014) to determine all potential coalitions of size $n = 2, 3, 4$. The coalition size refers to the number of municipalities participating in a merger. I simulate mergers with up to four municipalities to mirror coalition sizes of realized mergers in 1997 and 1998. Similarly, I restrict potential mergers to only occur within district boundaries – the exception being mergers with district-free cities – and between neighboring municipalities to ensure that the new jurisdiction forms a spatially contiguous unit. I investigate whether potential fiscal equalization gains incentivize voluntary mergers. To measure the incentive effect, I calculate the potential per capita transfer growth through mergers:

$$\Delta transfer_c = \frac{\frac{transfer_{c,post}}{pop_c}}{\frac{\sum_{i=1}^{n} transfer_{i,pre}}{pop_c}} - 1 \tag{4.3}$$

To calculate this quantity, I treat each merger as a single event. That is, I carry out the calculations as if the respective coalition (realized or potential) was the only one to form and keep the territorial status of all other municipalities constant. Thus, the *ceteris paribus* post-merger transfer level ($transfer_{c,post}$) is simulated by re-calculating the fiscal need and fiscal capacity for the new administrative unit, while keeping all "institutional" variables, like the overall transfer volume, at their 1996 levels.[12] This simulated transfer level is then compared to the individual pre-merger transfers aggregated to the coalition level ($\sum_{i=1}^{n} transfer_{i,pre}$) to get the potential transfer growth.

Empirical strategy

In voluntary mergers, municipalities might strategically choose potential partners following local political considerations (Hyytinen *et al.*, 2014; Bruns *et al.*, 2015) or fiscal motives (Hinnerich, 2009; Jordahl and Liang, 2010; Hansen *et al.*, 2014; Hirota and Yunoue, 2017). So

[12] Note that the basic amount is also re-calculated in each iteration.

far, transfer gains have not been considered as an element in merger decisions. Municipalities might strategically choose merging partners to maximize future per capita transfers via the fiscal equalization scheme. Statistical modeling of spatial merger decisions faces many challenges. In outlining these challenges and the statistical approach used to address them, I follow Saarimaa and Tukiainen (2014). First, a merger decision is two-sided, i.e. a voluntary merger is only observed if all potential partners agree to merge. Second, the choice set of potential partners and also the number of potential coalitions can be large given that the number of municipalities participating in a merger is not restricted. However, a municipality can only merge once in a given year. Third, merger choices are spatially interdependent, since a merger between two or more municipalities affects the choice set of their neighboring municipalities.

My empirical approach is based on the comparison between realized mergers and potential mergers that did not take place (Saarimaa and Tukiainen, 2014). The main empirical analysis is carried out at the coalition level so that the unit of observation is a group of municipalities that may or may not merge. By conducting the analysis at the coalition level, I abstract from municipalities' individual choices and avoid the problem of two-sided decision making. The merger decision is modeled using the binary logit model

$$Pr(Y_c = 1|X) = \Lambda(\alpha_c + X_c'\beta + u_c), \tag{4.4}$$

where Y_c is an indicator variable that equals one if the observation belongs to a realized merger and zero otherwise and $\Lambda(\cdot)$ indicates the logistic cumulative distribution function. A coalition is only realized if all municipalities agree to merge. To take this into account, I control for extreme values of some municipal characteristics at the coalition level.

As table 4.1 shows, the number of potential coalitions grows large in coalition size, even with the imposed spatial constraints on mergers. When using the full sample of potential coalitions, realized mergers would be a relatively uncommon occurence. For this reason, I use choice-based sampling (see Saarimaa and Tukiainen, 2014) which relies on the idea to deliberately oversample these rare events. Accordingly, I keep all realized mergers in the sample as treatment group and then randomly sample a control group from the number of potential coalitions. For each actual coalition of size *n*, I sample 15 potential coalitions of equal size that did not merge, i.e. I stratify the sampling based on coalition size. To avoid biased estimates arising from this sampling procedure, the empirical analysis is conditioned on strata fixed effects and uses the weighted exogenous maximum likelihood estimator (WESML) proposed by Manski and Lerman (1977).

Results

Figure 4.2 shows the spatial distribution of the 46 realized voluntary mergers observed in the years 1997 and 1998. Merging municipalities are relatively evenly distributed across Saxony and are always adjacent to each other. Results for the binary logit estimation according to

Table 4.1: Potential and realized mergers

Coalition size	Potential coalitions	Actual mergers	Control coalitions	WESML weights merger = 1	WESML weights merger = 0
2	1,936	41	615	0.338843	1.044077
3	6,812	4	60	0.0093952	1.06604
4	28,346	1	15	0.0005645	1.066629

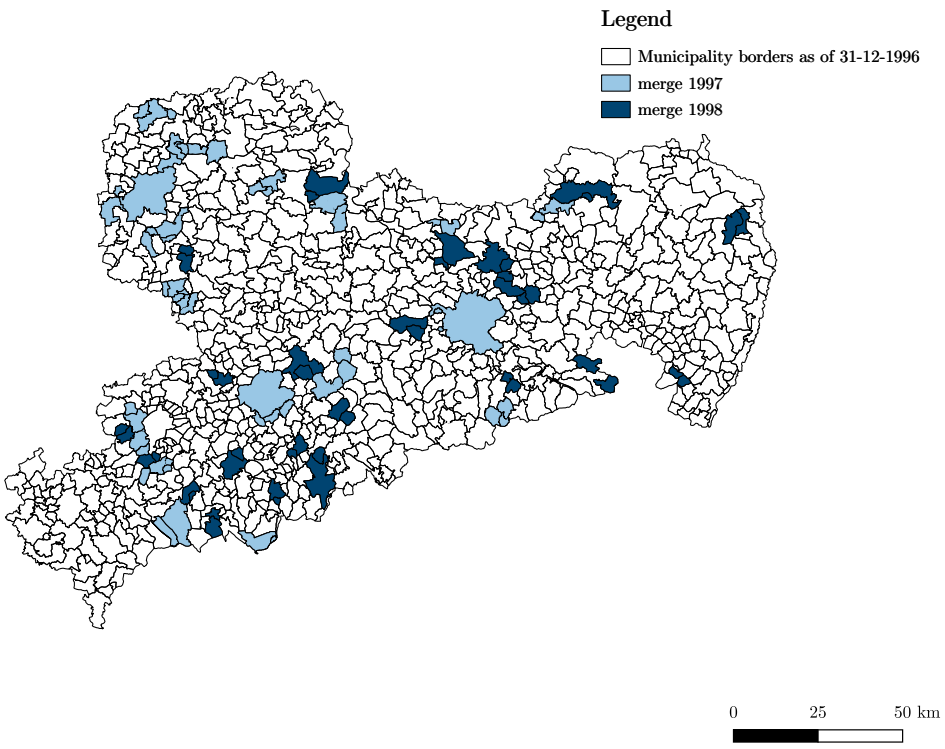
Notes: The table shows the number of potential coalitions resulting from the application of the ISMAGS algorithm to the list of neighboring municipalities 1996. Actual mergers refer to the realized mergers in 1997 and 1998. For each realized merger of coalition size *n*, I randomly sample 15 control coalitions from the set of potential coalitions, i.e. I stratify sampling on coalition size. WESML (weighted exogenous maximum likelihood estimator) weights are derived from the sampling and population proportions of merging and not merging.

equation (4.4) are shown in table 4.2. The most parsimonious specification in column (1) includes only the main variables of interest measuring the merger incentive of fiscal equalization transfers. Columns (2)-(3) add demographic and economic coalition-level controls. In columns (4) and (5), I present results for specifications using restricted samples. First, in an attempt to isolate mergers occurring in rather rural areas, I exclude coalitions including independent cities. Second, against the background that mergers are not temporally independent, I exclude coalitions where all partners had already merged before 1997.

Most importantly, there is no evidence that fiscal equalization transfers played a major role in incentivizing voluntary mergers. I proxy the incentive to merge inherent in the fiscal equalization system with the simulated transfer increase (see section 4.4.2). Furthermore, I include a dummy variable which equals one if the coalition contains a municipality that is located relatively close to the next population bracket of the fiscal equalization scheme. Being close means the municipality needs a theoretical population increase below 6% – this level represents the the 10^{th} percentile in the sample – to cross the next population cut-off. Both coefficients are negative, but mostly do not turn out statistically significant. These findings suggest that the realized mergers fall far behind the potential mergers in terms of maximizing fiscal equalization transfers. Also, proximity to the next population bracket deters mergers. However, this does not necessarily mean that the fiscal equalization system does not incentivize mergers, but that municipalities close to the next population bracket could hope for a natural population increase without having to merge. With the data at hand, however, I cannot distinguish between these two explanations.

The merger probability increases with a coalition's total population suggesting that municipalities are looking for economies of scale through merging. Furthermore, the minimum population within a coalition is negatively associated with merging. This means that a merger is more likely if it includes a small municipality. This is also in line with the results on the number of municipalities below the minimum population size threshold defined by the Saxon state government. The territorial reform targets primarily small municipalities, so intuitively

Figure 4.2: Merging municipalities 1997 and 1998



Notes: The map shows the municipal borders in Saxony as of December 31 1996 (831 municipalities). The shaded areas mark municipalities that merged during the year 1997 (light blue) or 1998 (dark blue).

the merger probability should increase with the number of municipalities that fall below the defined population targets. Although coefficients are positive, they do not reach statistical significance, potentially, because the minimum population variable already captures part of its effect. The cooperation dummy correlates strongly negatively with the merger probability, indicating that cooperative agreements between jurisdictions were viewed as substitutes for territorial reform.

Most economic variables do not influence the merger probability. There is some suggestive evidence that mergers are more likely when the differential in per capita income tax revenues between partners is large. The income tax is raised at the federal level, but municipalities receive a share of revenues on the basis of their inhabitants' income tax payments. Thus, differentials in terms of jurisdiction size between merging partners also carry through to the dimension of local economic activity. Reassuringly, the debt level does not seem to play any role for the merger choice, i.e. it is not the particularly indebted municipalities that choose to

Table 4.2: Binary logit – Choice based sample

	(1)	(2)	(3)	(4)	(5)
Fiscal equalization					
Increase p.c. transfers	-3.16***	-1.95	-1.99	-1.99	-1.90
	(1.21)	(1.40)	(2.02)	(2.08)	(2.05)
Close to next pop. bracket (min)	-0.59	-1.04*	-1.21	-0.69	-1.22
	(0.49)	(0.61)	(0.74)	(0.63)	(0.78)
Demography					
Population (log)		0.53***	0.46***	0.72***	0.44**
		(0.16)	(0.17)	(0.22)	(0.18)
Population (min)		-0.00***	-0.00***	-0.00***	-0.00***
		(0.00)	(0.00)	(0.00)	(0.00)
Cooperation		-0.90*	-1.03*	-1.03*	-1.07*
		(0.54)	(0.57)	(0.59)	(0.56)
Nb. below minimum pop. size		0.18	0.04	0.14	-0.04
		(0.30)	(0.30)	(0.33)	(0.31)
Economy					
Δ Income tax			0.04*	0.04**	0.03
			(0.02)	(0.02)	(0.02)
Δ Business tax			0.00	0.00	0.00
			(0.00)	(0.00)	(0.00)
Δ Property tax (agricultural prop.)			-0.04	-0.06	-0.03
			(0.03)	(0.04)	(0.04)
Δ Property tax (general prop.)			-0.01	-0.01	-0.01
			(0.01)	(0.01)	(0.01)
Δ Debt			-0.00	-0.00	-0.00
			(0.00)	(0.00)	(0.00)
Δ Staff			21.66	33.12**	20.29
			(18.53)	(16.73)	(19.03)
Coalition-size FE	Yes	Yes	Yes	Yes	Yes
W/o cities				Yes	Yes
W/o mergers pre-1997					Yes
Pseudo R^2	0.05	0.13	0.16	0.16	0.14
Observations	735	735	735	675	548

Notes: The table shows the results of five logit estimations. The dependent variable *merge* equals one if a group of municipalities merges in the year 1997 or 1998 and zero otherwise. The 735 realized and (sampled) potential coalitions (territorial status as of December 31 1996) in the years 1997 and 1998 are the observation units. All specifications include coalition-size fixed effects. All economic variables are measured in per capita values and Δ refers to the difference between the minimum and maximum value within the coalition. In column (4), I exclude all coalitions that include a district-free city and in column (6) I exclude coalitions consisting only of municipalities that had already merged before 1997.

merge in order to free-ride on the future common pool of taxes. The administrative capacity proxied by the difference in municipal staff seems to have a positive association with the merger probability.

4.4.2 Consequences of mergers

Data

The main part of the empirical analysis is dedicated to the fiscal consequences of municipal mergers. To compare municipalities across merger waves, I need to construct a dataset of spatially constant units. I use data provided by the Saxon Statistical Office which is aggregated to administrative boundaries as of January 1 2019. That means all municipalities that would eventually merge are aggregated to their post-reform size.

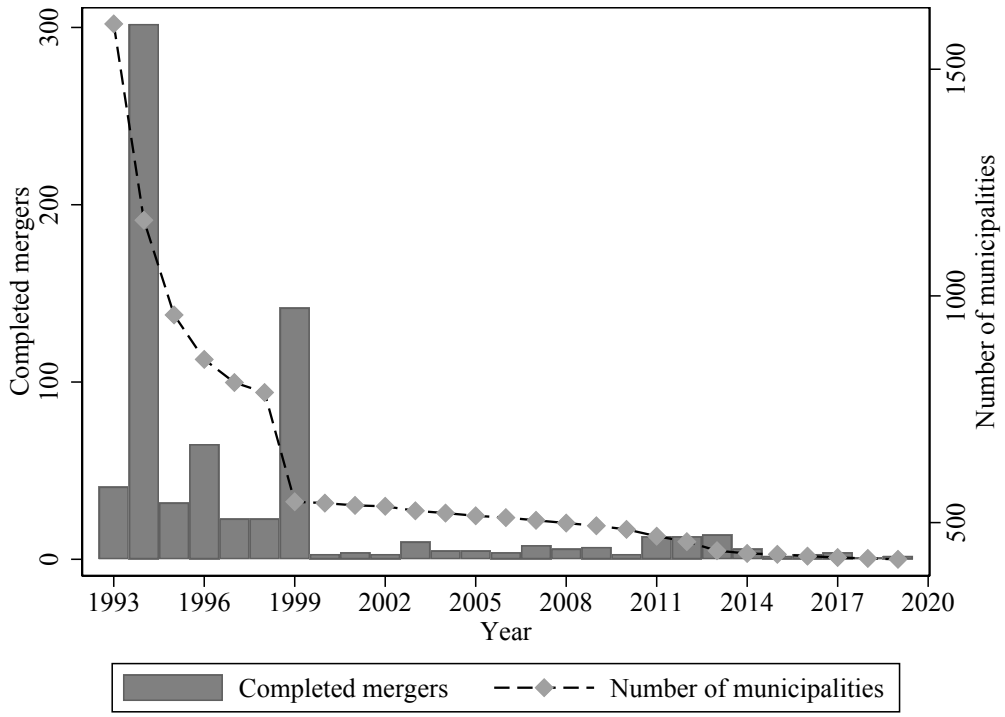
The aggregated dataset covers 419 municipalities over the period 1992-2010. However, I restrict the sample in several ways. First, I exclude municipalities that merged between 2010 and 2019 to avoid a mismatch between the end of my sample period and the territorial status the data reflects. Second, the analysis focuses on district municipalities. The three independent cities – Chemnitz, Dresden, and Leipzig – are discarded from the sample, since their tasks and responsibilities are more far-reaching than those of district municipalities.

The local level in Saxony was subject to many administrative changes (see figure 4.3). Over the course of 28 years, the number of municipalities decreased by 74% from 1,614 in 1992 to 419 in 2019. The two largest merger waves took place in 1994 (voluntary) and 1999 (compulsory). On average, a municipality – as of 2019 – was involved in 2.7 mergers. 74 municipalities did not merge, 164 merged only once and 75 merged twice (see table A4.1 in the appendix).

Mergers differ regarding their (in)voluntary nature, but also with respect to the legal independence of the participating municipalities. The most common merger types in Saxony are: incorporations (*Eingliederung*), equal mergers (*Zusammenschluss*), and new formations (*Neubildung*) (see table A4.2 in the appendix).[13] In the case of incorporations, municipality A ("incorporating") absorbs its merging partner municipality B ("incorporated") which loses its legal independence. Since municipality A in this case is typically a city, i.e. the dominant partner in terms of population and fiscal capacity, I distinguish between the incorporating and incorporated municipalities. When two municipalities form a new administrative unit as equal partners, it is defined as equal merger. The new administrative unit gets a new name. A new formation is identical to an equal merger with the additional option that the new administrative unit keeps the name of one of the merging partners. Most of the mergers occurring between 1994 and 1999 are incorporations (see figure 4.4). In the biggest merger waves 1994 and 1999, this type of merger accounted for 57.2% and 71.7% of the total territorial changes. New formations play a minor role. In voluntary reforms, between 1.4% and 17.3% are new formations, whereas the corresponding share in 1999 is only 3.1%.

[13] In the analysis, I focus on the most prominent merger types and will therefore exclude name changes, since they have no territorial consequences, as well as reclassifications, since they only play a minor role.

Figure 4.3: Local territorial reforms in Saxony

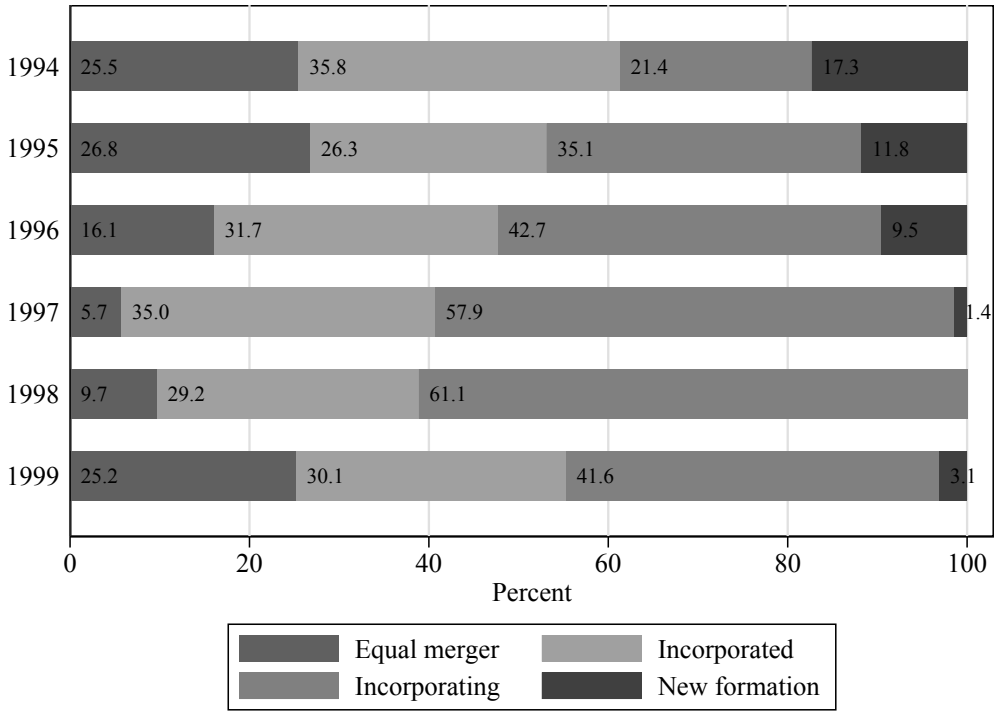


Notes: The figure shows the number of completed municipality mergers (bars, left axis) and the number of resulting municipalities (connected line, right axis) in Saxony over the period 1993 to 2020.

The aggregated data include detailed information on municipal revenues and expenditures in general or by policy area as main variables of interest. A description of the expenditure categories and policy areas is provided in tables A4.3 and A4.4 in the appendix. All monetary variables are in per capita values and in constant 2015 prices. As control variables, I use demographic, economic, and political characteristics of a municipality such as population, population density, the share of young and old, employment, and the share of left-wing parties in the local council. Table 4.3 shows descriptive statistics of variables used in the empirical analysis.

On average, municipalities over the sample period 1992-2010 receive €305 per capita in key allocations per year. Transfers from the state are an important source of revenue for the local level, contributing nearly one fifth to municipal revenues. The year-on-year change in fiscal transfers is 6%. In merger-years, however, fiscal transfers increase by around 9%. The distribution of per capita changes in fiscal transfers also varies substantially between merger and non-merger years, as figure A4.1 shows. The distribution for merger years is shifted to the right and has more mass in the right tail. Furthermore, merger-years exhibit much less variation in per capita transfers than other years.

Figure 4.4: Merger types 1994-1999



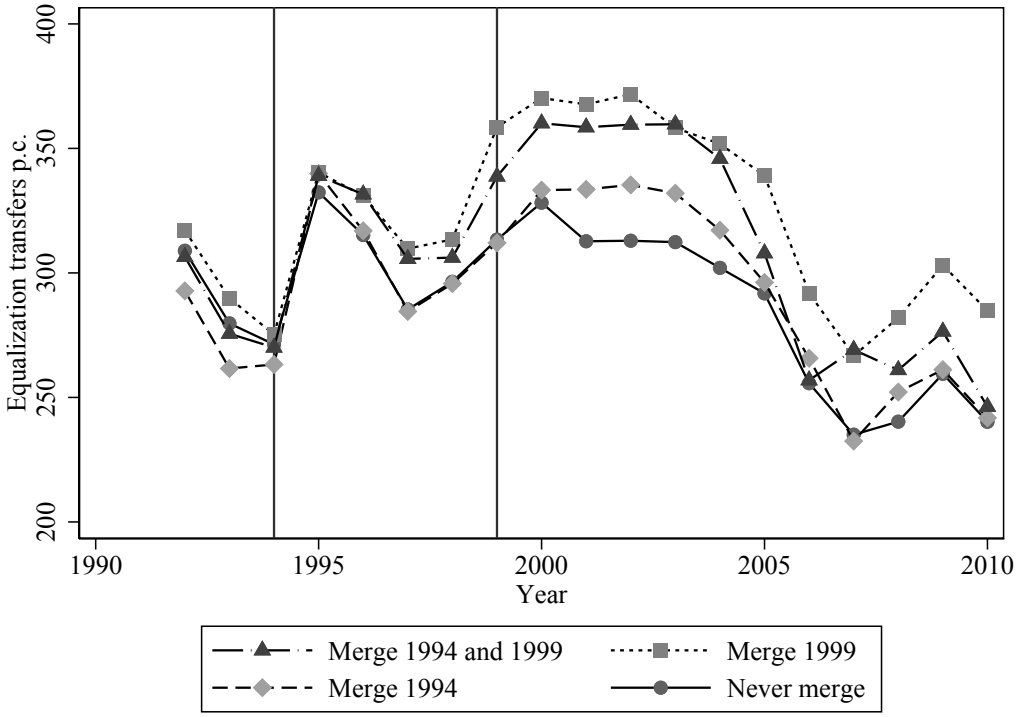
Notes: The figure shows the relative number of Saxon municipalities involved in different merger types by year. The merger types include incorporations, equal mergers, and new formations. I exclude partial reorganizations and spin-offs of territorial units, pure area reorganizations and name changes. For incorporations, I differentiate between the incorporating and incorporated municipalities.

Figure 4.5 shows the development of per capita transfers over the sample period 1992-2010 for different groups of municipalities. For the ease of interpretation, I focus on municipalities that merged during the largest merger waves in 1994 and 1999 and the control group of municipalities that never merged. The effect of increasing per capita transfers is immediate in the respective merger year (vertical line), because transfers are adjusted for the administrative boundaries of January 1 of the equalization year. In 1994, the increase in per capita transfers is only moderate, whereas in 1999, there is a sharp increase widening the gap between merging and non-merging jurisdictions. The development of transfers across municipality groups in non-merger years follows largely parallel trends which is reassuring for the empirical analysis.

Empirical strategy

Given the progressive relationship between a municipality's population and the scaling factor used to determine the fiscal need, transfer gains from mergers can be potentially large. Consequently, a particular focus lies on the estimation of the merger effect on fiscal equalization transfers. Furthermore, I investigate how mergers affect revenue and expenditure categories of the municipal budget. In particular, I want to test whether mergers increase

Figure 4.5: Development of per capita transfers by merger groups



Notes: The figure shows the mean values of per capita transfers for four groups of municipalities: those that merged in 1994 and 1999 (*N* = 16), those that merged only in 1994 or only in 1999 (*N* = 65 and 38, respectively) and the control group of municipalities that never merged (*N* = 75). Vertical lines mark the years of the two largest merger waves in 1994 and 1999. The 416 municipalities of Saxony (territorial status as of January 1 2019) over the period 1992-2010 are the observation units.

municipalities' tax capacity. If economies of scale are realized, expenditures should decrease in the wake of a merger. At the same time, gains in fiscal equalization transfers could widen the scope for expansionary fiscal policies.

The analysis is based on data aggregated to post-reform boundaries as of January 1 2019, as described in section 4.4.2. I employ generalized difference-in-differences specifications (DiD) to estimate the average effect of the changes in jurisdiction size on local transfers and expenditures for the treated units, i.e. the average treatment effect for the treated (ATT). The DiD-estimator is obtained from the following regression:

$$y_{it} = \alpha_i + \beta treat_{it} + X'_{it}\lambda + \delta_t + \epsilon_{it}, \tag{4.5}$$

where $treat_{it}$ is the main variable of interest. It is coded as an interaction between two dummy variables $merge_{it}$ and $post_{it}$, where $merge_{it}$ is one for a municipality i that merges in year t and zero otherwise and $post_{it}$ is one in all years after the merger and zero otherwise. As shown in table A4.1, municipalities as of 2019 have often undergone several mergers so that $post_{it}$ is not unambiguous. In the baseline estimations, $post_{it}$ is coded as one in the years after a

municipality's first merger. To control for time-invariant potentially unobserved municipality characteristics, I include municipality specific fixed effects α_i. Furthermore, δ_t controls for time-varying shocks that affect all municipalities – like adjustments in the overall transfers redistributed by the state – and ϵ_{it} is the error term.

The main identification assumption underlying equation (4.5) is that treatment and control municipalities would have had parallel trends in outcomes in the absence of treatment. To test whether this assumption holds, I estimate event study specifications:

$$y_{it} = \alpha_i + \sum_{j=c}^{C} \beta_j treat_{it}^{j} + \delta_t + \epsilon_{it} \tag{4.6}$$

Compared to equation (4.5), the dummy $treat_{it}$ is replaced by a vector of dummies measuring a symmetric time window around the merger. The coefficients of interest are described by $\sum_{j=c}^{C} \beta_j$. The variable $treat_{it}^{j}$ equals one if a municipality i merges in $(t+j)$ years and zero otherwise. I include five dummies measuring the years before a municipality merges (-5 *and less* to -1) and five dummies measuring the years after a municipality merges (1 to 5 *and more*). The year before the merger serves as base category. Therefore, j ranges from $c = -5$ *and less* to $C = +5$ *and more*, excluding -1 (base category). Event studies are a suitable tool to test the common trend assumption and to deliver a more detailed picture of the merger effects over time.

Another main assumption for a causal interpretation of the results is an exogenous source of variation. Municipalities could sort into treatment based on observable or unobservable characteristics that are correlated with the fiscal outcomes on the left-hand-side of equations (4.5) and (4.6). For example, if voluntary mergers were incentivized by the fiscal equalization scheme, then estimating the merger effect on fiscal transfers would suffer from reverse causality. I address these problems in several ways: first, I model the merger probability via logit estimations to rule out sorting into treatment based on observables.[14] Second, the difference-in-differences design controls for all time-invariant and potentially unobserved characteristics that could be correlated with whether a municipality underwent a territorial reform. Third, the assumption of exogeneity is more plausible for compulsory mergers, where the two-sided decision process is undermined. Even municipalities that were unwilling to merge were forced to do so during the 1999 merger reform enacted by the Saxon state. Naturally, this decision process by the upper-level government relied much less on location-specific characteristics.

[14] Of course, there still remain driving factors that are immeasurable with the data at hand, like how much a municipality values its political independence. This might also change during the consecutive merger waves and would thus not be absorbed by the fixed effects.

Results

Quantifying the "mechanical" transfer increase

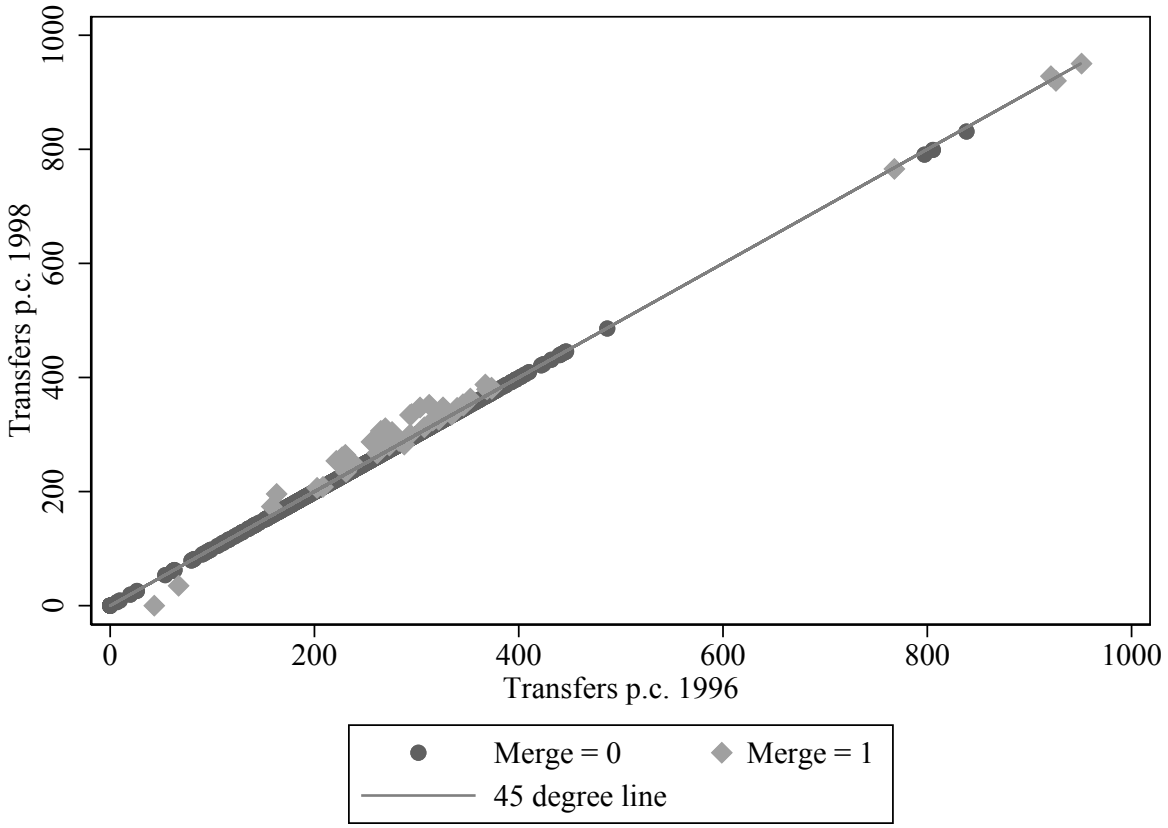
The fiscal equalization transfers I study are formula-based. So naturally, one part of the estimated effect of mergers on transfers is a mechanical increase due to the institutional design of the equalization scheme. As outlined in section 4.3.1, if a group of municipalities reaches a higher population bracket by merging, the coalition-level scaling factor will exceed pre-reform individual scaling factors. At the same time, this aggregate increase in fiscal need reduces the basic amount for all jurisdictions, dampening the potential revenue gains from mergers. To quantify these effects, I simulate pre- and post-reform transfer levels taking the fiscal equalization system 1996 as given. Instead of treating each merger as a single event – as I have done in section 4.4.1 for every realized and potential merger –, I simulate equalization transfers for the territorial status at the end of 1998. The analysis necessarily abstracts from changes in the equalization parameters – scaling factors remained constant between 1996 and 1998, though – and treats all mergers in the consecutive two years as if they happened in 1997. Overall, I can map realized transfers between 1996 and 1998 reasonably well.

Figure 4.6 shows the simulated transfers before and after territorial reform graphically. Several interesting patterns emerge. Municipalities that did not choose to merge display a nearly perfect fit with the 45 degree line, suggesting that their pre- and post-reform transfer levels are quite comparable. Summary statistics confirm that this group faced an average decrease in per capita transfers by negligible 0.24%. This decrease is entirely driven by the downward adjustment of the endogenous basic amount which falls from €425.06 to €424.51, since all economic and demographic characteristics that enter the equalization formula remain unchanged for non-merging municipalities.

Unsurprisingly, merging municipalities exhibit more variation in their transfer growth, with the majority of 85% reaching higher per capita transfer levels post-reform. On average, merging municipalities gained 2.5% in per capita transfers. Interestingly, the losing coalitions of the reform group can be found at the extremes of the per capita transfer distribution. There are two explanations for this phenomenon: first, if one merging partner is abundant, i.e. receives no transfers via the fiscal equalization scheme, the coalition-level tax capacity probably overcompensates the higher fiscal need. Second, mergers including independent cities are special, since the basic amount is calculated separately within this group of jurisdictions.[15] Changes in the fiscal need of a district-free city lead to potentially large adjustments in the basic amount, because transfer volumes are split between only a handful of jurisdictions. Moreover, for district-free cities constant scaling factors apply. Accordingly, of the four cities involved in mergers, only Chemnitz benefited in terms of per capita transfers with a moderate

[15] The Saxon state redistributes separate transfer volumes depending on a jurisdiction's status as district municipality or district-free city.

Figure 4.6: Simulated transfers before and after territorial reform (1996)



Notes: The graph shows simulated per capita transfers based on the 1996 fiscal equalization scheme pre- (x-axis) and post-reform (y-axis). Transfers 1996 are calculated at the territorial status of January 1 1996, while transfers 1998 refer to simulated transfers based on the territorial status of December 31 1998. Circles represent municipalities that did not merge in 1997 or 1998 and diamonds refer to municipalities that merged during this period.

increase from €921 to €928 per capita or 0.8%. As hypothesized, the fall in the basic amount – €1,080.76 vs. €1,073.57 – rendered territorial reforms a zero-sum-game for the remaining cities.

Of course, this analysis can only provide a snapshot of the "mechanical" increase in per capita transfers based on the institutions and economic fundamentals in 1996 and a year with 46 mergers. However, results are informative for the empirical analysis and conclusions to follow. First and foremost, the simulations suggest a rather moderate transfer gain for reform municipalities. Moreover, seven (of 46) coalitions even lost transfers through merging. For the realized merger waves, I would expect even larger heterogeneities across both merging and non-merging municipalities. Especially with regard to economic activity, I present estimates of a status quo where tax capacity of the new administrative unit simply reflects the sum of individual tax capacities pre-reform. As argued in section 4.2, this assumption does not have to hold in reality (Egger *et al.*, 2021).

Revenues

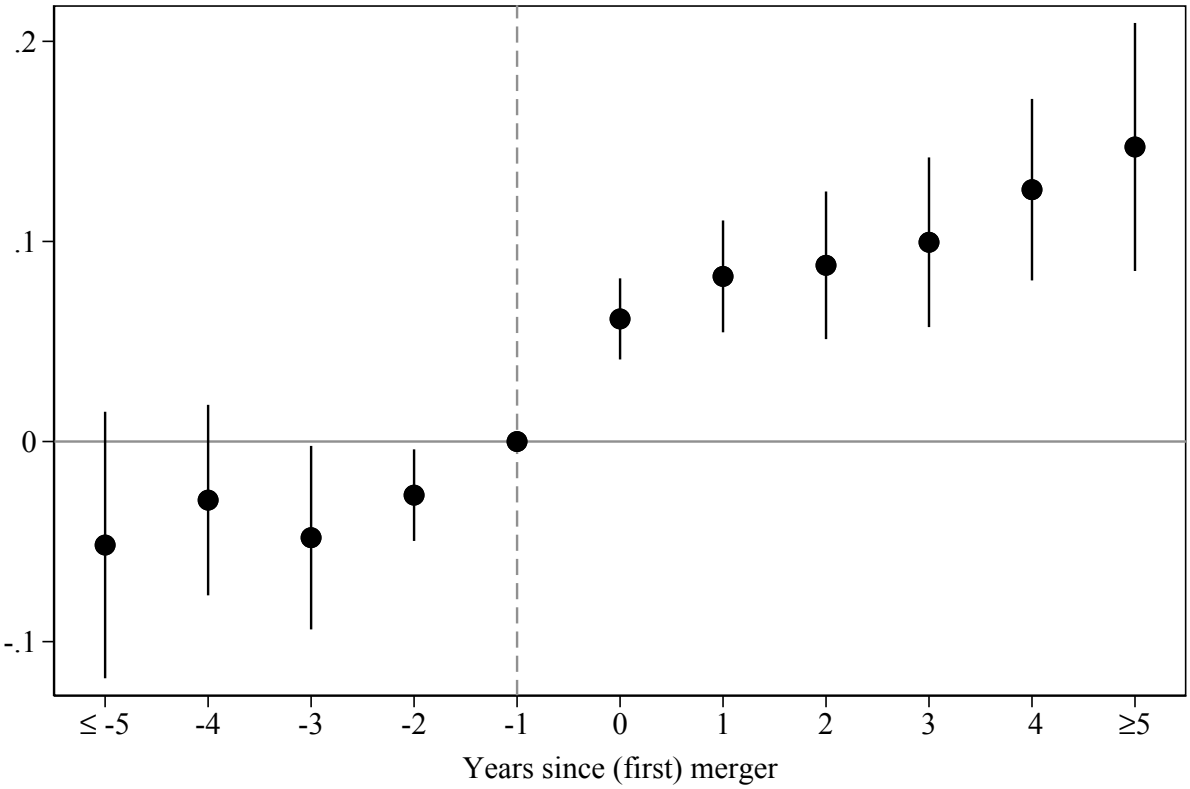
Table 4.4 shows difference-in-differences results for the equalization transfers. When moving from column (1) to column (4) of table 4.4, the respective specifications get consecutively more rigorous. The baseline specification includes only municipality and year fixed effects. In column (2), the following socio-demographic controls are added: population (log), population density, and the share of young (under 25 years) and old (over 65 years). In column (3), I include a municipality-specific linear time trend to account for the fact that treatment and control group might follow different revenue trajectories. In column (4), I further add the variables net-inflow of people to control for population dynamics and the number of pupils (log) by different school type which constitute the second main determinant of fiscal transfers since 1995. Note that this last addition of control variables reduces the sample to the period from 1995 onward due to data availability. Thus, identifying variation in this setting stems only from municipalities that merged after 1995. Event study results are presented in figure 4.7 and rely on the specification in column (3) of table 4.4.

The estimation results across both methods point to a positive merger effect on fiscal equalization transfers. In the wake of territorial reforms, per capita transfers increase by around 11% in merged municipalities compared to the control group of municipalities that did not merge. This effect is statistically significant and economically sizable. For the average municipality receiving around €305 per capita in yearly transfers, a merger results into an estimated gain of €34 per capita in transfer payments. What is more, this revenue gain is persistent over time (see figure 4.7). Even up to five years after their first merger, treated municipalities benefit from an upward shift in their transfer level. Large-scale merger reforms thus have considerable redistributive effects on fiscal equalization transfers.

I submit these baseline results to a battery of robustness tests in table 4.5. All robustness tests rely on the baseline specification as presented in column (3) of table 4.4 in terms of the included control variables. First, I omit municipalities that underwent multiple mergers during my sample period, since they likely introduce some noise into the estimation. Second, I employ a more balanced event window around the merger year. To render the pre- and post-reform periods more comparable, I exclude all municipalities with less than three pre- or post-treatment years.[16] Third, I re-run the baseline specification on a winsorized sample to minimize the influence of outliers on the estimation results. Before applying the logarithm, the tail areas of the per capita transfer distribution are set to the 1^{st} and 99^{th} percentile. Fourth, I use an alternative definition of the treatment variable. Instead of letting the first merger define the treatment status, the post-merger municipality is now coded as one in all years after the last merger of the municipality had been completed. As the results in table 4.5 show, the estimated coefficient of per capita transfers is fairly robust to the sample restrictions in columns (1) and (2). I now estimate a 13% increase in transfer payments in the wake of territorial reform, which is quite close to the baseline effect of 11%. Moreover, as

[16] Note that this robustness test omits the 1994 merger wave from the sample.

Figure 4.7: Event study – Fiscal transfers



Notes: The figure shows the results of an event study estimation, where the dependent variable is per capita transfers (log). The vertical dashed line represents the year of the first merger of a municipality. The 360 municipalities of Saxony (territorial status as of January 1 2019) over the period 1992-2010 are the observation units; for sample restrictions see section 4.4.2. Bullets are point estimates and black lines represent the 90% confidence interval. I include year and municipality fixed effects and control variables. Standard errors are robust to heteroskedasticity.

column (3) shows, the baseline results are apparently not driven by extreme values in transfer payments. The estimated coefficient in column (4) – around 9% – relying on the alternative treatment definition can be regarded as a lower bound for the merger effect on fiscal transfers. Municipalities that merge multiple times are part of the control group up until the year of their last merger, such that the control mean is inflated by their potentially higher transfer levels after earlier mergers.

Given the redistributive effects of territorial reform within the rule-based equalization system, the Saxon state government could have tried to compensate non-merging municipalities by other means. In principle municipalities can generate revenues from two other types of state-level transfers that are more discretionary: specific needs transfers and general transfers. Specific needs transfers are designed to compensate municipalities with special financial needs situations, for instance as a result of urgently required budget consolidation measures. General transfers are non-earmarked transfers within the equalization framework. Table A4.5

in the appendix shows results for the difference-in-differences specifications with these two types of transfers as the dependent variables. The evidence does not support the hypothesis of a substitution effect. In fact, the discretionary transfers do not differ between merging and non-merging municipalities.

Similarly, I repeat the analysis for overall municipal revenues and tax revenues. Results are displayed in table 4.6 and show that transfer gains do not translate into higher total revenues for reform municipalities (panel A). Coefficient estimates are very close to zero suggesting that total per capita revenues do not differ between treatment and control group. Moreover, merging municipalities do not succeed in attracting higher tax revenues. Although coefficients for per capita tax revenues in panel B are positive, they do not turn out statistically significant.

Expenditures

For municipal expenditures, results are less clear-cut (see table 4.7, panel A). Most notably, mergers seem to reduce total administrative costs by around 3% (column 1), an effect that is also confirmed by the corresponding event study plot in figure A4.2. Spending on the administrative budget accounts for 65% of total municipal spending, i.e. this expenditure position is economically important. Against the mean value of €1,176 spent per capita, the estimated savings correspond to €35 per capita. From columns (2)-(5), it remains unclear which sub-category is driving this aggregate result. For staff expenditures, the estimated coefficient is even positive although it does not turn out to be statistically significant. The capital budget makes up 35% of total expenditures and contains all expenditures that increase the assets of a municipality, i.e. are considered investments under budgetary law. Results in columns (6)-(10) suggest zero effects of mergers on aggregate capital spending and the considered sub-categories. These findings remain largely unchanged if I control for fiscal equalization transfers in panel B. Large revenue gains could have feedback effects on municipal expenditures when reform municipalities use additional transfers in areas where economies of scale would be expected. However, this does not seem to be the case. Coefficient estimates between both specifications are quite comparable and the negative effect on total administrative expenditures persists.

So far I have differentiated expenditures solely by the purpose of the expense, i.e. whether the money is spent on staff, material, grants etc. In the local accounting system, expenditures (as well as revenues) can further be characterized by the policy area within which they occur – such as general administration or social security, see table A4.4. Expenditure shares vary substantially between policy areas (table A4.6). While the general administration and social security areas spend most on staff, material expenses dominate in the areas of municipal companies and assets, health, and schooling. Unsurprisingly, expenditures for construction measures play the largest role for the building, housing, and infrastructure area.

To investigate whether expenditure effects differ between policy areas, I re-run the estimations including baseline controls and the municipality-specific linear time trend for five policy areas: administration, building, public institutions and business development, schools, and social security. These policy areas are quite representative for a municipality's voluntary and compulsory tasks. Results are displayed in table 4.8. Generally, I only observe merger effects on operating expenses (administrative budget) across policy areas, while the investment expenditures (capital budget) are unaffected. After the merger, the general administration spends 12% more on staff and 18% per capita less on material. In the area of building, housing and infrastructure, the increase in staff expenditures is even more pronounced (23%). Moreover, total expenditures for schools increase in merging municipalities.

Table 4.7: Expenditures

	Administrative budget					Capital budget				
	(1) Total	(2) Staff	(3) Material	(4) Grants	(5) Other	(6) Total	(7) Assets	(8) Building	(9) Debt	(10) Other
Panel A: Disregarding transfers										
Treat	-0.03**	0.04	-0.04	0.01	-0.02	0.00	0.04	0.08	0.03	-0.15
	(0.01)	(0.03)	(0.02)	(0.08)	(0.02)	(0.04)	(0.07)	(0.06)	(0.10)	(0.09)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls+trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Exp. share	64.58	22.35	18.29	4.44	19.53	35.42	2.84	21.34	5.86	5.00
Observations	6,840	6,840	6,840	6,782	6,840	6,840	6,799	6,834	6,382	5,597
Panel B: Controlling for transfers										
Treat	-0.03**	0.04	-0.03	-0.01	-0.03	0.01	0.04	0.08	0.02	-0.21**
	(0.01)	(0.03)	(0.02)	(0.08)	(0.02)	(0.04)	(0.06)	(0.06)	(0.10)	(0.09)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls+trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,714	6,714	6,714	6,656	6,714	6,714	6,673	6,709	6,262	5,522

Notes: Each panel shows the results of ten difference-in-differences estimations, where the dependent variables are different per capita expenditures (log); for a description of expenditure categories see table A4.3. The main variable of interest (Treat) takes on the value of one in the years after the first merger of a municipality. The 360 municipalities of Saxony (territorial status as of January 1 2019) over the period 1992-2010 are the observation units; for sample restrictions see section 4.4.2. In panel A, the set of control variables is identical to column (3) of table 4.4. In panel B, I additionally control for fiscal transfers per capita (log). Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

Heterogeneity

Heterogeneity of merger effects can occur across many dimensions in the treatment. Most importantly, I distinguish between voluntary and compulsory mergers. Similarly to Blesse and Baskaran (2016), my sample offers the advantage to investigate both types of mergers within the same institutional setting. However, multiple mergers are potential confounders, when one municipality merges both under the voluntary and compulsory regime. Therefore, I exclude municipalities that merged more than once between 1992-2010 so that every merger can be unambiguously classified into voluntary vs. compulsory.

Table 4.9 shows results for fiscal transfers (following the preferred specification of column 3 table 4.4). Results in column (1) rely on the whole sample and are estimated using separate treatment effects for voluntary and compulsory mergers. Columns (2) and (3) restrict the treatment group to municipalities that merged during the largest voluntary and the compulsory merger wave, in 1994 and 1999, respectively. Across all merger waves, the increase in fiscal transfers is quite comparable between voluntary and compulsory mergers (column 1), ranging between 10-12%. A t-test reveals that I cannot reject the equality hypothesis of both coefficients. When estimating the treatment effect separately for 1994 and 1999, however, the coefficient for 1999 turns out larger. In the wake of the compulsory 1999 reform, merging municipalities gained on average 17% in per capita transfers, compared to 12% for coalitions forming during the 1994 merger wave.

Table A4.7 repeats the analysis for expenditures. Coefficient estimates for compulsory compared to voluntary mergers mostly bear similar signs suggesting that spending effects across both regimes go into the same direction. One notable exception are building expenditures which increase significantly only after compulsory mergers. The increase amounts to 20%, which is sizable against a mean value of €321 per capita for building. The average negative effect of mergers on total administrative expenditures is driven by voluntary mergers, where estimates range between -5% and -8%.

How do municipalities spend the additional equalization transfers they gain from mergers? To answer this question, I conduct another heterogeneity test by including an interaction term between the treatment indicator and a dummy variable indicating whether reform municipalities reaped relatively large transfer gains by merging. The cut-off value between high vs. low transfer gains is defined as the per capita transfer increase at the 75^{th} percentile (6.9%) in merger years. Again, I only include municipalities that merged once to avoid the problem of multiple treatments. Table 4.10 shows the results. On the one hand, it seems that municipalities with high transfer gains spend their additional funds on staff: the positive expenditures differential between this group of municipalities and the control group amounts to 14%. On the other hand, the same municipalities economize on material expenditures. Municipalities with moderate transfer gains have lower total per capita administrative expenditures than the control group (-4%).

4.5 Discussion

Territorial reforms of local administrative units are often motivated by the realization of economies of scale. At the same time, the fiscal equalization systems of the German states "reward" larger municipalities with disproportionately higher grants. Although this institutional design provides clear incentives for mergers, Saxon municipalities did not choose their partners to maximize fiscal equalization transfers.

Nevertheless, the large-scale territorial reforms gave rise to a significant reallocation of fiscal transfers. Merging municipalities gained on average 11% in per capita transfers. This is quite sizable against the simulated benchmark of a mechanical increase of roughly 2%. I propose several explanations for the discrepancy: first, the simulation relied on a year with 46 mergers which is hardly representative for the encompassing merger waves in 1994 and 1999 where larger reallocation would be expected. Second, as the results for tax revenues suggest, merging municipalities did not succeed in attracting more economic activity post-reform. This effect would have worked against the transfer increase, especially in the long-run. Moreover, if merging municipalities collectively adjust their tax multipliers downwards, as shown for Swiss municipalities in Studerus (2016), they could drive state-wide average tax rates and as a result also tax capacities down. For reform municipalities, a disproportionately higher fiscal need would then be compared to a lower tax capacity intensifying the mechanical population-based transfer increase.

Empirical work on the fiscal consequences of municipal mergers has failed to corroborate the hypothesis of sizable economies of scale in many different settings. Also the evidence in my study warrants caution for policymakers who expect large cost-savings post-reform. For Saxony, the average estimated cost savings in total administrative expenditures amount to a moderate 3%. These cost savings seem to be realized primarily under the voluntary merger regime. During the encompassing reform wave in 1994, effect sizes for total administrative expenditures – similar to estimated cost reductions in Reingewertz (2012) and Blom-Hansen *et al.* (2014) – reach up to -8%. However, this differential effect might be subscribed to the timing rather than the nature of the reform, since a location's possibility to realize economies of scale decreases with its size. It is conceivable that the potential overall cost benefits from mergers were highest at the beginning of the Saxon reform process, i.e. when municipalities were still extremely small and mergers were still voluntary.

Finally, contrary to the empirical evidence on the flypaper effect (see e.g., Dahlberg *et al.*, 2008; Helm and Stuhler, 2021), the large transfer increases induced by the territorial reform do not increase municipal spending. If at all, high transfer recipient jurisdictions spend more on municipal staff, a purely consumptive function, while I do not observe any systematic increase of local investments. One possible interpretation put forward by Hailemariam and Dzhumashev (2019) is that municipal governments anticipate reduced fiscal entitlements in the future when funds are channeled towards more productive uses that boost local tax bases.

4.6 Conclusion

Mergers are often administered by upper-level governments to increase efficiency and realize economies of scale. At the same time, some municipal fiscal equalization systems "reward" larger or more densely populated jurisdictions based on the assumption of rising per capita administrative costs with increasing municipality size. Although both views have their theoretical foundation, territorial reforms can give rise to a reallocation of transfers within the fiscal equalization system. In this paper, I focus on the consequences of mergers on fiscal transfers, a question that has not been studied so far.

As a testing ground, I use large-scale merger waves in the East German state of Saxony. Although theoretically, mergers should reduce costs, the empirical evidence casts considerable doubt on this (Allers and Geertsema, 2016; Blesse and Baskaran, 2016; Blom-Hansen *et al.*, 2016; Roesel, 2017). Realizing economies of scale was also the explicit goal of the Saxon state government in the 1990s. However, the state did not want to interfere with the newly restored self-government of municipalities after German reunification. Thus, until 1998, municipal territorial reform took place exclusively on a voluntary basis. Between October 3 1990 and October 27 1998, the number of Saxon municipalities had been reduced from 1,626 to 773.

While nearly all OECD countries rely on fiscal equalization schemes to reduce sub-national fiscal disparities, some of them include adjustments for population size. All 13 territorial German states for example grant disproportionately higher grants to larger municipalities. In Saxony, the relationship between municipalities' population counts and the weight they receive in calculating fiscal need is especially progressive. To test whether gains in fiscal equalization transfers incentivize voluntary mergers, I simulate the expected increase in post-merger transfer levels. The analysis is conducted at the coalition level to abstract from two-sided decision making and compares realized to potential mergers. Results suggest that municipalities do not strategically choose merging partners to maximize transfer levels.

Large merger waves – as they have taken place in many industrialized countries – give rise to a significant redistribution of public funds within the equalization scheme. In Saxony, merging municipalities benefited from a significant and persistent increase in per capita equalization transfers. They received up to €34 additional per capita transfers compared to the control group of municipalities that never merged. This effect amounts to 11% of the mean value of per capita transfers in the sample, rendering it economically sizable. Although one might expect a corresponding systematic change in municipal expenditures to this revenue gain, results suggest only moderate increases, which are limited to the operating budget. In particular, municipalities with large transfer gains seem to divert additional funds to staff expenditures, while investive expenditures remain unchanged. Overall, cost-saving effects of municipal territorial reform are moderate: total administrative expenditures and material expenditures decrease by 3% and up to 18%.

My results have important implications from a policy perspective. Particularly, they raise the question of whether jurisdiction size should enter the equalization formula. Although it has been acknowledged that due to their institutional designs, fiscal equalization schemes offer (dis-)incentives to merge (Blöchliger *et al.*, 2007), this paper is the first to empirically test this hypothesis. In systems that compensate their sub-national jurisdictions for the costs of density, mergers artificially inflate fiscal need indicators in reform municipalities. On the one hand, higher equalization payments to larger municipalities can be justified on the grounds of higher per capita costs for (some) public services – like fire protection or public safety. On the other hand, this rationale is in stark contrast to the efficiency arguments often made in favor of territorial reforms. Especially when it comes to capital-intensive local infrastructure, such as energy and transport systems, per capita costs decrease with jurisdiction size. In their current form, the German municipal fiscal equalization systems fail to reconcile these views. Territorial reforms render population-based measures of fiscal need manipulable and can have large and persistent redistributive consequences with respect to equalization transfers. Although the focus of this paper is on municipal budgets, previous literature has shown that fiscal equalization transfers significantly impact regional convergence as well.

Table 4.3: Descriptive statistics

	Observations	Mean	SD	Min	Max
Transfers					
Transfers p.c.	7,777	305.69	92.30	0.74	1,109.71
Transfer revenue share	7,777	0.18	0.07	0.00	0.42
Δ Transfers p.c.	7,313	0.06	2.40	-1.00	194.72
Δ Transfers p.c. (merge = 0)	6,714	0.06	2.50	-1.00	194.72
Δ Transfers p.c. (merge = 1)	599	0.09	0.21	-0.80	1.62
Expenditures p.c. (log)					
Administrative budget					
Total	7,904	7.05	0.23	5.69	8.78
Staff	7,904	5.94	0.48	0.53	7.54
Material	7,904	5.74	0.36	3.59	7.67
Grants	7,846	3.61	1.40	-2.06	6.76
Other	7,904	5.79	0.38	3.13	8.57
Capital budget					
Total	7,904	6.41	0.65	3.55	9.14
Assets	7,863	3.52	1.07	-2.30	7.88
Building	7,898	5.75	0.95	-0.72	8.74
Debt	7,422	3.94	1.42	-2.21	7.80
Other	6,609	3.76	1.58	-2.28	8.17
Control variables					
Population (log)	7,904	8.47	0.89	5.98	11.69
Population density	7,904	205.31	210.79	16.97	1,747.88
Population: share under 25	7,904	25.59	3.71	16.10	41.11
Population: share over 65	7,904	19.56	3.74	8.07	31.81
Further control variables					
Population: net inflow	6,655	-23.32	145.00	-2,165.00	1,234.00
Pupils: primary school (log)	6,655	4.46	2.37	-2.30	9.70
Pupils: middle school (log)	6,655	3.04	3.91	-2.30	8.46
Pupils: secondary school (log)	6,655	-0.47	3.63	-2.30	8.32
Pupils: special needs (log)	6,655	-0.96	2.86	-2.30	6.70

Notes: The table shows descriptive statistics for the variables used in the empirical analysis. The 416 municipalities of Saxony (territorial status as of January 1 2019) over the period 1992-2010 are the observation units. The dummy variable *merge* equals one when a municipality participates in a merger and zero otherwise. Δ refers to yearly growth rates.

Table 4.4: Baseline – Transfers

	Fiscal transfers p.c. (log)			
	(1)	(2)	(3)	(4)
Treat	0.11***	0.12***	0.12***	0.11***
	(0.02)	(0.02)	(0.02)	(0.03)
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes
Linear time trend			Yes	Yes
Further controls				Yes
Observations	6,714	6,714	6,714	5,633

Notes: The table shows the results of four difference-in-differences estimations, where the dependent variable is per capita transfers (log). The main variable of interest (Treat) takes on the value of one in the years after the first merger of a municipality. The 360 municipalities of Saxony (territorial status as of January 1 2019) over the period 1992-2010 are the observation units; for sample restrictions see section 4.4.2. All specifications include year and municipality fixed effects. Control variables are socio-demographic variables, see table 4.3. In column (3), a municipality-specific linear time trend is included. Due to data availability, the sample in column (4) is reduced to the time period 1995-2010. Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

Table 4.5: Robustness – Transfers

	Fiscal transfers p.c. (log)			
	(1)	(2)	(3)	(4)
Treat	0.13***	0.13***	0.10***	0.09***
	(0.03)	(0.03)	(0.02)	(0.02)
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls+trend	Yes	Yes	Yes	Yes
Robustness	Once	± 3	Winsorized	Last
Observations	4,341	3,806	6,714	6,714

Notes: The table shows the results of four difference-in-differences estimations, where the dependent variable is per capita transfers (log). The main variable of interest (Treat) takes on the value of one in the years after the first merger of a municipality. The 360 municipalities of Saxony (territorial status as of January 1 2019) over the period 1992-2010 are the observation units; for sample restrictions see section 4.4.2. The set of control variables is identical to column (3) of table 4.4. In column (1), I exclude municipalities that merged more than once. In column (2), I focus on a more balanced event window around the first merger by excluding municipalities with less than three pre- or post-treatment years. In column (3), the estimation is repeated for a winsorized sample where the highest and lowest values of the dependent variable are set to the 99^{th} and 1^{st} percentile, respectively. Column (4) uses an alternative specification for the main explanatory variable, which now takes on the value of one in the years after the last merger of a municipality. Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

Table 4.6: Revenues

	Revenues p.c. (log)			
	(1)	(2)	(3)	(4)
Panel A: Total revenues				
Treat	-0.01	-0.00	-0.00	0.02
	(0.02)	(0.02)	(0.02)	(0.02)
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes
Linear time trend			Yes	Yes
Further controls				Yes
Observations	6,840	6,840	6,840	5,759
Panel B: Tax revenues				
Treat	0.02	0.02	0.02	0.00
	(0.02)	(0.02)	(0.02)	(0.02)
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls		Yes	Yes	Yes
Linear time trend			Yes	Yes
Further controls				Yes
Observations	6,836	6,836	6,836	5,755

Notes: Each panel shows the results of four difference-in-differences estimations, where the dependent variables are total and tax revenues per capita (log). The main variable of interest (Treat) takes on the value of one in the years after the first merger of a municipality. The 360 municipalities of Saxony (territorial status as of January 1 2019) over the period 1992-2010 are the observation units; for sample restrictions see section 4.4.2. Control variables are socio-demographic variables, see table 4.3. In column (3), a municipality-specific linear time trend is included. Due to data availability, the sample in column (4) is reduced to the time period 1995-2010. Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

Table 4.8: Expenditures by policy area

	Administrative budget				Capital budget		
	(1) Total	(2) Staff	(3) Material	(4) Grants	(5) Total	(6) Assets	(7) Building
Panel A: General administration							
Treat	-0.04	0.12***	-0.18***	-0.11	-0.08	0.03	0.03
	(0.03)	(0.04)	(0.03)	(0.13)	(0.08)	(0.07)	(0.13)
Controls+trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,840	6,840	6,840	2,858	5,801	5,351	2,976
Panel B: Building, housing, infrastructure							
Treat	-0.04	0.23***	0.04	-0.02	-0.08	0.05	-0.06
	(0.05)	(0.09)	(0.04)	(0.31)	(0.08)	(0.14)	(0.08)
Controls+trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,840	5,004	6,833	975	6,769	3,666	6,722
Panel C: Public institutions, business development							
Treat	-0.01	-0.01	-0.05	0.33**	0.03	-0.00	0.20
	(0.08)	(0.09)	(0.08)	(0.15)	(0.12)	(0.10)	(0.13)
Controls+trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,835	6,405	6,819	4,214	6,148	4,854	4,966
Panel D: Schools							
Treat	0.12**	-0.04	0.03	0.10	0.15	0.05	0.11
	(0.05)	(0.05)	(0.04)	(0.19)	(0.10)	(0.08)	(0.11)
Controls+trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,408	6,072	6,359	2,058	5,603	4,594	4,823
Panel E: Social security							
Treat	-0.02	-0.07	-0.05	0.03	-0.10	0.12	-0.12
	(0.03)	(0.09)	(0.08)	(0.12)	(0.11)	(0.10)	(0.11)
Controls+trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,837	6,140	6,782	6,119	5,566	3,518	4,789

Notes: Each panel shows the results of seven difference-in-differences estimations, where the dependent variables are different per capita expenditures (log) by policy area; for a description of expenditure categories and policy areas see tables A4.3 and A4.4. The 360 municipalities of Saxony (territorial status as of January 1 2019) over the period 1992-2010 are the observation units; for sample restrictions see section 4.4.2. The set of control variables is identical to column (3) of table 4.4. Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

Table 4.9: Heterogeneity – Transfers

	Fiscal transfers p.c.		
	(1) All waves	(2) 1994 only	(3) 1999 only
Voluntary merger	0.10***		
	(0.03)		
Compulsory merger	0.12**		
	(0.05)		
Treat		0.12***	0.17***
		(0.04)	(0.06)
Municipality FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls+trend	Yes	Yes	Yes
Observations	4,322	2,573	2,096

Notes: The table shows the results of three difference-in-differences estimations, where the dependent variable is per capita transfers (log). The 232 municipalities of Saxony that merged only once (territorial status as of January 1 2019) over the period 1992-2010 are the observation units. The set of control variables is identical to column (3) of table 4.4. Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

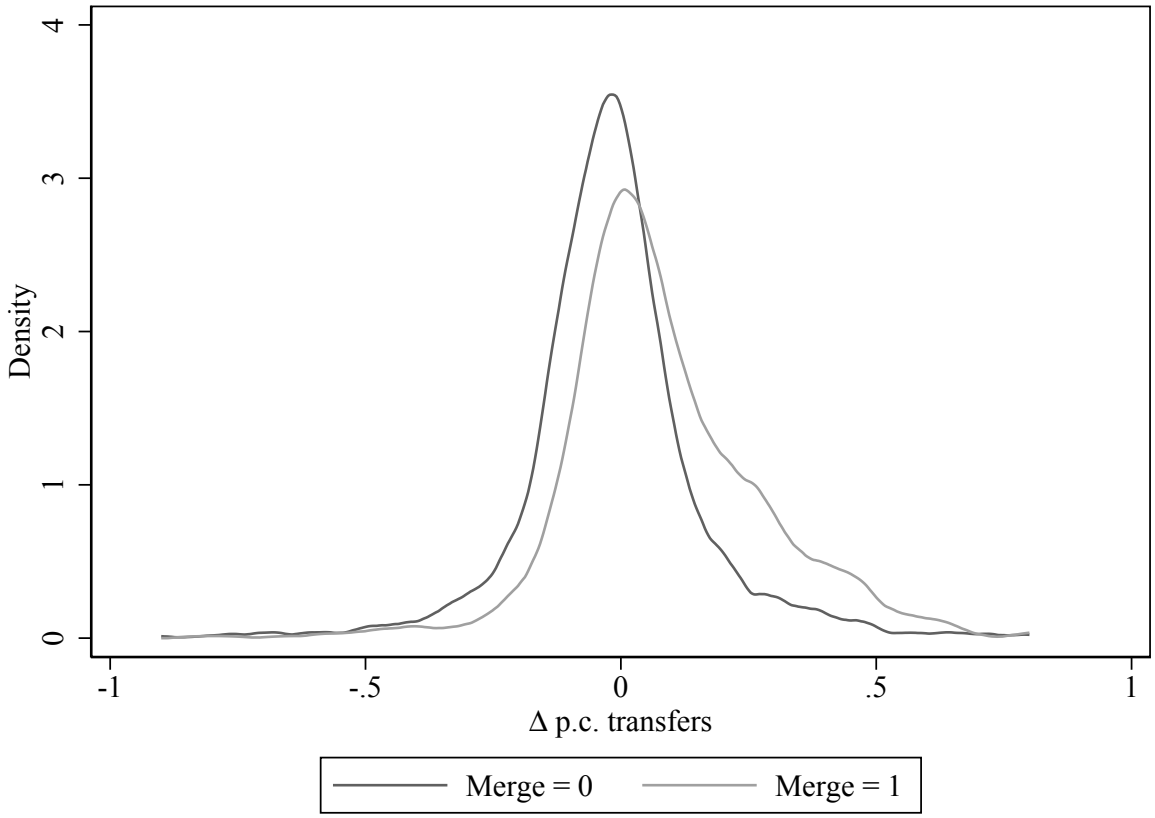
Table 4.10: Expenditures by transfer change intensity

	Administrative budget					Capital budget				
	(1) Total	(2) Staff	(3) Material	(4) Grants	(5) Other	(6) Total	(7) Assets	(8) Building	(9) Debt	(10) Other
Treat × high Δ transfer	-0.02	0.14***	-0.08**	-0.09	-0.02	-0.06	-0.00	-0.06	0.12	-0.08
	(0.02)	(0.05)	(0.04)	(0.17)	(0.05)	(0.07)	(0.13)	(0.11)	(0.20)	(0.18)
Treat × low Δ transfer	-0.04**	0.05	-0.02	-0.15	-0.06*	-0.03	0.15*	0.09	-0.17	-0.25*
	(0.02)	(0.04)	(0.03)	(0.10)	(0.03)	(0.05)	(0.09)	(0.08)	(0.13)	(0.13)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls+trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,408	4,408	4,408	4,357	4,408	4,408	4,372	4,402	4,038	3,507

Notes: Each panel shows the results of ten difference-in-differences estimations, where the dependent variables are different per capita expenditures (log); for a description of expenditure categories see table A4.3. The 360 municipalities of Saxony (territorial status as of January 1 2019) over the period 1992-2010 are the observation units; for sample restrictions see section 4.4.2. The set of control variables is identical to column (3) of table 4.4. I interact the main variable of interest (Treat) with a dummy variable for high (low) transfer growth. High/low transfer growth rates are defined as values above/below the 75^{th} percentile of 6.9% in merger years. Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

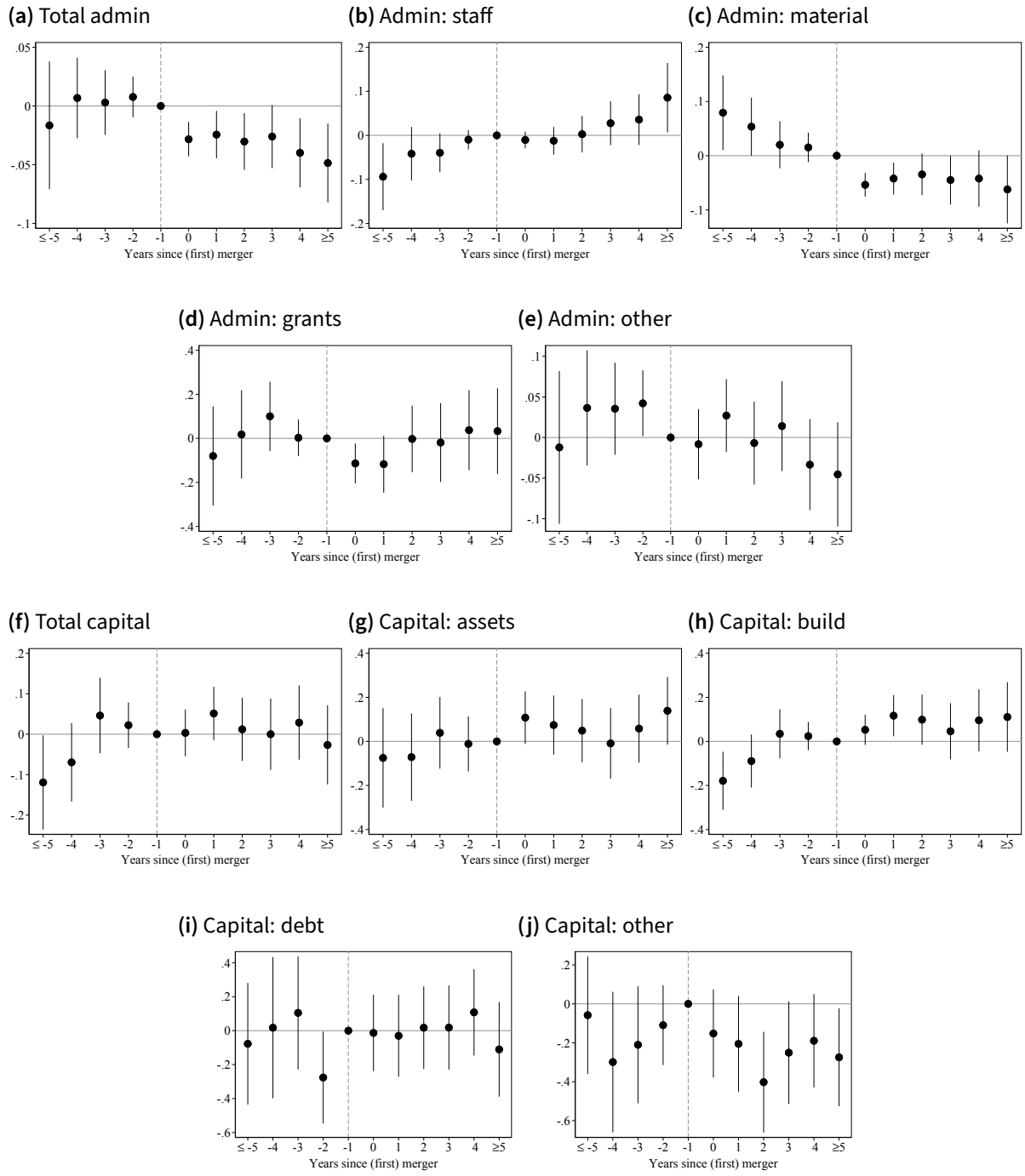
Appendix

Figure A4.1: Distribution of changes in per capita transfers



Notes: The figure shows the distribution (univariate kernel density estimate) of yearly growth rates in per capita transfers. The dummy variable *merge* equals one when a municipality participates in a merger and zero otherwise. The 416 municipalities of Saxony (territorial status as of January 1 2019) over the period 1992-2010 are the observation units.

Figure A4.2: Event study – Expenditures



Notes: The figure shows the results of ten event study estimations, where the dependent variables are different per capita expenditures (log); for a description of expenditure categories see table A4.3. The vertical dashed line represents the year before the first merger of a municipality. The 360 municipalities of Saxony (territorial status as of January 1 2019) over the period 1992-2010 are the observation units; for sample restrictions see section 4.4.2. Bullets are point estimates and black lines represent the 90% confidence interval. I include year and municipality fixed effects and control variables. Standard errors are robust to heteroskedasticity.

Table A4.1: Number of mergers

	Abs.	%
0	74	17.7
1	164	39.1
2	75	17.9
3	56	13.4
4	22	5.3
5	15	3.6
≥ 6	13	3.1
Total	419	100.0

Notes: The table shows the absolute and relative number of municipalities by the number of mergers they underwent. The 419 municipalities of Saxony (territorial status as of January 1 2019) over the period 1992-2010 are the observation units.

Table A4.2: Types of mergers

	Abs.	%
Incorporation	463	65.8
Equal merger	152	21.6
New formation	57	8.1
Name change	24	3.4
Partial reclassification	7	1.0
Reclassification	1	0.1
Total	704	100.0

Notes: The table shows the absolute and relative number of different mergers in Saxony. The 419 municipalities of Saxony (territorial status as of January 1 2019) over the period 1992-2010 are the observation units. If a municipality underwent two different types of mergers in a given year, only one type is kept.

Table A4.3: Expenditure categories in municipal budgets

Budget	Exp.	Description
Administrative	Staff	Salaries, social security contributions, pensions, compensation for volunteers
Administrative	Material	Maintenance and operation of fixed assets (land, buildings) and vehicles, rent and lease, purchase of movable property, expenses for public employees, taxes, insurance and claims, business expenses, reimbursement of general and administrative expenses, imputed costs
Administrative	Grants	For charitable, benevolent or ecclesiastical purposes, for ongoing purposes, debt servicing, welfare benefits (social, youth, asylum seekers, other)
Administrative	Other	Interest payments, trade tax levy, general grants and levies, transfers to the capital budget, run-off of previous years
Capital	Assets	Acquisition of assets, such as shareholdings, capital, land, movable assets (machinery, vehicle park etc.
Capital	Building	Construction measures, e.g. road construction, schools, sewage disposal
Capital	Debt	Repayments of credits and loans

Notes: The table contains a description of the expenditure categories used in the empirical analysis. Expenditures can occur within the administrative or the capital budget. The capital budget contains all revenues or expenditures of the municipality that have an effect on assets, i.e. all financial transactions that increase or decrease assets and therefore cannot be allocated to the operating budget.

Table A4.4: Policy areas

Policy area	Description
General administration	Municipal authorities, audit, central and fiscal administration, specialist services
Public safety	Police, public order, fire protection, disaster control
Schools	School administration, operating expenditures, other school-related functions (transport, meals, etc.)
Science, research, culture	Science and research, museums, collections and exhibitions, theaters, concerts, cultivation of music, local heritage, popular education, nature conservation, historic preservation, church affairs
Social security	Administration of social affairs, social assistance, implementation Asylum Seekers' Benefits Law, social institutions and services, assistance for war victims, child and youth welfare
Health, sports, recreation	Health administration, hospitals, sports initiatives, sports facilities, swimming pools, parks and gardens, other recreational facilities
Building, housing, infrastructure	Building authority, urban planning, surveying, building regulations, housing promotion, housing companies, municipal roads, district roads, federal and state roads, street lighting and cleaning, parking facilities, waterways
Public institutions, business development	Sewage and waste disposal, markets, stockyards, slaughterhouses, funeral activities, promotion of agriculture and forestry, tourism, business development
Municipal companies, assets	Administration of companies, utilities, transport companies, business development, agricultural and forestry businesses, spa and pool operations, fixed assets (land), special assets
Finance	Taxes, grants and levies, reserves, credits, run-off of previous years

Notes: The table contains a description of the ten policy areas on which the municipal budget system is based.

Table A4.5: Discretionary transfers

	Specific needs transfers			General transfers		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.27	0.23	0.23	0.05	0.10	0.06
	(0.17)	(0.17)	(0.17)	(0.41)	(0.41)	(0.40)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls		Yes	Yes		Yes	Yes
Linear time trend			Yes			Yes
Observations	2,335	2,335	2,335	3,004	3,004	3,004

Notes: The table shows the results of six difference-in-differences estimations, where the dependent variables are two types of discretionary transfers per capita (log). The main variable of interest (Treat) takes on the value of one in the years after the first merger of a municipality. The 360 municipalities of Saxony (territorial status as of January 1 2019) over the period 1992-2010 are the observation units; for sample restrictions see section 4.4.2. All specifications include year and municipality fixed effects. In columns (2) and (5), I add socio-demographic controls. In columns (3) and (6), a municipality-specific linear time trend is included. Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

Table A4.6: Expenditure shares within policy areas

	Staff	Material	Grants	Assets	Building	Debt
Administration	64.28	27.74	4.03	3.11	7.05	.
Building	15.35	26.45	3.72	3.11	57.72	.
Companies	9.98	46.67	14.40	19.24	34.08	.
Finance	.	3.63	.	.	.	17.28
Health	26.04	48.18	8.87	5.40	30.89	.
Public	40.91	27.68	10.44	6.54	25.03	.
Safety	26.06	41.86	2.64	16.22	28.40	.
Schools	25.04	51.25	5.33	4.83	24.71	.
Science	39.27	40.07	17.94	6.94	28.19	.
Social	57.71	15.97	27.22	1.07	7.96	.

Notes: The table shows the expenditure shares of the expenditure categories used in the empirical analysis by policy areas. All values are in % and expressed relative to total expenditures within the respective policy area. The 419 municipalities of Saxony (territorial status as of January 1 2019) over the period 1992-2010 are the observation units.

Table A4.7: Expenditures by voluntary and compulsory mergers

	Administrative budget					Capital budget				
	(1) Total	(2) Staff	(3) Material	(4) Grants	(5) Other	(6) Total	(7) Assets	(8) Building	(9) Debt	(10) Other
Panel A: Compulsory vs. voluntary mergers										
Compulsory merger	-0.01	0.14**	-0.07*	-0.04	-0.02	0.06	0.13	0.16*	-0.03	-0.14
	(0.02)	(0.05)	(0.04)	(0.17)	(0.04)	(0.06)	(0.11)	(0.08)	(0.15)	(0.16)
Voluntary merger	-0.05**	0.03	-0.03	-0.11	-0.05	-0.09	0.10	-0.02	-0.11	-0.23
	(0.02)	(0.04)	(0.03)	(0.11)	(0.04)	(0.06)	(0.10)	(0.09)	(0.16)	(0.14)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls+trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,408	4,408	4,408	4,357	4,408	4,408	4,372	4,402	4,038	3,507
Panel B: 1999 mergers only										
Merge 1999	-0.03	0.15**	-0.11**	0.02	-0.03	0.03	0.15	0.19*	-0.11	-0.07
	(0.02)	(0.06)	(0.05)	(0.18)	(0.05)	(0.07)	(0.12)	(0.10)	(0.18)	(0.19)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls+trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,147	2,147	2,147	2,131	2,147	2,147	2,121	2,143	1,932	1,696
Panel C: 1994 mergers only										
Merge 1994	-0.08***	-0.02	-0.05	-0.14	-0.06	-0.13	0.29*	-0.04	-0.24	-0.29
	(0.03)	(0.06)	(0.05)	(0.18)	(0.06)	(0.10)	(0.17)	(0.14)	(0.28)	(0.23)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls+trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,641	2,641	2,641	2,608	2,641	2,641	2,610	2,635	2,365	2,051

Notes: Each panel shows the results of ten difference-in-differences estimations, where the dependent variables are different per capita expenditures (log); for a description of expenditure categories see table A4.3. The 360 municipalities of Saxony (territorial status as of January 1 2019) over the period 1992-2010 are the observation units; for sample restrictions see section 4.4.2. The set of control variables is identical to column (3) of table 4.4. In panel A, I interact the main variable of interest (Treat) with a dummy variable for compulsory (voluntary) mergers. In panels B and C, I re-estimate the baseline specification (panel A table 4.7) for municipalities that merged in 1999 and 1994, respectively. Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

5 Conclusion

Reducing disparities across sub-national jurisdictions to align economic perspectives and ensure equal access to public services has been at the top of the regional policy agenda for decades. To design well-targeted regional policy measures, it is key to understand the consequences they might yield. In this thesis, I investigate how political, spatial, and administrative changes shape the local level. In particular, I focus on a wide range of outcomes which are representative for local economic and socio-demographic development and also relate to the social fabric of local communities.

Local politicians, like mayors, are key players in determining regional economic outcomes. In chapter 2, my coauthors and I investigate the policies of directly elected Carinthian mayors, which rank among the strongest local leaders in Europe. Our results show that far-right populist mayors deepen the divide within municipalities along partisan lines and within civil society. Their election likely makes nationalist and xenophobic views more acceptable within the public (Bursztyn *et al.*, 2020) with real consequences for local communities: immigrants are warded off and foreign residents withdraw from civil society, as measured by their participation in local football teams. On a global scale, with the controversial policies of Donald Trump, Victor Orban, and Jair Bolsonaro, polarization has emerged as a major societal concern, complicating the search for policy solutions.

Chapter 3 focuses on investments in new road infrastructure as a traditional instrument of regional policy. In line with the core-periphery model (Krugman, 1991), our results suggest differential effects across the "regional hierarchy" (Baum-Snow *et al.*, 2020): when peripheral municipalities are connected to the new highway, they see lower numbers of employment due to a lower volume of inbound commuters. Commuter flows within the periphery decrease significantly, while central municipalities benefit in terms of all economic outcomes and become more attractive places to live. These findings certainly urge caution for a policy debate that tends to focus exclusively on the benefits of improved accessibility especially for poorer regions. Although improvements in connections in our setting were large, rather than fostering economic development in peripheral areas, the highway intensified the core-periphery structure. Policymakers should adjust new infrastructure to regional-specific needs and particularly consider the potential "threats" of new transport infrastructure through increased competitive pressure.

In chapter 4, I investigate how shocks to jurisdiction size influence fiscal equalization transfers. In a system that grants disproportionately higher grants to larger municipalities, large-scale territorial reforms give rise to a sizable reallocation of transfers from non-merging to merging municipalities. The estimated effects exceed the mechanical population-based transfer increase by far, suggesting that larger jurisdictions do not succeed in attracting more economic

activity and in raising additional tax revenues. My findings raise doubts about the adequacy of relying on population-based indicators to determine fiscal need within equalization schemes. While I have focused on a system favoring larger municipalities, policy conclusions carry through to systems favoring smaller or less densely populated municipal structures. Such policies bear the risk of preserving inefficient public services and excessively dispersed settlement structures within a country and provide disincentives to merge.

This thesis demonstrates that regional policymakers and regional policy measures shape local development in various ways. A careful design of regional policies requires an understanding of the spatial distribution of economic activity to avoid unintended consequences. The same is true for the design of fiscal equalization systems aimed at reducing sub-national fiscal disparities. The results of this thesis offer important insights for policymakers, particularly in other federalist, high-income countries such as Germany and Austria. As the descriptive evidence in chapter 1.3 has shown, within-country disparities are an OECD-wide phenomenon. While the attention of regional policy often revolves around economic outcomes, social cohesion within local communities is equally important. Further research should devise innovative measures for the social divide within society which is particularly challenging in small jurisdictions. Investigating the feedback effect social divisions have on economic outcomes is an intriguing question which will be all the more relevant against the background of rising societal tensions in a globalized world.

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Curriculum Vitae

Luisa Dörr

08/2016 - 02/2022	Junior Economist and Doctoral Student ifo Institute – Leibniz Institute for Economic Research at the University of Munich Ph.D. Program in Economics (Dr. oec. publ.) Munich Graduate School of Economics Ludwig-Maximilians-Universität München
09/2015 - 12/2015	Graduate studies in Economics University of Dundee, Scotland
04/2014 - 10/2016	Master of Science in Economics University of Bayreuth Undergraduate studies in Economics University Jean Moulin Lyon III, France
10/2010 - 09/2013	Bachelor of Science in European Economic Studies Otto-Friedrich-Universität Bamberg