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Infrastructure, Institutions and Identity – Determinants of Regional Development

Empirical Evidence from Germany

Stefanie Gäbler

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Preface

Stefanie Gäbler prepared this study while she was working at the ifo Center for Public Finance and Political Economy. The study was completed in March 2020 and accepted as doctoral thesis by the Department of Economics at the University of Munich. It consists of six distinct empirical essays investigating various aspects of regional development using data from Germany. Chapters 2 and 3 investigate the impact of transport infrastructure in the form of an regional airport and highway expansion. Chapters 4 and 5 examine institutions at the local level, namely local public accounting standards and local election systems. Chapter 6 deals with the topic of regional identity. The chapters employ difference-in-differences, event study and synthetic control estimations to answer the respective research questions.

Keywords: Airports, tourism, regional development, transportation infrastructure, highway, accessibility, tax factors, municipalities, local government, fiscal rules, public accounting, budget transparency, sustainability, government efficiency, accountability, direct elections, constitutions, government form, economic performances, public services, common identity, regional identity, formation of identity, voter turnout, Germany, GDR, reunification, synthetic control method, differences-in-differences, event studies

JEL-No: D02, D72, D73, H40, H54, H70, H71, H75, H83, L93, O18, R50, Z19, Z38

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I thank my colleagues at the ifo Center for Public Finances and Political Economy, Luisa Dörr, Manuela Krause, Björn Kauder, Florian Dorn, Martin Mosler, Klaus Gründler, Fabian Ruthardt and Johannes Blum, for a pleasant working atmosphere, wonderful company, and productive talks. I am particularly grateful to Luisa and Manuela for our daily morning coffee breaks, their extensive moral encouragement and their friendship. Luisa contributed significantly to the completion of my dissertation as my coauthor, but most of all as the best office neighbor I can imagine. Interns, research assistants, the administrative staff at the ifo Institute and colleagues from other ifo centers also provided me with helpful support during the entire time. Among many others, I am especially grateful to Kristin Fischer, Florian Neumeier and Robert Lehmann.

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I owe a great deal to my mother, Katja Gäbler, who always believed in me. I am more than thankful for her unconditional love and support throughout my life. Although he will never be able to read these words, I like to express deepest gratitude to my father Christian Gäbler. I miss him every day.

Stefanie Gäbler
March 2020

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	Prof. Dr. Andreas Haufler

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1 Introduction

1.1 Importance of regional development

Between regional diversity and disparity, helping regions lagging behind to "catch up" has become a political paradigm. Regional policies are intended to diminish (severe) regional inequalities using public interventions. Despite much policy effort[1] spatial patterns of disparities across regions seem to intensify (see, for example, Rosés and Wolf 2018 for Europe and OECD 2016 for the OECD). Even though interregional gaps will always exist, reducing regional inequalities constitutes an overarching goal to promote regional and social cohesion (OECD, 2016). If citizens feel left behind, differences in regional welfare might translate into social consequences (Hüther *et al.*, 2019). Before launching policy measures, however, it is essential to understand the consequences these measures can unfold on different aspects of regional characteristics and potential inequalities.

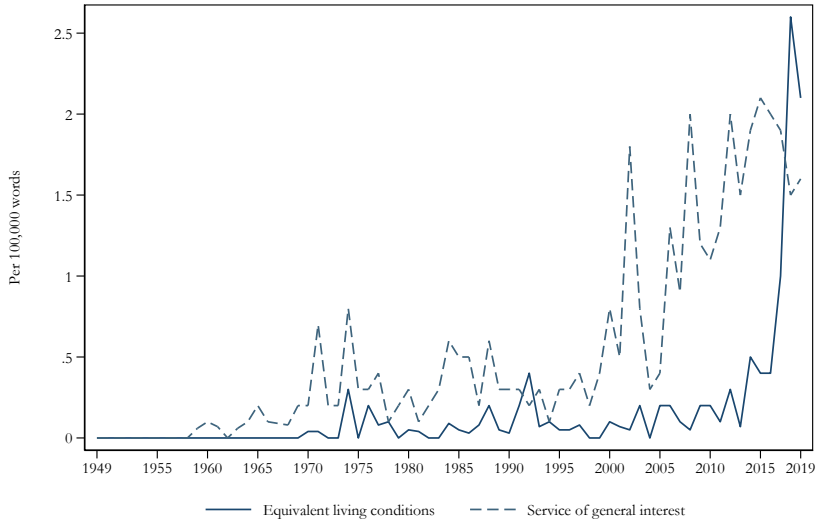
This thesis contributes to the field of regional economics by investigating how policy measures influence regional development. I use the example of Germany, where promoting regional development and a spatially balanced economic structure are important issues and established by law.[2] Ensuring equivalent living conditions constitutes a major political goal in Germany.[3] Providing services of general interest, like access to health services, education and mobility, are considered to be a fundamental prerequisite. In recent years, the establishment of equivalent living conditions as well as providing services of general interest to promote regional development and reduce regional disparities gained more and more attention in the public and political debate (figure 1.1).

[1] The EU, for example, spends almost one third of its budget on regional cohesion policies (European Commission, 2020).

[2] "A balanced social, infrastructural, economic, ecological and cultural environment is to be sought in the Federal Republic of Germany as a whole and in its sub-areas." (§2 (2) Nr. 1 Raumordnungsgesetz).

[3] See, for example, the coalition agreement between CDU, CSU and SPD from 2018, or the government programs of the individual parties from 2017 (CDU/CSU/SPD, 2018; SPD, 2017; CDU, 2017). Furthermore, a commission for the establishment of equivalent living conditions was set up in 2018 (CDU/CSU/SPD, 2018). However, the concept of equivalent living conditions is not explicitly defined, neither are indicators to measure whether living conditions are equivalent or not.

Figure 1.1: Importance of "equivalent living conditions" and "service of general interest" in the German Bundestag



Source: Zeit Online (2019).
Notes: This figure shows the appearance of the words *"gleichwertige Lebensverhältnisse"& "gleichwertiger Lebensverhältnisse"* (equivalent living conditions) and *"Daseinsvorsorge"* (service of general interest) per 100,000 words in debates of the German Bundestag. It is based on an analysis of 4.216 plenary minutes from the German Bundestag between 1949 and July 24, 2019.

1.2 Measures of regional development

Regional disparities become apparent in numerous aspects. Inequalities between German regions in private income and economic power[4] decreased over the last years (Fuest and Immel, 2019; Braml and Felbermayr, 2018). Nevertheless, depending on the perspective and focusing, for example, on public finances, demography, or labor market characteristics, regions in Germany face different challenges. Figure 1.2 shows the dispersion of taxable capacity, public debt, population density and unemployment rates across German counties in 2017. The chosen parameters reveal a non-uniform picture. While there seems to be a east-west difference in taxable capacity and unemployment with especially high taxable capacity and low unemployment in the south (figures 1.2a and 1.2b), the patterns for public debt and population density are diverse. Public debt is particularly high in western parts of Germany, with low levels in the east and south (figure 1.2c). Population density shows a clear distinction between rural and urban areas (figure 1.2d).

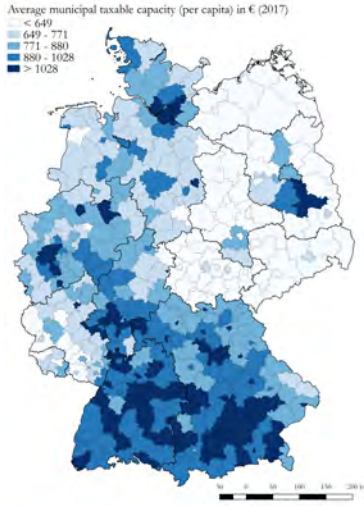
The aim of this thesis is to investigate how infrastructure, institutions and identity affect different measures of regional development. Chapters 2 and 3 examine infrastructure in the form of airports and highways and their influence on tourism, public finances, population and economic activity. Chapters 4 and 5 deal with institutions, such as public sector accounting standards and electoral systems and how they affect unemployment, public finances as well as efficiency and accountability of the public administration. Chapter 6 turns toward a different aspect that may shape regional prosperity and examines the formation of regional identity. Besides classical measures like GDP or household income, the aspects covered in this thesis – public finances, public administration, demography, unemployment, tourism and identity – are important indicators of regional prosperity and disparities.

Sound public finances and an efficient public administration are fundamental prerequisites for a local government capable of action. High levels of debt (figure 1.2c), for instance, indicate a reduced scope of action, as indebted municipalities or counties cannot easily undertake necessary investments or provide sufficient services of general interest. The taxable capacity is an important benchmark for economic and fiscal performance (figure 1.2a). Major tax revenues at the local level are the business and income tax, both closely tied to economic activity. Demography is another factor influencing regional development. A low population (figure 1.2d) and negative development are associated with increasing costs for public services of general interest. Among others, public transport or health infrastructure have to be provided for fewer and fewer inhabitants. Also the age structure of the population might have severe consequences for local development. If the working age population decreases, local enterprises are likely to face a shortage of (skilled) workers. The unemployment rate (figure 1.2b) is another proxy to evaluate economic conditions and development of a region. The unemployment rate also signals the degree to which citizens participate in socio-economic processes (Oberst *et al.*, 2019). Tourism and identity seem on the first glance not es closely

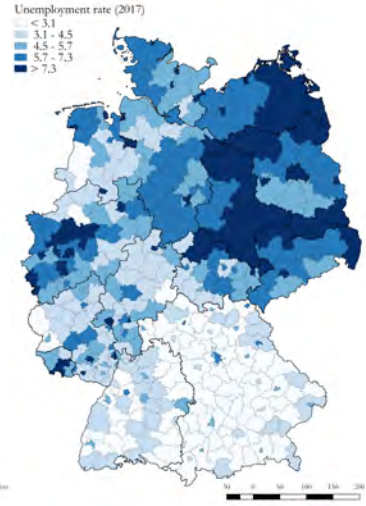
[4] Measured with gross value added.

Figure 1.2: Regional disparities in Germany

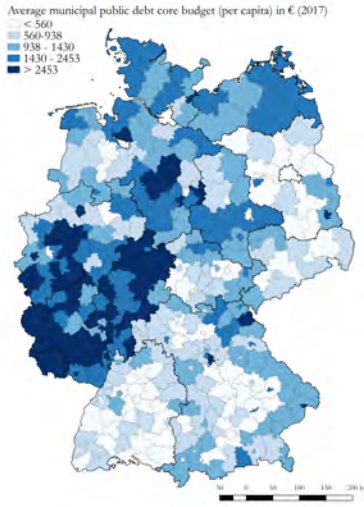
(a) Taxable capacity



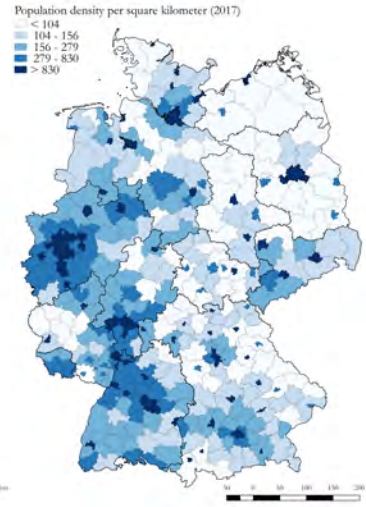
(b) Unemployment rate



(c) Public debt



(d) Population density



Source: BBSR (2019).
Notes: This figure shows indicators of regional prosperity and their dispersion across counties in Germany.

linked to economic development. However, tourism constitutes an important cross-sector industry, with growth-impulses for rural and structural weak areas (BMWi 2017). Moreover, regional identity might have consequences for economic prosperity, as the connection to ones community is closely tied to social cohesion and civic engagement.

1.3 Regional layers

This thesis focuses on counties and municipalities in different German states. Figure 1.3 portrays the regional layers and states covered in each chapter. The federalistic structure of Germany consists of two tiers: the federal level and the state level. Municipalities and counties belong to the state level and form the two layers of local government.[5] They play an important role in shaping regional development and providing services of general interest at the local level.

Municipalities are guaranteed the right to self government and exercise autonomy in their expenditures and revenues to a certain degree. Municipal tasks comprise, among others, local services of general interest, promotion of the local economy, the establishment and operation of culture and sport facilities. Municipalities form counties and consolidated city-counties (*Landkreise* and *kreisfreie Städte*), who posses the right of self-government within the scope of the tasks assigned to them by the states. Counties and consolidated city-counties are also responsible for numerous public services. Their main responsibility is social care, like public employment services and youth welfare. Moreover, the development of the local economy, county hospitals, public transport, maintaining county roads and school buildings as well as school capacity planing fall within the jurisdiction of counties and consolidated city-counties.[6]

1.4 Chapter overview

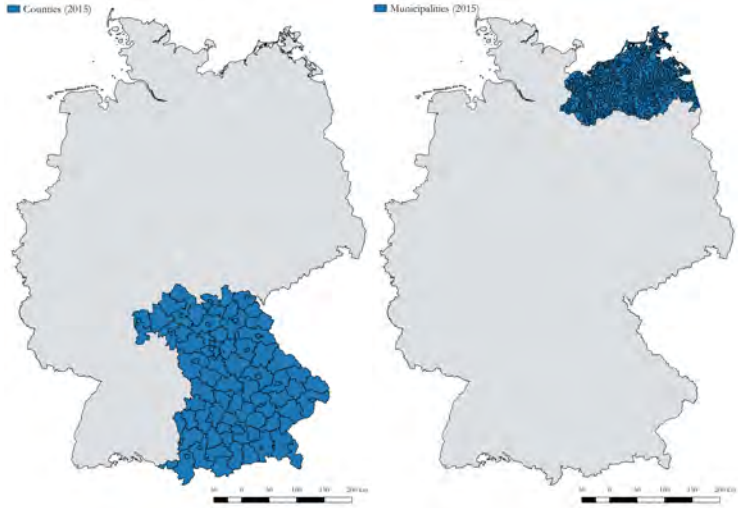
Chapters 2 and 3 examine the local consequences of transport infrastructure. Transport infrastructure is often described as the backbone of regions, the base for economic and social interactions (Demary *et al.*, 2019). Infrastructure connects regions at the global and local level. The expansion of roads, railroads and airports reduces transportation costs for people and products, which might attract new businesses and jobs and eventually lead to regional (economic) development (see for example Duranton and Turner, 2012; Donaldson, 2018). Chapter 2 focuses on the global connection of regions with airports, while chapter 3 considers the local connection via highways.

[5] For an introduction in the federal system of Germany and its administrative structure, see Brümmerhoff and Büttner (2015).
[6] The specific tasks, however, vary in each state.

Figure 1.3: Regional layers

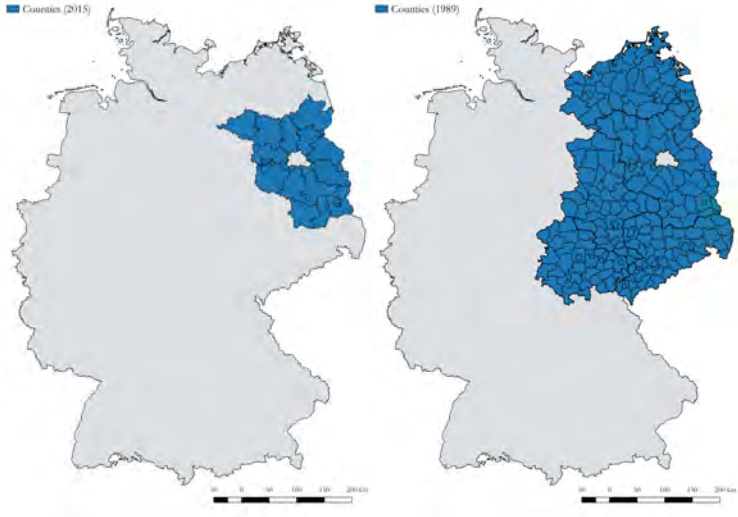
(a) Chapters 2 & 4 – Counties in Bavaria

(b) Chapter 3 – Municipalities in Mecklenburg Western Pomarania



(c) Chapter 5 – Counties in Brandenburg

(d) Chapter 6 – Counties in East Germany



Notes: This figure shows the regional layers and areas in Germany that are used in this thesis to investigate aspects of regional development.

Chapter 2, which is joint work with Luisa Doerr, Florian Dorn and Niklas Potrafke, based on Doerr *et al.* (2020), investigates how new airport infrastructure influences tourism. Infrastructure is a basic determinant of tourism flows, while tourism often endorses regional economic development as tourists demand accommodations and services. In 2015 national and international tourists spend some Euro 287 billion in Germany. These expenditures are associated with almost three million jobs and a value added of around Euro 105 billion (BMWi 2017).

Investigating the causal effect of transportation infrastructure comes with some challenges. Infrastructure is built to connect economic units, which makes it difficult to disentangle the causal effect between infrastructure and economic development. We identify the effect of new airport infrastructure by exploiting the conversion of a military airbase in Bavaria into the regional airport Memmingen. The conversion constitutes an exogenous positive infrastructure shock for the tourism sector. We concentrate on guest arrivals at the county level in the German state of Bavaria (figure 1.3a). Using a synthetic control approach we show that guest arrivals from abroad increased in the counties close to the airport. Together with a passenger survey conducted at the airport Memmingen, covering, among others, expenditures, length and place of stay, the results suggest that the airport not only increased tourism but may also promote regional economic development.

Public finances are another important topic for regional development. Financial endowment and the possibility to raise taxes is not evenly distributed across municipalities (figures 1.2a and 1.2c). Structurally weak municipalities and counties might simply not have the financial scope to fulfill public services and maintain infrastructure (Oberst *et al.*, 2019). It is therefore essential to investigate the impact certain policy measures, like infrastructure expansion or institutions at the local level, might unfold on public finances.

In chapter 3, which is joint work with Luisa Doerr, based on Doerr and Gaebler (2020), we therefore investigate how highway accessibility influences municipal tax policy. The chapter exploits the stagewise expansion of a highway in the East German state of Mecklenburg-Western Pomerania (figure 1.3b). Within their scope of self-government, German municipalities can decide independently about the tax factors for business tax, general and agricultural property tax. Using difference-in-differences and event study methods we show that highway access influences local tax setting. Tax rates for the immobile factor property increase to keep tax revenues stable while the tax rates for businesses are not altered. To reduce concerns about endogeneity we follow the inconsequential units approach. For non-agglomeration municipalities that are located at a convenient route between two larger cities, access to a highway can be regarded as close to random. Highways are primarily built to connect the larger cities and not the municipalities in between (Chandra and Thompson, 2000; Redding and Turner, 2015).

Even though transportation infrastructure is often viewed as a catalyst for economic growth, its impact is, from a theoretical point of view, ambiguous. On the one hand, infrastructure increases the accessibility of a region *to* other regions. This increases market potential and infrastructure thus acts as an agglomeration force. On the other hand, infrastructure also increases the accessibility of a region *from* other regions, which fosters competitive pressure and infrastructure might act as a deagglomeration force ("two ways road problem", Cheshire *et al.* 2014). Whether the agglomeration or deagglomeration effect prevails may depend on the location of a region. While central areas often profit from infrastructure expansion and better market connections, peripheral areas do not necessarily benefit (Baum-Snow *et al.*, 2018; Asher and Novosad, 2020). When distinguishing between peripheral and central municipalities we find a similar pattern. Our results in chapter 3 are driven by peripheral municipalities, while we do not find an influence of highway accessibility on tax factors in core municipalities. Additionally, when examining possible drivers of the tax policy effect we find that improved accessibility gives rise to a shift of economic activity and population from the periphery to the core.

Similar to chapter 3, chapter 4 analyses how policy measures influence public finances. The policy measure under investigation is a change in institutions: namely in public sector accounting standards. In the public sector two different accounting standards are used. Within the New Public Management movement countries started to replace traditional cash-based accounting with business-like accrual accounting. International institutions like the OECD, IMF or the EU promote the switch to accrual accounting. The European Commission is planing and developing an harmonized accrual accounting regime for all EU member states.[7] Accrual accounting is expected to enhance budget transparency, efficiency, and accountability. Nevertheless, accrual accounting comes with high implementation costs. France, for example, spend about $ 1.7 billion (European Commission, 2013), while estimates for Germany range around $ 3.5 billion (German SAI, 2017), without considering higher operation costs.

Local governments in Germany traditionally used cash-based accounting. But in the beginning of the 2000s accrual accounting became mandatory for local governments in almost all German states. In joint work with Florian Dorn and Felix Roesel, based on Dorn *et al.* (2019b), we evaluate the effect of public sector accrual accounting on sustainable budgeting, efficiency and accountability of the administration. We exploit the gradual and partial shift in accounting standards at the county level in Bavaria (figure 1.3a). Results from a difference-in-differences and an event study approach show that expenditures, public debt, voter turnout, technical efficiency, and accidents on county roads hardly reveal any policy change after switching to accrual accounting. However, costs to run the administration increase. We conclude that in high income countries public sector accounting standards do not seem to matter much for the performance of local governments.

[7] European Public Sector Accounting Standards – EPSAS.

The electoral system is another institution that might affect outcomes at the local level. There is an ongoing debate whether politicians appointed by parliaments perform different than directly elected politicians (Persson and Tabellini, 2003). Even though the form of government should, following the median voter theorem, theoretically not matter, reality looks different. The Turkish constitutional change in 2017, which included a directly elected president, was followed by violent protests. Also at the local level the form of government is a topic of interest and is shown to influence public finances (see, for example, Hessami, 2018; Baqir, 2002).

In joint work with Felix Roesel and based on Gaebler and Roesel (2019), chapter 5 evaluates how directly elected politicians affect economic and administrative performance beyond public finances. We focus at the county level in the German state of Brandenburg (figure 1.3c), where a quorum applies to local elections. The head of local government needs the absolute majority and votes for the winning candidate have to represent at least 15% of eligible voters. If the winner does not reach this quorum, the direct election is suspended and the local council appoints the head of government. For election outcomes around the quorum the form of government can be regarded as close to random. Using a difference-in-differences and an event study approach results show that the public employment service becomes somewhat more effective under directly elected politicians, while they do not seem to speed up administrative acts or attract more businesses.

More difficult to measure but nevertheless essential for local prosperity are social cohesion and participation. Especially in rural areas, voluntary civic engagement plays an important role (Heinze and Orth, 2019). Social participation is closely linked to regional identity, which is an essential underlying factor of social capital and functioning societies. Citizens will take interest and participate in society if they feel to be a part of it (Lipset *et al.*, 1954). Local civic engagement and volunteer work, for example, tends to be higher if citizens feel a strong local connection to their community (Foertsch and Roesel, 2019).

Chapter 6, therefore, investigates the formation of regional identity. I use a historical quasi experiment to examine how a change in administrative areas influences sub-national and national identity. In 1952 the German Democratic Republic dissolved states and replaced them by districts. During the process of German reunification in 1990, states had to be reestablished. The district borders did not follow the old state borders. As a result, future state affiliation was not clear in all East German counties and referendums were conducted. I use a revealed measure of regional identity, namely voter turnout in state and federal elections, to examine attachment to the community at different layers. Using difference-in-differences estimates and focusing on the county level in East Germany (figure 1.3d) I show that identification with the state is lower in counties were future state affiliation was not clear. Hence, administrative regions seem to influence common regional identity. Changing them does on the aggregate not lead to identity formation with the new region, even within a time window of 30 years. A reduced regional identity, in turn, might have long-lasting implications. A significant decline in the electorate, for instance, indicates that counties where the attachment to the state is weak are less attractive for their citizens.

For carefully targeted regional policy measures, designed to help regions lagging behind to “catch up”, it is essential to understand the consequences policy measures might unfold. This thesis contributes to the debate and investigates how different policy measures shape regional prosperity and development. The following chapters are self-contained and can be read independently. Chapter 7 provides some concluding remarks.

2 How new airport infrastructure promotes tourism: Evidence from a synthetic control approach in German regions[1]

Abstract

The chapter examines how new airport infrastructure influences regional tourism. Identification is based on the conversion of a military airbase into a regional commercial airport in the German state of Bavaria. The new airport opened in 2007 and promotes traveling to the touristic region of Allgäu in the Bavarian Alps. A synthetic control approach is used to show that the new commercial airport increased tourism in the Allgäu region over the period 2008-2016. The positive effect is especially pronounced in the county in which the airport is located. The results suggest that new transportation infrastructure promotes regional economic development.

[1] This chapter is joint work with Luisa Doerr, Florian Dorn and Niklas Potrafke. It is based on our paper "How new airport infrastructure promotes tourism: Evidence from a synthetic control approach in German regions" published in Regional Studies, *forthcomming*, 2020.

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2.1 Introduction

Transportation infrastructure connects regions and promotes regional (economic) development. Investments in roads, railroads and airports reduce transportation costs for products and people and help to attract new businesses, production plants and jobs. Moreover, infrastructure constitutes the basic determinant of (inter)national tourism flows. Tourists may well travel to rural areas when roads, railways and airports facilitate convenient and low-cost journeys. They demand accommodation and amenities, cultural affairs such as theaters and exhibitions, amusement parks, etc. and their expenditures in these areas often endorse regional economic development.

We examine how new airport infrastructure influences regional tourism. Empirical studies show that building or extending airports and airport services enhanced international tourism flows (Khadaroo and Seetanah, 2007; Eugenio-Martin, 2016; Khan *et al.*, 2017), increased production and employment (Hakfoort *et al.*, 2001; Klophaus, 2008; Zak and Getzner, 2014), endorsed regional economic development (Halpern and Bråthen, 2011; Mukkala and Tervo, 2013; Kazda *et al.*, 2017)[2], and might even generate positive spillover effects to neighboring regions (Percoco, 2010). However, hardly any empirical studies identify the causal effect of airport infrastructure on tourism or economic development. Empirical studies that examine how infrastructure influences economic development have to deal with identification issues. Transportation infrastructure is built to connect economic units, hence, disentangling causality between new infrastructure projects and economic development is difficult. New empirical studies use identification strategies such as instrumental variables (IV) or synthetic control to estimate causal effects of infrastructure programs on population and employment (Duranton and Turner, 2012; Möller and Zierer, 2018; Gibbons *et al.*, 2019), or economic development in individual regions (Chandra and Thompson, 2000; Ahlfeldt and Feddersen, 2018). Castillo *et al.* (2017) use a synthetic control approach to estimate the causal effect of an encompassing infrastructure program (including a new airport) on employment in the tourism sector in Argentina. However, they do not isolate the effect of the airport. Scholars employing IV approaches show that airports or air passenger traffic increased the local population (Blonigen and Cristea, 2015), employment in service-related industries (Brueckner, 2003; Green, 2007), and local employment in services that directly benefit from the air connection (Sheard, 2014). Koo *et al.* (2017), however, also use an IV and find no effect of direct air services on tourism inflow. Tsui (2017) uses IV and Difference-in-Differences approaches and shows that low-cost carriers (LCCs) have a positive effect on domestic tourism demand.

We investigate how new airport infrastructure (specialized in LCCs) influences additional guest arrivals in the tourism sector. The identification is based on the conversion of the military airbase of Memmingerberg into the regional commercial airport of Memmingen (Munich-West) in the German state of Bavaria. The military airfield was built by the Nazi regime in 1935/36

[2] Tveter (2017), however, finds small positive effects of regional airports on employment and population in Norwegian municipalities.

and was reused by the German Bundeswehr after the Second World War. In 2003, it was closed because the federal government decided to reorganize and consolidate the German Bundeswehr. We exploit the conversion of the airfield to a commercial airport specialized on LCCs as an exogenous positive infrastructure shock for the touristic sector in counties close to the airport. The commercial airport opened in 2007 and facilitates traveling to the touristic region of Allgäu in the Bavarian Alps. We use a synthetic control approach comparing tourism inflows in counties close to the new commercial airport and their synthetic counterparts when the new commercial airport started operating. Counties from other regions in Bavaria that are not affected by the new airport constitute the donor pool to construct the synthetic counterfactuals. The results show that the new commercial airport increased incoming tourism from abroad in the Allgäu region over the period 2008-2016. The positive effect is especially large in the county where the airport is located (Lower Allgäu): Memmingen Airport increased total arrivals of tourists and business travelers at touristic accommodations in Lower Allgäu on average by 54,000 (22%) and arrivals from abroad on average by 23,000 (69%) per year over the period 2008-2016. The results suggest that new transportation infrastructure may promote regional economic development.

2.2 Background: History, geography, airlines and passengers

The Regional Airport of Memmingen (FMM), internationally also known as Munich-West or Allgäu-Airport, was opened on the former military airbase in Memmingerberg in Bavaria. The military airbase was built by the Nazis in 1935/36 for strategic military reasons, and was reconstructed and reused by the German Bundeswehr and its NATO partners after the Second World War. In 2003, it was closed because the federal government decided to reorganize and consolidate the German Bundeswehr. Local companies decided to start a commercial civil airport on the former NATO airbase because of the high technical endowment and size of the runway. Local governments and the state government supported the civil airport with investments and subsidies for conversion and construction measures. Memmingen Airport, however, does not receive subsidies for its operating business and has reported a positive operating result (earnings before interest and taxes – EBIT) for several years.[3]

FMM started operating commercial air service in mid-2007. The airport already had over 450,000 passengers in 2008 and over 800,000 passengers in 2009, with scheduled flights operated by TUIfly and Air Berlin in the first years. The regional airport is specialized in services

[3] Many regional airports do not report positive operating results and operate at inefficient levels (Adler *et al.*, 2013). One reason for inefficiency lies in the importance of LCCs (Červinka, 2017). Their market power enables LCCs to negotiate favorable agreements, for example, marketing charges (Barbot and D'Alfonso, 2014).

by LCCs, such as the Irish airline Ryanair (scheduled flights since 2010) or the Hungarian airline Wizz Air (since 2009).[4] The number of passengers increased to 1.17 million by 2017, a decade after its opening (figure A2.1 in the appendix).

The airport connects several countries in Europe and the Mediterranean region to the Allgäu region. German domestic flights were the most important in the first two years after the launch of air services at FMM, but have been discontinued since 2011. In 2018, connections to and from Spain, Portugal, Romania, Bulgaria, Ukraine and the UK had the highest passenger volume at Memmingen Airport (table A2.1 in the appendix). A passenger survey conducted in 2018 has shown that 40% of all passengers at Memmingen Airport are incoming passengers, similarly during the winter (46%) and summer season (35%) (Bauer *et al.*, 2019).[5]

Memmingen Airport is located in the touristic region of Allgäu in the southwest of the German state of Bavaria (figure A2.2 in the appendix). The Allgäu is a popular touristic region in Germany. It is famous, for example, for hiking and skiing in the Alps, wellness and health hotels, and Germany's best-known castle: Neuschwanstein. Allgäu ranks second after the state capital city Munich among the most popular touristic regions regarding arrivals and overnight stays in Bavaria. The 2018 passenger survey has shown that Allgäu (21%) and Munich (33%) account for more than half of all overnight stays by incoming passengers via Memmingen Airport (Bauer *et al.*, 2019).[6] Growth rates in guest arrivals and overnight stays in the touristic region of Allgäu have exceeded those of Bavaria in total since 2007.

Connectivity via airport infrastructure depends on air services being offered (Derudder and Witlox, 2005). An airport's attractiveness for airlines is influenced by its catchment area size (Humphreys and Francis, 2002; Lieshout, 2012) and airport competition in multiple airport regions (Pels *et al.*, 2001; Alberts *et al.*, 2009; Derudder *et al.*, 2010; Lian and Rønnevik, 2011; Wiltshire, 2018). Memmingen Airport is often advertised abroad as Munich-West and Munich's LCC airport. Flights to FMM tend to be cheaper than to Munich's International Airport (MUC). Travel times between Memmingen Airport and Munich's city center, however, are about 1.5 h (by car and bus/railway likewise), that is, about 0.5-0.75 h more than from MUC. On the contrary, travel times to several touristic places in the Allgäu are reduced when arriving at Memmingen Airport rather than at any other airport.[7]

[4] The emergence of LCCs has led to an overall increase in the number of tourists (Rebollo and Baidal, 2009). Tourists choosing LCCs are likely to have different preferences than tourists choosing other carriers (Eugenio-Martin and Inchausti-Sintes, 2016).

[5] Flight connections to the source regions of Bulgaria, Poland, Romania and Russia had among the highest shares of incoming passengers ($>$ 50%) for all air services in 2018. Air services offered to Sweden and the Mediterranean region including Croatia, Greece, Italy, Portugal or Spain are mainly used by outgoing passengers (incoming share $<$ 30%).

[6] About 75% among all incoming passengers who stay in the Allgäu region report touristic or private motives; about 20% report business reasons.

[7] The only exception is the West Allgäu region close to Lake Constance. For several municipalities in West Allgäu, travel times to the Bodensee Airport Friedrichshafen at Lake Constance are less than to Memmingen Airport. The airport in Friedrichshafen, located in the German state of Baden-Württemberg, was built in 1918

2.3 Empirical strategy and data

2.3.1 Estimation strategy

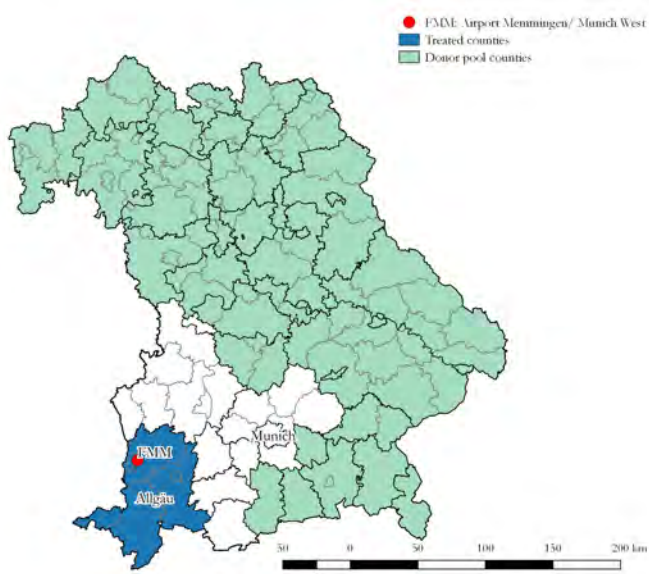
We compare the development of tourism across counties in the German federal state of Bavaria. A total of 96 Bavarian counties form 36 tourism regions (figure 2.1), which merchandise as Bavarian touristic destinations. Therefore, the treatment and control areas (donor pool) are counties belonging to different touristic regions. Memmingen Airport is located in the touristic region of Allgäu which consists of seven counties constituting the treatment group (blue counties in figure 2.1). Counties in touristic regions located in the north and east of Bavaria form the control group (donor pool, green counties). Counties from touristic regions bordering the Allgäu, as well as the capital Munich and its vicinity, are excluded from the analysis, that is, they are neither in the treatment nor control groups (white counties). Touristic regions bordering the Allgäu are likely to be treated to some extent as well. Munich attracts most incoming passengers at Memmingen Airport and is by far the most populous and economically powerful area in Bavaria and, therefore, not comparable with other regions especially in terms of tourism inflows.

Identification relies on the main assumption that sorting into treatment was exogenous. The placement of the military airbase in 1935/36 and its closure by a decision of the federal government in 2003, hence, the timing of treatment, are obviously independent of touristic considerations. What is more, other former airbases in Bavaria are located relatively close to the international airports in Munich and Nuremberg or the technical equipment and size of the airfield was not as suitable for a commercial airport. They are reused as special airfields, sport airfields, or industrial areas. Memmingen Airport, however, has proximity to the catchment and metropolitan area of Munich. Thus, it was in an ideal location for establishing a specialized LCC airport close to Munich. Its geographical location combined with the circumstances of its conversion renders FMM an ideal testing ground to examine how new transport infrastructure influences tourism indicators in the (peripheral) counties around the airport.

To identify how Memmingen Airport influences tourism in the Allgäu region, we use the synthetic control approach to compare actual developments in tourism with a hypothetical situation, which would probably have arisen without the opening of the commercial airport. The synthetic control method is a powerful approach for comparative case studies when the number of treated units is small, and only aggregated outcomes are observable (Abadie and

and has been operating as a commercial airport since 1929. Bodensee Airport, however, cannot be described as an LCC airport for Munich such as Memmingen Airport. Passenger numbers at Friedrichshafen Airport have been fluctuating around an annual 550,000 since 2005. Most importantly, passenger numbers of the airport in Friedrichshafen were not altered by the opening of Memmingen Airport (figure A2.1 in the appendix). St. Gallen Airport in Switzerland is another small regional airport close to Friedrichshafen, but it has even smaller passenger numbers, which are constantly around 100,000. Innsbruck Airport in Austria and Memmingen Airport might have overlapping catchment areas in the Alps. Innsbruck Airport, however, also increased its passenger numbers since the opening of FMM. We conclude that other airports in the catchment area of Memmingen Airport are no close substitutes (figures A2.1 and A2.2 in the appendix).

Figure 2.1: Treatment and donor pool regions



Notes: The map shows the federal state of Bavaria with its touristic regions (black boundaries) and the Bavarian counties (gray boundaries). Blue counties form the treatment region of Allgäu. Green counties form the donor pool. White-shaded counties are not included because they are likely to be treated to some extent as well.

Gardeazabal, 2003; Abadie *et al.*, 2010, 2015; Chernozhukov *et al.*, 2018). The approach allows one to construct accurate counterfactuals of the counties of interest.[8] The identifying assumption in the present context is that tourism in the treated counties close to the new commercial airport would have evolved in the same manner as in their synthetic counterfactuals in a hypothetical world without the opening of the commercial airport. Synthetic controls for the treated counties are constructed by using lagged values of the outcome variable as predictors (Firpo and Possebom, 2018; Kaul *et al.*, 2018). The counterfactual outcome is determined as a weighted average of the untreated donor pool counties.[9] Counties from other Bavarian regions that are not affected by the new airport constitute the donor pool in order to construct the synthetic counterfactuals (figure 2.1). The difference in the outcome variable between treated counties and their synthetic counterfactuals following the treatment measures the causal effect of the airport if the following assumptions hold. First, there is a sufficient match between the trends in the outcome variable for synthetic and treated counties over a long pre-treatment period. We provide evidence for this fit in the next section. Second, there are no further interventions that affected treated and untreated counties differently in the treatment period. All counties are part of touristic regions in Bavaria. General policies of the Bavarian state government and actions of the Bavarian Tourism Marketing agency to attract tourists from abroad are supposed to target all Bavarian counties in the post-intervention period. Third, the counties of the donor pool are not affected by the treatment. Counties in touristic regions bordering the Allgäu and the capital Munich are not included in the donor pool. A passenger survey conducted at Memmingen Airport in 2018 shows that only up to 7% of all incoming passengers visit one of the 69 donor pool counties in the rest of Bavaria (Bauer *et al.*, 2019).[10] By estimating placebo treatment effects in the robustness tests, we show that tourism in donor pool regions is not affected by the opening of the new commercial Memmingen Airport.

We provide parametric estimates from a traditional difference-in-differences model using Weighted Least Squares (WLS) to discuss the significance of the causal inference. When estimating the model with WLS, we weight all counties with the weights derived by the synthetic control approach. In the robustness tests, we also discuss the results when estimating the difference-in-differences model with Ordinary Least Squares (OLS) where all counties receive an equal weight.[11]

[8] The synthetic control approach using algorithm-derived weights is supposed to describe better the characteristics of the counties of interest than any single comparison or an equally weighted combination of several control counties. Scholars, however, discuss caveats in the optimal selection of economic predictors for counterfactuals to avoid biased estimates (Kaul *et al.*, 2018).

[9] The synthetic control approach is described in technical detail in the appendix.

[10] If at all, the airport effect might be biased towards zero if tourists travel to donor pool regions.

[11] The method is described in technical detail in the appendix.

2.3.2 Data

We use county-level data on registered guest arrivals at touristic accommodations, including business travelers and guests with touristic motives. Guests who do not stay at a touristic accommodation, for example, those staying with friends and relatives, are not registered.[12] The main dependent variable is guest arrivals from abroad because domestic flights were discontinued since 2011. We also use data on total guest arrivals (including domestic and foreign arrivals). The dataset encompasses the period 1996-2016.[13] We therefore cover 11 years before the opening of the commercial airport (pre-treatment) and nine years afterwards (post-treatment). The year 2007, when commercial flights started operating, is excluded. We use four treatment regions: East Allgäu, Lower Allgäu, Upper Allgäu and West Allgäu.[14]

2.4 Results

2.4.1 Baseline

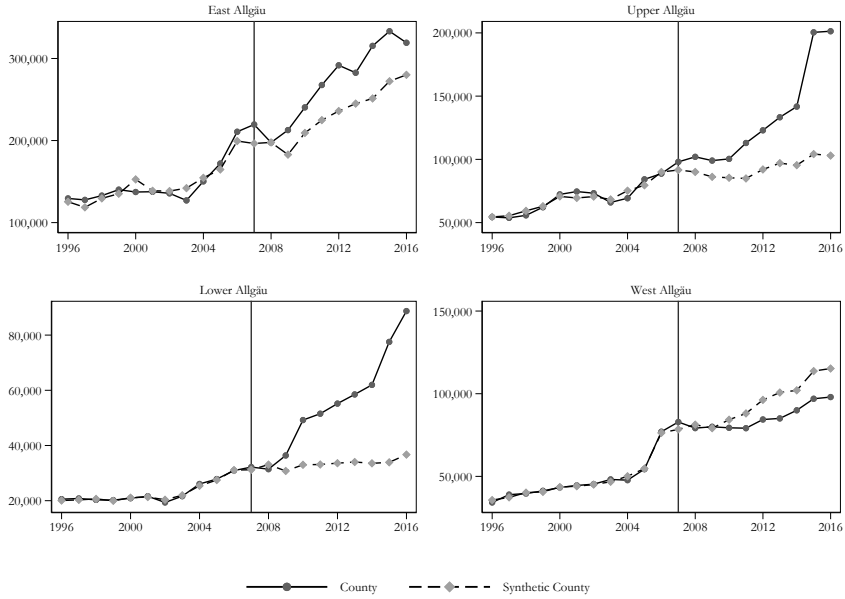
The results of the baseline synthetic control model are shown in figure 2.2 and table A2.2 in the appendix. We report results for guest arrivals from abroad in the four regions of East, Lower, Upper and West Allgäu. Table A2.2 in the appendix shows that the fitting procedure yields comparable outcomes in treatment and synthetic control units over the pre-treatment period. The ratios of arrivals between the real Allgäu regions and their synthetic counterfactuals amount to almost 100% in all four regions before 2007 (table A2.2 in the appendix). Figure 2.2 shows the pre-treatment matching trends graphically. Table A2.3 in the appendix shows the corresponding individual donor pool weights. The results indicate that the number of total arrivals increased in Lower, Upper and East Allgäu after FMM started operating, compared with their synthetic counterfactuals. The positive effect of Memmingen Airport on arrivals is in relative terms largest in Lower Allgäu, that is, in the counties where Memmingen Airport is based. More precisely, Memmingen Airport increased arrivals from abroad in Lower Allgäu by 69% in the 2008-2016 period. The positive effect of the airport on guest arrivals from abroad in Upper and East Allgäu is 45% and 17% (compare the ratios in table A2.2, column 2, in the appendix). In West Allgäu, however, the results do not suggest that Memmingen Airport increased the number of arrivals from abroad.

[12] Using arrivals at touristic accommodations as the dependent variable underestimates the total effect of the airport on tourism as about half of all incoming passengers reported visiting friends and relatives in a 2018 passenger survey at FMM (Bauer *et al.*, 2019).

[13] For a raw data plot, see figure A2.3 in the appendix.

[14] We merge rural counties and independent city counties in the treatment region because the independent city counties are regional centers and geographically enclosed by the rural counties: East Allgäu, including the rural county of Ostallgäu and the city of Kaufbeuren; Lower Allgäu, including the rural county of Unterallgäu and the city of Memmingen; Upper Allgäu, including the rural county of Oberallgäu and the city of Kempten; and West Allgäu, including the rural county of Lindau-Bodensee. For a detailed map see figure A2.4 in the appendix.

Figure 2.2: Synthetic control method, arrivals from abroad



Notes: This figure shows arrivals from abroad in the four treated regions of East Allgäu, Upper Allgäu, Lower Allgäu and West Allgäu (dark gray) and in their synthetic counterparts (light gray). The donor pool consists of counties in Bavaria that were not treated. The vertical line in each graph marks the opening of Memmingen Airport in 2007.

We compare the synthetic control results to estimates from a difference-in-differences model using WLS where we weight the observations in the regression with the weights derived by the synthetic control approach (for individual weights, see table A2.3 in the appendix). Hence, we apply the difference-in-differences estimation with the synthetic control group (Roesel, 2017). Estimating the effect of the airport on arrivals from abroad using WLS yields similar results to the pre-post-treatment differences of the synthetic control approach (panels A and B of table 2.1). When we use the parametric WLS model, the effect of the airport on guest arrivals from abroad is positive and significant in Upper and Lower Allgäu, but does not turn out to be statistically significant in East and West Allgäu (panel B in table 2.1). The results suggest that the opening of the commercial airport in Memmingen increased the number of guest arrivals from abroad compared with a counterfactual development without an airport by roughly 42,000 in Upper Allgäu and about 23,000 in Lower Allgäu per year over the 2008-2016 period.

Table 2.1: Difference-in-differences results using weighted least squares (WLS), arrivals from abroad

	Arrivals from abroad			
	(1) East Allgäu	(2) Upper Allgäu	(3) Lower Allgäu	(4) West Allgäu
A: Synthetic control group				
Pre-Post-Treatment difference	40,001	41,906	23,141	-9,863
B: Difference-in-differences (WLS)				
Allgäu · Airport	40,001	41,930***	23,141***	-9,911
	(44,659)	(3,422)	(4,968)	(11,059)
County fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	100	140	180	120
Within R^2	0.82	0.85	0.79	0.85

Notes: The table compares results from the synthetic control approach to difference-in-differences results. The synthetic control approach results in panel A are calculated from table A2.2 in the appendix as the difference in before-after treatment differences of the treated regions and their synthetic counterparts. Panel B shows the results of difference-in-differences estimations using a WLS regression with weights derived from the synthetic control method (see table A2.3 in the appendix). We use yearly data over the period of 1996 to 2016 (without 2007). Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

We also examine whether the opening of Memmingen Airport influenced total arrivals at touristic accommodations in the Allgäu region (including guests from domestic and abroad). Synthetic control results for total arrivals are very similar to those for arrivals from abroad (figure A2.5 in the appendix). Estimates using WLS, however, do not turn out to be statistically significant in East, West and Upper Allgäu. The Upper Allgäu county is by far the most popular region for domestic tourists in Bavaria (next to the capital, Munich). Thus, more arrivals from abroad may not translate into more total arrivals in Upper Allgäu. The results suggest that the positive effect of Memmingen Airport on total guest arrivals is only significant in Lower Allgäu, that is, in the counties where FMM is based. The opening of Memmingen Airport increased total guest arrivals in touristic accommodations in Lower Allgäu by year by 54,000 over the

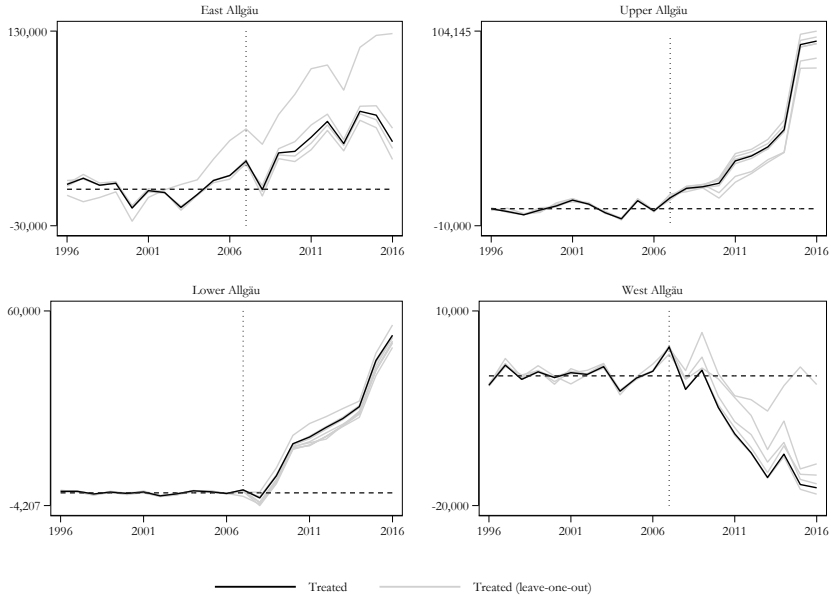
2008-2016 period (table A2.4 in the appendix). The ratio of real and synthetic total arrivals is 122% for Lower Allgäu over the treatment period 2008-2016 (table A2.2 in the appendix). Lower Allgäu had the lowest number of guest arrivals among all Allgäu regions. Hence, increasing tourism because of the airport is large in relative terms for Lower Allgäu, but, for example, not for the Upper Allgäu (figure A2.3 in the appendix). Moreover, the counties where Memmingen Airport is based may likewise benefit from incoming and outgoing passengers, for example, if passengers stay in accommodations close to the airport before their departure or after arrival.

2.4.2 Robustness

We submit the results to several robustness tests. First, following Abadie *et al.* (2015), we employ variations in the county weights by constructing leave-one-out distributions of the synthetic control for the Allgäu regions. We re-estimate the baseline model for every treated region and iteratively omit one county from the donor pool that received a positive weight. Results for this robustness test are shown in figure 2.3, which reproduces the baseline results (black line) from figure 2.2 with the light gray lines representing the leave-one-out estimates. We focus on the gap in arrivals from abroad between each treated region and its synthetic counterfactual, that is, we calculate the difference between the lines shown in figure 2.2. The estimates excluding individual donor pool counties follow the baseline estimates quite closely in all considered Allgäu regions. The leave-one-out distributions are particularly robust for the Upper Allgäu and Lower Allgäu regions. This finding is in line with the parametric WLS results that only show a significant effect of the airport on guest arrivals from abroad in the Upper and Lower Allgäu regions.

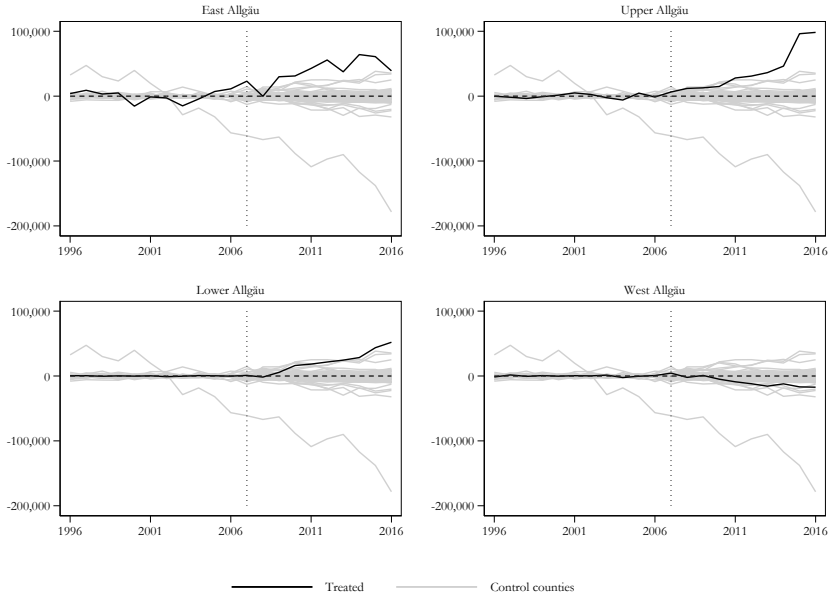
Second, we estimate placebo specifications to verify the validity of the estimation design. We iteratively apply the synthetic control method on every county of the donor pool using them as a placebo-treatment group. If donor pool counties are not affected by the treatment, we should not observe any differences in the development of tourism between the placebo-treatment and control groups, that is, we should estimate zero gaps in guest arrivals for every iteration. The results of this test are shown in figure 2.4, where every light gray line indicates one placebo estimate. This robustness check also corroborates the baseline findings showing that the previously estimated positive treatment effects on arrivals from abroad (black line) in the Allgäu regions are unusually large when compared with the bulk of placebo estimates. What is more, the large majority of placebo estimates reveals a good fit and also produces estimated zero gaps for the control counties. Thus, the selected control counties seem to be a valid comparison group for the treatment regions, since the opening of Memmingen Airport did not influence tourism or coincide with other shocks to touristic inflows in the selected donor pool counties. The positive treatment effect of Memmingen Airport on guest arrivals is indeed considerably larger in East, Lower and Upper Allgäu than in the placebo counties. On the one hand, this validates the choice of control units, but on the other hand this also increases confidence that the significant baseline estimates for the Upper and Lower Allgäu regions are indeed attributable to the opening of Memmingen Airport.

Figure 2.3: Robustness (I) – Leave-one-out



Notes: This figure shows the gap of arrivals from abroad between the treated regions and their synthetic counterfactuals. The black line represents the gap for the four treated regions of East Allgäu, Upper Allgäu, Lower Allgäu and West Allgäu (baseline synthetic control estimate). The light gray lines represent estimates from repeated synthetic control analyses while iteratively leaving out one donor pool county. The vertical line in each graph marks the opening of Memmingen Airport in 2007.

Figure 2.4: Robustness (II) – Placebo test



Notes: This figure shows the gap of arrivals from abroad between the treated regions and their synthetic counterfactuals. The black line shows the gap for the four treated regions of East Allgäu, Upper Allgäu, Lower Allgäu and West Allgäu. The light gray lines show 72 placebo gaps for each county in the donor pool. Nuremberg is omitted as an outlier, since it is the upper bound in guest arrivals of the donor pool counties. The vertical line in each graph marks the opening of Memmingen Airport in 2007.

Third, we compare the baseline results with estimates from a traditional *difference-in-differences* regression using OLS with equal weights of the counties in the control group. Estimating the impact of the airport using difference-in-differences gives rise to positive effects for arrivals from abroad in all the treated regions if we consider all 69 counties of the donor pool (panel A in table A2.5 in the appendix). Compared with the baseline results, the regions East and West Allgäu also experienced a significant positive increase of arrivals from abroad. For the regions of East and West Allgäu the common trend assumption of the difference-in-differences estimation is, however, not fulfilled. Figure A2.6 in the appendix shows the development of arrivals from abroad in the treatment and control regions between 1996 and 2016. Guest arrivals in the regions of East and West Allgäu experience an increase some years before the airport started operating, compared with the rest of Bavaria. For Upper and Lower Allgäu, in contrast, the common trend assumption fits quite well. Guest arrivals develop similarly compared with the rest of Bavaria before 2007 and start to diverge and increase after Memmingen Airport was opened.[15] In addition, we restrict the counties in the control group to counties that received non-zero weights in the synthetic control approach (but contribute now with an equal weight). The results turn out to be quite similar in economic terms and significance to the baseline estimates using WLS (table 2.1). When we use the restricted OLS model the effect of the airport on guest arrivals from abroad is again positive and significant in Upper and Lower Allgäu, but does not turn out to be statistically significant in East and West Allgäu (panel B in table A2.5 in the appendix).

2.5 Effects on overall economic development

The results show that new airport infrastructure increases registered arrivals at touristic accommodations. The synthetic control results suggest that every year around 95,000 additional registered guests from abroad arrived in the Allgäu region in the period 2008-2016 than would have been the case if the airport had not been opened (table A2.2 in the appendix).[16] The effect is significant and robust for the Upper and Lower Allgäu regions which amounts to 65,000 additional arrivals from abroad per year. An important question is how the increasing guest arrivals translate into higher revenues in the regional tourist industry. More guests may influence revenues in the tourist industry via numerous channels: they spend on food and accommodation, go shopping and demand, among others, local transport, amenities, spa and skiing, or cultural affairs. At the same time, expenditures in the regional touristic industry induce multiplier effects on other regional industries and often endorse regional economic development. A passenger survey conducted at FMM in 2018 shows that incoming passengers from abroad via Memmingen Airport spent about Euro 131 on average per day, whereas each

[15] Similar to Roesel (2017), we find that the results from the difference-in-differences and synthetic control method yield similar results if pre-treatment outcomes follow a common trend. However, if pre-treatment trends are not alike, the synthetic control method delivers more reliable results.

[16] The number 95,000 refers to the sum of the differences between the actual and synthetic arrivals from abroad of the four treatment regions in the period 2008-2016.

additional euro in expenditure by an incoming passenger increased purchasing power inflows by a multiplier of around Euro 1.43 in counties located around the airport (Bauer *et al.*, 2019).[17]

Increasing revenues in the tourism industry because of guest arrivals from abroad are arguably a lower bound of regional economic benefits generated by the opening of the commercial airport. Airport infrastructure is also likely to influence business location and investment decisions, and foster regional economic development by increased production and employment, accounting for the direct effects of production and employment at the airport itself, and indirect effects because of subcontractors benefiting from the new airport infrastructure (Hakfoort *et al.*, 2001; Klophaus, 2008; Zak and Getzner, 2014).[18] In any event, a commercial airport is attractive for tourists and business travelers and might influence business location decisions by helping to enhance a region's image or facilitate the recruitment of foreign professionals.[19] In 2018, Dorn *et al.* (2019a) conducted a survey asking local entrepreneurs about the extent to which their business benefits from Memmingen Airport and whether their investment decisions have been affected by the airport.[20] The results suggest some positive effects of Memmingen Airport on business connections. A total of 21% of the respondents believe that Memmingen Airport improved business connections and about one third reported

[17] The survey includes 1,002 incoming passengers at Memmingen Airport in 2018 (487 during the winter season; 515 during the summer season). Incoming passengers visiting the Allgäu region reported staying around 6.4 days per visit. This would sum up to around Euro 838 direct expenditures and additional Euro 361 indirect multiplier effects in the Allgäu region per incoming passenger from abroad. Considering the total of yearly (significant) 65,000 additional guest arrivals from abroad at accommodations and employing a back-of-the-envelope calculation, Memmingen Airport is supposed to increase direct and indirect tourism revenues by incoming guests from abroad in the Allgäu region by around Euro 77.9 million per year (all in 2018 prices). The calculation must be interpreted with caution as interviewed incoming passengers at the airport and registered guest arrivals at accommodations are different concepts. On the one hand, one incoming passenger may well count twice in the guest arrivals statistics if they stay in two different accommodations within the same region. On the other hand, average expenditures refer to all surveyed passengers staying at touristic accommodations or not. While the first could overestimate the economic effect, the latter would underestimate it.

[18] One may well want to investigate whether Memmingen Airport had any effect on the overall economic development in the Allgäu region. We cannot use synthetic control techniques to estimate the causal effect of Memmingen Airport on overall economic development measures such as gross domestic product (GDP), because the military air base that operated until the year 2003 also had economic impacts on the Allgäu region. The former air base hosted some 2,200 soldiers who stimulated local consumption. They needed to be supplied with necessities including food, etc., which were provided by local enterprises.

[19] Scholars examine the extent to which business travelers and tourists have similar preferences regarding airports and airlines. In the San Francisco Bay Area, preferences of business travelers and tourists were quite similar (Pels *et al.*, 2001).

[20] The survey asked participants in the monthly ifo Business Survey, whose enterprises are located in 28 counties around Memmingen Airport. The ifo Business Survey is conducted every month among 7,000 German firms; it provides the basis for the ifo Business Climate Index, Germany's leading business cycle indicator. Among a total of 7,000 German firms, 770 are located around Memmingen Airport and have been asked. The response rate was 30.5% (235 firms).

that the new airport infrastructure helped to improve conditions regarding location and attracting specialist workers from abroad. Breidenbach (2019), however, finds no evidence for spillover effects of regional airports on the surrounding economies in Germany.

Governments and public stakeholders often argue that subsidies and investments in new airport infrastructure pay off because of its regional economic impact. New airport infrastructure has many benefits, but also external costs: "the costs are clearly localized in terms of noise, reduced property values, and degradation of health and quality of life" (Cidell, 2015, pp. 1125f., see also Boes and Nüesch, 2011; Ahlfeldt and Maennig, 2015). Politicians should consider the total cost-benefit ratio and sustainability of public investment decisions in infrastructure projects.

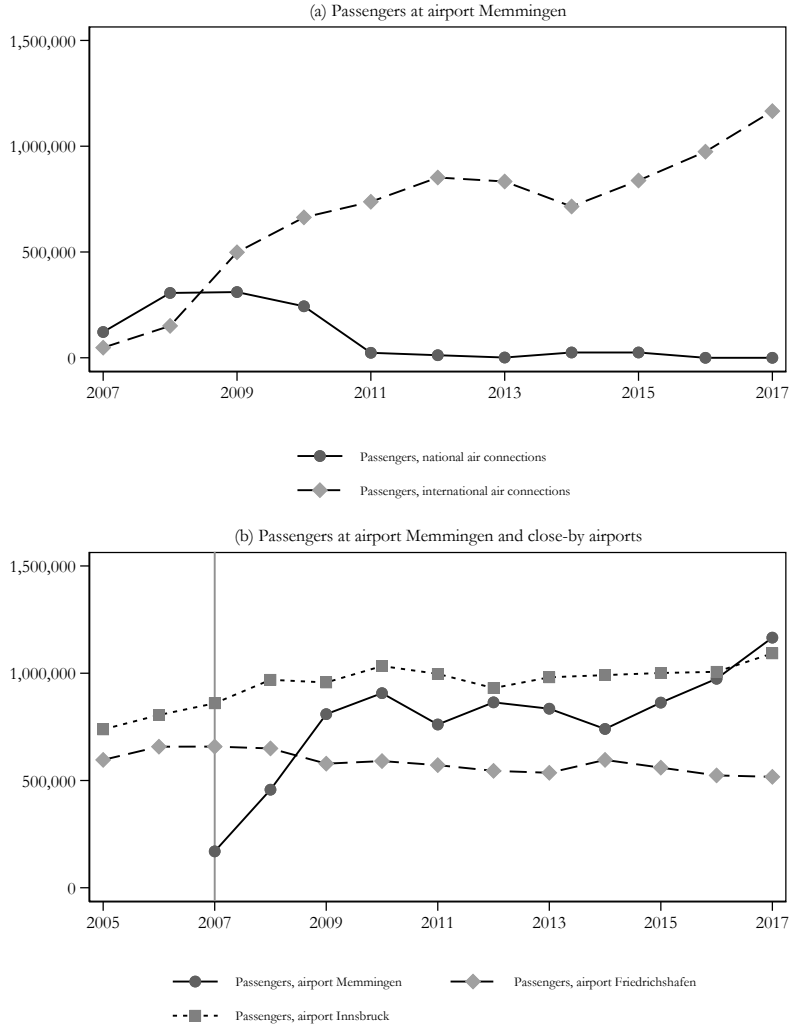
2.6 Conclusion

Scholars examine the extent to which new transportation infrastructure promotes economic development. Many studies describing the effects of airport infrastructure on economic development employed input-output methods or show correlations. Clearly, the input-output methods and correlations are useful when assessing the benefits of new airport infrastructure, but they do not measure causal effects. Studies examining the causal effect of new airport infrastructure on regional tourism are scarce. We employ a synthetic control approach and estimate how new airport infrastructure increases arrivals of tourists in the Bavarian (peripheral) region of Allgäu. Identification is based on converting a military airbase into the regional commercial airport Memmingen. The results show that additional tourist inflows are particularly pronounced and robust in the county where the airport is located and are driven by guest arrivals from abroad. The results suggest that new transportation infrastructure promotes regional economic development. The economic effects, however, might also differ among airports in their scale and direction (Allroggen and Malina, 2014), and may well depend on the geographical catchment area size and airport competition in multiple airport regions (see Pels *et al.*, 2001; Lian and Rønnevik, 2011; Wiltshire, 2018). Future research should employ empirical techniques to estimate causal effects of new airport infrastructure in other regions and on other economic outcome variables such as employment and production.

Appendix

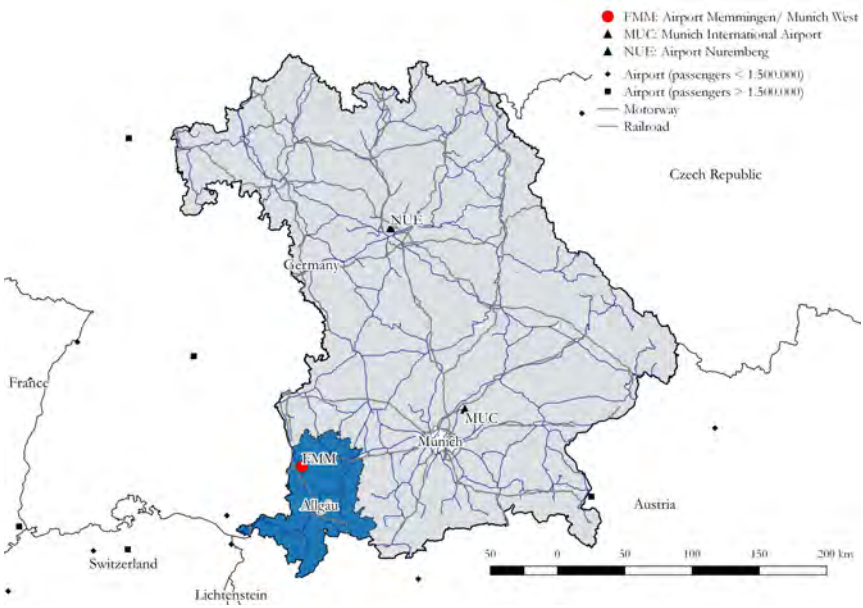
Figures and tables

Figure A2.1: Passengers at Memmingen Airport and close-by airports



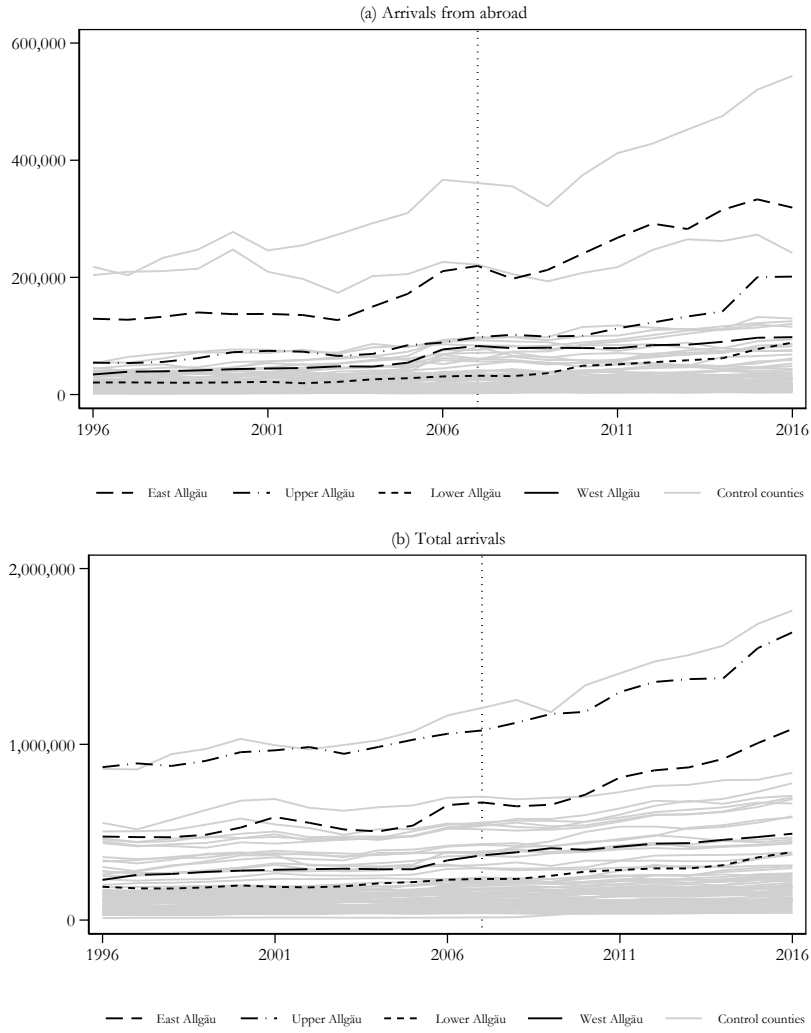
Notes: Figure A2.1a shows the development of passengers at national air connections (dark gray) and international air connections (light gray) at the Airport Memmingen. Figure A2.1b shows the development of passengers overall at the Airport Memmingen and the close-by airports Friedrichshafen and Innsbruck.

Figure A2.2: Map of Bavaria



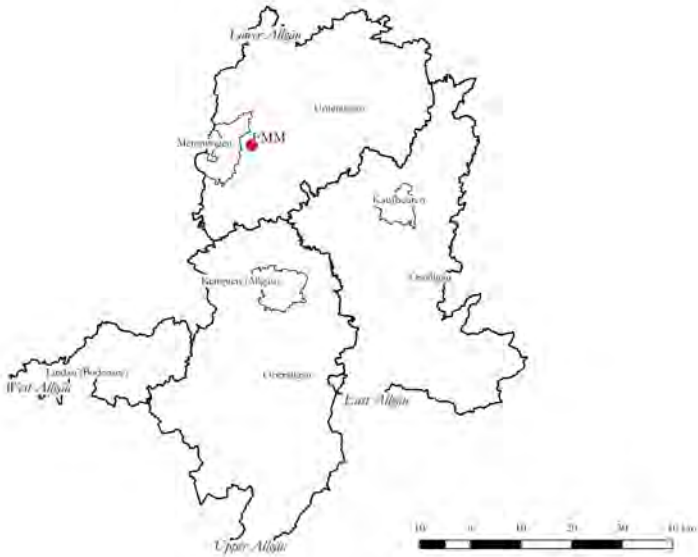
Notes: The map shows the federal state of Bavaria (light gray) with its two international airports in Munich and Nuremberg and the regional airport in Memmingen (red circle). Gray lines show the motorway network, blue lines the railroad network in Bavaria. The blue region (Allgäu) is our treatment region. Passenger numbers of 2018.

Figure A2.3: Raw data plots



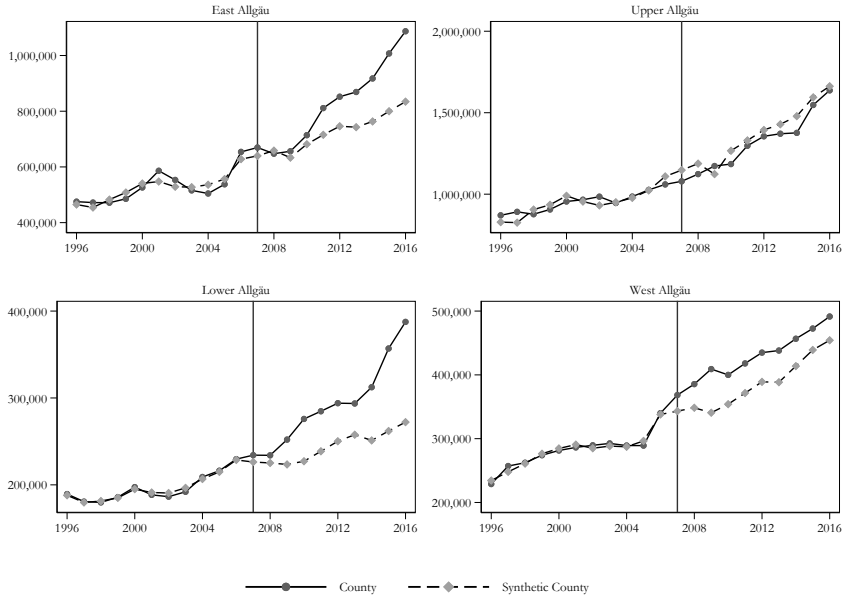
Notes: This figure shows how the two dependent variables evolve over our period of investigation. Black lines represent treated counties, light gray lines control counties (see figure 2.1).

Figure A2.4: Treatment regions



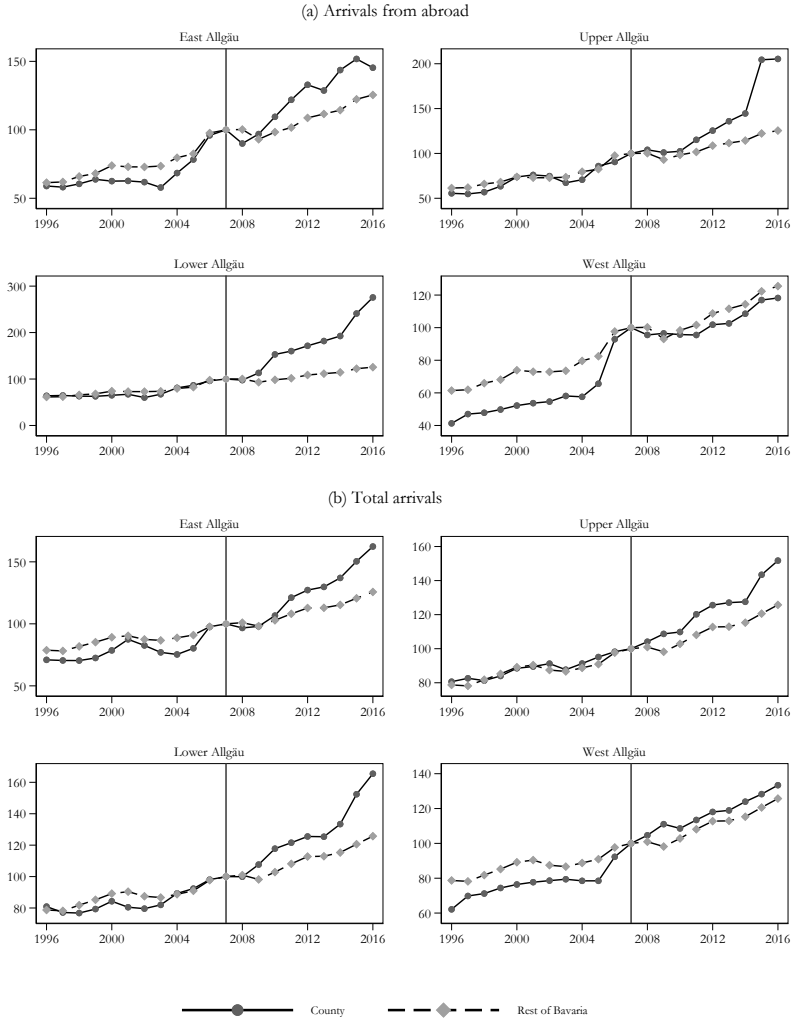
Notes: This map shows the treatment regions (italic, thick boundaries) and their counties (thin boundaries): Lower Allgäu (Memmingen and Unterallgäu), East Allgäu (Kaufbeuren and Ostallgäu), Upper Allgäu (Kempten (Allgäu) and Oberallgäu) and West Allgäu (Lindau (Bodensee)).

Figure A2.5: Synthetic control method, total arrivals



Notes: This figure shows total arrivals in the four treated regions of East Allgäu, Upper Allgäu, Lower Allgäu and West Allgäu (dark gray) and their synthetic counterparts (light gray). The donor pool consists of counties in Bavaria that were not treated. The vertical line in each graph marks the opening of the Airport Memmingen in 2007.

Figure A2.6: Development of arrivals in Bavarian regions, 1995-2016



Notes: This figure shows the development of total and abroad arrival in the four treated regions of East Allgäu, Upper Allgäu, Lower Allgäu and West Allgäu (2007=100). Donor pool counties form the control group (see figure 2.1). The vertical line in each graph marks the opening of the Airport Memmingen in 2007. We use yearly data over the period of 1996 to 2016.

Table A2.1: Passengers at Memmingen Airport in 2018, by destination and source country

	Passengers			Incoming Share of Passengers	
	Total Volume	Outbound Flights	Incoming Flights	Winter Season	Summer Season
Total	1,486,493	737,908	748,585	46%	35%
Spain	241,465	121,097	120,368	26%	18%
Romania	178,347	87,041	91,306	58%	52%
Bulgaria	142,208	70,001	72,207	58%	40%
Portugal	99,223	49,767	49,456	28%	15%
United Kingdom	92,635	47,241	45,394	35%	30%
Ukraine	89,977	44,056	45,921	70%	58%
Serbia	78,556	38,869	39,687	74%	45%
Italy	74,007	37,010	36,997	22%	25%
Macedonia	59,575	29,349	30,226	69%	30%
Greece	55,831	27,955	27,876	25%	15%
Poland	48,659	24,032	24,627	33%	53%
Ireland	45,189	22,604	22,585	50%	24%
Bosnia and Herz.	43,491	21,309	22,182	37%	34%
Russia (Europe)	43,074	22,025	21,049	71%	64%
Marocco	36,586	18,495	18,091	50%	30%
Montenegro	32,710	15,803	16,907	50%	30%
Sweden	32,137	16,115	16,022	19%	25%

Source: Bauer *et al.* (2019); Federal Statistical Office (2019).
Notes: This table shows passenger numbers for outbound and incoming flights in total and for selected countries as well as the share of incoming passengers in the winter and summer season at the Airport Memmingen in 2018.

Table A2.2: Descriptive statistics

	Arrivals from abroad		Total arrivals	
	(1) Before 2007	(2) After 2007	(3) Before 2007	(4) After 2007
West Allgäu	46,768	85,797	280,982	434,06
Synthetic West Allgäu	46,756	95,647	280,946	388,83
Ratio	100.03%	89.70%	100.01%	111.63%
East Allgäu	145,527	273,391	525,653	840,258
Synthetic East Allgäu	145,405	233,268	525,031	730,588
Ratio	100.08%	117.20%	100.12%	115.01%
Upper Allgäu	68,588	134,901	951,71	1,340,634
Synthetic Upper Allgäu	68,734	93,141	948,213	1,385,142
Ratio	99.79%	144.83%	100.37%	96.79%
Lower Allgäu	22,745	56,714	195,918	299,033
Synthetic Lower Allgäu	22,699	33,527	196,173	245,308
Ratio	100.20%	169.16%	99.87%	121.90%

Notes: This table shows the absolute numbers of arrivals from abroad and total arrivals for the four treated regions of East Allgäu, Upper Allgäu, Lower Allgäu and West Allgäu and their synthetic counterparts. For the composition of the synthetic regions see table A2.3 in the appendix. We use yearly data over the 1996-2016 period (without 2007).

Table A2.3: Synthetic control donor pool weights

Donor pool	Weights							
	Arrivals from abroad				Total arrivals			
	(1) West Allgäu	(2) East Allgäu	(3) Upper Allgäu	(4) Lower Allgäu	(5) West Allgäu	(6) East Allgäu	(7) Upper Allgäu	(8) Lower Allgäu
Rosenheim	0	0	0.057	0	0	0	0	0
Berchtesgadener Land	0.447	0	0	0	0.100	0	0	0
Ebersberg	0	0	0	0.144	0	0	0	0
Eichstätt	0	0.355	0.065	0.106	0.002	0.580	0	0
Miesbach	0	0	0.454	0	0.011	0	0	0.160
Rosenheim	0.369	0.167	0	0	0	0.020	0	0
Landshut	0	0	0	0.153	0	0	0	0.095
Passau (city)	0	0	0.133	0	0.217	0	0	0
Freyung-Grafenau	0	0	0	0	0	0	0	0.265
Passau (county)	0	0	0	0	0	0.156	0	0
Dingolfing-Landau	0	0	0	0.091	0	0	0	0
Regensburg	0.166	0	0	0	0.020	0	0	0
Hof	0	0	0	0	0	0	0	0.127
Erlangen	0	0	0	0.168	0	0	0	0
Fürth (city)	0	0	0	0.091	0	0	0	0
Nuremberg	0.010	0.381	0	0	0.097	0.244	0.910	0
Ansbach	0.007	0.097	0.082	0.004	0	0	0.090	0.009
Fürth (county)	0	0	0	0	0	0	0	0.320
Weißenburg-Gunzenhausen	0	0	0	0	0.553	0	0	0
Würzburg	0	0	0.210	0	0	0	0	0.024
Schweinfurt	0	0	0	0.243	0	0	0	0

Notes: This table shows the weights derived from the synthetic control approach for the four treated regions of East Allgäu, Upper Allgäu, Lower Allgäu and West Allgäu, and the two dependent variables total arrivals and arrivals from abroad. We omit counties that have never received a positive weight in any specification.

Table A2.4: Difference-in-differences resutls using WLS, total arrivals

	Total arrivals			
	(1) East Allgäu	(2) Upper Allgäu	(3) Lower Allgäu	(4) West Allgäu
A: Synthetic control group				
Pre-Post-Treatment difference	109,048	-48,006	53,979	45,194
B: Difference-in-differences (WLS)				
Allgäu · Airport	109,048	-48,006	53,979*	45,194
	(106,094)	(69,229)	(27,25)	(49,923)
County fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	100	60	160	160
Within R^2	0.83	0.96	0.76	0.68

Notes: The table compares results from our synthetic control approach to difference-in-differences results. Synthetic control approach results in panel A are calculated from table A2.2 in the appendix as the difference in before-after treatment differences of the treated regions and their synthetic counterparts. Panel B shows the results of four difference-in-differences estimations using a WLS regression with weights derived from the synthetic control method (see table A2.3 in the appendix). We use yearly data over the 1996-2016 period (without 2007). Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

Table A2.5: Robustness (III) – Difference-in-differences using OLS, arrivals from abroad

	Arrivals from abroad			
	(1) East Allgäu	(2) Upper Allgäu	(3) Lower Allgäu	(4) West Allgäu
A: All counties from donor pool				
Allgäu · Airport	116,015***	54,465***	22,121***	27,180***
	(2,632)	(2,632)	(2,632)	(2,632)
County fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	1.460	1.460	1.460	1.460
Within R^2	0,35	0,25	0,22	0,22
B: Only synthetic counterpart counties				
Allgäu · Airport	57,268	44,058***	19,873***	-22,589
	(34,967)	(5,147)	(4,721)	(30,018)
County fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	100	140	180	120
Within R^2	0.69	0.68	0.50	0.53

Notes: The table reports difference-in-differences results using OLS. In panel A all counties from the donor pool form the control group (see figure 2.1). In panel B only the counties that received a weight in the synthetic control approach form the control group (see table A2.3 in the appendix) but each receive a weight of 1. We use yearly data over the 1996-2016 period (without 2007). Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

Synthetic control approach

The synthetic counterfactual is calculated as a weighted average of the untreated control counties from the donor pool such that the fit in the variable of interest in the pre-treatment period is maximized. The counterfactual outcome $\hat{Y}_{it}$ of county i in period t is determined by a weighted average of the untreated donor pool counties j:

$$\hat{Y}_{it} = \sum_{i \neq j} w_j Y_j, \qquad \sum w_j = 1 \tag{A2.1}$$

The counterfactual weights w across all donor pool counties j sum up to unity and are selected to minimize the pre-treatment Root Mean Square Prediction Error (RMSPE) of the observed pre-treatment outcome of the treated county Y_{it} and the counterfactual pre-treatment outcome of its synthetic county $\hat{Y}_{it}$

$$\min RMSPE_i = \min \sqrt{\sum_{t=1}^{T_0} \frac{(Y_{it} - \hat{Y}_{it})^2}{T_0}} \tag{A2.2}$$

The synthetic control estimator is given by the comparison between the outcome for the treated county and the outcome for the synthetic control county at the post-treatment period t (with $t \geq T_0$):

$$Y_{it} - \hat{Y}_{it} \tag{A2.3}$$

The difference in the outcome variable between treated counties and their synthetic counterfactuals following the treatment measures the causal effect of the airport if the following assumptions hold: first, there is a sufficient match between the trends in the outcome variable for synthetic and treated counties over a long pre-treatment period. That is, the RMSPE in equation (A2.2) is sufficiently minimized. Second, no further interventions affected treated and untreated counties unevenly in the treatment period.

Difference-in differences approach

Our difference-in-difference model takes the following form:

$$Y_{it} = \alpha_i + \theta_t + \gamma(Allgäu_i \cdot Airport_t) + \epsilon_{it} \tag{A2.4}$$

where Y_{it} describes our dependent variables arrivals in county i and year t (1996-2016). $Allgäu_i$ is a dummy variable that takes on the value one for our treatment counties in the touristic region Allgäu and zero otherwise, while $Airport_t$ is a dummy variable denoting the years after the Memmingen Airport was opened (2008-2016) with one, and zero otherwise. $Allgäu_i \cdot Airport_t$ measures the interaction of the two dummies and γ thus estimates our treatment effect. We include county and year fixed effects (α_i and θ_t). The coefficient γ can be interpreted as a causal effect of the airport if the common pre-trend assumption between the treated counties and the control group holds.

We estimate equation (A2.4) with Weighted Least Squares (WLS) and Ordinary Least Squares (OLS) and use three different control groups. WLS and OLS regressions differ in their regression weights. First, we estimate WLS where we combine the synthetic control approach with the difference-in-differences estimation. We use the donor pool weights derived from the synthetic control approach as regression weights (the counties in the control group are weighted according to table A2.3 in the appendix). Second, we estimate a difference-in-differences model using OLS where all counties from our donor pool are included (green counties, see figure 2.1) and contribute with equal weights to the control group. Third, we estimate a difference-in-differences model using OLS where only the counties that received a weight in our synthetic control approach are included in our control group, but all with an equal weight.

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

Abstract

We examine how highway accessibility influences tax policy. We exploit the stagewise expansion of the "Baltic Sea highway" in the East German state Mecklenburg-Western Pomerania as the largest contiguous highway construction project in Germany since 1945. For non-agglomeration municipalities that lie on a convenient route between two larger cities the access and opening year are close to random. Results from difference-in-differences estimations and an event study approach show that highway access influences local tax setting in municipalities within 5 to 10 km road distance. Improved accessibility increases property tax factors persistently by roughly 6 percentage points. Our effects are driven by peripheral municipalities, while we do not find an influence on core municipalities. Additionally, improved accessibility gives rise to a shift of population and economic activity from the periphery to the core.

[1] This chapter is joint work with Luisa Doerr. It is based on our paper "Does Highway Access Influence Local Tax Factors? Evidence from German Municipalities", *ifo Working Paper*, 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 9th 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, policymakers might react to changing local economic conditions and adapt their tax policies. If, for example, the agglomeration benefits outweigh the deagglomeration force, agglomeration rents can be taxed.

The theoretical prediction of taxable agglomeration rents has been widely established by the new economic geography literature (see e.g., Krugman, 1991; Baldwin and Krugman, 2004). While empirical contributions have shown a reduced sensitivity of firm location to corporate taxes in the presence of agglomeration economies (Devereux *et al.*, 2007; Jofre-Monseny and Solé-Ollé, 2012; Brülhart *et al.*, 2012), more direct assessments of whether local policymakers tax agglomeration rents are relatively scarce. Notable exceptions include Charlot and Paty (2007) and Koh *et al.* (2013), who both find that agglomeration effects increase local tax rates. Similar to Charlot and Paty (2007), we argue that market access is the main agglomerating force that affects local taxation. We consider business and property tax factors[2] that are set by local policymakers. To test the relationship between tax factors 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[3] economic development (Banerjee *et al.*, 2020; Ahlfeldt and Feddersen, 2018; Hornung, 2015; Donaldson and Hornbeck, 2016), 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.*, 2018). Highways have also 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] The actual business and property tax rates are determined by multiplying the tax factor with a uniform base rate.

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

Our empirical study relates to the new economic geography literature predicting taxable agglomeration rents and the literature evaluating the effects of new transportation infrastructure. Contrary to previous contributions to infrastructure evaluation, we investigate how infrastructure development influences economic policies rather than economic outcomes. In a new economic geography framework, infrastructure development, i.e., a reduction in trade costs, might create taxable rents due to agglomeration economies. When examining possible channels that drive the tax policy effect, we test for variables that have been shown to react to new transportation infrastructure, e.g., employment (Lin, 2017; Möller and Zierer, 2018; Duranton and Turner, 2012), population (Baum-Snow, 2007; Ángel Garcia-López *et al.*, 2015), commuting flows (Baum-Snow, 2010; Heuermann and Schmieder, 2019) and house prices (Mikelbank, 2004).

Our sample covers the period 1995-2015 in the German state of Mecklenburg-Western Pomerania (MV). More specifically, 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 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; Banerjee *et al.*, 2020; Faber, 2014; Möller and Zierer, 2018). 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 municipalities located within a road distance of 5-10 km to the next highway access increase their property tax factors. 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. In any event, when examining possible channels, we provide some suggestive evidence of counteracting effects between central and peripheral municipalities. It seems that central localities benefit from highway accessibility in terms of population and employment effects, but do so at the expense of the periphery.

3.2 Theoretical considerations and hypothesis

The new economic geography literature shows that improved accessibility can have ambiguous effects on tax policies (Fujita *et al.*, 2001; Krugman, 1991). 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 could lose economic activity (competition or deagglomeration effect). Whether the market potential or the competition effect prevails could depend 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.* (2018), 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, and vice versa for core regions. If deagglomeration forces dominate, one might expect municipalities to reduce tax factors to stay competitive or to increase tax factors to sustain their tax revenues. If, on the other hand, agglomeration forces should dominate, business and property tax factors might increase as agglomeration rents can be taxed (see Baldwin and Krugman 2004; Krogstrup 2008; Kind *et al.* 2000; Ludema and Wooton 2000 for theoretical studies and Luthi and Schmidheiny 2014; Buettner 2001; Charlot and Paty 2007 for empirical investigations). Thus, it is not clear from a theoretical point of view whether and how increased accessibility affects tax factors. We will assess this question empirically.

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.[4] The tax liability then results from multiplying the tax factor with a tax assessment base. For property tax, this assessment base is in turn determined by the rateable value of real estate (*Einheitswert*), which was last assessed in 1935 for East Germany and in 1964 for West Germany.[5] The assessment base for the business tax is the companies' profits. Along with the income and value-added tax, business and general property taxes are important sources of municipal revenue. In 2015, the business tax and income tax[6] amounted to 38% and 37% of municipality tax revenue in the German state of Mecklenburg-Western Pomerania. The general property tax had a share of 16%. The property tax for agriculture is rather unimportant and contributed merely 1.5% of all tax revenues.

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 Western 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 1930's. In construction plans from 1926, two streets connect Lübeck with Stettin[7], 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

[4] In the GDR, the business tax was not a municipal tax: it was a federal tax with a uniform tax factor of 400%. The property tax was a municipal tax in the sense that its revenues accrued to the municipalities only, but tax factors for both types, general and agricultural property tax, were uniform across the GDR (Duda, 2010).
[5] Adherence to this outdated reference point actually led to the Constitutional Court ruling in April 2018 that these provisions violated the general principle of equality.
[6] Municipalities cannot change the tax factors for income tax.
[7] Szczecin, Poland.

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[8]; 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 3.1). 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.[9] Other highways that run through MV and have already been open in 1992 are the BAB 24, connecting 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 3.1 shows the highway network in MV in the year 1995 (gray) and 2015 (black). Further, it shows the change in municipalities' road distance to the highway network. Darker shaded areas mark those municipalities with the highest distance changes. 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 53 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 25 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.

3.4 Empirical analysis

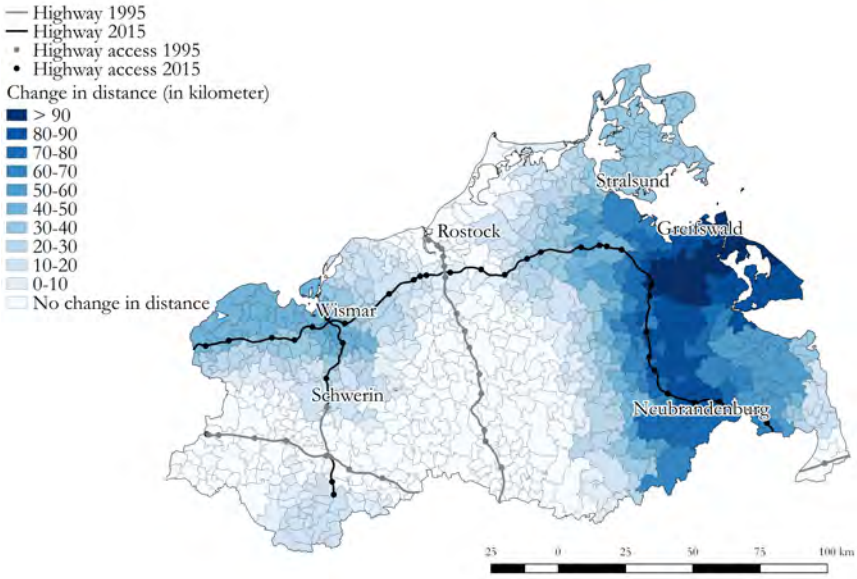
3.4.1 Data and sample

We use a panel of yearly fiscal and geographical data at the municipal level in Mecklenburg-Western Pomerania. As dependent variables and tax policy measures we use tax factors for business and property tax. Due to the small revenue share of the property tax for agriculture, we focus on the general property tax. 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.

[8] 196 km in Lower Saxony are still planned

[9] In 2006, BAB 241 was renamed and became part of the BAB 14.

Figure 3.1: Highway network in Mecklenburg-Western Pomerania, 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.

We focus on the time period between 1995 and 2015. 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).[10] There have been several municipality reforms during this period. We adjust the data to the territorial status of 2015.[11] We exclude the cities Schwerin, Rostock, Wismar, Stralsund, Greifswald, and Neubrandenburg for two reasons. First, they have been consolidated city-counties until 2011 (after 2011, only Schwerin and Rostock remained consolidated city-counties). Consolidated city-counties 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, after adjusting for municipality reforms and excluding consolidated city-counties, includes 745 municipalities over the period 1995-2015.

We control for lagged demographic and electoral control 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, since the demand for public expenditures may vary over the life cycle (Buettner, 2001). As electoral controls, we use the share of left-wing votes[12] 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 potential redistributive motives of left-wing governments (Krause and Potrafke, 2020). In a robustness test, we further include the share of the unemployed.[13] 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 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.

Table 3.1 shows the descriptive statistics. We use data in levels. The business tax factor was on average 292, ranging between 100 and 450. The average property tax factor (316) was slightly higher and varied between 200 and 600. 12% of all municipalities lie within a road distance of 10 km to the next highway access point.

[10] By starting our panel in 1995, we also avoid possible tax-mimicking between East and West German municipalities after reunification. Baskaran (2019) shows that East-German municipalities at the inner-German border mimicked tax rates from West German municipalities after reunification, but only in the first two years and only for the business tax.

[11] Our baseline results are unchanged when we exclude all merged municipalities from our sample; see table A3.2 in the appendix.

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

[13] Unemployment rates at the municipal level are available since 1998; for 37 observations, there are no unemployment data available due to municipality mergers.

Table 3.1: Descriptive statistics

	Obs.	Mean	SD	Min	Max
Dependent variables					
Business tax factor	15,645	291.83	38.14	100.00	450.00
Property tax factor	15,645	316.11	27.75	200.00	600.00
Highway access (<10 km)					
Access (yes = 1)	15,645	0.12	0.32	0.00	1.00
Control variables					
Population (log)	15,645	6.74	0.98	4.62	10.46
Age: < 15, share	15,645	13.71	3.37	3.33	31.27
Age: between 15 and < 25, share	15,645	11.46	3.47	0.98	40.18
Age: between 25 and < 40, share	15,645	19.05	4.16	5.15	41.88
Age: between 40 and < 65, share	15,645	38.41	6.03	12.37	62.14
Population density	15,645	52.11	63.88	4.91	632.94
Election county assembly, share left	15,645	46.17	6.89	32.50	61.80
Election state assembly, share left	15,645	57.64	4.62	50.40	62.70
Election mayor, share left	15,645	17.09	19.40	0.00	100.00
Further control variables					
GDP Poland · distance	15,645	7,344.06	26,243.78	546.88	683812.00
Unemployment rate	13,410	11.81	5.12	0.00	36.42

Notes: The 745 municipalities of the German state of Mecklenburg-Western Pomerania are our observation units. We use yearly data between 1995 and 2015. The dummy variable *Access* equals one when a municipality is within a certain 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 tax factors, we exploit variation across space and variation in time, since the highway was opened in different segments throughout our period of study (see figure A3.1 in the appendix). We estimate the following difference-in-differences model:

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

with τ_{it} as our dependent variable, the local tax factor 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 1 when a municipality is within a road distance of 10 km to the next highway access point, and zero otherwise.[14] X'_{it} is a vector of location and time-specific covariates

[14] Additionally, we estimate specifications for $Access_{it}$ with road distance of 5 km, 15 km, 0-5 km, 5-10 km and 10-15 km as well as continuous, see table 3.6.

(see section 3.4.1). δ_i denotes location-specific time-invariant unobservables (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' choice of tax factors.

Identification relies on the main assumption that municipalities with a highway access would have evolved similar 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:

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

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. $Access_{it}^{j}$ takes on the value of 1 when a municipality i is within a road distance of 10 km to the next highway access point in $(t + j)$ years and 0 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 not only enable us to test the common trend assumption equation (3.1) rests on, they 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 endogeneous to regional patterns because they are built to connect economic units. Location-specific factors, like productivity or amenity, which are generally unobserved, may influence the location of infrastructure and the choice of tax factors (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; Banerjee *et al.*, 2020; Faber, 2014; Möller and Zierer, 2018). Non-agglomeration 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[15], 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

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

the highways in MV connect the larger 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, and it covered 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 municipality 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 (3.1).

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.[16] 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 have 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 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 3.3 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, unemployment rate, number of firms, employment, commuter pattern and election outcomes

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

Table 3.2: Sorting into treatment – t-tests

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)	6.78	6.73	0.05	0.10	745
Poulation age 15-65 (log)	6.39	6.34	0.04	0.10	745
Population density	52.55	49.28	3.27	6.33	745
B: Location of municipalities	Mean central municipalities	Mean peripheral municipalities	Diff.	SD	Obs.
Population (log)	6.77	6.81	-0.05	0.24	745
Poulation 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. Significance levels: *** 0.01, ** 0.05, * 0.10 (no significant values to report).

do not turn out to significantly alter the hazard rate. We conclude that pre-reform characteristics do not predict sorting into treatment. Also, municipalities that are classified as tourism destinations do not turn out to statistically influence the hazard rate.[17]

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.

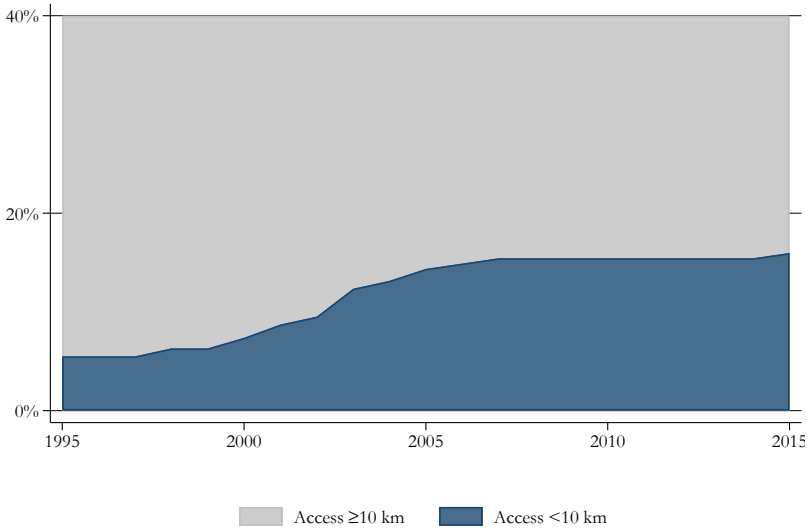
3.5 Results

3.5.1 Difference-in-differences

Table 3.4 columns 1 and 4 show our baseline regression results for the property tax factor and the business tax factor. All specifications include municipality-fixed effects to account for variation in average tax factors between municipalities and year-fixed effects to address temporary shocks that are common to all municipalities. Differences in local tax factors could be a result of different local preferences for public goods. 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 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.

[17] Tourism destinations are municipalities listed as resorts, health resorts, spa, coastal resorts, and coastal health resorts by the Statistical Office of Mecklenburg-Western Pomerania in 2015.

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.

Table 3.3: Sorting into treatment – Cox regression

	(1)	(2)	(3)	(4)
Population (log)	-1.29	-1.40	-1.32	-0.83
	(1.69)	(2.27)	(2.33)	(2.35)
Population age 15-65 (log)	1.27	0.93	0.87	0.48
	(1.67)	(2.41)	(2.52)	(2.51)
Population density	-0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Unemployment rate		0.02	0.02	0.02
		(0.03)	(0.04)	(0.04)
Firms (per capita)		-22.82	-23.26	-11.92
		(14.23)	(14.64)	(17.57)
Employed place of residence (log)		0.37	0.38	0.29
		(0.83)	(0.84)	(0.85)
Employed place of work (log)		0.22	0.23	0.22
		(0.39)	(0.39)	(0.38)
Inbound commuter (log)		-0.12	-0.12	-0.15
		(0.34)	(0.33)	(0.33)
Outbound commuter (log)		-0.13	-0.13	-0.14
		(0.13)	(0.13)	(0.13)
Election mayor, share left			-0.00	-0.00
			(0.01)	(0.01)
Election county assembly, share left			-0.01	-0.01
			(0.03)	(0.03)
Tourism destination				-1.71
				(1.26)
Pseudo R^2	0.00	0.01	0.01	0.01
Observations	13,909	9,751	9,751	9,751

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. Significance levels (standard errors in brackets): *** 0.01, ** 0.05, * 0.10 (no significant values to report).

For the property tax factor, the influence of highway access is positive and statistically significant at the 1% level. Municipalities whose road distance to the next highway access does not exceed 10 km have property tax factors that are on average 6.2 percentage points higher compared to less accessible municipalities. This effect corresponds to roughly 2% relative to the average property tax factor and is therefore economically sizable, but moderate. For the business tax factor (column 4), however, we do not find any statistically significant effect of improved highway accessibility. Generally, the baseline model fits the property tax factor relatively well, accounting for 49% of its within variation, while explaining only 30% of the within variation in business tax factors.

Strengthening the argument of the inconsequential units approach, we in turn exclude central and predominantly urban municipalities.[18] 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

[18] The spatial categories are defined by the Federal Institute for Building, Urban Affairs, and Spatial Research (*BBSR*).

Table 3.4: Baseline results

	Property tax factor			Business tax factor		
	(1)	(2)	(3)	(4)	(5)	(6)
Access (<10 km)	6.21***	6.89***	6.26***	2.74	4.16	2.69
	(2.24)	(2.59)	(2.24)	(3.16)	(3.60)	(3.16)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Central mun.	Yes	No	Yes	Yes	No	Yes
Urban mun.	Yes	Yes	No	Yes	Yes	No
Within R^2	0.49	0.49	0.49	0.31	0.31	0.31
Number of mun.	745	728	733	745	728	733
Observations	14,900	14,560	14,660	14,900	14,560	14,660

Notes: The table shows the results of difference-in-differences estimations. The 745 municipalities of the German state of Mecklenburg-Western Pomerania are our observation units. We use data in levels over the period 1995 to 2015. Our variable of interest (Access) takes on the value of one for municipalities within a road distance of 10 km to the next highway access point, and zero otherwise. Columns 1 and 4 show our baseline specification; columns 2 and 5 show regression results for the sub-sample where municipalities classified as being "central" (location) are excluded; columns 3 and 5 show regression results for the sub-sample where municipalities classified as being "predominantly urban" (structure) are excluded. Control variables are lagged demographic and political variables; see table 3.1. Significance levels (standard errors robust to hereroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

area measures. 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 11% increase in the coefficient for the 10 km dummy in column 2 of table 3.4 when excluding these municipalities is sizable. The effect size corresponds to 2.2% of the average property tax factor, while the respective coefficient for the business tax factor (column 5) again does not turn out to be statistically significant. Thus, the overall positive property tax differential after the highway opening seems to be primarily driven by (very) peripheral localities without immediate access to populous and economically active urban centers. Structural factors, i.e., whether the municipality itself is of (predominantly) urban or rural type, however, do not play a crucial role. Results in columns 3 and 6 closely resemble those in columns 1 and 4.

In an attempt to corroborate the baseline findings of table 3.4, we conduct several robustness checks. 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 tax factors even in the absence of the construction of the BAB 20. However, these concerns are not supported by the results as columns 1 and 4 in table 3.5 demonstrate. For the property tax factor, the positive differential between accessible and less accessible municipalities persists. Compared to the baseline estimates, coefficients are slightly smaller, suggesting that part of the treatment effect is absorbed by the time trends. Second, we extend the set of control variables and control for the unemployment rate and

market access to Poland. Since the unemployment rate could at the same time be influenced by highway accessibility, we include it with a lag. As columns 2 and 5 in table 3.5 show, results remain unchanged. Third, we exclude all control variables (table 3.5, columns 3 and 6). The results fairly reproduce our baseline findings.

Table 3.5: Robustness

	Property tax factor			Business tax factor		
	(1)	(2)	(3)	(4)	(5)	(6)
Access (<10 km)	5.93***	5.64**	6.24***	2.19	0.70	3.16
	(2.24)	(2.35)	(2.25)	(3.18)	(3.08)	(3.18)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	No	Yes	Yes	No
Time trend	Yes	No	No	Yes	No	No
Further controls	No	Yes	No	No	Yes	No
Within R^2	0.49	0.49	0.49	0.31	0.33	0.29
Number of mun.	745	745	745	745	745	745
Observations	14,900	12,665	15,645	14,900	12,665	15,645

Notes: The table shows the results of difference-in-differences estimations in three different robustness tests. The 745 municipalities of the German state of Mecklenburg-Western Pomerania are our observation units. We use data in levels during the period 1995 to 2015. Our variable of interest (Access) takes on the value of one for municipalities within a road distance of 10 km to the next highway access point, and zero otherwise. Column 1 and 4 show regression results including a municipality specific time trend; column 2 and 5 show regression results including lagged unemployment rate and GDP in Poland multiplied with the inverse linear distance of each municipality to the polish border; column 3 and 6 show regression results without control variables. Control variables are lagged demographic and political variables; see table 3.1. Significance levels (standard errors robust to hereroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

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 (see table A3.3 in the appendix). 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. Table A3.4 in the appendix shows that our baseline results hold.

Finally, table 3.6 repeats our baseline analysis when using alternative specifications of the distance variable to ease concerns about arbitrary cutoff-values. In columns 1 and 2 as well as 5 and 6, we specify the treatment dummy to equal one for municipalities within a road distance of 5 km and 15 km, respectively. In columns 3 and 7, 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 positive property tax differential is restricted to municipalities within a distance band of 5 to 10 km from the next highway access (column 3). The fact that municipalities located in even closer proximity to the highway – less than 5 km – or between 10 and 15 km do not react in terms of property tax factors (columns 1 and 3), explains the estimated average zero effect in column 2. What is more, coefficients for the maximum distance band in column 3 even display opposite signs

suggesting counteracting effects on the property tax factor. This is also true for the business tax factor (column 6), where the coefficient of the 10 to 15 km distance dummy is negative and statistically significant at the 5% level. In columns 4 and 8 we include the road distance to the next highway access as a continuous measure. Our baseline results are confirmed.[19]

Table 3.6: Different distance bands

	Property tax factor				Business tax factor			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Access (<5 km)	3.88		3.86		9.63		8.64	
	(2.96)		(2.99)		(5.94)		(6.05)	
Access (<15 km)		1.46				-1.80		
		(1.60)				(2.00)		
Access (5-10 km)			6.81**				-1.63	
			(3.05)				(3.52)	
Access (10-15 km)			-1.98				-4.63**	
			(1.69)				(2.05)	
Distance to access (km), cont.				-0.04**				0.02
				(0.02)				(0.03)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.49	0.49	0.49	0.49	0.31	0.31	0.31	0.31
Number of mun.	745	745	745	745	745	745	745	745
Observations	14,900	14,900	14,900	14,900	14,900	14,900	14,900	14,900

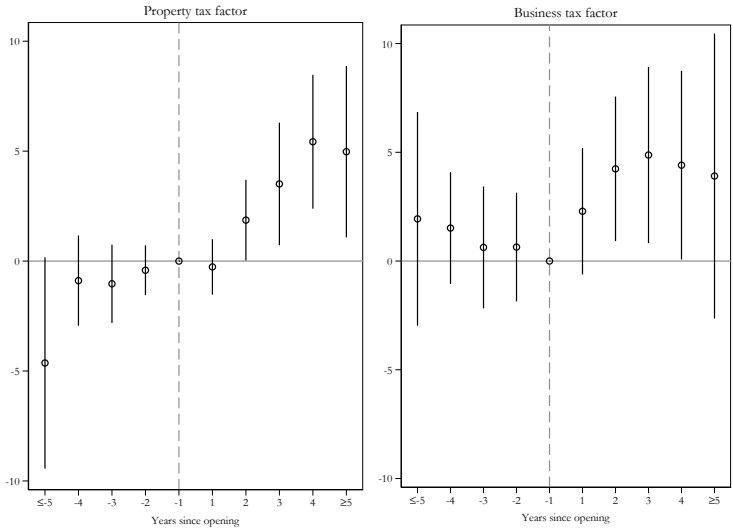
Notes: The table shows the results of difference-in-differences estimations. The 745 municipalities of the German state of Mecklenburg-Western Pomerania are our observation units. We use data in levels during the period 1995 to 2015. Our variable of interest (Access) takes on the value of one for municipalities 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 hereroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

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). To discern temporary patterns in the local tax setting, we normalize the year of improved accessibility for all 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 3.3 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).

[19] The effect on the property tax factor is negative, which confirms our baseline finding of a positive effect when municipalities get closer to highway access. During our sample period, no highway was closed, only opened. Municipalities therefore experienced no change or a reduction in their distance to the next highway. With negative distance changes, the coefficient has to be multiplied by -1 to interpret it as the effect of getting closer to the next highway access.

Figure 3.3: Event study results



Notes: The figures show the results of two event-study estimations. Vertical dashed lines represent the year when a municipality falls within a road distance of 10 km to the next highway access point. The 745 municipalities of the German state of Mecklenburg-Western Pomerania are our observation units. We use yearly data in levels during the period 1995 to 2015. Circles 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.

The observed patterns for the property and business tax factors are relatively similar, although confidence bands for the estimates on the business tax factor are larger, which explains why we do not find an average effect in our baseline regressions. For both tax factors, effect sizes oscillate around five percentage points, although – in line with previous results – the increase in the property tax factor is more precisely estimated and persistent compared to that of the business tax factor. Four years after the opening of the highway segment – and thus after falling under the 10 km distance band – the estimate for the positive business tax differential gets less precise and lacks statistical significance. Reassuringly, tax factors are not adapted in the four years before the opening which is consistent with the relatively quick planning procedure precluding local decision makers from targeting specific locations. As we do not observe statistically significant differences in tax factors in the years before the highway was opened (90% confidence intervals always include the zero), the common trend assumption seems to be met.

3.5.3 Mechanisms

The average estimated effects of the baseline specifications might mask substantial heterogeneities in municipal tax setting. Conditional on the spatial characteristics of a municipality, for example, policymakers might have more or less scope for tax rate changes in the first place, and, more importantly, might be affected differently by increased accessibility. For this reason, we re-estimate our baseline specification by adding multiplicative interactions between the treatment indicator and dummy variables for central and peripheral municipalities. Table 3.7 displays the results. Peripheral municipalities that receive a highway access within 10 km increase their property tax factors by 6.91 percentage points (column 1). This effect corresponds quite closely to the one displayed in column 2 of table 3.4 where we excluded central municipalities from the specification. The positive property tax differential for peripheral municipalities persists for the different control variants in columns 2 to 4. Central municipalities do not seem to adjust their tax factors. Similar to previous results, the effects for business tax factors do not turn out to be statistically significant.

We investigate possible mechanisms to explain the positive property tax factor differential for accessible municipalities. Effects could run through population, employment, or area-based channels, which we investigate in turn in table 3.8 with a parsimonious specification. Panel A shows how these variables are affected by accessibility. Most of the coefficients are very imprecisely estimated, but some employment measures seem to matter. Receiving a highway access within 10 km is associated with a decrease in the number of firms, the number of employed workers in the municipality, and the number of inbound commuters. 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

Table 3.7: Baseline results with location heterogeneity

	Property tax factor				Business tax factor			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Access (<10 km) periphery	6.91***	6.61**	5.90**	7.21***	4.28	3.71	1.45	5.09
	(2.58)	(2.58)	(2.60)	(2.59)	(3.60)	(3.62)	(3.38)	(3.63)
Access (<10 km) central	2.22	2.03	3.34	0.94	-6.13	-6.50	-6.04	-7.43*
	(2.66)	(2.65)	(2.37)	(2.42)	(4.00)	(4.05)	(3.96)	(3.81)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Time trend	No	Yes	No	No	No	Yes	No	No
Further controls	No	No	Yes	No	No	No	Yes	No
Within R^2	0.49	0.49	0.49	0.49	0.31	0.31	0.33	0.29
Number of mun.	745	745	745	745	745	745	745	745
Observations	14,900	14,900	12,665	15,645	14,900	14,900	12,665	15,645

Notes: The table shows the results of difference-in-differences estimations. The 745 municipalities of the German state of Mecklenburg-Western Pomerania are our observation units. We use data in levels during the period 1995 to 2015. We interact our Access dummy (one for municipalities within a road distance of 10 km to the next highway access point, and zero otherwise) with a dummy for central (peripheral) municipalities, which is one for municipalities that are classified as being central (peripheral), and zero otherwise. Columns 1 and 5 show our baseline specification; columns 2 and 6 show regression results including a municipality specific time trend; columns 3 and 7 show regression results including lagged unemployment rate and GDP in Poland multiplied with the inverse linear distance of each municipality to the polish border; columns 4 and 8 show regression results without control variables. Control variables are lagged demographic and political variables, see table 3.1. Significance levels (standard errors robust to hereroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

overall and working population, rendering them less densely populated. Also the overall employment situation deteriorates, with the adverse effects on the number of local employees and inbound commuters being especially pronounced. And buildings with residential areas, which are a main part of the tax base of the property tax, decrease. 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 employment with increased highway accessibility. The small number of municipalities classified as central, however, raises questions about sample size and how reliable the estimate for central municipalities actually is.

The effect of highway accessibility on tax factors can also run via property values reflected in 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.[20] Table A3.5 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. Unfortunately, the data coverage does not overlap with the main construction window of the

[20] 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.

BAB 20 such that variation in the access variable is low.[21] 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.

Moreover, we investigate how highway accessibility influences tax revenues with our parsimonious specification. Table 3.10 shows results for absolute tax revenues (*Istaufkommen*, columns 1 and 3) and tax factor adjusted revenues (*Grundbetrag*, columns 2 and 4).[22] Tax factor adjusted revenues allow for a comparison of tax revenues between municipalities with different tax factors as municipalities with a similar tax base may have different revenues simply because they set tax factors differently. Overall, property tax revenue is not affected (panel A). Disentangling effects between peripheral and central municipalities in panel B, however, shows that changes in accessibility may well map into changes in tax revenues. Peripheral municipalities experience a decrease in tax factor adjusted property tax revenues. This implies that peripheral municipalities close to a new highway access – compared to the baseline category of peripheral municipalities without an access – increase their property tax factors to keep their absolute property tax revenues stable (see table 3.7). In central municipalities, on the other hand, absolute and adjusted property tax revenues remain unchanged. Turning to business taxes, neither absolute nor tax factor adjusted revenues are significantly altered in the medium-run.

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 increased their property tax factors. This effect proves to be persistent and very robust across specifications, conditioning factors, and estimation methods. Business tax factors, the other local tax that German municipalities have discretion over, appear to be unaffected by improved accessibility.

We examine population and employment-related channels for the property tax effect. On average, improved highway accessibility is associated with a decrease in firms per capita, the number of employees working (but not living) in the municipality, and inbound commuters.

[21] Because of the low variation in the shorter time window we cannot conduct a heterogeneity analysis between peripheral and central municipalities.

[22] *Tax factor adjusted revenues* $=$ *revenue* $\cdot$ *100* $/$ *tax factor*.

Furthermore, house prices seem to decline in municipalities close to a highway access. Since our baseline 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 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, leads 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 is 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.

As a result of a shift in employment and population, peripheral municipalities lose part of their tax base. On the one hand, adverse effects for employees commuting to work in peripheral municipalities are especially pronounced, and they suggest a general decline in local economic activity that might in the long-run be reflected in business tax revenues. On the other hand, the municipality share in income tax revenue is distributed according to employees' place of residence, another location condition that is negatively affected by accessibility via the BAB 20. Counteracting these tax base effects with an increase in property tax factors to keep tax revenues stable seems a rational response by local policymakers. First, property tax revenues are relatively stable due to the immobile nature of the tax base.[23] Second, the outdated legal provisions of the German property tax do not take value adjustments into account, i.e., tax factors are the only instrument at the discretion of local policymakers. This argument is especially important given the suggestive evidence of falling house prices close to the new highway network.

[23] Property tax does not depend on whether a property is inhabited or not. If one moves to another municipality, one has to pay property tax to the "old" property until it is sold, then the new owner pays property tax. The tax base can be reduced, for instance, by the conversion of built-up land into undeveloped land.

3.7 Conclusion

We focus on an episode of extensive highway construction in East Germany following reunification and examine how access to the highway network influences municipal tax policy. 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 and induce changes in tax policy choices of the local government.

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 since 1945 in Germany. 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 stagewise 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 1995 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 municipalities within 5 to 10 km road distance to a new highway access increase their property tax factors. Using event studies, we show that there is no adjustment in tax factors in the four years prior to the highway opening, but tax factors start to rise immediately afterwards. In line with the baseline findings, more accessible municipalities increase their property tax factors persistently, while the business tax differential falls to zero four years after the opening of the highway segment. Finally, we reconcile our findings with the literature by examining the role of economic outcomes as possible drivers of the tax policy 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. As a result, municipalities might resort to increased property tax factors to stabilize their tax revenues.

Table 3.8: Channels

	Population			Firms	Employment		Commuter		Buildings
	(1) All (log)	(2) Age 15-65 (log)	(3) Density	(4) (log)	(5) Place of residence (log)	(6) Place of work (log)	(7) Inbound (log)	(8) Outbound (log)	(9) With residential areas (log)
A: Baseline									
Access (<10 km)	-0.00	0.00	0.14	-0.05*	-0.02	-0.18*	-0.17*	0.01	0.00
	(0.02)	(0.02)	(1.31)	(0.03)	(0.02)	(0.09)	(0.10)	(0.12)	(0.01)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.31	0.39	0.07	0.06	0.10	0.08	0.06	0.00	0.51
Number of mun.	745	745	745	737	745	745	745	745	745
Observations	15,645	15,645	15,645	11,934	11,175	11,141	11,124	11,175	2,980
B: Heterogeneity									
Access (<10 km) periphery	-0.04***	-0.04**	-2.41**	-0.07**	-0.04**	-0.27***	-0.26***	-0.01	-0.02**
	(0.02)	(0.02)	(1.03)	(0.03)	(0.02)	(0.09)	(0.10)	(0.13)	(0.01)
Access (<10 km) central	0.20***	0.22***	14.07***	0.15***	0.20***	0.64***	0.59***	0.23	0.12***
	(0.04)	(0.04)	(4.34)	(0.05)	(0.02)	(0.19)	(0.21)	(0.15)	(0.04)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.33	0.41	0.09	0.06	0.12	0.09	0.07	0.00	0.52
Number of mun.	745	745	745	737	745	745	745	745	745
Observations	15,645	15,645	15,645	11,934	11,175	11,141	11,124	11,175	2,980

Notes: The table shows the results of difference-in-differences estimations. The 745 municipalities of the German state of Mecklenburg-Western Pomerania are our observation units. We use data in levels during the period 1995 to 2015. Data on firms for the years 1999 to 2015; data on employed and commuters for the years 1999 to 2013; data on buildings with residential areas aggregated for the periods 1996-2000, 2001-2004, 2005-2008, 2009-2011. We interact our Access dummy (one for municipalities within a road distance of 10 km to the next highway access point, and zero otherwise) with a dummy for central (peripheral) municipalities which is one for municipalities that are classified as being central (peripheral), and zero otherwise. Significance levels (standard errors robust to hereroskedasticity 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)
A: Baseline				
Access (<10 km)	-165.45***	-205.92***	-345.72***	-248.47***
	(23.54)	(50.57)	(50.94)	(81.38)
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 fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	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: The table shows the results of 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. Our variable of interest (Access) takes on the value of one for municipalities within a certain road distance to the next highway access point, and zero otherwise. All specifications include municipality and year fixed effects, housing controls and municipality controls. Municipality controls are demographic and political variables, see table 3.1 in the appendix. Housing controls are the number of rooms, total area, year of construction, object type and postal code, see table A3.5. Significance levels (standard errors robust to hereroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

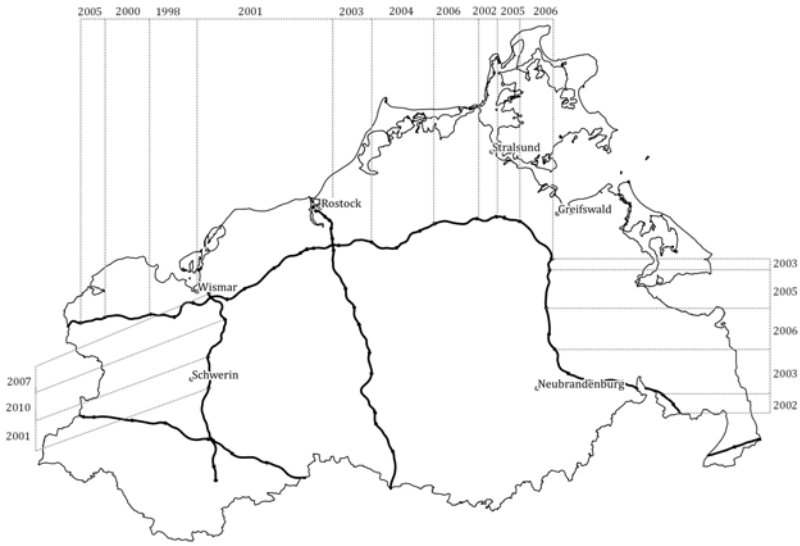
Table 3.10: Tax revenues

	Property tax revenue		Business tax revenue	
	(1) Absolute (log)	(2) Tax factor adjusted (log)	(3) Absolute (log)	(4) Tax factor adjusted (log)
A: Baseline				
Access (<10 km)	0.00	-0.02	0.06	0.06
	(0.01)	(0.01)	(0.10)	(0.10)
Municipality fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Within R^2	0.53	0.42	0.30	0.26
Number of mun.	745	745	745	745
Observations	13,405	13,405	12,730	12,730
B: Heterogeneity				
Access (<10 km) periphery	-0.00	-0.02**	0.09	0.08
	(0.01)	(0.01)	(0.11)	(0.11)
Access (<10 km) central	0.04	0.03	-0.19	-0.17
	(0.06)	(0.05)	(0.14)	(0.13)
Municipality fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Within R^2	0.53	0.42	0.30	0.26
Number of mun.	745	745	745	745
Observations	13,405	13,405	12,730	12,730

Notes: The table shows the results of difference-in-differences estimations. The 745 municipalities of the German state of Mecklenburg-Western Pomerania are our observation units. We use data in levels during the period 1998 to 2015. *Tax factor adjusted revenue* is defined as *revenue* $\cdot$ *100* / *tax factor*. It is used for comparisons between municipalities as it offsets the impact of different tax factors. We interact our Access dummy (one for municipalities within a road distance of 10 km to the next highway access point, and zero otherwise) with a dummy for central (peripheral) municipalities which is one for municipalities that are classified as being central (peripheral), and zero otherwise. Control variables are lagged demographic and political variables, see table 3.1. Significance levels (standard errors robust to hereroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

Appendix

Figure A3.1: 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.

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

Table A3.2: Baseline results without merged municipalities

	Property tax factor			Business tax factor		
	(1)	(2)	(3)	(4)	(5)	(6)
Access (<10 km)	5.69**	6.40**	5.72**	1.50	2.89	1.44
	(2.52)	(3.05)	(2.52)	(3.29)	(3.91)	(3.30)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Central mun.	Yes	No	Yes	Yes	No	Yes
Urban mun.	Yes	Yes	No	Yes	Yes	No
Within R^2	0.47	0.47	0.47	0.28	0.28	0.28
Number of mun.	564	548	552	564	548	552
Observations	11,280	10,960	11,040	11,280	10,960	11,040

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

Table A3.3: Baseline results without tourism destinations

	Property tax factor			Business tax factor		
	(1)	(2)	(3)	(4)	(5)	(6)
Access (<10 km)	4.74*	5.2442*	4.67*	1.59	2.96	1.53
	(2.41)	(2.92)	(2.41)	(3.37)	(4.02)	(3.37)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Central mun.	Yes	No	Yes	Yes	No	Yes
Urban mun.	Yes	Yes	No	Yes	Yes	No
Within R^2	0.47	0.47	0.47	0.28	0.28	0.28
Number of mun.	513	497	504	513	497	504
Observations	10,260	9,940	10,080	10,260	9,940	10,080

Notes: This table reproduces table 3.4 without 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. Significance levels (standard errors robust to hereroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

Table A3.4: Pooled time dimension

	Property tax factor			Business tax factor		
	(1)	(2)	(3)	(4)	(5)	(6)
Access (<10 km)	6.20***	6.30**	6.21***	2.22	3.05	2.16
	(2.18)	(2.52)	(2.18)	(3.17)	(3.59)	(3.17)
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Central mun.	Yes	No	Yes	Yes	No	Yes
Urban mun.	Yes	Yes	No	Yes	Yes	No
Within R^2	0.44	0.44	0.44	0.27	0.27	0.27
Number of mun.	745	728	733	745	728	733
Observations	14,900	14,560	14,660	14,900	14,560	14,660

Notes: This table reproduces table 3.4 where the observations are pooled and the time dimension is ignored. Significance levels (standard errors robust to hereroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

Table A3.5: Discriptive statistics - Real estate offers

	Obs.	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.

4 Ineffective fiscal rules? The effect of public sector accounting standards on budgets, efficiency, and accountability[1]

Abstract

International organizations have encouraged national governments to switch from traditional cash-based to business-like accrual accounting, on the presumption that long-run benefits may outweigh substantial implementation and operating costs. We use a quasi-experimental setting to evaluate whether changing public sector accounting standards is justified. Some local governments in the German federal state of Bavaria introduced accrual accounting while others retained cash-based accounting. Difference-in-differences and event-study results do not show that (capital) expenditures, public debt, voter turnout, or government efficiency developed differently after changes in accounting standards. Operating costs of administration, however, increase under accrual accounting.

[1] This chapter is joint work with Florian Dorn and Felix Roesel. It is based on our paper "Ineffective Fiscal Rules? The Effect of Public Sector Standards on Budgets, Efficiency, and Accountability" published in Public Choice, *forthcomming*, 2019.

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"Majorities rule often nicely,
If still concerned with public goods;
But even with all voting wisely
Irrational cycles swamp the books."
Bernholz (1980)

4.1 Introduction

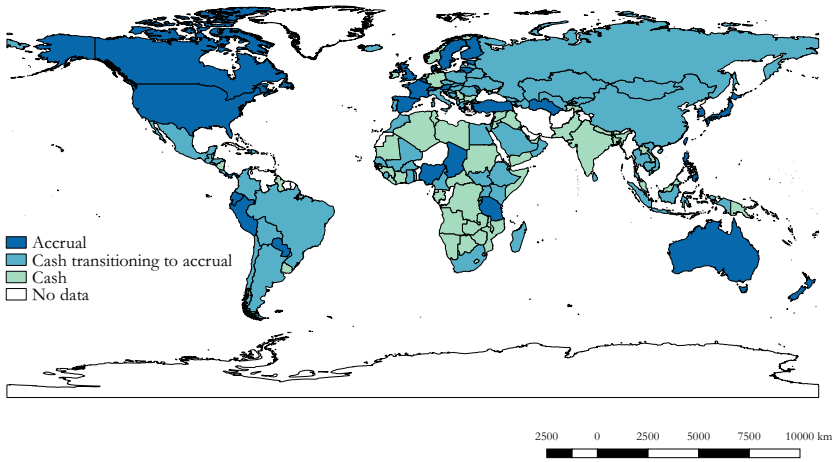
Two different accounting standards are used for reporting in the public sector: traditional cash-based accounting and business-like accrual-based accounting. Pure cash accounting statements do not report assets, liabilities, or depreciation. Business-like accrual accounting statements, by contrast, provide intertemporal fiscal information by complementing the cash-based information with resource-based information. International organizations such as the OECD, the International Monetary Fund (IMF), and the European Union (EU), have advocated public sector accrual accounting, with the intention of enhancing budget transparency, efficiency, and accountability of decision makers. The European Commission have urged EU members and candidate states to adopt the business-like accounting system in their public sector.[2] Increasing numbers of countries around the globe have replaced traditional cash-accounting with business-like accrual accounting. By 2018, 119 out of some 200 national governments around the world were using some form of full or modified accrual accounting or have plans for transitioning from cash-based to accrual-based standards (figure 4.1).

Accrual accounting does not come for free. The main obstacle to adopting public accrual accounting is high implementations costs, resulting from expensive valuations of assets and liabilities. France, for example, spent some $ 1.7 billion to switch from cash-based to accruals-based accounting (European Commission, 2013). Implementation costs for Germany are estimated at around $ 3.5 billion[3], without taking permanent higher operating costs into account (German SAI, 2017). Surprisingly, there has been little research into whether accrual accounting improves public finances. Surveys among governments yield subjective impressions (Kuhlmann *et al.*, 2008; Andriani *et al.*, 2010; Burth and Hilgers, 2014; Moretti, 2016, among others). Khan and Mayes (2009) discuss technical details. Carlin (2005) and Christensen (2007) report no research on effects of accrual accounting based on objective budget outcomes. Two recent studies examine the effect of the public accounting system on fiscal policy outcomes in Germany. Christofzik (2019) uses state-level aggregates and does

[2] The European Commission proposes a harmonized accrual accounting regime (EPSAS) for all EU member states assuming that "[t]he appropriateness of the accruals principle is indisputable" (European Commission, 2013, p. 5). The underlying assumption is that harmonized public accrual accounting among the EU members may strengthen confidence in the financial stability in the European Union and facilitates fiscal surveillance in order to avoid future sovereign debt crisis (Council of the European Union, 2011; European Commission, 2013). A majority of EU member states have already implemented full accrual-based public accounting or plan to do so. See also Cavanagh *et al.* (2016) for the IMF, and OECD and IFAC (2017) for the OECD.

[3] The cost estimates refer to the introduction of the accrual-based EPSAS.

Figure 4.1: Accounting standards of national governments



Source: Deloitte (2015); PwC (2015); OECD and IFAC (2017); IFAC and CIPFA (2018).
Notes: The map reports the current public-sector accounting standard (cash or accrual) at the national government level around the world as of 2018. The map also indicates countries which are in a transition from cash-based to a full accrual-based reporting system or have plans to do so in the next years.

not find that switching accounting standards had affected financial balances. Her findings suggest that accrual accounting somewhat altered the composition of revenues. Raffer (2019) investigates municipalities in the German federal state of Baden-Württemberg and finds that investment expenditure decreases under accrual accounting. In this federal state, all municipalities were obliged to change to accrual accounting.[4]

We estimate the effect of public sector accrual accounting on fiscal and political outcomes in a high-income country. Because (budget) institutions are likely to be endogenous (Aghion *et al.*, 2004; Heinemann *et al.*, 2018), we apply difference-in-differences estimation and event studies to a quasi-experimental setting at the local level in Germany.[5] Some local governments in the federal state of Bavaria gradually switched to accrual accounting between 2005 and 2012, but a substantial number of local governments retained cash-based accounting, making for an interesting case of institutional competition at the community level (Bernholz, 2008). We investigate the extent to which budgeting, efficiency, and accountability changes under accrual accounting. The results do not show that switching counties develop differently from counties with cash-based accounting – neither before nor after implementing accrual accounting. We find no significant impact on expenditures, public debt, government efficiency, nor on voter participation even after eight and more years after implementation. Local governments seem to sell fewer non-financial assets but more financial assets under accrual accounting. Rural counties somewhat reduce outsourcing after implementing accrual accounting. Operating costs to run the administration steadily increase under accrual accounting. Our findings therefore do not support proposals of international organizations such as the OECD, IMF or EU that public sector accrual accounting outperforms cash-based accounting. We thus question the standard expected benefit-cost evaluation of switching accounting standards. Politicians do not seem to take advantage of accruals-based information and adjust their behavior accordingly, at least when the levels of development and transparency are already high.

This chapter contributes to the discussion of fiscal rules. Fiscal rules are usually designed to limit government spending and to enhance sustainable budgeting. Empirical evidence suggests that this kind of political self-constraining works well.[6] Following the seminal contributions by Alesina *et al.* (1999); Alt and Lowry (1994); Poterba (1996); Alesina and Perotti (1999) and Von Hagen and Harden (1995), follow-up studies have shown that budget institutions contribute to sound public finances. For example, balanced-budget rules (Bohn and Inman, 1996; Asatryan *et al.*, 2018), deficit reduction rules (Grembi *et al.*, 2016), Swiss-style debt brakes (Burret and Feld, 2018), checks and balances in the budgeting process (Fabrizio and Mody, 2006), supervision by fiscal overseers (Christofzik and Kessing, 2018), or budget transparency

[4] Lampe *et al.* (2015) use a stochastic frontier approach and show that accrual accounting comes with initial gains in cost efficiency which diminish rapidly. In their setting of German local governments in the state of North Rhine-Westphalia in the very short run over three years, however, accrual accounting overlaps with further policy changes such as withdrawing fiscal supervision (see Christofzik and Kessing, 2018).

[5] Asatryan *et al.* (2018) use a similar strategy.

[6] Tóth (2019) shows that fiscal rules successfully bind the implementing but also later governments.

(Benito and Bastida, 2009) reduce debt and the likelihood of sovereign debt crises. Debrun *et al.* (2008); Krogstrup and Wälti (2008); Dabla-Norris *et al.* (2010); Blume and Voigt (2013); Dove (2016), and the meta-regression by Heinemann *et al.* (2018) report very similar results. Previous studies therefore favor fiscal rules as a policy against unsustainable budgeting. Our empirical findings, by contrast, suggest that not all fiscal rules and improvements in financial reporting have a clear beneficial impact on budget outcomes. This is in line with theoretical papers by Halac and Yared (2014) and Landon and Smith (2017) showing that the same fiscal rules may well produce different outcomes and vary substantially in effectiveness and efficiency. We conclude that the literature on fiscal rules is in need of qualification.

Literature in public choice has a long tradition of investigating which institutions and legal systems provide efficiency and democracy (Bernholz, 1993). Previous research has shown that governments may well use "creative accounting" tricks to circumvent fiscal rules (Von Hagen, 1991; Milesi-Ferretti, 2004), and to decrease budget deficits or public debt without changing government net worth (Easterly, 1999). In particular, creative accounting increases before regular elections (Reischmann, 2016), before a country joined the European Monetary Union (EMU) (Dafflon and Rossi, 1999; Milesi-Ferretti and Moriyama, 2006), and after the introduction of the European Stability and Growth Pact (SGP) to sugarcoat the budget balance requirements (Von Hagen and Wolff, 2006; Buti *et al.*, 2007; Beetsma *et al.*, 2009; Alt *et al.*, 2014). Our study is one of the first that does not view accounting as a dependent variable but as an explanatory variable. We examine whether accounting affects government budgeting, efficiency, and accountability. We contribute to the literature by studying whether and how institutions may map into incentives for decision makers and may prevent fiscal manipulation.

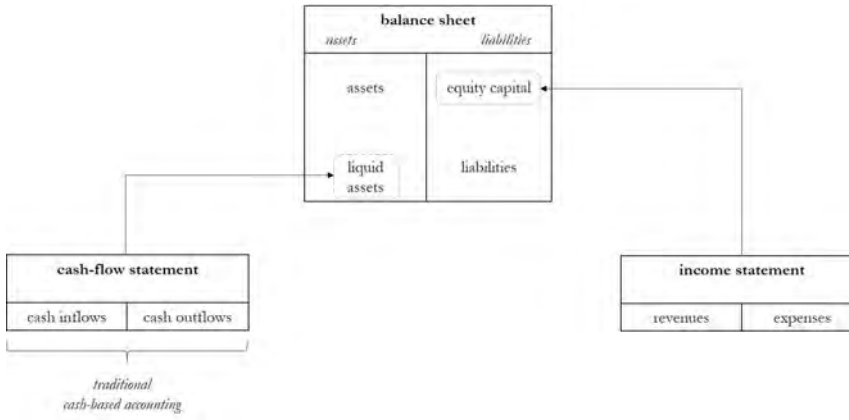
4.2 Public sector accounting standards

4.2.1 Key features of cash-based and accrual accounting

Technically, traditional cash-based accounting consists of a cash flow statement. Accrual accounting is more complex and complements the cash-based view with a resource-based view reported in an income statement on revenues and expenses (see figure 4.2). Accrual accounting links the surplus or deficit of the cash flow and income statements in a balance sheet on assets, liabilities and equity. As illustrated in figure 4.2, the balance of cash flows affects the liquid assets or the debt level in the balance sheet. The balance of revenues and expenses together report complete resource consumption in the period and directly affect equity capital.

Besides the pure components, accrual accounting differs from cash-based accounting in two main dimensions: (1) the timing of transactions and (2) information on assets and liabilities. First, cash-based accounting records transactions when cash is received or paid out, but not consumption of already purchased resources. Accrual accounting income statements, by contrast, record all kinds of resource consumption (revenues and expenses) in real time.

Figure 4.2: Components of a simplified accrual accounting system



Source: See Lueder (2001), p. 37.
Notes: The figure shows a simplified three-component accounting system.

For example, traditional cash-based accounting reports production costs for public roads when cash is paid out, but does not directly mirror liabilities and subsequent deterioration, while income statements under accrual accounting also mirror annual depreciation. Second, accrual accounting balance sheets take assets and liabilities into account. Conventional cash-based statements do not report government assets and liabilities. Changes in revenues and expenses, for example caused by the depreciation of assets or future pension liabilities, also do not show up in traditional cash-based accounting systems. Thus, public sector accrual accounting not only provides information on complete resource consumption but also on equity capital. Moreover, accruals-based reports often come as consolidated statements including the core administration and public enterprises.[7]

[7] The difference of the two accounting systems and its components becomes more obvious by discussing some examples: If an investment good (e.g., non-financial asset) is acquired, cash-based accounting reports only the cash outflow in the period when cash is paid out. Under accrual accounting, however, the balance sheet reports the decrease of liquid financial assets (or an increase of debt (liabilities)) at the price of the purchased asset, but also the increase of non-financial assets at the value of the purchased asset. Equity capital, however, does not change if the price equals the value of the purchased asset. This is similar if non-financial assets such as land properties, buildings or machineries are sold. While cash-based accounting only reports the cash inflow in the cash flow statement, the balance sheet of accrual accounting takes the rise of liquid assets on the one hand and the decline in the value of non-financial assets on the other hand into account. In the case of borrowing, cash-based accounting records again only the inflow of cash in the cash flow statement. Accrual accounting, by contrast, reports the rise of liquid assets (due to cash inflow) and the rise of liabilities. Moreover, future interest costs of the credit are considered in the income statement as expenses. The income statement also reports an increase in expenses when capital assets depreciate. If the balance of revenues and expenses is negative, equity capital is decreasing in the balance sheet. Table A4.1 in the appendix gives a numerical example.

Accrual accounting is not a completely new concept. Bringing business-like accounting standards to the public sector was one of the main issues raised by the New Public Management movement in the 1980s. National governments in Australia, Canada, the United States, and New Zealand already started to adopt public sector accrual accounting in the 1990s or around the turn of the millennium. Among OECD countries, 82 % of national governments implemented accrual accounting or have plans to do so (OECD and IFAC, 2017). Similar adoption rates apply to the local level: in 75 % of all OECD countries, local governments use full accrual accounting. A growing number of low-income countries around the world is also following the trend of switching accounting standards and implemented reports on an accrual base or have plans to do so in the future. Changes in accounting standards usually are accompanied by debates about the pros and cons; we discuss the main arguments in the next section. A summary of the main key features of cash-based and accrual-based accounting and the pros and cons of public sector accrual accounting are shown in table 4.1.

Table 4.1: Key features and pros and cons of cash-based and accrual-based accounting

Key features of cash-based and accural accounting	
Cash-based accounting	Accrual-based accounting
records transactions when cash is received or paid out	records transactions when they occur
real transactions are not covered	complements cash-flow by a resource-based view (revenues and expenses)
does not report balance sheets including assets, liabilities and depreciation	records assets and liabilities
	consolidated statements include budgets of the core administration and public enterprises
Pros and cons of public sector accrual accounting	
Pros	Cons
accrual accounting statements provide more information	business accounting standards ill-fitting in a public sector context
increased transparency is expected to map into sustainable budgeting, efficiency and accountability	evaluation of public goods for accrual accounting is time consuming and often arbitrary estimation
	substantial implementation costs of accrual accounting

Notes: The table summarizes key features of cash-based and accrual-based accounting (section 4.2.1) and the pros and cons of public sector accrual accounting (section 4.2.2).

4.2.2 Pros and cons of public sector accrual accounting

All arguments favoring public sector accrual accounting over cash-based accounting (for an overview, see Carlin, 2005; Christensen, 2007) come down to one key argument: transparency. Transparency increase information, which is key for democratic societies (Bernholz, 1993). Accrual accounting statements include income statements and balance sheets, and therefore provide more comprehensive information than cash-only statements. This, in turn, may

enable and empower decision makers for more sustainable budgeting (i.e., intergenerational equity), increase efficiency, and give rise to accountability in elections. The main argument against accrual accounting is that income statements and balance sheets are based on time-consuming and often arbitrary estimates of values of public assets for which market values are usually not available. Thus, while accrual accounting may provide more information, the information may not be reliable. We now discuss the pros and cons in more detail, starting with potential benefits.

Accrual accounting statements provide much more information than cash-based statements, which can enable more sustainable budgeting decisions. Accrual accounting reports multi-annual flows of resources and reveals future benefits of assets and non-cash costs hidden under conventional cash-based public sector accounting, mainly depreciation costs.[8] Accrual accounting balance sheets thus show the entire intertemporal resource formation and consumption of the government and reflect the scope and quality of the public capital stock more transparently. Accrual accounting reveals the allocation of public resources over time, which may give rise to greater intergenerational equity and sustainable budgeting because under- and overinvestment is reduced. For example, consuming public capital stock because of too little investment in roads or schools is invisible under cash-based accounting but in principle is mirrored in accrual accounting statements. Accrual accounting also avoids overinvestment because follow-up costs and intergenerational consequences of current decisions are made more visible. Another benefit relates to privatization and outsourcing. If public core administrations use the same accounting standards as public enterprises, integrated or consolidated financial statements covering the universe of public entities become available. Anecdotal evidence reports that incentives for outsourcing decrease drastically because public enterprises are treated like core budgets, and vice versa.[9] Accrual accounting may thus prevent politicians from engaging in opaque and costly off-budget activities to reduce deficits and debt of the core administration, for example by outsourcing to public enterprises.

Efficiency is argued to increase under accrual accounting. For example, real-time information on capital and valuation of assets provided under accrual accounting should allow for more efficient allocation of public resources. Accrual-based budgets reveal priorities for road or school maintenance, for example, which can facilitate targeting public investment and lead to a higher quality of public assets. Accrual accounting can also prevent public decision makers from selling assets below market value. Sales of non-financial assets such as land properties, buildings or machinery can reduce deficits or public debt by the sale price, while accrual accounting also reports the decline in net worth by the value of the asset (see Easterly, 1999) (see table A4.1).

[8] Traditional cash-based accounting statements do not systematically report the use of resources.

[9] See, Delmenhorster Kurier, June 30, 2019, "Misstrauische Politiker", `https://www.weser-kurier.de/region/delmenhorster-kurier_artikel,-misstrauische-politiker-_arid,1841297.html`.

Transparency increases accountability of public decision makers. Reliable intertemporal fiscal information enhances management capabilities and responsibilities. Accrual accounting may also prevent politicians from timing manipulation ("creative accounting") to finance or reduce budget deficits, as resource consumption is recorded when it is due (*income statement*), while cash-based accounting records transactions only when cash is received or paid out (*cash-flow statement*). For example, sale-and-lease-back contracts may reduce budget deficits in the short-run but often have little budgetary effect and are not worthwhile in a long-term perspective. Hiring civil servants creates pension liabilities that are rather opaque under traditional cash-based accounting, but become transparent in balance sheets of accrual-based statements. Finally, public finances become more comparable to private-sector finances under accrual accounting. Voters may therefore become better informed and more interested in politics.

There are, however, arguments against public sector accrual accounting. Accounting standards developed for businesses may well be appropriate for market-based transactions but not in a public sector context. Profit and loss statements, balance sheets and other accrual accounting tools are designed for profit-seeking organizations. The public sector is non-profit and in principle has social-welfare objectives. Technical problems also arise. Valuating public assets is challenging because publicly provided goods such as local public roads, police stations, or women's shelters are not allocated via markets. Assumptions must be made to value long-term liabilities (e.g., pensions) or assets without market prices. Identifying returns on investments of public infrastructure or consumption is almost impossible. Thus, in a public sector context, the accuracy of accrual accounting can be spurious. There are transition problems, including inconsistent and contradictory statements, time consuming asset valuation, internal resistance by the administration, and requirements for new IT systems, staff training and external support services.[10] For such reasons, implementation costs are substantial. OECD and IFAC (2017) estimate that switching a central government's account from cash-based to accruals costs some 0.05 % of gross domestic product (GDP). In addition, permanent follow-up costs of accrual accounting can be underestimated (Carlin, 2006).

Altogether, theoretical predictions on the effect of switching the accounting standards on fiscal outcomes, government efficiency and accountability are ambiguous. There are reasons for believing that accrual accounting improves the performance of the public sector; increasing transparency of assets and liabilities seems the most prominent argument. However, practitioners and scholars question whether accrual accounting is appropriate for the public sector, which is non-profit. Therefore, it is an empirical matter whether accrual accounting is beneficial.

[10] See, e.g., Boehme *et al.* (2013), and Selb-Live.de, November 29, 2018, "Aus dem Stadtrat notiert - Rückumstellung des Rechnungswesens", `http://www.hochfranken-live.de/index.php/aus-dem-rathaus/6300-aus-dem-stadtrat-notiert-31.html`.

4.3 Institutional background

Examining the effect of budget accounting standards is impossible at the national government level because national governments are not comparable in size and functions. Moreover, accrual accounting also often comes with further New Public Management tools; effects of multiple reforms overlap. We use a quasi-experiment at the local level in the German state of Bavaria that allows us to isolate the effects of accrual accounting. Between 2005 and 2012, around one third of county governments gradually switched to accrual accounting, with the remainder keeping cash-based accounting. County governments that did not switch are an ideal control group for governments changing accounting standards within the same German state. Institutions and responsibilities of county governments differ somewhat among German states. In Bavaria, responsibilities or other institutions do not change, accounting standards are the only difference across both groups.

Germany has two layers of local government similar to the US: municipalities (*Gemeinden*), and counties (*Landkreise*). The 96 counties in the German state of Bavaria approximately correspond to US counties in population size (135,000 inhabitants on average in 2016). Consolidated city-counties (*kreisfreie Städte*) combine responsibilities of counties and municipalities like in the US. Our study treats counties and consolidated city-counties as county governments. German county governments are mainly responsible for social care and youth welfare, but also for building and maintaining county roads, the development of the local economy by granting subsidies, county hospitals and schools, household waste collection, and specific administrative tasks such as drivers' licenses, car registrations or building permits (see Roesel, 2017). Powers are shared between a directly elected head of a county administration (*Landrat*) and the county council (*Kreistag*). In Bavaria, the Landrat and county council elections are usually held simultaneously every six years. The county council decides on the budget proposed by the Landrat. Counties do not directly levy taxes but raise tax-like contributions from municipalities' tax revenues (by the so-called "county rate") and receive grants from the state government. Bavarian counties (including consolidated city-counties) spent some $ 30 billion (Euro 25 billion) in 2016, which is around 4.3 % of Bavarian GDP.

Local governments in Germany traditionally use cash-based accounting. In 1999 German states agreed on New Public Management guidelines including implementing accrual accounting elements for local governments. Reform laws passed all state parliaments between 2004 and 2009. Almost all German states implemented mandatory accrual accounting for local governments. Three German states including Bavaria, however, allowed local governments to choose between cash-based and accrual accounting.[11] Because tasks and responsibilities of local governments vary across German states, we use only Bavaria. The governing party

[11] The states of Bavaria and Thuringia allow local governments to choose between accrual-based and traditional cash-based accounting. In the state of Schleswig-Holstein, local governments can select full accrual-based or cash-based accounting extended by some accrual accounting elements. All county governments have switched to accrual accounting. In Thuringia, four out of 23 county governments changed accounting standards.

in Bavaria, the conservative right-wing Christian-Social-Union (CSU), believed that the cost-benefit-ratio of implementing accrual-based accounting standards may not pay off for all local governments. The left-wing political opposition in the Bavarian parliament voted against the new law, criticizing allowing local governments to select their accounting standards. The Social Democrats (SPD), as largest oppositional party in parliament favored mandatory accrual accounting. The new Bavarian budgetary law passed the Bavarian parliament in November 2006 and came into force in January 2007. By switching to accrual accounting, local governments in Bavaria must balance their resource-based accounting statements, while governments keeping cash-based accounting must simply balance their cash-flow statements on an annual basis (see figure 4.2). According to the new budgetary law, county governments that start with accrual-based budgeting and accounting have to present their first full consolidated financial statement five years after implementing accrual-based budgeting.

Three county governments were allowed to experiment with accrual accounting before 2007. Between 2005 and 2012, 35 % of the 96 Bavarian county governments introduced accrual accounting; 65 % kept cash-based accounting. Local governments that decided to switch to accrual accounting expected gains from transparency, generational equity, and improved management capabilities based on business-like tools; whereas governments that kept traditional accounting report that they did not believe that accrual-based accounting is superior to the cash-based rule (see Boehme *et al.*, 2013). The county government and administration or a council committee (selected members of the elected county council) usually discussed the benefits and costs of switching accounting standards. If the county government or any other group in the council proposed to implement accrual accounting, the final decision was taken by the majority on the county councils. Anecdotal evidence does not report large public discussions within counties.[12]

[12] See Pressestelle Landratsamt Bamberg, December 21, 2004, "Landkreis Bamberg entscheidet sich für die Doppik; Einstimmiger Grundsatzbeschluss des Kreistages", https://www.landkreis-bamberg.de/showobject.phtml?object=tx,1633.10.1&ModID=7&FID=1633.5682.1; Stadt Regensburg, March 21/29, 2007, "Vorlage - VO/07/2212/020: Umstellung der Haushaltsführung von der kameralistischen auf die doppelte kommunale Buchführung", https://srv19.regensburg.de/bi/vo020.asp?VOLFDNR=2121; Pressestelle Landkreis Würzburg, March 04, 2009, "Landkreis führt Doppik ein", https://www.landkreis-wuerzburg.de/Auf-einen-Klick/Pressebereich/Landkreis-f%C3%BChrt-Doppik-ein.php?object=tx,2680.5.1&ModID=7&FID=1755.226.1&NavID=2680.127&La=1; Die Augsburger Zeitung, November 13, 2009, "Pro Augsburg gibt Doppik nicht auf", https://www.daz-augsburg.de/pro-augsburg-gibt-doppik-nicht-auf/; Landkreis Schwandorf, March 14, 2011, "11. Sitzung des Kreisausschusses: Bericht zum neuen Kommunalen Haushaltsrecht", https://landkreis-schwandorf.de/index.phtml?La=1&sNavID=1901.67&mNavID=1901.1&object=tx%7C1901.416.1&kat=&kuo=1&sub=0.

4.4 Methods

4.4.1 Data

We use annual data on different performance measures for the 96 county governments of the German state of Bavaria over the time period 1995 to 2016.[13] Twelve different outcome variables cover the main dimensions expected to differ under accrual accounting: sustainable budgeting, efficiency, and accountability. Nine budget-related variables represent our main outcomes of interest. Three further variables cover possible changes that are beyond budgets.

Fiscal outcomes

Accrual accounting may provide transparency, which, in turn, has been shown to increase sustainable budgeting (Benito and Bastida, 2009). One could therefore expect public debt to decrease, and resources to be shifted from current operating expenditures to investment expenditures such as the construction of public schools and streets. All assets have to be valued and reported in financial statements of county governments that switched to accrual accounting. Therefore, incentives to sell non-financial assets to balance the budget may decrease as the simultaneous decline in net worth become visible in accrual-based statements.

In our dataset, per capita expenditures are in three main categories[14] (staff, administrative material and services, and investment expenditure). Sources for short-term revenues to balance the budget (the county rate, per capita sales of financial and non-financial assets), and public debt per capita (core budget, public enterprises) cover fiscal outcomes of county governments and allow examining whether accounting standards affect budgeting. Table 4.2 shows descriptive statistics for county-year observations from 1995 to 2016. On average, counties spent Euro 285 ($ 320) per capita on staff and Euro 210 ($ 240) per capita on administrative material and services. Investment expenditure accounted for Euro 140 per capita ($ 160).[15]

Sales of assets can be used to increase revenues in the short term, for example to balance the budget of the cash-flow statement. Per capita sales of non-financial and financial assets are on average Euro 22 ($ 25) and Euro 4 ($ 5) respectively. The main income source for rural counties, however, is the county rate. The county rate defines a percentage contribution

[13] Data on accounting standards are from the Bavarian State Parliament (Bayerischer Landtag, Drs. 17/12909). All other data are obtained from the State Statistical Office of Bavaria.

[14] The collection of these expenditure categories are hardly affected by different accounting standards. Spurious statistical effects can be ruled out to large extent. By contrast, other expenditure categories as well as gross total expenditures (*Bruttoausgaben*) might be biased by artificial statistical breaks. The State Statistical Office of Bavaria confirmed that our fiscal performance categories are comparable between cash-based and accrual-based accounting statements.

[15] Investment expenditures include the acquisition of land, facilities, and movable fixed assets as well as construction expenditures. This chapter also discusses whether accrual accounting affects local government decisions on total construction expenditure and investments in schools or county streets in the results section.

(tax levy) of municipalities within the county from the annual municipality tax income to the county budget.[16] The percentage contribution is determined by the county council each year. We use the determined percentage contribution and the resulting per capita contribution of the county rate. The average county rate is 46 %, that is Euro 340 ($ 385) per capita.

Public debt in core budgets amounts to around Euro 565 ($ 635) per capita on average, and ranges from almost zero debt per capita to a maximum of Euro 3,430 ($ 3,860) per capita. Local governments also outsource tasks to local public enterprises (*Kommunale Eigenbetriebe*). Outsourcing costly tasks to local public enterprises is attractive for local governments, by reducing debt in statements of the core administration. Budgets and debt of local public enterprises, however, must be included in the full consolidated financial statement of local governments five years after switching to accrual accounting standards. To rule out an outsourcing bias, we account for both debt in core budgets and in public enterprises. Note, however, that debt figures only include public enterprises directly controlled by the local government. Debt figures do not include, for example, funds for public housing.[17] The average debt level of the county governments' enterprises is Euro 140 ($ 160) per capita. As public debt of both the core budget and public enterprises become more transparent and must be balanced in the consolidated statement, one could therefore expect public debt to decrease in counties using accrual accounting.

Government efficiency

There are proposed effects of accrual accounting for government efficiency and counterarguments. Accrual accounting may increases government efficiency because financial transparency and output-oriented management capabilities improve. However, increasing costs to run the administration may rather decrease efficiency of governments that switch to accrual accounting. County governments are efficient in a technical sense when they produce a given amount of outputs using a minimum of inputs. We estimate technical efficiency via a pooled nonparametric data envelopment analysis (DEA) approach using data between 1996 and 2016 (see Farrell, 1957; Charnes *et al.*, 1978; Banker *et al.*, 1984). DEA generates an efficiency frontier from multiple inputs and outputs and computes an efficiency score for each county-year observation. Efficiency scores report relative positions with respect to the frontier. The most efficient county-year observation defines the frontier and receives an

[16] County governments do not raise own taxes. County rates, however, do not occur in consolidated city counties.
[17] Data on debt of all local government enterprises is not available as panel dataset in the period of observation.

Table 4.2: Descriptive statistics

	Obs.	Mean	SD	Min	Max
Sustainable budgeting					
Staff expenditure (per capita)	2,112	286.21	298.99	12.86	1,244.56
Administrative expenditure (per capita)	2,112	211.72	157.84	0.01	1,205.66
Investment expenditure (per capita)	2,112	139.70	143.48	8.77	954.09
Sales of non-financial assets (per capita)	2,016	21.66	51.71	-0.60	1,076.44
Sales of financial assets (per capita)	2,016	4.30	42.15	-0.55	1,574.81
County rate contributions (per capita)	1,562	342.35	87.19	25.81	1,064.96
County rate (%)	1,562	46.48	3.86	33.50	59.85
Public debt core budget (per capita)	2,112	564.19	662.30	0.38	3,343.30
Public debt public enterprises (per capita)	2,112	140.89	360.60	0.00	2,332.89
Efficiency					
Technical efficiency	2,001	89.75	16.65	11.45	100.00
Accidents on county roads (per 1,000 capita)	1,632	0.55	0.33	0.00	2.12
Accountability					
Voter turnout in county council elections	384	62.09	9.22	29.00	82.30
Accounting standard					
Accrual accounting (yes = 1)	2,112	0.13	0.34	0.00	1.00
Control variables					
Population (log)	2,112	11.59	0.53	10.54	14.20
Old-young population dependency ratio	2,112	50.72	3.67	38.40	60.80
Population share of foreigners	2,112	7.77	4.11	2.10	25.87
GDP (Euro 1,000 per capita)	2,112	32.67	15.15	14.43	122.30
CSU seat share council	2,112	43.61	8.60	0.00	60.00
CSU head of county government	2,112	0.64	0.48	0.00	1.00

Notes: The table reports descriptive statistics of the dataset. The 96 counties of the German state of Bavaria are the unit of observation; data span the period from 1995 to 2016. Technical efficiency multiplied by 100, starts in 1996. Data for accidents on county roads starts in 2000. County rates for 71 rural counties.

efficiency score of 100.[18] Observations of county governments with efficiency scores below 100 are technically inefficient, i.e., governments should be able to produce the same amount of outputs with less inputs.[19]

Table A4.2 in the appendix provides descriptive statistics for input and output variables used in the DEA analysis. We use total government expenditures (*bereinigte Gesamtausgaben*) as input factor, which reflects the costs of producing output and public services that are included

[18] DEA report the maximum efficiency score of 1. We multiply all efficiency scores by 100 and report the maximum efficiency score as 100.

[19] The calculations of the efficiency scores are based on an input-orientation rather than an output-oriented model. This approach seems appropriate because county governments have large autonomy in expenditure decisions (input factors). A decrease or increase in input factors such as expenditures (given a constant output) seems always possible (for example by raising the county rate to finance expenditures), whereas a change in the amount of outputs and services is not always feasible. Scholars have shown that per capita public expenditures or legislative tasks may depend on the size and density of the population (see, for example, Breunig and Rocaboy, 2008; Holcombe and Williams, 2008; Egger and Koethenbuerger, 2010). Efficiency scores therefore rely on the assumption of variable returns to scale. Inferences of our results hardly change by using constant returns to scale.

in the DEA. The six output variables reflect the multitude of county government services. The number of building permits and registered vehicles represents administrative performance. The length of county roads proxies for public infrastructure. School age population (6 to 17 years) reflects county tasks for school infrastructure, public transport for pupils and youth welfare, all provided by county governments. The number of beds in hospitals indicates hospital policies in the county. Total population proxies for general administration tasks and long-term development of a county. Performing DEA analyses yields average efficiency scores of county governments of around 90 in the period 1996 to 2016 (see table 4.2). Efficiency scores vary substantially and range from 11 to the maximum value of 100. The results are in line with recent studies on the efficiency of German county governments (see, for example, Fritzsche, 2019).

Technical efficiency scores mainly focus on the quantity of outputs rather than on quality. Assessment of the efficiency of county governments, however, should also include the quality of public service provision (see Balaguer-Coll *et al.*, 2007). A main task of Bavarian counties is building and maintaining county roads. If resources are allocated more efficiently under accrual accounting, one would expect better quantity and quality of county roads to result in less congestion and fewer accidents. Accidents on county roads have been used as indicator of the quality of county infrastructure (see Kalb, 2014; Fritzsche, 2019). If accrual accounting improves the quality of local roads, this may well translate into fewer accidents. We include data on accident rates on county roads as a proxy for the quality of governments' expenditure decisions. There were around 0.55 accidents per 1,000 capita on county roads on average (see table 4.2).

Accountability

Advocates of accrual accounting standards maintain that transparency can increase accountability of politicians. It has been shown that communication and information increase citizen participation (e.g. Lassen, 2005; Ebdon and Franklin, 2006). We use voter turnout in county elections as a proxy for voters' interest in county politics. County managers and county councils are usually elected at the same day. One may expect that voter turnout increases after switching to accrual accounting standards. Data on voter turnout covers the election years 1996, 2002, 2008 and 2014. Turnout in counties range from 29 % to 82 % between 1996 and 2014 (see table 4.2).

4.4.2 Empirical strategy

We take advantage of Bavarian county governments having introduced accrual accounting at different points of time. The main assumption to identify causal effects of accrual accounting is that counties that switched to accrual accounting would have evolved in a similar way as counties with cash-based accounting if they had *not* changed accounting standards. Twelve empirical baseline difference-in-differences regressions using OLS formalize this assumption.

Each model explains one of the twelve performance variables (nine budget outcomes, two efficiency measures, and voter turnout) with a dummy taking on the value of one for governments using accrual accounting, and zero otherwise (before adopting accrual accounting or never adopting accrual accounting). In around 13 % of all observations, governments use accrual accounting (see table 4.2). All models control for time-invariant differences across counties (county fixed effects), temporal shocks and time trends (year fixed effects), as well as for economic and demographic effects. Control variables are GDP per capita, total population (log), the share of foreigners, and the old-young dependency ratio (population below the age of 15 and above 65 over the working-age population between 15 and 65). We control for the seat share of the CSU in the county council and a dummy that takes the value of one if the head of the county government is of the CSU, and zero otherwise. The CSU is by far the main and dominating party, usually relying on absolute majorities in the state parliament during our period of investigation. In the year before the first switch to accrual accounting, around two third of all counties had a CSU head of government, and the CSU held 124 out of 180 seats in the state parliament (legislation period 2003-2008). Therefore, the CSU implemented the new budgetary law as the governing party with absolute majority in the Bavarian state parliament (see section 4.3). Other parties played only a minor role. The CSU dummy therefore measures not only a conservative ideology but also alignment with the state government.[20] Standard errors are clustered at the county level. Against the institutional homogeneity of county governments in Bavaria, these specifications allow isolating the effect of accrual accounting. Our baseline difference-in-differences regression equation takes the form:

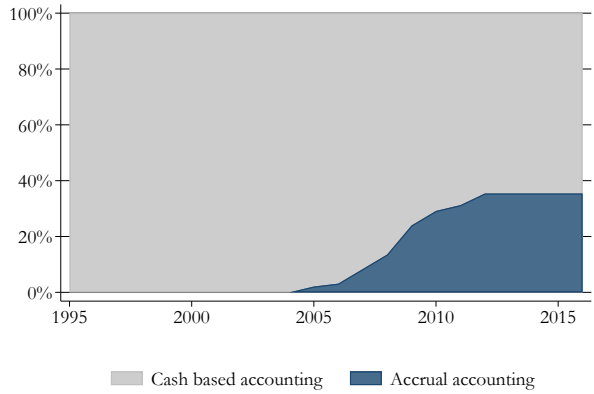
$$y_{it} = \alpha_i + \delta_t + \beta(Accrual_{it}) + X'_{it}\gamma + \epsilon_{it} \tag{4.1}$$

where y_{it} describes outcome y in county i in year t. α_i and δ_t are county and year fixed effects, X'_{it} is a vector of control variables, and ϵ_{it} denotes the error term. The coefficient of interest is β referring to the dummy variable $Accrual_{it}$ which takes on the value of one if a county i uses accrual accounting in year t, and zero otherwise. One main concern might be that sorting into different accounting standards is not exogenous. If counties applying accrual accounting already perform better than other counties, both may follow different trends and correlations might be spurious. Figure 4.3 provides some "eye-ball evidence" against temporal or spatial self-selection concerns. The upper figure shows that the share of counties with accrual accounting gradually increased to 35 % between 2005 and 2012. There is no temporal clustering. The map in figure 4.3 indicates some spatial clustering, especially in the north-west of Bavaria. Results do not change when we add district (*Regierungsbezirk*) and district-year fixed effects (see table A4.3 and table A4.4 in the appendix).

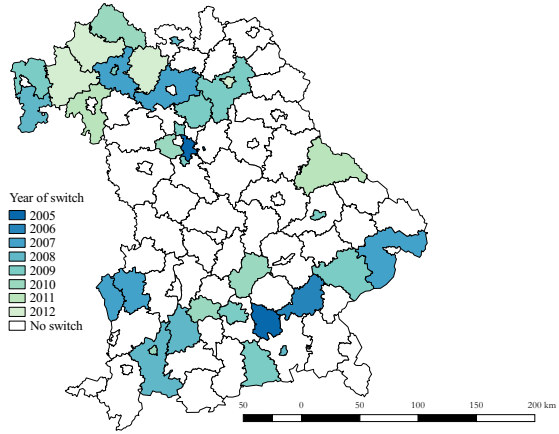
[20] The SPD was the second largest party in the Bavarian parliament during our period of observation and clearly preferred mandatory accrual accounting in the parliamentary debate. We have also tested the SPD seat share and SPD head of government as additional control variables. Inferences regarding our main results, however, do not change.

Figure 4.3: Accounting standards in Bavarian county governments

(a) Sample balancedness



(b) Map of Bavaria



Notes: The upper figure shows the cumulative share of accounting standards in the 96 counties of the German state of Bavaria between 1995 and 2016. The map shows regional adoption patterns. 34 shaded counties switched from cash-based to accrual accounting between 2005 and 2012 (the darker the shade intensity the earlier the switch). 62 white-shaded counties keep cash-based accounting.

Pre-reform characteristics do not predict the selection into accounting standards. Table 4.3 shows that socioeconomic, political and fiscal outcomes in the pre-reform period are not correlated with switching to accrual accounting.[21] First, we estimate survival models with switching accounting standards as the failure event using cox regressions (columns 1-3). Socioeconomic, political and fiscal outcomes do not significantly alter the hazard rate. Second, we use probit models to estimate the probability of switching accounting standards where we take average outcomes of the years 1996 to 2004, that is the time period before counties were allowed to switch to accrual accounting (columns 4-5). Again, neither socioeconomic outcomes such as population variables or GDP per capita, nor political outcomes such as party seat shares or fiscal outcomes such as total expenditures or public debt, significantly predict whether a county decides to switch to accrual-based accounting. Additionally, table A4.5 in the appendix shows that mean values in socioeconomic, political and fiscal pre-reform characteristics do not differ among counties that switched later to accrual accounting and counties that retained cash-based accounting.

Parallel pre-reform trends of switching and non-switching counties can be tested empirically by extending the twelve empirical models to event study regressions. In event study regressions, dummies for each year before and after switching to accrual budgeting replace the baseline dummy variable for accrual accounting. Three dummies measure the years before the treatment ($\leq$4, 3, and 2 years before switching), and eight dummies measure years after switching to accrual-based budgeting (1, ..., 7, and $\geq$8 years after switching). The year before switching to accrual accounting serves as the base category. There is large variation in the event study dummy variables because counties switched at different points of time between 2005 and 2012. The event-study design allows establishing whether accrual accounting counties performed differently than cash-based counties after, but also before, switching accounting standards. Our event-study regressions take the form:

$$y_{it} = \alpha_i + \gamma_t + \sum_{j=c}^{C} \beta_j (Accrual_{it}^{j}) + X'_{it}\gamma + \epsilon_{it} \tag{4.2}$$

where y_{it} describes outcome y in county i in year t. α_i and δ_t are county and year fixed effects, X'_{it} is a vector of control variables following equation (4.1), and ϵ_{it} denotes the error term. $\sum \beta_j$ refers to the vector of coefficients of interest. $Accrual_{it}^{j}$ takes on the value of one if a county i uses accrual accounting in $(t + j)$ years, and zero otherwise. j ranges from $c = -4$ and less to $C = +8$ and more, excluding -1 (base category).

[21] Inferences hardly change when we include *Regierungsbezirk*-year fixed effects instead of year fixed effects (see, table A4.3 for cox and probit regressions with district-year fixed effects; table A4.4 for the difference-in-differences results and figures A4.1 and A4.2 for the event-study results in the appendix). Bavarian counties are grouped into seven administrative districts (*Regierungsbezirke*); interactions among heads of government could be somewhat more intense within districts. We found a statistically significant effect of CSU heads of government on the cox regression but not in the probit estimations.

Table 4.3: Previous development does not predict switching to acrual accounting

	Cox			Probit		
	(1)	(2)	(3)	(4)	(5)	(6)
City county	0.40	0.46	1.08	0.47	0.48	-0.39
	(0.81)	(0.84)	(1.44)	(0.59)	(0.58)	(1.70)
Population (log)	0.26	0.25	0.34	0.19	0.17	0.08
	(0.45)	(0.48)	(0.56)	(0.34)	(0.34)	(0.43)
Old-young population dependency ratio	-0.02	-0.03	-0.03	-0.05	-0.05	-0.05
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Population share of foreigners	0.05	0.06	0.06	0.03	0.03	0.02
	(0.08)	(0.08)	(0.08)	(0.06)	(0.06)	(0.06)
GDP (Euro 1,000 per capita)	-0.02	-0.02	-0.01	-0.02	-0.02	-0.02
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
CSU seat share council		0.01	0.01		0.01	0.01
		(0.03)	(0.03)		(0.02)	(0.02)
CSU head of county government		0.59	0.55		-0.14	-0.11
		(0.40)	(0.42)		(0.36)	(0.36)
Expenditure (Euro 1,000 per capita)			-0.44			0.08
			(0.91)			(0.90)
Public debt core budget (per capita)			0.00			0.00
			(0.00)			(0.00)
Public debt public enterprises (per capita)			-0.00			0.00
			(0.00)			(0.00)
Pseudo R^2	0.01	0.02	0.03	0.03	0.04	0.05
Observations	1,869	1,869	1,869	96	96	96

Notes: The table reports the results of three cox regressions (columns 1-3) and three probit regressions (columns 4-6) where the 96 counties of Bavaria are the units of observations. The cox regressions estimate a survival model with the introduction of accrual accounting as the failure event. In the probit regressions the dependent variable is a dummy which is one if the country will switch to accrual accounting and zero otherwise. We average over the years 1996 to 2004, before the first counties switched to accrual accounting. Significance levels (standard errors in brackets): *** 0.01, ** 0.05, * 0.10 (no significant values to report).

4.5 Results

4.5.1 Baseline

Table 4.4 reports the baseline results for all fiscal outcome variables which are of main interest in our study.[22] Turning to expenditures first, administrative spending on material and services increase, while expenditure on staff and investment decrease. The difference-in-differences estimates do not meet the conventional levels of statistical significance, but are close to (t-value of 1.99 in the case of administrative expenditure). Similar to total investment expenditures, coefficients for construction expenditures in different categories such as schools or streets show a negative sign but do also not turn out to be statistically significant (see table A4.7 in the appendix). Public debt and the per capita county rate do also decrease on average. However, again, effects are also not statistically significant at the 10 % level. However, the structure of revenues from sales of assets changes after implementing accrual accounting. Politicians seem to sell fewer non-financial assets under accrual accounting. Revenues from

[22] Table A4.6 in the appendix shows the results for our control variables.

sales of non-financial assets decrease by around Euro 8 ($ 9) per capita on average, whereas revenues from sales of financial assets increase by around Euro 6 ($ 7) per capita. Among budget outcomes, however, increasing revenues from sales of financial assets such as bonds, investment funds or financial derivatives are the only statistically significant finding among our baseline results. The effect is statistically significant at the 10 % level. Our results are fully in line with Christofzik (2019) in showing that accrual accounting reduces investment expenditures and sales of non-financial assets but increases administrative spending. However, our results suggest that a reduction in sales of non-financial assets seems to be offset by increases in sales of financial assets. Therefore, accrual accounting seems to affect the composition of revenues.

Table 4.4: Baseline results (I) – Fiscal outcomes

	Expenditure			Revenues				Public debt	
	(1) Staff	(2) Administrative	(3) Investment	(4) Sales of non-financial assets	(5) Sales of financial assets	(6) County rate contributions	(7) County rate (%)	(8) Core budget	(9) Public enterprises
Accrual accounting	-9.40	11.57	-7.57	-7.58	5.91*	-8.81	0.01	-67.92	-24.08
	(7.73)	(8.94)	(10.80)	(4.68)	(3.14)	(6.74)	(0.45)	(60.43)	(30.86)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.16	0.13	0.10	0.05	0.01	0.67	0.56	0.19	0.04
Observations	2,112	2,112	2,112	2,016	2,016	1,562	1,562	2,112	2,112

Notes: The table reports difference-in-differences estimates. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10.

We also do not observe statistically significant effects of accrual accounting on non-budget outcomes. Table 4.5 shows that neither traffic accidents on county roads nor voter turnout in county elections change significantly after accrual accounting was implemented.[23] Accrual-based budgets do not seem to improve the transparency of public activities and to attract some marginal non-voters. If accrual accounting increases the quality in the provision of public goods, we had expected that accidents on county roads would decrease. A substantial part of accidents on county roads is caused by bad quality of the road surface. Road accidents therefore mirror the quality of local roads but we do not observe statistically significant effects of accrual accounting. Finally, effects on DEA technical efficiency are also not statistically significant at any conventional level in our baseline difference-in-difference results. Thus, we do not find that accrual accounting improves the way in which local governments translate inputs into outputs.

Table 4.5: Baseline results (II) – Non-fiscal outcomes

	(1) Technical efficiency	(2) Accidents on county roads	(3) Voter turnout
Accrual accounting	0.14	0.05	-0.09
	(0.49)	(0.04)	(0.81)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes
Within R^2	0.08	0.11	0.82
Observations	2,001	1,632	384

Notes: The table reports difference-in-differences estimates. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10. Technical efficiency multiplied by 100.

4.5.2 Event studies

County governments in Bavaria have to publish their first full consolidated financial statements five years after implementing accrual accounting. It may well take several years that transparency maps into policy changes. Pooled effects over the entire post-switching period may mask that effects fade in slowly. We therefore estimate event studies showing how effects of accrual accounting on our fiscal and non-fiscal outcome variables evolve over time – after and before counties introduced accrual accounting. Each dot in figures 4.4 and 4.5 represent one coefficient, vertical bars are 90% confidence intervals. Note that all estimates include year and county fixed effects and, similar to our baseline specification, control for population, age structure, foreigners, GDP per capita, party council seat shares and the party affiliation of the head of county government.[24] The base category is the last year before accrual accounting was introduced (year: -1).

[23] Results do not change for time lags of voter turnout. See table A4.8 in the appendix.
[24] The appendix provides full event study regression outputs in tables A4.9 and A4.10.

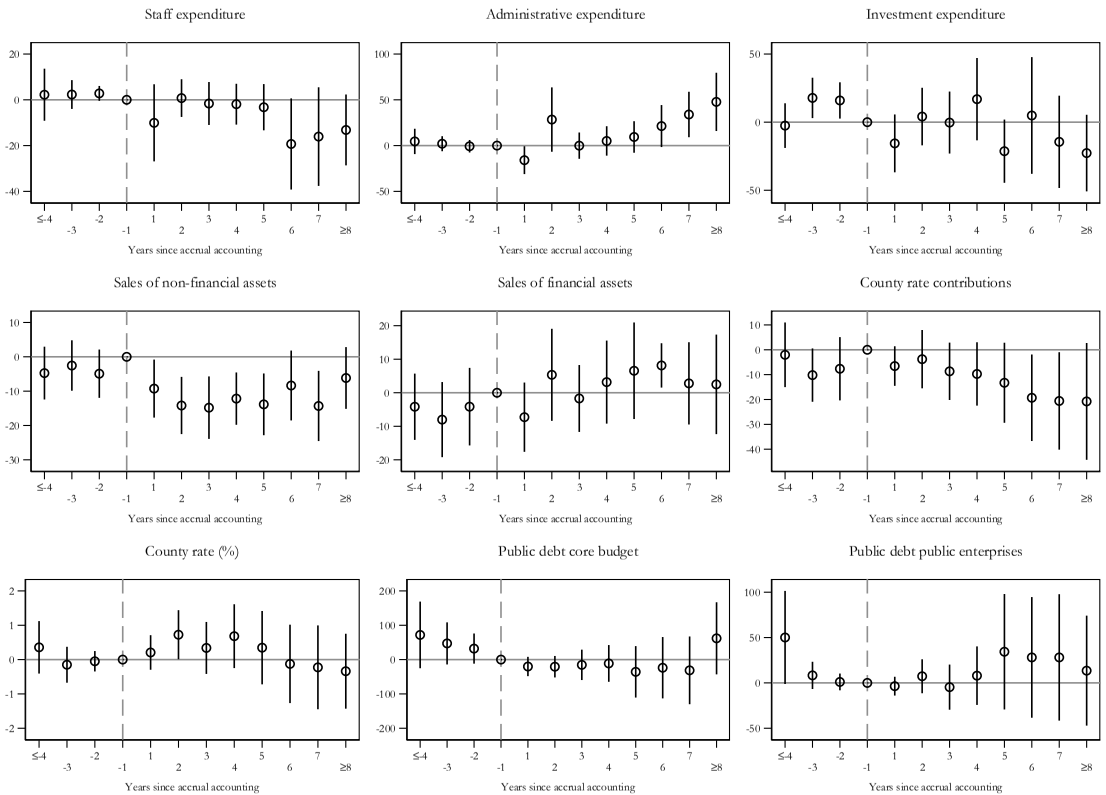
Again, we first turn to fiscal policies representing our main outcome variables of interest (figure 4.4). Pre-reform trends look promising: counties switching to accrual accounting do not deviate from counties using cash-based accounting before changing accounting standards. Both changing and not-changing counties follow common trends in pre-switching years as represented by circles on the left-hand side of the dashed vertical lines. As an exception, investment expenditure increases shortly before switching to accrual accounting. That might be due to an anticipation effect of county governments, which could decide to invest more before implementing accrual-based accounting standards. This is plausible as the investment decision that policy makers face differ under the two accounting systems: using cash-based accounting, the question is whether one can afford the investment in *this year* as only the cash outflow is reported; whereas under accrual accounting the question is whether one can also afford the investment in the *years to come*, that is including future depreciation costs.[25]

Post-reform coefficients plotted on the right-hand side of the dashed vertical lines report the effects of accrual accounting over time. The event-study findings shown in figures 4.4 and 4.5 corroborate our baseline findings. First, staff and investment expenditures tend to decrease after accrual accounting is implemented, but the effects are not statistically significant. Second, public debt does not seem to change at all. Even eight years (and more) after changing accounting standards, counties using accrual accounting do not perform differently in terms of borrowing than their counterparts keeping cash-based accounting. The same holds true for the efficiency and accountability measures (see figure 4.5). Technical efficiency steadily increases after introducing accrual accounting, but effects are never statistically significant at the 10 % level.

However, figure 4.4 also shows that changes in accounting standards may well map into outcomes. First, effects on operating costs of accrual accounting increase steadily over time. Figure 4.4 shows that administrative expenditures increase in years after county governments started to publish full consolidated financial statements. Six and more years after switching, counties using accrual accounting spend significantly more on administrative expenditures than counties using cash-based accounting. Second, sales of non-financial assets decrease immediately after introducing accrual-based accounting. The effect is statistically significant in six out of the seven years after switching accounting standards. Revenues from sales of financial assets, by contrast, significantly increase some six years after changing to accrual accounting. Both effects are in line with our baseline point estimates, which may indicate that outsourcing and selling public property below market values become less attractive under accrual accounting. Under cash-based accounting, policy makers can sell public property (even below market value and without asset valuation) to balance their annual cash-flow statement. This is not possible under accrual accounting, where the reduction in assets does

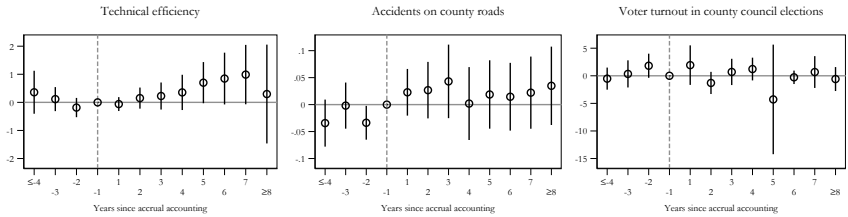
[25] Another minor exception is that road accidents are somewhat lower some two years before switching (10% significance level). See table A4.10 in the appendix.

Figure 4.4: Event study results (I) – Fiscal outcomes



Notes: Circles represent point estimates from event study estimations, bars are 90% confidence intervals (equivalent to * 0.1). -1 on the x-axis is the base category and denotes one year before the introduction of accrual accounting; 1 denotes the first year of implementing accrual accounting.

Figure 4.5: Event study results (II) – Non-fiscal outcomes



Notes: Circles represent point estimates from event study estimations, bars are 90% confidence intervals (equivalent to * 0.1). -1 on the x-axis is the base category and denotes one year before the introduction of accrual accounting; 1 denotes the first year of implementing accrual accounting. Technical efficiency multiplied by 100.

not help to balance the income statement (see figure 4.2). Finally, we observe that revenues from county rate contributions decrease significantly (at the 10 % significance level) after counties switched to accrual accounting after some six to seven years.

4.5.3 Robustness

Our main findings hold in several robustness and heterogeneity tests. Excluding control variables (table A4.11 in the appendix), or including further control variables such as unemployment rates and dummies for flood events in 2002 and 2013 (table A4.12) barely change the results.[26] When we exclude consolidated city-counties from the sample (table A4.13), however, our findings suggest less outsourcing to public enterprises under accrual accounting: in rural counties, debt levels of core public enterprises decrease by some Euro 28 ($ 31) per capita after the introduction of accrual accounting, whereas debt levels in the core administration increase to a similar amount. We also split the dataset at the median of GDP per capita county ranking in 2005 to assess heterogeneous effects on poor and rich counties (table A4.14). Effects of accounting standards may well depend on wealth and the level of development. Not all regions in Bavaria are as wealthy as the capital Munich. The poorest counties in Bavaria had a GDP per capita comparable to Slovenia, Portugal or Saudi Arabia as of 2016. However, estimates in poor counties are not statistically significant in any of the fiscal or non-fiscal outcome variables (table A4.14 in the appendix). In richer counties, by contrast, revenues from sales of non-financial assets such as land properties, buildings or machineries as well as the percentage county rate decrease after implementing accrual accounting (for both variables, the effect is statistically significant at the 10 % level). Despite many coefficients that differ

[26] We do not use unemployment rates as a baseline control variable because we do not observe unemployment rates for the entire period under investigation. Dummies for flood events are one in 2002 and 2013 when a county government declared emergency alert, and zero otherwise.

between both samples, point estimates showing increases in administrative expenditures are very similar but not statistically significant. Thus, if anything, accrual accounting matters more to rich than to poor administrations.

4.6 Discussion

Our results suggest that accounting standards do not have a large impact on the performance of governments. Public sector accrual accounting mainly targets investment expenditure and sustainable budgeting. Investment expenditure hardly changes after counties adopt accrual accounting. There are no significant differences even eight years after switching accounting standards. Similar findings apply to public debt. We find neither differences for the core budget, nor for outsourced budgets to public enterprises in our full sample. Rural county governments, however, somewhat shift debt from public enterprises to the core administration after introducing accrual accounting. This may indicate that accrual-based accounting prevents politicians from engaging in outsourcing in rural areas.

A major element of the case for public sector accrual accounting over cash-based accounting is efficiency. Our findings do not support this case at any conventional level of statistical significance.

Overall, accrual accounting hardly maps into superior budget and efficiency outcomes compared to cash-based accounting. One reason could be a lack of new public management skills of current public managers and political decision makers, who cannot make any use of the additional information and lack management capabilities. Another explanation might be that cash-based accounting already provides sufficient information to make effective budget and investment decisions. Many local governments, for example, added elements of valuating and monitoring their assets and debt under cash-based accounting. Voter turnout in county elections does not change with the introduction of accrual accounting. Even if accrual accounting enhances budget transparency, effects are not translated into greater accountability or increasing interest by the general public. The marginal voter does not seem to value accrual accounting. This could also be a reason why we do not observe an impact of accrual accounting. Voters do not seem to use the information provided by accrual accounting to evaluate the performance of politicians. Therefore, politicians do not have an incentive to change their behavior.

Our results show that adopting accrual accounting somewhat changes the structure of revenues of county governments, corroborating findings of Christofzik (2019). Revenues from sales of non-financial assets decrease after counties adopting accrual accounting, but this reduction is somewhat compensated for by increasing revenues from sales of financial assets. The findings are more pronounced among richer than among poorer counties. Sales of non-financial assets require time-consuming asset valuation after adopting accrual accounting

and become visible as losses in the resource-based accruals income statements. This might prevent public decision makers from selling non-financial assets such as land properties and buildings to balance cash-flow statements.

Finally, accrual accounting comes with implementation costs but also with permanent additional costs (Carlin, 2006). Government expenditures for materials and services increase around six years after implementing accrual accounting.[27] That is exactly the time when county governments have to present their first full consolidated financial statements after implementing accrual-based budgets. Higher administrative costs mirror the implementation costs of the full consolidated financial statements and reflect increasing budgeting complexity under accrual accounting leading to additional consulting services, staff training, and permanent software updates. These additional operating costs are not matched by benefits in other spending categories and efficiency gains are not found to be significantly different from zero.[28]

[27] Anecdotal evidence reports, for example, that introducing accrual accounting gave rise to transition problems including inconsistent and contradictory statements, time consuming asset valuation, costly expenses for new IT systems, staff training and external support services. Some counties even report severe mistakes in creating the new balance sheets and asset valuations due to overloading of the staff. After 2012 no further counties decided to implement accrual accounting in Bavaria. Quite the contrary, some local governments are discussing to switch back to cash-based accounting. See Süddeutsche Zeitung, April 9, 2015, "Sinn und Unsinn – Befürworter der Doppik", https://www.sueddeutsche.de/muenchen/landkreismuenchen/befuerworter-der-doppik-sinn-und-unsinn-1.2427815; Süddeutsche Zeitung, April 9, 2015, "Pioniere mit Problemen", https://www.sueddeutsche.de/muenchen/landkreismuenchen/vorreiter-gemeinde-pioniere-mit-problemen
-1.2427817; Nordbayerischer Kurier, May 16, 2015, "Bayreuth: Buchhalterpanne kostet 1,5 Millionen Euro", https://www.kurier.de/inhalt.stadt-beginnt-mit-aufarbeitung-der-falschen-bilanz-bayreuth-\buchhalterpanne-kostet-1-5-millionen-euro.221eeee7-9a0b-4d48-83e8-f92fdb2dd729.html; Selb-Live.de, November 29, 2018, "Aus dem Stadtrat notiert - Rückumstellung des Rechnungswesens", http://www.hochfranken-live.de/index.php/aus-dem-rathaus/6300-aus-dem-stadtrat-notiert-31.html.

[28] We show that observable pre-reform characteristics do not predict the selection into treatment (see section 4.2). Even more, event study results corroborate that the common trends assumption in our outcome variables hold (see section 5.2). One may still argue that unobserved characteristics such as the motivation of the head of the county administration and the members of the county council influence the selection into treatment decision and the government performance as more motivated decision makers more likely use the new management tools provided by accrual-based financial statements. The benefits of accrual accounting might then be overestimated due to an omitted variable bias. Our results, however, do not show significant effects which suggest that unobserved characteristics cause an overestimation of benefits. Thus, our results do not seem to be biased.

4.7 Conclusion

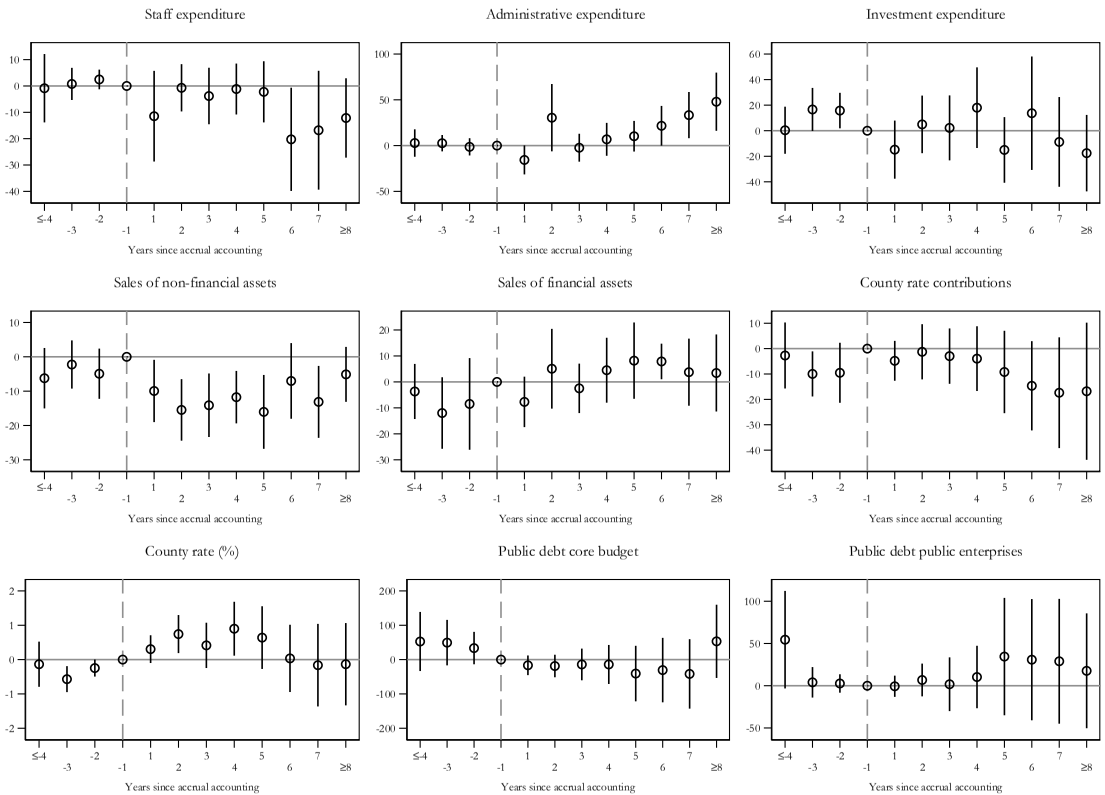
Our results suggest that public sector accounting standards do not matter much for the performance of local governments in high-income countries. Our findings question whether switching public sector accounting from cash-based to accrual-based standards is warranted in developed countries.

More generally, we have shown that fiscal rules do not always translate into preferable outcomes. Sound public accounting and budgeting are certainly important preconditions for the effectiveness of fiscal rules, but our results suggest that accounting standards themselves do not significantly affect public finance and government performance. Our data are drawn from a low corruption environment with monitoring by the media and public. The scope for benefit from improvements in transparency is greater in low-income countries where corruption may be prevalent. Further research is needed to investigate whether effects of accounting standards depend on the institutional context and the level of development.

An important next research step includes examining whether inferences change in the very long run when governments are used to accrual-based accounting for several years. Results may depend on specific public management skills of decision makers and on the institutional context. Reforms at other levels of government (for example, at the municipality, the state or the national level) can also be studied. Exploiting temporal and spatial differences in accounting standards across subnational governments appears to be a promising avenue.

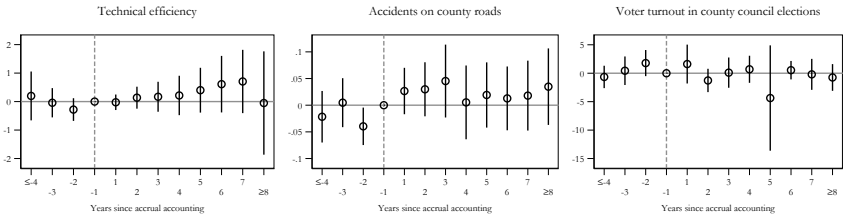
Appendix

Figure A4.1: Event study results (I) – Fiscal outcomes including district-year fixed effects



Notes: Circles represent point estimates from event study estimations where we include district-year fixed effects, bars are 90% confidence intervals (equivalent to * 0.1). -1 on the x-axis is the base category and denotes one year before the introduction of accrual accounting; 1 denotes the first year of implementing accrual accounting.

Figure A4.2: Event study results (II) – Non-fiscal outcomes including district-year fixed effects



Notes: Circles represent point estimates from event study estimations where we include district-year fixed effects, bars are 90% confidence intervals (equivalent to * 0.1). -1 on the x-axis is the base category and denotes one year before the introduction of accrual accounting; 1 denotes the first year of implementing accrual accounting. Technical efficiency multiplied by 100.

Table A4.1: Components of cash-based and accrual accounting

	Cash-based accounting	**Accrual accounting**		
Components	cash-flow statement (cash inflows, cash outflows)	cash-flow statement (cash inflows, cash outflows)	balance sheet (assets, liabilities, equity)	income statement (revenues, expenses)
Examples				
(a) sale of investment good: (market value: 10,000 sales value: 10,000)	financial cash inflow (+10,000)	financial cash inflow (+10,000)	non-financial asset (-10,000); financial asset (+10,000)	
(b) sale of investment good: (market value: 10,000 sales value: 12,000)	financial cash inflow (+12,000)	financial cash inflow (+12,000)	non-financial asset (-10,000); financial asset (+12,000); equity (+2,000)	revenues (+2,000)
(c) sale of investment good: (market value: 10.000 sales value: 8.000)	financial cash inflow (+8,000)	financial cash inflow (+8,000)	non-financial asset (-10,000); financial asset (+8,000); equity (-2,000)	expenses (-2,000)

Notes: The table shows a simplified three-component accounting system. While cash-based accounting consists only of the cash flow statement and accounts for cash inflows and outflows, the accrual accounting system consists of three parts. Similar to cash-based accounting, the cash flow statement covers cash inflows and outflows. Additionally, the balance sheet reports assets, liabilities and equity, and the income statement covers revenues and expenses. Furthermore, the table displays three examples to illustrate the differences between the accounting systems. All three examples (a-c) deal with the sale of an investment good. If an investment good is sold, cash-based accounting reports only the cash inflow, independent of the market and sales value. Under accrual accounting, however, the balance sheet reports the increase of liquid financial assets (at the sales value), but also the decrease of non-financial assets (at the market value). If the price equals the value of the sold assets, equity capital does not change (a). If the investment good is sold at a higher price than its market value, the revenues are reported in the income statement, which increases equity capital (b). If, in contrast, the investment good is sold under its market value, expenses are reported and the equity capital decreases (c).

Table A4.2: Descriptive statistics for DEA inputs and outputs

	Obs.	Mean	SD	Min	Max
Outputs					
County population (total, in 1000)	2,112	129.38	135.62	37.64	1,464.30
School age population (age 6 to 17)	2,112	16,172.28	13,143.39	3,891.00	135,446.00
Building permits	2,112	940.52	915.83	46.00	10,530.00
Length of county roads (km)	2,112	195.38	149.19	0.70	598.10
Registered vehicles	2,016	91,562.49	81,172.74	23,333.00	812,545.00
Beds in hospitals	2,102	831.67	1,329.14	20.00	13,398.00
Inputs					
Expenditure (Euro, in million)	2,112	193,045.46	564,741.84	43,405.09	6,615,576.00

Notes: The table reports descriptive statistics of the DEA input and output dataset. The 96 counties of the German state of Bavaria are the unit of observation; data span the period from 1996 to 2016. Length of county roads are imputed for the years 1996 to 1998 with values from 1999.

Table A4.3: Cox and probit regression including fixed effects

	Cox			Probit		
	(1)	(2)	(3)	(4)	(5)	(6)
City county	0.35	0.15	1.09	0.65	0.67	0.09
	(0.98)	(0.93)	(1.27)	(0.79)	(0.79)	(1.91)
Population (log)	0.35	0.37	0.52	0.21	0.18	0.11
	(0.41)	(0.40)	(0.47)	(0.37)	(0.38)	(0.48)
Old-young population dependency ratio	0.01	-0.03	-0.03	-0.08	-0.08	-0.09
	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Population share of foreigners	0.09	0.13	0.16	0.02	0.03	0.00
	(0.10)	(0.10)	(0.10)	(0.08)	(0.08)	(0.08)
GDP (Euro 1,000 per capita)	-0.03	-0.02	-0.03	-0.03	-0.03	-0.03
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
CSU seat share council		0.04	0.04		0.03	0.03
		(0.03)	(0.03)		(0.03)	(0.02)
CSU head of county government		0.88**	0.81*		-0.22	-0.20
		(0.42)	(0.45)		(0.42)	(0.42)
Expenditure (Euro 1,000 per capita)			-0.05			-0.09
			(0.35)			(0.97)
Public debt core budget (per capita)			-0.00			0.00
			(0.00)			(0.00)
Public debt public enterprises (per capita)			-0.00			0.00
			(0.00)			(0.00)
District-year fixed effects	Yes	Yes	Yes			
District fixed effects				Yes	Yes	Yes
Pseudo R^2	0.18	0.21	0.21	0.14	0.16	0.17
Observations	1,869	1,869	1,869	96	96	96

Notes: The table replicates the regressions from table 4.3 with district-year fixed effects for the cox regressions in columns 1 to 3 and district fixed effects for the probit regressions in columns 4 to 6. Significance levels (standard errors in brackets): *** 0.01, ** 0.05, * 0.10.

Table A4.4: Baseline results including district-year fixed effects

	Expenditure			Revenues				Public debt				
	(1) Staff	(2) Administrative	(3) Investment	(4) Sales of non-financial assets	(5) Sales of financial assets	(6) County rate contributions	(7) County rate (%)	(8) Core budget	(9) Public enterprises	(10) Technical efficiency	(11) Accidents on county roads	(12) Voter turnout
Accrual accounting	-7.77	13.45	-6.36	-6.67	6.71*	-4.32	0.53	-56.19	-23.22	0.13	0.04	-0.06
	(7.64)	(9.97)	(11.32)	(5.30)	(3.86)	(8.45)	(0.46)	(60.35)	(35.71)	(0.58)	(0.04)	(0.87)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.22	0.20	0.17	0.12	0.07	0.71	0.66	0.23	0.08	0.13	0.15	0.84
Observations	2,112	2,112	2,112	2,016	2,016	1,562	1,562	2,112	2,112	2,001	1,632	384

Notes: The table reports difference-in-differences estimates. The table replicates the regressions from tables 4.4 and 4.5 including district-year fixed effects. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10. Technical efficiency multiplied by 100.

Table A4.5: Pre-reform characteristics (2004)

	Mean cash-based accounting	Mean accrual accounting	Diff.	SD	Obs.
City county	0.24	0.29	-0.05	0.09	96
Population (log)	11.54	11.65	-0.11	0.11	96
Old-young population dependency ratio	50.14	49.04	1.10	0.72	96
Population share of foreigners	7.33	8.26	-0.94	0.87	96
GDP (Euro 1,000 per capita)	30.64	31.05	-0.40	2.88	96
CSU seat share council	44.65	45.59	-0.94	1.84	96
CSU head of county government	0.66	0.66	0.00	0.09	96
Expenditure (Euro 1,000 per capita)	1.19	1.33	-0.14	0.20	96
Public debt core budget (per capita)	551.14	704.36	-153.22	144.59	96
Public debt public enterprises (per capita)	124.42	199.56	-75.15	75.26	96

Notes: The table compares pre-reform characteristics of switching counties to counties keeping cash-based accounting. Significance levels: *** 0.01, ** 0.05, * 0.10 (no significant values to report).

Table A4.6: Baseline results

	Expenditure			Revenues				Public debt				
	(1) Staff	(2) Administrative	(3) Investment	(4) Sales of non-financial assets	(5) Sales of financial assets	(6) County rate contributions	(7) County rate (%)	(8) Core budget	(9) Public enterprises	(10) Technical efficiency	(11) Accidents on county roads	(12) Voter turnout
Accrual accounting	-9.40	11.57	-7.57	-7.58	5.91*	-8.81	0.01	-67.92	-24.08	0.14	0.05	-0.09
	(7.73)	(8.94)	(10.80)	(4.68)	(3.14)	(6.74)	(0.45)	(60.43)	(30.86)	(0.49)	(0.04)	(0.81)
Population (log)	-109.56	23.52	230.85*	-71.15	-10.58	202.46**	-5.15	-1,041.23	-20.43	3.26	0.57	-0.72
	(81.55)	(76.41)	(125.89)	(66.93)	(50.98)	(78.23)	(4.94)	(743.43)	(506.57)	(6.15)	(0.44)	(4.09)
Old-young population	2.98**	-0.38	3.07*	0.64	0.40	3.10*	-0.06	6.45	-1.37	-0.22	0.01*	-0.04
dependency ratio	(1.16)	(1.10)	(1.66)	(0.75)	(0.52)	(1.69)	(0.09)	(7.66)	(4.24)	(0.18)	(0.00)	(0.15)
Population share of	1.95	-3.46	-7.79	1.57	-1.70	6.39	0.35	60.08**	-4.76	-0.04	0.01	-0.22
foreigners	(3.20)	(3.45)	(6.68)	(3.00)	(1.89)	(5.30)	(0.25)	(26.01)	(18.43)	(0.27)	(0.02)	(0.26)
GDP (EURO 1,000	0.75	0.84	-1.11	0.28	-0.55**	5.75*	-0.09	-16.20***	-4.11	-0.05	0.00**	-0.06
per capita)	(0.66)	(0.80)	(1.00)	(0.59)	(0.22)	(2.94)	(0.09)	(5.39)	(3.41)	(0.05)	(0.00)	(0.04)
CSU seat share	0.32	0.58	1.59	0.46	-0.01	-0.49	-0.02	-0.34	1.54	-0.04	-0.00	0.02
	(0.50)	(0.37)	(1.08)	(0.35)	(0.13)	(0.43)	(0.03)	(3.44)	(2.46)	(0.05)	(0.00)	(0.03)
CSU head of county	1.03	-3.25	-2.98	0.32	-1.40	-4.22	-0.28	-10.74	-26.24	-0.65	0.03	-0.39
government	(5.22)	(6.77)	(8.43)	(3.21)	(1.51)	(4.18)	(0.35)	(29.68)	(21.60)	(0.47)	(0.03)	(0.31)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.16	0.13	0.10	0.05	0.01	0.67	0.56	0.19	0.04	0.08	0.11	0.82
Observations	2,112	2,112	2,112	2,016	2,016	1,562	1,562	2,112	2,112	2,001	1,632	384

Notes: The table replicates the results from tables 4.4 and 4.5 but shows all control variables. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10. Technical efficiency multiplied by 100.

Table A4.7: Construction expenditure

	Construction expenditure		
	(1) All	(2) Schools	(3) Streets
Accrual accounting	-6.32	-8.40	-4.18
	(8.50)	(6.26)	(3.09)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes
Within R^2	0.12	0.12	0.06
Observations	2,112	2,112	2,112

Notes: The table reports difference-in-differences estimates. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10 (no significant values to report).

Table A4.8: Time lags for effects on voter turnout

	Voter turnout		
	(1) Time lag: 1 year	(2) Time lag: 2 years	(3) Time lag: 3 years
Accrual accounting	0.01 (0.80)	0.54 (0.62)	0.86 (0.62)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes
Within R^2	0.82	0.83	0.87
Observations	384	384	288

Notes: The table reports difference-in-differences estimates where we lag voter turnout as dependent variable by 1, 2, or 3 years. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10 (no significant values to report).

Table A4.9: Event study regression output (I) – Fiscal outcomes

	Expenditure			Revenues				Public debt	
	(1) Staff	(2) Administrative	(3) Investment	(4) Sales of non-financial assets	(5) Sales of financial assets	(6) County rate contributions	(7) County rate (%)	(8) Core budget	(9) Public enterprises
Year: ≤ -4	2.22	4.55	-2.54	-4.74	-4.17	-2.02	0.36	71.83	50.10
	(6.85)	(8.34)	(9.87)	(4.63)	(5.95)	(7.81)	(0.46)	(58.44)	(30.89)
Year: -3	2.31	2.03	17.78**	-2.54	-8.01	-10.19	-0.15	47.12	8.29
	(3.79)	(4.94)	(8.92)	(4.42)	(6.75)	(6.44)	(0.31)	(36.94)	(9.04)
Year: -2	2.80	-0.73	15.94**	-4.92	-4.15	-7.63	-0.05	32.10	1.08
	(1.96)	(4.11)	(8.02)	(4.23)	(6.97)	(7.64)	(0.18)	(26.44)	(5.57)
Year: -1					*Baseline*				
Year: 1	-10.06	-15.93*	-15.59	-9.25*	-7.29	-6.54	0.21	-20.13	-3.60
	(10.14)	(9.16)	(12.78)	(5.09)	(6.22)	(4.78)	(0.30)	(16.97)	(6.25)
Year: 2	0.75	28.42	4.11	-14.18***	5.35	-3.77	0.72*	-20.71	7.33
	(4.96)	(21.18)	(12.72)	(5.02)	(8.27)	(7.04)	(0.43)	(18.85)	(11.28)
Year: 3	-1.61	-0.08	-0.30	-14.81***	-1.71	-8.66	0.34	-15.35	-4.69
	(5.65)	(8.64)	(13.70)	(5.48)	(6.01)	(6.93)	(0.45)	(26.66)	(14.98)
Year: 4	-1.92	5.07	16.86	-12.18***	3.18	-9.72	0.68	-11.21	8.00
	(5.37)	(9.68)	(18.20)	(4.59)	(7.46)	(7.65)	(0.56)	(32.20)	(19.42)
Year: 5	-3.23	9.40	-21.31	-13.85**	6.56	-13.27	0.35	-35.69	34.36
	(6.08)	(10.37)	(13.96)	(5.41)	(8.67)	(9.66)	(0.64)	(45.12)	(38.32)
Year: 6	-19.29	21.32	4.90	-8.35	8.15**	-19.30*	-0.13	-23.71	28.12
	(11.99)	(13.80)	(25.79)	(6.13)	(3.98)	(10.45)	(0.68)	(53.79)	(40.05)
Year: 7	-16.07	33.98**	-14.45	-14.32**	2.80	-20.58*	-0.23	-31.33	28.10
	(12.97)	(14.95)	(20.39)	(6.16)	(7.39)	(11.76)	(0.73)	(59.39)	(41.95)
Year: ≥ 8	-13.16	47.74**	-22.69	-6.16	2.50	-20.77	-0.34	61.96	13.53
	(9.33)	(19.17)	(16.92)	(5.41)	(8.94)	(14.10)	(0.65)	(63.21)	(36.50)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.16	0.16	0.10	0.05	0.02	0.68	0.56	0.19	0.04
Observations	2,112	2,112	2,112	2,016	2,016	1,562	1,562	2,112	2,112

Notes: The table reports the event study estimates corresponding with figure 4.4. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10.

Table A4.10: Event study regression output (II) – Non-fiscal outcomes

	(1) Technical efficiency	(2) Accidents on county roads	(3) Voter turnout
Year: $\leq$ -4	0.36	-0.03	-0.52
	(0.46)	(0.03)	(1.20)
Year: -3	0.12	-0.00	0.34
	(0.26)	(0.03)	(1.48)
Year: -2	-0.18	-0.03*	1.82
	(0.21)	(0.02)	(1.32)
Year: -1		*Baseline*	
Year: 1	-0.06	0.02	1.93
	(0.15)	(0.03)	(2.16)
Year: 2	0.15	0.03	-1.30
	(0.23)	(0.03)	(1.21)
Year: 3	0.23	0.04	0.70
	(0.29)	(0.04)	(1.43)
Year: 4	0.35	0.00	1.22
	(0.38)	(0.04)	(1.24)
Year: 5	0.70	0.02	-4.28
	(0.44)	(0.04)	(5.98)
Year: 6	0.85	0.01	-0.28
	(0.55)	(0.04)	(0.73)
Year: 7	0.99	0.02	0.68
	(0.64)	(0.04)	(1.74)
Year: $\geq$ 8	0.30	0.03	-0.59
	(1.06)	(0.04)	(1.31)
County fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes
Within R^2	0.08	0.11	0.84
Observations	2,001	1,632	384

Notes: The table reports the event study estimates corresponding with figure 4.5. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10. Technical efficiency multiplied by 100.

Table A4.11: Baseline results excluding control variables

	Expenditure			Revenues				Public debt				
	(1) Staff	(2) Administrative	(3) Investment	(4) Sales of non-financial assets	(5) Sales of financial assets	(6) County rate contributions	(7) County rate (%)	(8) Core budget	(9) Public enterprises	(10) Technical efficiency	(11) Accidents on county roads	(12) Voter turnout
Accrual accounting	-7.79	11.36	-1.44	-7.51	6.78*	-12.23	-0.05	-67.78	-23.64	-0.06	0.05	-0.06
	(7.26)	(9.27)	(11.01)	(5.08)	(3.75)	(8.97)	(0.47)	(69.03)	(32.17)	(0.47)	(0.04)	(0.90)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	No	No	No	No	No	No	No	No	No	No	No	No
Within R^2	0.13	0.12	0.07	0.04	0.01	0.61	0.55	0.08	0.03	0.05	0.08	0.82
Observations	2,112	2,112	2,112	2,016	2,016	1,562	1,562	2,112	2,112	2,001	1,632	384

Notes: The table reports difference-in-differences estimates where we exclude control variables. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10. Technical efficiency multiplied by 100.

Table A4.12: Baseline results controling for unemployment and flood in 2002 and 2013

	Expenditure			Revenues				Public debt				
	(1) Staff	(2) Administrative	(3) Investment	(4) Sales of non-financial assets	(5) Sales of financial assets	(6) County rate contributions	(7) County rate (%)	(8) Core budget	(9) Public enterprises	(10) Technical efficiency	(11) Accidents on county roads	(12) Voter turnout
Accrual accounting	-5.81	10.32	-8.60	-9.90*	6.53*	-7.10	0.26	-58.84	-16.29	0.18	0.05	-0.08
	(6.26)	(7.56)	(10.55)	(5.55)	(3.34)	(6.28)	(0.44)	(53.85)	(24.68)	(0.43)	(0.04)	(0.88)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.16	0.13	0.09	0.06	0.02	0.64	0.55	0.22	0.04	0.08	0.11	0.72
Observations	1,728	1,728	1,728	1,728	1,728	1,278	1,278	1,728	1,728	1,714	1,536	288

Notes: The table replicates the regressions from tables 4.4 and 4.5 controlling for the unemployment rate and a dummy, which is 1 in the year a country was affected by a flood and zero otherwise. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10. Technical efficiency multiplied by 100.

Table A4.13: Baseline results excluding city-counties

	Expenditure			Revenues				Public debt				
	(1) Staff	(2) Administrative	(3) Investment	(4) Sales of non-financial assets	(5) Sales of financial assets	(6) County rate contributions	(7) County rate (%)	(8) Core budget	(9) Public enterprises	(10) Technical efficiency	(11) Accidents on county roads	(12) Voter turnout
Accrual accounting	1.54	4.66	-6.25	0.62	2.71	-8.81	0.01	31.68	-28.09*	0.70	0.07	-0.71
	(1.74)	(5.76)	(6.87)	(0.95)	(1.68)	(6.74)	(0.45)	(22.66)	(15.34)	(0.46)	(0.05)	(1.08)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.54	0.15	0.13	0.03	0.03	0.67	0.56	0.09	0.06	0.11	0.17	0.81
Observations	1,562	1,562	1,562	1,491	1,491	1,562	1,562	1,562	1,562	1,480	1,207	284

Notes: The table reports difference-in-differences estimates where we exclude city-counties *(Kreisfreie Städte)*. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10. Technical efficiency multiplied by 100.

Table A4.14: Poor and rich counties

	Expenditure			Revenues				Public debt				
	(1) Staff	(2) Administrative	(3) Investment	(4) Sales of non-financial assets	(5) Sales of financial assets	(6) County rate contributions	(7) County rate (%)	(8) Core budget	(9) Public enterprises	(10) Technical efficiency	(11) Accidents on county roads	(12) Voter turnout
A: GDP per capita 2005 below state median (poor)												
Accrual accounting	-4.23	10.95	-1.95	0.06	7.14	4.13	0.89	-11.10	-1.94	-0.62	0.01	0.37
	(9.98)	(12.95)	(17.45)	(4.21)	(5.69)	(7.43)	(0.64)	(77.47)	(25.99)	(0.60)	(0.05)	(1.50)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.18	0.17	0.11	0.07	0.02	0.81	0.63	0.13	0.08	0.26	0.15	0.76
Observations	1,034	1,034	1,034	987	987	770	770	1,034	1,034	973	799	188
B: GDP per capita 2005 above state median (rich)												
Accrual accounting	-16.66	9.32	-13.44	-14.85*	8.76	-17.83	-0.99*	-128.70	-68.56	0.20	0.08	-0.13
	(10.72)	(10.84)	(12.09)	(8.76)	(5.35)	(10.56)	(0.54)	(102.77)	(53.32)	(0.64)	(0.06)	(0.59)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.22	0.14	0.12	0.06	0.05	0.63	0.52	0.25	0.05	0.16	0.11	0.92
Observations	1,078	1,078	1,078	1,029	1,029	792	792	1,078	1,078	1,028	833	196

Notes: The table reports difference-in-differences estimates for two subsamples of our dataset (poor and rich counties). Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10. Technical efficiency multiplied by 100.

5 Do direct elections matter? Quasi-experimental evidence from Germany[1]

Abstract

We estimate the causal effect of direct elections on the economic performance of politicians. Candidates running in direct elections to head local governments in the German state of Brandenburg need an absolute majority, and votes for the winner must represent at least 15% of eligible voters. If the quorum is not reached, direct elections are suspended, and local councils appoint the head of government. We examine election outcomes around the quorum, where the form of government is arguably exogenous. Event study results show that the public employment service becomes somewhat more effective under directly elected politicians. However, directly elected politicians do not seem to attract more businesses or expedite administrative acts.

[1] This chapter is joint work with Felix Roesel. It is based on our paper "Do Direct Elections Matter? Quasi-experimental Evidence from Germany" published in International Tax and Public Finance, 26 (6), 1416–1445, 2019.

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5.1 Introduction

There is an ongoing debate whether directly elected politicians perform differently than politicians appointed by parliaments (Persson and Tabellini, 2003). From a pure median voter perspective, the form of government should not matter at all. If a parliament mirrors voters' preferences, appointed and directly elected politicians should equally represent the median voter. In reality, however, the form of government is a topic of interest and subject to strategic actions (Robinson and Torvik, 2016). Violent protests followed the Turkish constitutional referendum in 2017, when a close majority of 51.4% voted for a new constitution that included a directly elected president. Discussions also apply to the local level, where the mayor-council system competes with the council-manager system. There is still no dominant form of government. In 2014, approximately 50% of US municipalities reported a council-manager system; 40% had direct elections (mayor-council system).[2]

Empirical studies indicate that the form of government influences public finances. Taxes tend to be lower if politicians are directly elected, and transfers from higher levels of government increase (Ade, 2014; Hessami, 2018). On the expenditure side, evidence is mixed. Studies have found that expenditures increase (Saha, 2011; Ade, 2014; Koethenbuerger *et al.*, 2014; Garmann, 2015), decrease (Baqir, 2002; Coate and Knight, 2011; Lewis, 2018) or do not change (MacDonald, 2008) when politicians are directly elected. Koeppl-Turyna (2016) stratifies categories of expenditures and finds that directly elected politicians spend less on public administration and staff but more on infrastructure, which she assumes is more visible to voters. Enikolopov (2014), by contrast, shows that the number of public employees is higher if chief executives are directly elected.[3]

In this chapter, we estimate the causal effect of direct elections on the administrative and economic performance of politicians beyond fiscal policy. A quorum applies to local elections in the German state of Brandenburg and determines the form of government. Like the USA, Brandenburg has two layers of local government: municipalities and counties. At both levels, candidates running to head local governments need an absolute majority of votes in direct elections, and votes for the winner must also represent at least 15% of eligible voters. If the quorum is not reached, direct elections are suspended, and local councils appoint the head of government. All other rules and institutions are equal. At the municipality level, candidates always easily exceed the quorum. Elections at the county level, by contrast, scatter around the 15% threshold because voter turnout is low. We examine close outcomes around the quorum in 14 county elections. In Brandenburg, the head of county administration (*Landrat*) is a powerful player who heads and organizes a county administration with an average of 850 employees. In direct elections for the head of county administration between 2010 and 2017, nine counties missed and six counties exceeded the 15% quorum, some by

[2] The remaining 10% report mixed or other forms of government. See the ICMA Form of Government Statistics – Municipalities (2014), April 02, 2018.

[3] There are further papers showing that election rules matter to economic outcomes. See, for example, Ferraresi *et al.* (2015).

only a few votes. In those cases, the form of government is arguably as good as exogenous. We employ difference-in-differences, event study, and synthetic control methods based on high-frequency monthly county data. Our results suggest that the form of government matters to the performance of politicians, but details are important. The public employment service for the long-term unemployed (*Jobcenter*), which is jointly administered by the county government and the federal public employment agency, becomes more effective. We find that long-term unemployment decreases under directly elected politicians, indicating that they are interested in delivering visible policies. By contrast, we do not find that directly elected politicians attract more businesses or expedite administrative acts.

Our study adds three novel aspects to the literature. First, our setting rules out self-selection of institutions to a large extent. Usually, the form of government is likely to be endogenous to political outcomes and strategies (Robinson and Torvik, 2016). Therefore, self-selection into forms of government is an issue with many prior studies. Governments may change the form of government for strategic reasons; the Turkish referendum in 2017 is an excellent case in point. Many empirical studies have exploited settings where jurisdictions self-select the form of government, which may lead to biased estimates. Other studies use temporal differences resulting from a gradual fade-in of direct elections. Direct elections were gradually introduced because election terms expired at different points in time. Even in those cases, governments are able to manipulate the time schedule (e.g., by enforcing retirements). Additionally, the pool of candidates may change when direct elections apply. Furthermore, changes in the form of government often coincide with additional overlapping reforms, for example, the duration of election terms or conditions for dismissing officials. We are able to abstract from self-selection and overlapping effects by simultaneous reforms. Using marginally met or missed quorums enables us to identify causal effects more properly. Moreover, in our setting, all other rules (responsibilities, role of the local council, election terms, dismissing rules, and so on) remain equal; only the election mode changes with the quorum.

Second, against the background of broad evidence on fiscal policy, little is known about whether the direct election of politicians influences public services and economic outcomes. This is particularly surprising because local administrations control public services *directly*, and business-friendly administrations can set the stage for economic growth. By contrast, decisions on public finance often require approval by councils. We investigate the effect of direct elections on economic and administrative outcomes, which are at the discretion of the head of government and do not require further council approvals. To account for changes in office during the year, we use high-frequency (monthly) data on unemployment, business registrations, and building permits at the county level.[4] We find effects in policy areas where local officials have some discretion. County governments and the federal public employment agency are jointly responsible for public employment services for the long-term unemployed in Germany. The short-term unemployed, by contrast, are served by the federal employment agency alone. Accordingly, we find that direct county elections have effects

[4] We can show that our results do not change when we use data on an annual basis.

on long-term unemployment but no effects on short-term unemployment. The number of business registrations and building permits do not change under directly elected heads of county administrations.

Third, our setting addresses some theoretical channels about *why* the form of government should matter to government performance. In our setting, directly elected and appointed politicians are equally accountable (same suspension rules). In most cases, the county council also appointed the candidate who has won the direct election when the quorum was not reached. Selection issues or information asymmetries among voters should therefore play a minor role. Neither directly elected nor appointed heads of government can be sure they will meet the 15% quorum in the next election. We can therefore also abstract from re-election motives. Finally, we compare directly elected politicians to council-appointed politicians who won the direct election but failed the quorum. Inferences do not change. Ruling out plenty of other channels, one possible interpretation of our findings is that direct elections create a different psychological climate between voters and politicians. Laboratory experiments have shown that followers accept deviating behavior from elected leaders rather from appointed leaders, and elected leaders behave more socially responsibly than appointed ones (De Cremer and Van Dijk, 2008).

5.2 Theoretical considerations

In a simple election model, both parliament and the president should equally represent the median voter. In this case, it should not matter to governmental outcomes whether a politician is appointed by the parliament or elected separately. In reality, however, there are large and emotional debates about whether to directly elect the head of government or not. Theoretical studies model differences across forms of government along five main dimensions: accountability, re-election motives, information asymmetries, selection of politicians, and psychological factors regarding self-conscious leadership.

First, accountability differs among different forms of government. Voters can hold a parliament or a president accountable if there are separate elections. In parliamentary systems, voters have to punish the parliament even if they (only) disagree with the appointed head of government. Maskin and Tirole (2004) argue that the "most important decisions should be taken by elected rather than nonaccountable officials" (p. 1050) in order to maximize welfare. Second, re-election motives may play a role. Elected politicians may face stronger incentives to gratify voters *directly* in order to increase re-election probabilities. Appointed politicians, by contrast, may reward the appointing institution rather than rewarding voters. Catering to the universe of voters or only a few councilors may well implicate different strategies and policies for the head of government. Third, Coate and Knight (2011) model information asymmetries among voters, which translate into different outcomes of governmental forms. If voters have only partial information about the policy preferences of local politicians, the form of government may matter to spending levels. Coate and Knight (2011) expect lower spending under direct

elections.[5] Fourth, direct elections may change the pool of candidates. Politicians appointed by councils are often described as managers or bureaucrats, while directly elected politicians are more likely to be charismatic leaders. Variations in characteristics are likely to translate into differences in policies. Fifth, the selection process itself may matter to the *perceived* backing of politicians. Direct elections and council elections "create [a] different psychological climate between leaders and follower[s]" (Hollander, 1992, p. 48). Direct election campaigns focus on personal characteristics and leadership, while council elections are dominated by political and party issues. Directly elected politicians are often said to feel more self-conscious because they know that they are backed by a majority of voters casting their votes explicitly for her name on the ballot sheet and not for an anonymous party organization.

However, even if one accepts that the form of government creates different psychological environments, consequences for policy outcomes are far from obvious. On the one hand, directly elected leaders feel more responsive to the interests and needs of their followers. Therefore, they might be more courageous in making difficult decisions, for example, when it comes to cut expenditures, reduce deficits, and implement reforms. On the other hand, directly elected politicians feel "closer" to the voters. They might have a stronger sense of social responsibility because followers put higher expectations on them to serve their interests (Julian *et al.*, 1969; Hollander and Julian, 1970; Ben-Yoav *et al.*, 1983; Kenney *et al.*, 1996; Grossman, 2014). This, in turn, may induce incentives to run popular but less-sustainable policies. Appointed leaders are different. Voters perceive that they have less legitimacy and followers place fewer expectations on them. Appointed leaders themselves are expected to have less interest in the needs of their followers (Hollander and Julian, 1970; Hollander, 1992; De Cremer and Van Vugt, 2002). In laboratory experiments, De Cremer and Van Dijk (2008) show that followers accept deviating behavior by appointed rather than by elected leaders and that elected leaders exhibited more socially responsible behavior than their appointed counterparts.[6] Hollander (1985) concludes that "appointment or election [...] affects a leader's actions" (p. 507). The direction of the effects, however, is unclear and remains an empirical issue that we aim to investigate in this chapter. We will return to this issue in section 5.3.2.

[5] Besides heterogeneous preferences of politicians which are only partially observed by voters, further key assumptions leading to this result are: (i) under mayor-council systems projects need the support of the mayor and the council, (ii) under the council-manager system projects need the support of the council. Therefore, less projects are realized under the mayor-council system. Coate and Knight (2011) find support for their model prediction in cross-sectional and panel analysis.

[6] Dal Bó *et al.* (2010) and Grossman and Baldassarri (2012) show that voter also perceive differences between elections and appointments. For example, the results by Grossman and Baldassarri (2012) indicate that individuals who can elect a leader contribute more to public goods than individuals who cannot chose their leader.

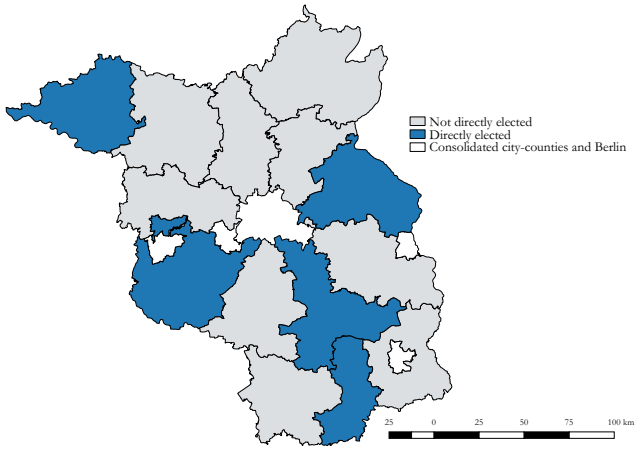
5.3 Background

5.3.1 Institutions

In the German state of Brandenburg, outcomes in local elections determine the form of government in a specific way. Votes for the winner in direct elections must represent at least 15% of all eligible voters. Otherwise, the direct election is suspended, and the local council decides on the head of government. We will return to this setting in detail later.

Brandenburg has two layers of local government: municipalities (*Gemeinden*) and counties (*Landkreise*). We focus on the county level because direct elections often fluctuate around the 15% quorum in county elections, while the quorum is always clearly passed in municipal elections. The state of Brandenburg surrounds the German capital of Berlin. Brandenburg has 14 counties, which roughly correspond with US counties regarding population size (150,000 inhabitants on average; for a map, see figure 5.1). There are also four consolidated city-counties (*kreisfreie Städte*, white shaded in figure 5.1), which are hardly comparable to the more rural counties in scope and responsibilities. For this reason, we use only counties in this study.

Figure 5.1: Counties in the German state of Brandenburg



Notes: The map shows the 14 counties of the German state of Brandenburg. In blue shaded counties, the 15% quorum was reached and the head of local government was directly elected. In gray shaded counties, the winner of direct elections missed the quorum of 15% and the head of local government was appointed by the local council. Four consolidated city-counties (Brandenburg an der Havel, Cottbus, Frankfurt/Oder, and Potsdam) and the state of Berlin (in the very center) are excluded (white shaded).

In Brandenburg, counties are responsible for plenty of public services. Tasks include education (school buildings, school capacity planning), public transport, social care, county roads, development of the local economy by granting subsidies, and administrative tasks such as drivers' licenses or building permits (Roesel, 2017). Counties account for approximately 7 to 10% of total government spending in Germany.[7] Social care, however, is the main responsibility of German counties. By 2016, Brandenburg county administrations spent some Euro 1,400 ($ 1,600) per capita on social care, which was approximately 75% of total county expenditures (Euro 1,900 per capita).[8]

Among social care, the public employment service for persons who are unemployed for more than one year (long-term unemployment) is the major service. The county administration and the federal public employment agency (*Bundesagentur für Arbeit*) jointly organize the *Jobcenter*, which is the local public employment agency for the long-term unemployed. Additionally, the costs are shared. The county reimburses the accommodation costs of the long-term unemployed; unemployment benefits are paid by the federal public employment agency. In some counties, the *Jobcenter* is fully decentralized to the county administration (*Optionskommunen*, see Mergele and Weber 2020). *Jobcenters* do not only provide job offers and qualifications but can also cut unemployment benefits if unemployed people are not willing to cooperate. County administrations thus have powerful measures but also plenty of discretion in designing labor market policies for the long-term unemployed. Services for short-term unemployed people (unemployed for less than one year), by contrast, are fully centralized and provided by the federal public employment agency. Counties do not have any influence on public employment services for the short-term unemployed; we will examine this difference later.

Responsibilities at the county level are shared between the powerful head of the county government (*Landrat*) and the local county council (*Kreistag*). Even though county politics sometimes lack public attention, the *Landrat* is considered a powerful political player, even sometimes described as a "regional prince"[9]. The *Landrat* in Brandenburg is the head of an administration with an average of 850 employees, he or she also has considerable discretion in designing county public services. First, the *Landrat* decides on the *location* of public services, which is likely to have implications for policy outcomes. For example, there have been heated debates in the Brandenburg county of Potsdam-Mittelmark on where the head of county government aims to concentrate public services from four branches to a single location in order to increase efficiency. Local politicians have discussed whether there should be

[7] Excluding social insurance expenditures. Tasks and expenditure shares vary across German states.

[8] Consolidated city-counties spend some Euro 3,600 per capita because they also perform the tasks of municipalities. Given that (rural) counties spend some Euro 1,900 per capita, county administration accounts for around one half of total local government expenditure in Brandenburg.

[9] Zeit Online, "Wenn der Wahlverlierer gewinnt", April 23, 2018, `https://www.zeit.de/politik/deutschland/2018-04/landratswahlen-brandenburg-wahlbeteiligung-gueltigkeit`.

exceptions for the public employment agency for the long-term unemployed (*Jobcenter*).[10] Second, *employment and organization* of county administrations are at the full discretion of the head of county governments. In the Unstrut-Hainich-Kreis county in the German state of Thuringia, for example, the *Landrat* proposed reorganizing the county administration, hiring managers and introducing controlling tools for social expenditures that are far beyond the state average.[11] Third, the head of county government has some discretion in designing the *scope and form of public services*. For example, the *Landrat* of Hersfeld-Rotenburg county in the German state of Hesse has not only completely reorganized the administration but also changed the office hours of the county offices where citizens can access the public employment agency.[12] There are many more examples from different counties in Brandenburg and other German states showing that the heads of county governments can exercise discretion in organizing their administration.

We now turn to election rules. After Germany's reunification in 1990, the East German state of Brandenburg introduced a council-manager system at the county level. The head of the county government was appointed by the county council. By 2010, Brandenburg had modified the electoral rules for county elections and introduced the direct election of the heads of local governments in order to increase voter participation. Winning candidates, however, do not only require the majority of votes cast but votes also must represent at least 15% of the eligible voters. Such a quorum for elections is unique to Germany; quotas usually only apply to referendums. If the 15% threshold is not reached, the direct election is suspended and the local council decides on the head of government. Direct election outcomes are not binding. The local council can choose from one of the candidates running in the direct election but can also appoint someone else. The mode of election does not influence any further rule or function. The head of local government in Brandenburg is elected for eight years, but he or she can be recalled from office by the voters via referendum, independent of whether he or she was elected or appointed.[13] The 15% quorum in Brandenburg dates back to 1993, when direct elections for mayors of municipalities were implemented. Rules for direct elections, including the 15% quorum, were simply rolled over to the county level in 2010. Thus, there is no link between voter turnout performance in county elections and the choice of a specific threshold.[14]

[10] Märkische Allgemeine, "Landrat will Verwaltung nach Beelitz umsiedeln", August 13, 2018, `http://www.maz-online.de/Lokales/Potsdam-Mittelmark/Mittelmarks-Landrat-blaest-zum-grossen-Umzug`.
[11] Thüringer Allgemeine, "Landrat will Kreisverwaltung neu strukturieren", August 13, 2018, `https://muehlhausen.thueringer-allgemeine.de/web/muehlhausen/startseite/detail/-/specific/Landrat-will-Kreisverwaltung-neu-strukturieren-1781514774`.
[12] Hersfelder Zeitung, "Das Landratsamt stellt sich neu auf", April 27, 2016, `https://www.hersfelder-zeitung.de/bad-hersfeld/zieht-einem-strang-6350821.html`.
[13] Similar rules apply to mayoral elections at the municipality level in Brandenburg. There was, however, not a single case of a missed quorum in mayoral elections.
[14] Accordingly, table 5.1 shows that voter turnout in elections before 2010 did not predict "successful" direct elections after 2010.

Direct elections have been gradually phased in since 2010 according to the remaining term of the head of local government in office. Temporal differences are a result of history. There were simultaneous elections in all counties in 1994. Afterward, however, not all heads of county governments served a full term because of, for example, retirement, dismissal, sickness, or death. Therefore, election schedules began to diverge across counties. The first direct election took place in Oberspreewald-Lausitz county in January 2010, while the counties of Oder-Spree and Potsdam-Mittelmark did not hold direct elections for the first time until January 2017. Despite the influential role of the head of county government, very low voter turnout in elections is widespread in all East German states, including Brandenburg (e.g., Mecklenburg-Western Pomerania 2018: 27.5% on average). Voter turnout in elections for the head of county government is often much higher in West German states (for example, Bavaria 2017: 61% on average). Therefore, since 2010, only five out of 14 counties in Brandenburg exceeded the quorum and elected a head of local government directly (blue shaded counties in figure 5.1). In the other nine counties, by contrast, the 15% quorum was not reached, direct elections were suspended, and the county council decided on the head of government (gray-shaded counties in figure 5.1). In six of the nine cases where the quorum was not reached, the county council elected the candidate who won the direct election. In two cases, the council appointed a candidate who was defeated in the direct election. In a single case, the council elected an external candidate who did not run in the direct election.

5.3.2 Hypotheses

How do we expect direct elections to influence policy outcomes in Brandenburg counties? County administrations have three main tasks that could be subject to changes: administrative services, local economic development, and the public employment service for the long-term unemployed. First, in section 5.2, we hypothesized that direct elections increase incentives to introduce popular policies. Expediting administrative acts is certainly popular. Heads of government may increase staff, reduce internal approvals, and reorganize employees in efficient team sizes. Reducing internal standards in favor of the citizens may also increase, for example, the turnaround of building permits. However, discretion in administrative services is often very limited due to state and federal law. If anything, we would expect that the number of administrative acts should increase under directly elected politicians. Policy measures regarding economic development are even more restricted at the county level, and the effect of direct elections is not that clear. On the one hand, appointed officials typically appear to be managers rather than politicians. The term council-*manager* system already indicates the perceived role of the head of government. Council-appointed managers, for example, are said to act more professionally and deliver efficiency. One may therefore expect that direct elections reduce efficiency and decrease economic activities, which we proxy with new business registrations. On the other hand, directly elected politicians may be more responsive to the needs of their voters. Delivering on local economic growth and jobs is certainly among the main objectives of politicians seeking re-election. Thus, it is not clear from a theoretical point of view whether and how direct elections affect economic outcomes.

Finally, the public employment service for the long-term unemployed is one of the main tasks of German counties. County administrations have considerable discretion on policy measures (for example, providing well-matching job offers and qualifications or cutting unemployment benefits). Unemployment is of the utmost importance to citizens of East Germany, including the state of Brandenburg, where unemployment rates increased to approximately 20% after reunification. One out of two East German citizens was unemployed for some time between 1990 and 2010 (25% in West Germany).[15] In 2009, 58% of East Germans worried about losing their job (West Germany: 46%).[16] In 2014, 43% of East Germans still feared becoming unemployed compared to 30% in West Germany.[17] Therefore, East German politicians who are responsive to their electorate are well advised to address the issue of unemployment. We would therefore expect that directly elected politicians are more inclined to reduce unemployment than appointed managers. For institutional reasons, county administrations can barely influence short-term unemployment but can impact long-term unemployment. We are therefore expecting differences between long-term and short-term unemployment and expect to see effects only for the former.

5.4 Empirical analysis

5.4.1 Identification

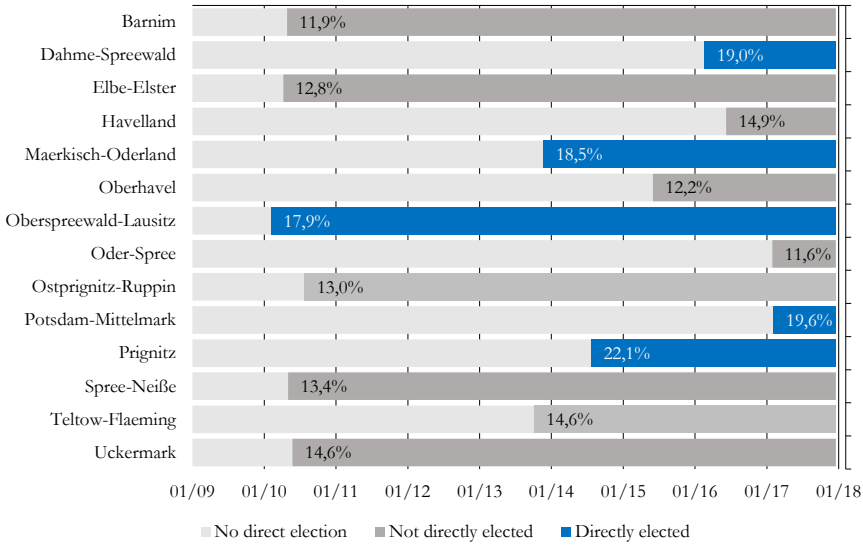
Our main identification assumption is that counties with a directly elected head of county government will have evolved in a similar way to counties with an appointed head of government if there had been no (successful) direct election. Two conditions must be met to estimate a causal effect. First, both groups of counties should follow parallel trends absent of treatment. This counterfactual scenario cannot be tested, but counties should follow a parallel trend in the period before the direct elections to indicate that this assumption is fulfilled. We later show in our event studies that this condition is fulfilled (see section 5.5.2). For example, we do not see that counties with directly elected heads of government differ regarding long-term unemployment from counties with an appointed head of government prior to inauguration (see the right-hand graph in the upper panel of figure 5.4). However, we also employ the synthetic control method to model common trends more explicitly.

Second, identification requires some exogenous source of variation in the form of government. Endogeneity of the form of government and policy outcomes is the main concern, and it is highly likely to bias regression results. We use both temporal variation in the introduction of direct elections and spatial variation resulting from the 15% quorum to identify the causal

[15] Deutsche Welle, "Jeder zweite Ostdeutsche war schon arbeitslos", February 20, 2010, https://www.dw.com/de/jeder-zweite-ostdeutsche-war-schon-arbeitslos/a-5265094.
[16] FAZ, "Die größten Ängste der Deutschen", September 3, 2009, https://www.faz.net/aktuell/gesellschaft/studie-die-groessten-aengste-der-deutschen-1855968.html.
[17] Wirtschaftswoche, "Wovor wir uns fürchten", September 4, 2014, https://www.wiwo.de/politik/deutschland/aengste-der-deutschen-wovor-wir-uns-fuerchten/10653642-all.html.

effect of direct elections. Figure 5.2 provides an overview of the timing and outcomes of the first direct elections in the counties of Brandenburg since 2010. Light gray bars represent periods without direct elections. In these periods, the head of local government (*Landrat*) was appointed by the county council. After 2010, direct elections were held when the term of the head of county government ended. The termination date is exogenous because, as explained in section 5.3, not all heads of county governments served a full term since 1994, inducing different timings to the elections. Ade (2014); Koethenbuerger *et al.* (2014); Martinez-Bravo *et al.* (2017) and Hessami (2018) examine differences in the timing of expiring election terms in a similar fashion.

Figure 5.2: Direct elections in Brandenburg counties



Notes: The figure shows the timing and the outcomes of direct elections for the head of local government (*Landrat*) in the 14 counties of Brandenburg. Transitions represent the day of inauguration, numbers in bars are the share of eligible voters casting their vote for the winning candidate. Light gray bars represent times of no direct elections (election by the local council). Dark gray bars represent direct elections that were suspended because the 15% quorum was not reached; the local council decided on the head of government. Blue bars show "successful" direct elections when the votes for the winning candidate represent at least 15% of the eligible voters.

However, we can rely on a second source of arguably exogenous variation: a missed or reached 15% quorum. Examining close election outcomes is a well-established strategy in the political economy literature (for example, Freier and Thomasius, 2016; Pettersson-Lidbom, 2008). The

blue bars in figure 5.2 indicate countries where direct elections were held and the quorum was reached; i.e., at least 15% of all eligible voters cast their vote in favor of the winning candidate. Dark gray bars, by contrast, represent direct elections that were suspended because the quorum of 15% was not reached. In those cases, the county council appointed the head of local government. The numbers in each bar represent the share of eligible voters casting their vote for the winning candidate, which is the crucial condition for the quorum. In some cases, the quorum was only missed by a few votes (14.6% and 14.9%). For example, in the county of Havelland, the winning candidate missed the quorum by only 175 votes (0.13%). At the threshold, the form of government can be assumed to be essentially exogenous. We later compare trends in counties close to around the 15% quorum threshold (see section 5.4.3).

Previous voter behavior does not predict whether the 15% quorum will be reached. Table 5.1 shows that voter turnout in county council elections prior to the first direct elections of the head of local government is not correlated with reaching (or missing) the quorum. We use voter turnout in the last county council election before 2010 in 2008 (column 1), average voter turnout in the elections in 2003 and 2008 (column 2), and long-term average voter turnout for all county council elections since reunification (column 3). Voter turnout in previous county elections does not predict whether the 15% quorum was reached or missed in either specification. Additionally, also the year of election does not predict whether the quorum was reached or not (column 4). We conclude that neither previous turnout performance nor general time trends influence the propensity of reaching the 15% quorum.

Table 5.1: Previous voter turnout and year of election do not predict direct election outcomes

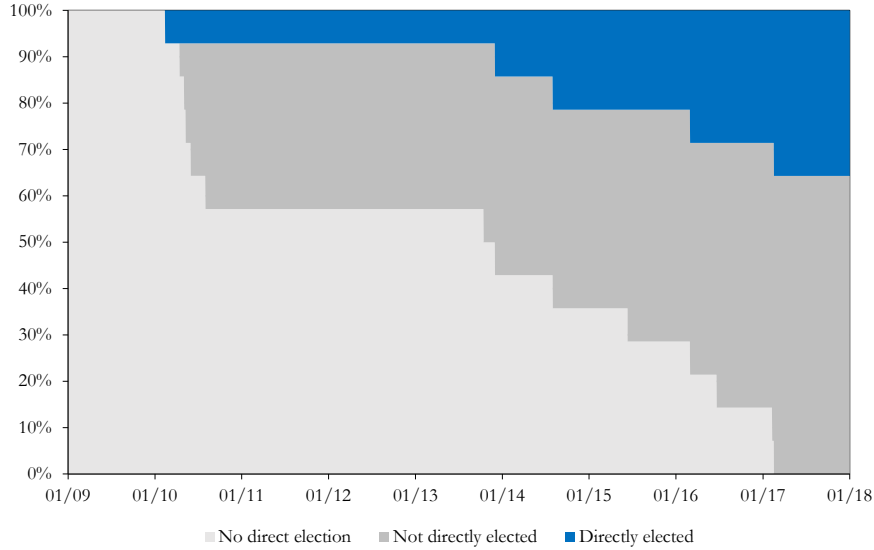
	Quorum reached (= 1)			
	(1) 2008	(2) 2003-2008	(3) 1993-2008	(4)
Turnout	0.11	0.10	0.01	
	(0.12)	(0.08)	(0.01)	
Election year				0.04
				(0.15)
Pseudo R^2	0.05	0.05	0.00	0.01
Observations	14	28	56	14

Notes: The table shows the results of probit regressions where the 14 counties of the state of Brandenburg are the units of observation. The dependent variable is the dummy *Direct*, which is one if the 15% quorum in the direct election for the head of local government was reached and zero otherwise. In column 1 to 3 voter turnout in previous county council elections is the regressor. We average over the elections in 2008 (column 1), 2003 and 2008 (column 2), and 1993, 1998, 2003, 2008 (column 3). In column 4 the year of the election is the regressor.

Accordingly, figure 5.3 shows that our sample is also well-balanced over time. Between 2009 and 2010, the head of local government was appointed in all counties. The share of counties with direct elections steadily increased over time. The share of counties with directly elected politicians developed somewhat proportionally to the share of counties with direct elections, although our sample is comparably small. We can therefore rule out temporal clustering; having earlier or later elections does not predict the “success” of direct elections. The map in

figure 5.1 also does not suggest any spatial clustering of "successful" direct elections. We are confident that – at least in counties scattering closely around the 15% quorum threshold – sorting into forms of government is essentially exogenous.

Figure 5.3: Sample balancedness



Notes: The figure shows the cumulative shares for no direct elections (light gray), direct elections that were suspended because the 15% quorum was not reached (dark gray) and "successful" direct elections when the votes for the winning candidate represent at least 15% of the eligible voters.

5.4.2 Data

We use monthly performance data for the 14 counties of the state of Brandenburg for the time period 01/2009 to 12/2017,[18] leading to a total of 1,512 observations. Because elections and inaugurations of newly elected heads of local government take place during the year, high-frequency monthly data are well-suited to capture the short-term effects of direct elections. Data on an annual basis less precisely mirror changes in government during the year. However, we show that inferences do not change when we use annual data. Consistent and reliable budget data are not available because accounting standards changed in 2011.

[18] Data cover the period 01/2008–12/2017; because we use first differences to the same month of the previous year in our analysis, first differences cover the period 01/2009–12/2017.

We begin by presenting results for data in levels but will quickly move on to first differences. We conduct tests for stationarity and trend stationarity. The results clearly show that we cannot reject the hypothesis that data in levels have a unit root (see table A5.1 in the appendix for Breitung test results). Taking first differences is a straightforward way to eliminate autocorrelation. When we use first differences with respect to the same month of the previous year, we can clearly reject the null of a unit root. Thus, we use first differences for all variables in our baseline specification, dummy variables being the exception. However, using data in levels does not change any results; we will also use variables in levels when we apply the synthetic control method as a robustness check.[19]

The dependent variables under investigation cover all the main tasks of the head of local government: local public employment services, administrative services, and local economic development. To describe the performance of local public employment services, we use series on short-term and long-term unemployment rates and the overall unemployment rate. Building permits represent the day-to-day administrative tasks of county administration. Finally, we use data on gross business registrations and net business registrations (registrations minus de-registrations) to proxy economic activity. As control variables, we include total population (log), the vote share for left-wing parties in the county council[20], and a dummy variable indicating decentralized counties self-administering the public employment service (no joint administration with the federal public employment agency).

Table 5.2 shows the descriptive statistics of our data. On average, unemployment rates (first differences to the same month in the previous year) decreased in Brandenburg, but long-term unemployment rates decreased more than short-term unemployment rates. The number of building permits increased in our sample period by 10.4 per million capita and per year. Gross and net business registrations decreased on average. In 14% of all observations, a directly elected head of government served in office. Total population and vote shares for left-wing parties in county council elections barely changed over time. Finally, in 45% of all observations, counties run a decentralized public employment service.

5.4.3 Regression specifications

We estimate difference-in-differences models, which we later extend to event studies. Our baseline model takes the following form:

$$\Delta Y_{itm} = \alpha_i + \theta_t \cdot \gamma_m + \beta Direct_{itm} + \Delta X'_{itm}\lambda + \epsilon_{itm} \tag{5.1}$$

where ΔY_{itm} describes our performance variables of interest in county i, year t, and month m. We use six different labor market but also administrative and economic outcomes on a monthly basis (first differences to the previous year) as performance variables. $Direct_{itm}$ is a

[19] Figure A5.1 in the appendix shows the development of our dependent variables between 2009 and 2017 for all 14 counties in levels.
[20] See Mechtel and Potrafke (2013); Hibbs (1977).

Table 5.2: Descriptive statistics

	Obs.	Mean	SD	Min	Max
Dependent variables					
Δ Unemployment rate, overall	1,512	-0.70	0.61	-2.80	1.50
Δ Unemployment rate, short-term	1,512	-0.24	0.28	-1.40	0.60
Δ Unemployment rate, long-term	1,512	-0.46	0.58	-2.60	2.00
Δ Building permits	1,512	10.37	134.91	-811.87	1,053.25
Δ Business registrations, gross	1,512	-23.73	157.18	-891.19	802.75
Δ Business registrations, net	1,512	-9.34	159.71	-824.57	711.15
Directly elected politician in office					
Direct (yes = 1)	1,512	0.14	0.35	0.00	1.00
Control variables					
Δ Population (log)	1,512	-0.00	0.01	-0.02	0.03
Δ Left-wing vote share	1,512	0.06	2.23	-7.72	7.78
Decentralized (yes = 1)	1,512	0.45	0.50	0.00	1.00

Notes: The 14 counties of the German state of Brandenburg are our units of observation. We use monthly data over the period 01/2009 to 12/2017. All variables are in first differences with respect to the previous year, dummy variables being the exception. Long-term and short-term unemployment rate refer to unemployed under *SGB II* and *SGB III*. Decentralized counties are fully responsible for the public employment service for long-term unemployed, not decentralized counties have joint public employment service for long-term unemployed with the federal public employment agency. Data on building permits and business registration are per million capita.

dummy variable and refers to our difference-in-differences estimator of interest. It takes on the value of one for counties with a directly elected head of local government, and zero if there was no directly elected head of local government (either there were no direct elections or the quorum was not reached). We include interacted year and month fixed effects ($\theta_t \cdot \gamma_m$), county fixed effects (α_i), and a vector of control variables, $\Delta X'_{itm}$ as described in section 5.4.2. ϵ_{itm} denotes the error term. We cluster standard errors at the county level. Because the number of clusters (14) is rather low, we also report wild bootstrapped p-values.[21]

As we discussed in section 5.4.1, we can rule out endogeneity to a reasonable extent. Election terms expired for historical reasons, inducing exogeneity in the timing of direct elections. However, we already described that we cannot fully rule out that election terms are manipulated according to county government performance. Therefore, we also investigate two subsamples of counties within a window of $\pm$3.0% and $\pm$4.5% around the 15% threshold. In counties close to the 15% threshold, reaching or failing to reach the quorum, and therefore the form of government, depends on factors that are arguably beyond political and economic outcomes. Some voters, for example, may have not cast their vote because of bad weather, holidays, or local festivals. Taking up the idea of regression discontinuity designs (RDD) (Lee and Lemieux, 2010; Ferreira and Gyourko, 2009), election outcomes near the 15% threshold can be treated as exogenous. Counties that barely reached the quorum should be similar in

[21] Inferences do not change when we use robust standard errors instead of clustering.

terms of unobservables to the counties that just marginally missed the quorum. Because we have a low number of observations, we restrict our sample to counties around the threshold and cannot estimate RDD.

We also estimate event studies where we replace $Direct_{itm}$ with a vector of dummies measuring the months before and after a directly elected head of government takes office. We include 11 dummies for the months before taking office (-12 and less to -2) and 36 dummies for the months after taking office (1 to 36 and later). The month before inauguration serves as the base category. Our high-frequency data allow us a more precise picture of the effects around changes in office. This also enables us to test the validity of the parallel trend assumption that our main specification in equation (5.1) rests on, because the event study design allows visualizing whether counties with directly elected politicians perform differently than counties with appointed politicians both before and after a new politician enters office. Our event study takes the following form:

$$\Delta Y_{itm} = \alpha_i + \theta_t \cdot \gamma_m + \sum_{j=c}^{C} \beta_j (Direct^j_{itm}) + \Delta X'_{itm}\lambda + \epsilon_{itm} \tag{5.2}$$

where similar to equation (5.1), ΔY_{itm} denote our monthly performance variables in first differences to the same month in the previous year, α_i, θ_t, and γ_m are county, year, and month fixed effects, $\Delta X'_{itm}$ is a vector of control variables, and ϵ_{itm} captures the error term. The vector of coefficients of interest is described by $\sum_{j=c}^{C} \beta_j$. $Direct_{itm}$ takes on the value of 1 if a directly elected politician enters office in county i in $(t + j)$ years and 0 otherwise. j ranges from $c = -12$ and less to $C = +36$ and more, excluding -1 (base category).

Finally, to account for the low number of treated units, we apply the synthetic control method developed by Abadie and Gardeazabal (2003) and Abadie *et al.* (2010, 2015) as a robustness test. We construct a synthetic counterfactual for all five Brandenburg counties surpassing the 15% quorum from a donor pool of the nine counties that failed the 15% quorum. The counterfactual is a weighted average of donor pool counties. Weights are derived in such a way that the synthetic counterfactual matches the pre-direct election period of the treated county best. This enables us to compare the development of all counties with "successful" direct elections with its synthetic counterpart before and after the inauguration of the politician.

5.5 Results

5.5.1 Difference-in-differences

Table 5.3 shows the results of our difference-in-differences estimations, where we move stepwise from annual data in levels to our preferred baseline specification using monthly data in first differences and control variables. We start with the most intuitive and basic specification in panel A, using annual data in levels and without any control variable. We

then turn to first differences and estimate equation (5.1) using annual data in first differences (to the previous year) in panel B. We showed in section 5.4.2 that our level data are likely to have a unit root, which can be fixed by first differencing. In panel C, we employ monthly data in first differences (to the same month in the previous year) instead of annual data to capture the timing of inaugurations during the year more precisely. Finally, we derive the most sophisticated specification in panel D, where we include control variables as discussed in section 5.4.2. This is our baseline specification.

Turning to the regression outcomes, we find a negative and significant effect of a directly elected head of government on the overall unemployment rate in all specifications (column 1). County administrations have substantial discretion in organizing the public employment service for the long-term unemployed (*Jobcenter*) but cannot directly influence services for short-term unemployment. Accordingly, we observe that the effect of directly elected politicians on overall unemployment rates is entirely driven by long-term unemployment (column 3). By contrast and as expected, we find no effect on short-term unemployment rates (column 2), an area where county administrations have hardly any influence. The effect on long-term unemployment is equivalent to around one third of a standard deviation in long-term unemployment changes and is therefore also economically substantial. We do not find systematic effects of direct elections on administrative acts (in this case: building permits, column 4) or business activity, which we proxy with business registrations (columns 5).

Our results are robust to different specifications. First, we substitute interacted month-year fixed effects with separate fixed effects for years and months. The results shown in panel A of table 5.4 fairly reproduce our baseline findings in table 5.3 panel D. Clustering standard errors by county-per-year (panel B) or excluding the month of inauguration from our analysis (panel C) does not change any inferences. We also resample our dataset in a manual jackknife procedure by leaving out each of the 14 counties. Inferences barely change when we exclude individual counties. In 14 (13) out of 14 jackknife-like regressions, we find a statistically significant effect of direct elections on total unemployment (long-term unemployment). Net and gross business registrations, building permits, and short-term unemployment rates, by contrast, turn out to be barely significant (only in 1, 4, 0 and 1 out of 14 regressions).[22] Thus, our results are robust to technical modifications.

A potential concern might be that the selection of candidates may drive the results, if the councils appoint individuals systematically differing from the winners in direct elections.[23] To rule out the concern of selection, we therefore exclude all three counties where the council did *not* appoint the candidate who won the direct election but failed to achieve the 15% quorum (panel A in table 5.5). The only systematic difference between directly elected politicians surpassing the 15% quorum and winners of direct elections who missed the 15% quorum but were appointed by the local council is the mode of election. Table 5.5 shows that the results

[22] Results are available upon request.
[23] However, one may also take this as one potential channel explaining differences across forms of government. See, section 5.2.

do not change when we adjust the control group as described. Another issue might be the presence of direct elections. In panel B, we include a dummy taking the value of one after the first direct election was held. Inferences do not change. Thus, not having yet held direct elections does not explain the differences between directly elected and council-appointed politicians in regard to long-term unemployment rates.

Finally, there is concern whether counties easily surpassing the quorum and counties clearly failing the quorum may also be different in terms of unobservables. We therefore restrict our sample to counties close to the 15% quorum threshold. Counties in this subsample should be even more comparable in unobservable characteristics because assignment into different forms of government is essentially exogenous. Table 5.6 reports the results for two different bandwidths, which provide a sufficient number of observations. In the upper panel, we reduce the bandwidth around the 15% threshold to $\pm$4.5%. Accordingly, the number of observations decreases from 14 to 12 counties, but the results remain robust. In the lower panel of table 5.6, we further reduce the bandwidth to $\pm$3.0%, which roughly halves the dataset and leaves us with eight counties. Inferences do not change; in fact, point estimates increase in this homogenous sample, and we find effects that are statistically significant at the 1% level. In conclusion, we find robust and economically substantial effects of directly elected politicians on long-term unemployment but not on short-term unemployment or other administrative outcomes.

5.5.2 Event studies

Because elections and inaugurations took place at different points in time across counties, we re-estimate our baseline specification using event studies as described in equation (5.2). Event studies allow for a more precise picture of the months before and after entering office. Event studies also allow us to indicate whether the difference-in-differences common trend assumption is met and to investigate the timing of the effects. We denote the first month of a directly elected head of local government in office (first month after inauguration) in each county by 1; the last month before she or he enters office is defined by -1 and serves as the base category. Vertical dashed lines indicate the moment of inauguration and therefore the transition from council-manager to mayor-council system.

Figure 5.4 shows the results. We do not observe significant differences in unemployment rates between counties with directly elected heads of government and appointed heads of government in the months ahead of inauguration; 90% confidence intervals always include the zero (see upper panel of graphics in figure 5.4). The results, however, change for the period after a directly elected head of county administration comes into office. Total unemployment rates decrease some months after inauguration. This effect is mainly driven by short-term unemployment, which also decreases. However, the effects are fairly small in size and become insignificant after few months. After a period of around two and a half years (30 months), long-term unemployment begins to decrease sharply and remains significantly lower under

directly elected heads of government. Overall unemployment rates decrease accordingly. Thus, it takes around two and a half years until changes in the form of government translate into substantial labor market effects. There are no striking patterns to report for building permits or business registrations. Coefficients fluctuate around zero, confirming difference-in-differences results that are not statistically significant.

5.5.3 Synthetic control

Inferences do not change when we include or exclude individual counties (see section 5.5.1). However, due to the comparably low number of observations, we also use the synthetic control method to model counterfactuals for all five counties surpassing the 15% quorum for direct elections. The nine counties failing to reach the 15% quorum are the donor pool.[24] The results are in line with our difference-in-differences estimations when trends before a direct election do not differ among counties and their synthetic counterfactual; however, we observe some diverging trends afterward.

Figure 5.5 shows the results of the synthetic control approach for short- and long-term unemployment rates in our five counties with "successful" direct elections using monthly data in levels.[25] Vertical lines indicate the month when a directly elected politician was inaugurated. First, we turn to long-term unemployment rates (right-hand side). In the case of the counties of Maerkisch-Oderland, Oberspreewald-Lausitz, and Prignitz, pre-inauguration trends of the synthetic counterparts reproduce the "real" counties very well. In Dahme-Spreewald and Potsdam-Mittelmark, the synthetic control method does not deliver overlapping but somewhat parallel trends. After the directly elected head of county government has been inaugurated, trends clearly start diverging in the cases of Maerkisch-Oderland and Oberspreewald-Lausitz. Additionally, in Prignitz and Dahme-Spreewald, real outcomes in long-term unemployment rates decline compared to the synthetic counterfactual. The post-election period in Potsdam-Mittelmark is too short to infer any trend. In any event, none of our synthetic control findings contradict our difference-in-difference results; some are strongly supportive. Patterns change when we turn to short-term unemployment (left-hand side of figure 5.5). The short-term unemployment rates of our treated counties and their synthetic counterparts follow very similar pre- and post-election trends. As expected, and in line with all previous findings, short-term unemployment rates do not change with the form of government.

[24] We cannot include counties from other states because all other comparable federal states have direct elections at the county level. The only German state not holding direct elections at the county level in our period of interest is Baden-Wuerttemberg, which differs drastically from Brandenburg (average unemployment rates 2009—2017: 4% vs. 10%).

[25] We cannot include annual data because our data start in 2009 and we have only one observation for elections held in 2010.

Figures A5.2 to A5.6 in the appendix show the full results for all five treated counties and for all performance variables, including overall unemployment rates, building permits, and business registrations. For building permits and business registrations, we do not find differences between treated counties and their synthetic counterfactual corroborating difference-in-differences and event study findings. Overall unemployment rates diverge after inauguration, which is driven by the decrease in long-term unemployment rates. In conclusion, the synthetic control approach supports all the main results. In at least three out of five counties with "successful" direct elections, long-term unemployment clearly decreases after a directly elected politician enters office. By contrast, we do not find an effect on short-term unemployment rates, building permits or business registrations.

5.5.4 Mechanisms

What are the driving forces behind our findings, and how do politicians change unemployment rates? County administrations account for only a small proportion of total county employment; hiring unemployed workers in the county administration is therefore hardly an option. There are two other, more reasonable ways. First, county administrations can increase subsidies, become more business-friendly and thus boost the economy and employment. Second, county administrations can change the incentives to work, i.e., the public employment service may become more efficient. For example, the public employment service (*Jobcenter*) has substantial discretion in sanctioning unemployed people who do not comply with job offers (Mergele and Weber, 2020).

Our evidence is more in line with the second channel: County administrations can become more efficient. First, our labor market effects are entirely driven by long-term unemployment rates, which can be influenced by the county administration to some extent. If a booming economy drives the results, we would expect to see short-term unemployment declining as well. Short-term unemployment, however, barely changes. Evidence on local economic activity supports this finding. Because there are no monthly GDP data available at the county level, we proxy the local economic performance by registrations of new businesses. Difference-in-differences results (see columns 5 in tables 5.3, 5.4, 5.5 and 5.6) indicate that gross business registrations go up while de-registrations also increase. Net business registrations (column 6) therefore reveal barely any significant effect; event study evidence (figure 5.4) is also inconclusive. Thus, we have little evidence that economic performance improves under directly elected politicians. Second, we have shown that it takes several months to observe any labor market effect. Reorganizing an administration takes time. In the case of Potsdam-Mittelmark (see section 5.3), for example, the directly elected head of county administration introduced reform proposals some two years after the election, which coincides with our findings.

However, increasing efficiency in one administrative task might be offset by decreases in efficiency in other tasks if politicians simply shift resources. We investigate whether the number of building permits changes in counties with a directly elected head of government. Building permits are one of the most demanding and important administrative services delivered by German county administrations. We find some weak negative effects on the number of building permits only in the subsample that is close to the 15% threshold (see column 4 in table 5.6). Against the background of robust labor market effects, this may indicate that personnel resources were moved to the social administration. Unemployment is likely to be more visible to the media and the voters than other administrative tasks. However, the effects on building permits are comparably weak, indicating that increases in public employment services do not necessarily come at the cost of other services.

We now return to the mechanisms we discussed in section 5.2. Our specific setting allows the discussion of some of the theoretical channels that may explain differences across forms of government. First, in our setting, directly elected and council-appointed heads of government are equally accountable for their decisions because the same suspension rules apply. Directly elected politicians cannot be held "more accountable" than their appointed counterparts. Thus, accountability cannot drive the results. Second, we can also widely rule out re-election motives playing a major role. Almost all election results are around the 15% quorum; the head of local government cannot be sure whether she or he will be re-elected in the next election, even if their political performance is good and there is no serious challenger. Third, information asymmetries, as modeled by Coate and Knight (2011), cannot account for the differences we revealed in our setting. In the majority of cases, the county council elected the candidate winning the direct election even when the 15% quorum was not reached. Therefore, the selection of candidates rarely changed when direct elections were replaced by council appointment. Finally, the selection of candidates should also not play a major role. We compare directly elected politicians surpassing the 15% quorum and winners of direct elections who miss the 15% quorum but were appointed by the local council afterward. The only difference between both groups of winners is that one group directly enters office while the other is appointed by the council a few weeks later. Because direct elections always apply and candidates do not change, the selection of candidates should not drive the results.

Thus, accountability, re-election motives, information asymmetries, and the selection of candidates are not able to explain our findings. We conclude that the only remaining theory, factors attributed to the self-consciousness of political leaders, is the most likely explanation off differences across forms of government. Elected leaders feel more socially responsible in serving their followers, while followers place higher expectations on them. Appointed leaders are said to be somewhat less interested in the needs of their followers and are perceived to have less legitimacy. Moreover, followers accept deviating behavior more from an appointed than from an elected leader (De Cremer and Van Dijk, 2008; Kenney *et al.*, 1996; Ben-Yoav

et al., 1983; Hollander and Julian, 1970; Hollander, 1985, 1992; De Cremer and Van Vugt, 2002; Grossman, 2014). Direct elections create stronger ties between politicians and voters that in turn can translate into differences in policy outcomes.

5.6 Conclusion

We examine a quasi-experimental setting in the German state of Brandenburg, where a quorum applies to direct elections of the head of local governments. Votes for the winning candidate must represent at least 15% of all eligible voters; otherwise, the direct election (mayor-council or presidential system) is suspended and replaced by council appointment (council-manager or parliamentary system). We use election outcomes of county elections around the 15% threshold where the form of government is arguably exogenous.

Our results show that the public employment service for long-term unemployment operates more efficiently under directly elected politicians, but it takes several months to reorganize the administration. We find little evidence that directly elected politicians attract more businesses or expedite administrative acts such as building permits. Our findings are in line with the recent literature showing that directly elected politicians enact more visible policies.

Our setting allows us to rule out some theoretical explanations of why direct elections matter. We find differences between directly elected politicians surpassing the 15% quorum and winners of direct elections who miss the 15% quorum but were appointed by the local council afterward. The only systematic difference between both groups is the mode of election. The literature has shown that follower-leader relations are different under direct elections compared to council appointments. Directly elected politicians seem to be more self-conscious and are therefore more likely to implement reforms. However, more research is required to examine the mechanisms at work more explicitly. Future studies may also focus more on administrative efficiency, which is widely considered one major reason to change the form of local government.

Table 5.3: Baseline results

	Unemployment rate			Building permits	Business registration	
	(1) Overall	(2) Short-term	(3) Long-term	(4) All	(5) Gross	(6) Net
A: Levels, year						
Direct	-0.53*	-0.25	-0.29	18.54	6.68	17.03
	(0.26)	(0.25)	(0.22)	(21.85)	(12.40)	(13.35)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	No	No	No	No	No	No
Within R^2	0.92	0.91	0.84	0.54	0.86	0.66
Wild bootstrapped p-value	0.08	0.44	0.26	0.44	0.62	0.27
Observations	126	126	126	126	126	126
	Δ Unemployment rate			Δ Building permits	Δ Business registration	
	(1) Overall	(2) Short-term	(3) Long-term	(4) All	(5) Gross	(6) Net
B: First differences, year						
Direct	-0.27**	-0.01	-0.26**	5.87	8.12	7.12
	(0.10)	(0.04)	(0.11)	(11.45)	(8.59)	(8.15)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	No	No	No	No	No	No
Within R^2	0.41	0.57	0.52	0.23	0.32	0.31
Wild bootstrapped p-value	0.03	0.78	0.05	0.73	0.37	0.52
Observations	126	126	126	126	126	126
C: First differences, month						
Direct	-0.30***	-0.05	-0.25**	-3.65	8.46	1.05
	(0.10)	(0.04)	(0.11)	(8.91)	(4.93)	(10.25)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	No	No	No	No	No	No
Within R^2	0.35	0.54	0.39	0.12	0.16	0.16
Wild bootstrapped p-value	0.00	0.16	0.02	0.69	0.15	0.92
Observations	1,512	1,512	1,512	1,512	1,512	1,512
D – Baseline: First differences, month						
Direct	-0.32**	-0.06	-0.25**	-5.43	10.10	2.33
	(0.11)	(0.04)	(0.11)	(8.67)	(6.19)	(10.09)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.36	0.55	0.40	0.12	0.16	0.17
Wild bootstrapped p-value	0.01	0.17	0.03	0.54	0.21	0.80
Observations	1,512	1,512	1,512	1,512	1,512	1,512

Notes: The table shows the results of difference-in-differences estimations. The 14 counties of the German state of Brandenburg are our units of observation. We use data over the period 01/2009 to 12/2017. Our variable of interest (*Direct*) takes on the value of one for directly elected heads of county government, and zero otherwise. The first panel (A) shows regression results for data in levels and annual data; the second panel (B) for first differences (to the previous year) and annual data; the third panel (C) for first differences (to the same month in the previous year) and monthly data; the fourth panel (D) for first differences (to the same month in the previous year) and monthly data with control variables. Additional control variables: total population (log), left-wing vote share in county council elections, dummy for decentralized public employment service for long-term unemployed. Data on building permits and business registration are per million capita. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10.

Table 5.4: Robustness (I)

	Δ Unemployment rate			Δ Building permits	Δ Business registration	
	(1) Overall	(2) Short-term	(3) Long-term	(4) All	(5) Gross	(6) Net
A: Year fixed effects, month fixed effects						
Direct	-0.33***	-0.06	-0.27**	-7.02	7.41	-0.91
	(0.11)	(0.04)	(0.11)	(8.84)	(7.23)	(10.79)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.25	0.39	0.32	0.02	0.04	0.04
Wild bootstrapped p-value	0.01	0.21	0.03	0.44	0.43	0.94
Observations	1,512	1,512	1,512	1,512	1,512	1,512
B: County-year cluster						
Direct	-0.32***	-0.06	-0.25**	-5.43	10.10	2.33
	(0.12)	(0.05)	(0.13)	(14.54)	(14.19)	(19.00)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.36	0.55	0.40	0.12	0.16	0.17
Wild bootstrapped p-value	0.02	0.34	0.09	0.73	0.52	0.90
Observations	1,512	1,512	1,512	1,512	1,512	1,512
C: W/o inauguration month						
Direct	-0.33**	-0.06	-0.26**	-5.45	8.49	4.94
	(0.11)	(0.04)	(0.11)	(8.10)	(7.06)	(10.43)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.35	0.55	0.40	0.12	0.16	0.17
Wild bootstrapped p-value	0.01	0.14	0.04	0.50	0.37	0.63
Observations	1,498	1,498	1,498	1,498	1,498	1,498

Notes: The table shows the results of difference-in-differences estimations. The 14 counties of the German state of Brandenburg are our units of observation. We use monthly data in first differences to the previous year over the period 01/2009 to 12/2017. Our variable of interest (*Direct*) takes on the value of one for directly elected heads of county government, and zero otherwise. The first panel (A) shows regression results with year fixed effects and month fixed effects (instead of their interaction); the second panel (B) shows regression results with standard errors clustered at the county-year level; the third panel (C) shows regression results excluding the month of inauguration. Additional control variables: total population (log), left-wing vote share in county council elections, dummy for decentralized public employment service for long-term unemployed. Data on building permits and business registration are per million capita. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10.

Table 5.5: Robustness (II)

	Δ Unemployment rate			Δ Building permits	Δ Business registration	
	(1) Overall	(2) Short-term	(3) Long-term	(4) All	(5) Gross	(6) Net
A: Smaller sample						
Direct	-0.32**	-0.04	-0.27**	1.12	14.23**	5.73
	(0.11)	(0.04)	(0.11)	(7.90)	(6.27)	(10.48)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.38	0.58	0.41	0.13	0.19	0.20
Wild bootstrapped p-value	0.01	0.36	0.03	0.90	0.10	0.56
Observations	1,188	1,188	1,188	1,188	1,188	1,188
B: Election dummy						
Direct	-0.55***	-0.10	-0.44***	-1.89	7.33	4.22
	(0.15)	(0.07)	(0.12)	(14.34)	(8.57)	(20.47)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.38	0.55	0.41	0.12	0.16	0.17
Wild bootstrapped p-value	0.00	0.21	0.00	0.91	0.41	0.85
Observations	1,512	1,512	1,512	1,512	1,512	1,512

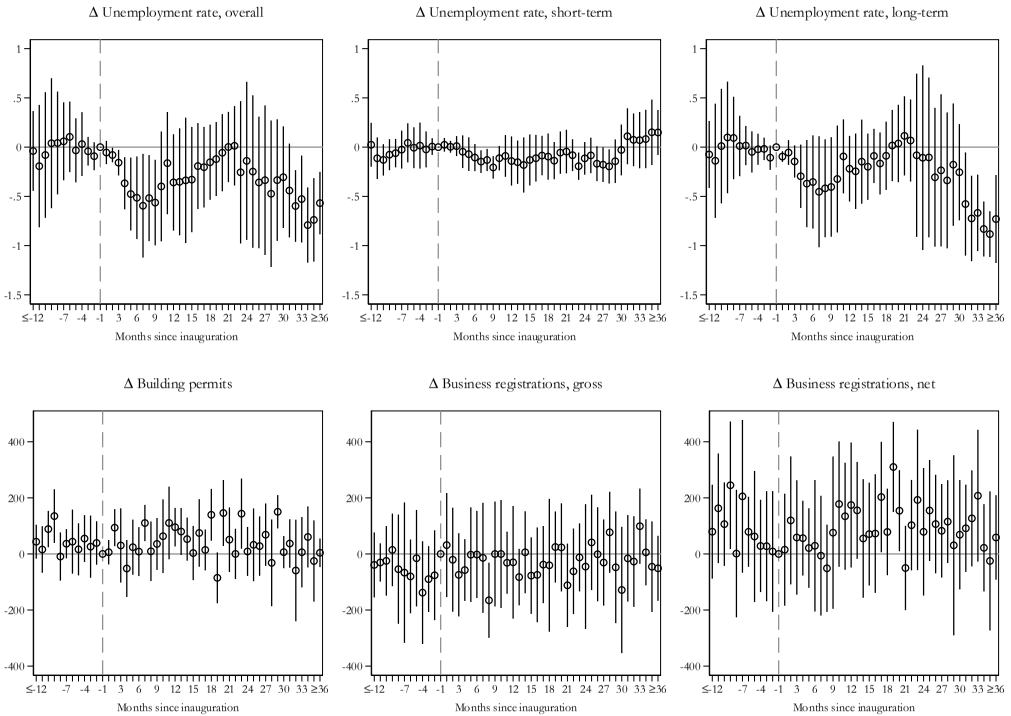
Notes: The table shows the results of difference-in-differences estimations. The 14 counties of the German state of Brandenburg are our units of observation. We use monthly data in first differences to the previous year over the period 01/2009 to 12/2017. Our variable of interest (*Direct*) takes on the value of one for directly elected heads of county government, and zero otherwise. The first panel (A) shows regression results for a smaller sample excluding counties where the council did *not* appoint the candidate winning the (suspended) direct election; the second panel (B) shows regression results when we include a dummy taking the value of one after the first direct election was hold. Additional control variables: total population (log), left-wing vote share in county council elections, dummy for decentralized public employment service for long-term unemployed. Data on building permits and business registration are per million capita. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10.

Table 5.6: Subsamples around the 15% quorum

	Δ Unemployment rate			Δ Building permits	Δ Business registration	
	(1) Overall	(2) Short-term	(3) Long-term	(4) All	(5) Gross	(6) Net
A: Bandwidth 4.5%						
Direct	-0.26*	0.01	-0.26	-11.54	15.17*	15.67
	(0.14)	(0.03)	(0.16)	(8.81)	(7.44)	(13.01)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.40	0.57	0.40	0.13	0.17	0.17
Wild bootstrapped p-value	0.08	0.83	0.09	0.26	0.22	0.34
Observations	1,296	1,296	1,296	1,296	1,296	1,296
B: Bandwidth 3.0%						
Direct	-0.82***	0.06	-0.88***	-35.90*	-1.31	-27.21
	(0.15)	(0.09)	(0.15)	(16.03)	(21.89)	(34.43)
County fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Further controls	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.45	0.58	0.42	0.16	0.19	0.18
Wild bootstrapped p-value	0.23	0.55	0.16	0.41	0.95	0.51
Observations	864	864	864	864	864	864

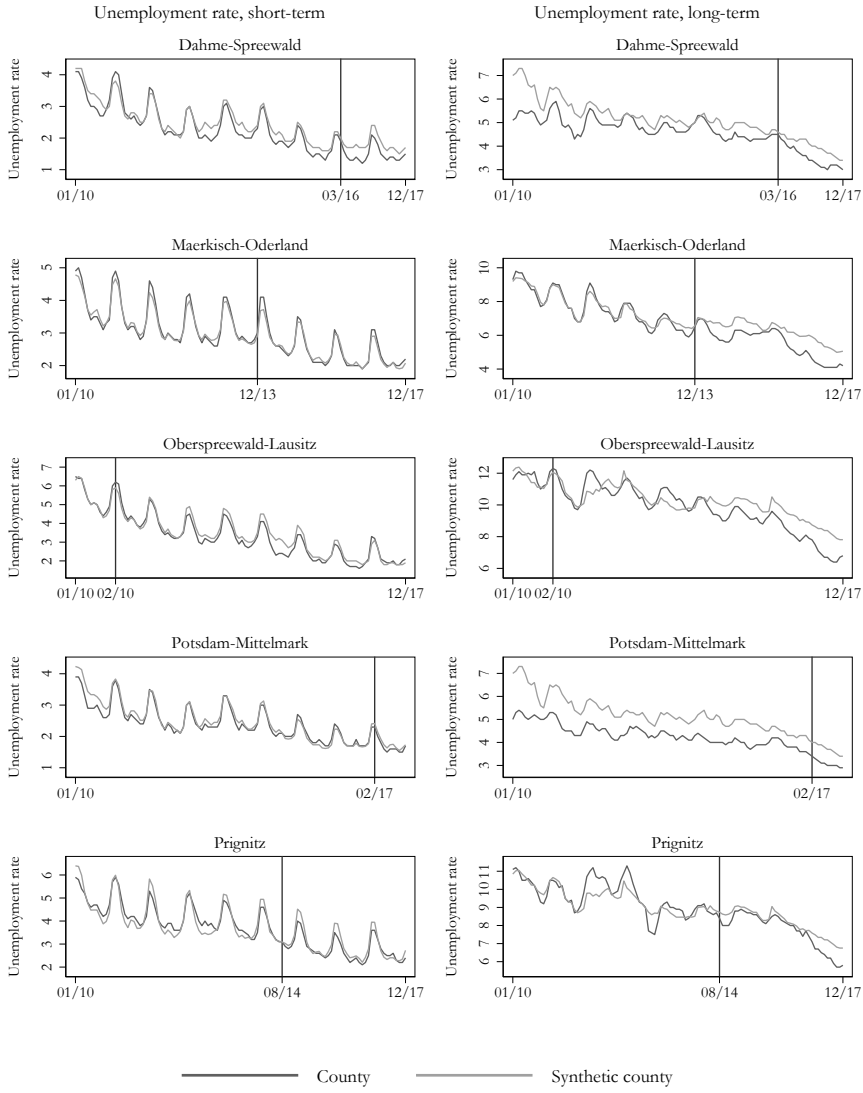
Notes: The table shows the results of difference-in-differences estimations. The 14 counties of the German state of Brandenburg are our units of observation. We use monthly data in first differences to the previous year over the period 01/2009 to 12/2017. Our variable of interest (*Direct*) takes on the value of one for directly elected heads of county government, and zero otherwise. The upper panel refers to a subsample of counties within a bandwidth of $\pm$4.5% around the 15% quorum, the lower panel refers to a subsample within a bandwidth of $\pm$3.0% around the 15% quorum. Additional control variables: total population (log), left-wing vote share in county council elections, dummy for decentralized public employment service for long-term unemployed. Data on building permits and business registration are per million capita. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10.

Figure 5.4: Event study results



Notes: The figures shows the results of event study estimations. Vertical dashed lines represent the inauguration of a directly elected politician. The 14 counties of the German state of Brandenburg are our units of observation. We use monthly data in first differences to the previous year over the period 01/2009 to 12/2017. Circles are point estimates, black lines represent the 90% confidence interval. We include year-month and county fixed effects and control variables (see notes for table 5.3, panel D). Month -1 is the base category. We cluster standard errors at the county level.

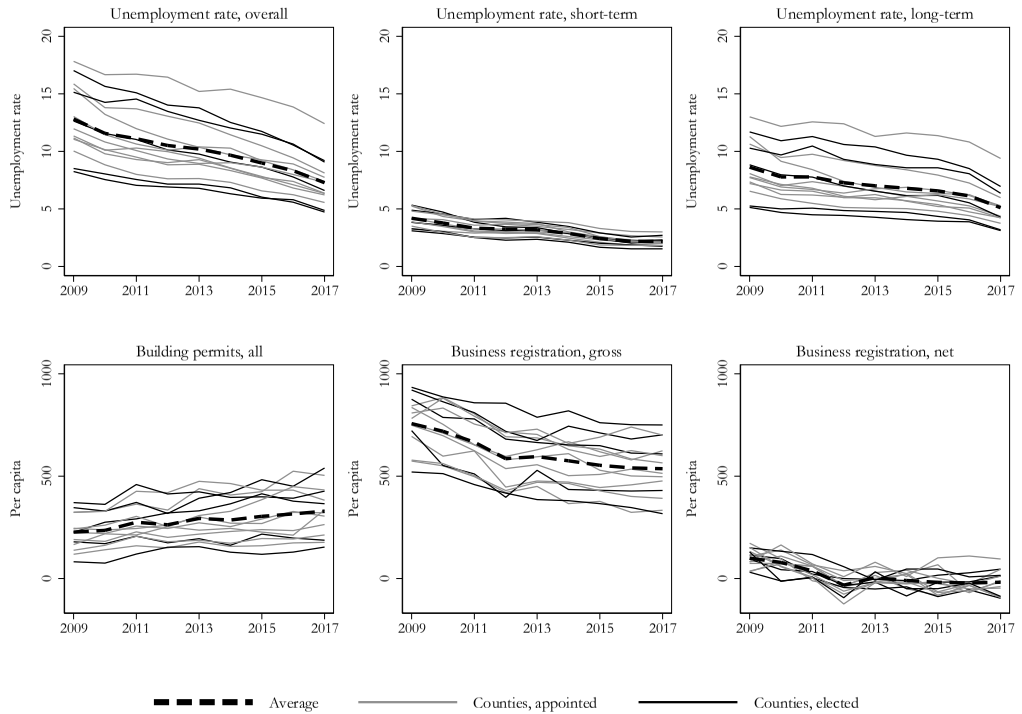
Figure 5.5: Synthetic control method – Unemployment rate



Notes: The figure shows short-term and long-term unemployment rates in five counties where the head of local government was directly elected (black) and their synthetic counterparts (gray). The vertical lines represents the month of inauguration of the directly elected head of local government. The donor pool consists of nine counties in the German state of Brandenburg where the 15% quorum was not reached.

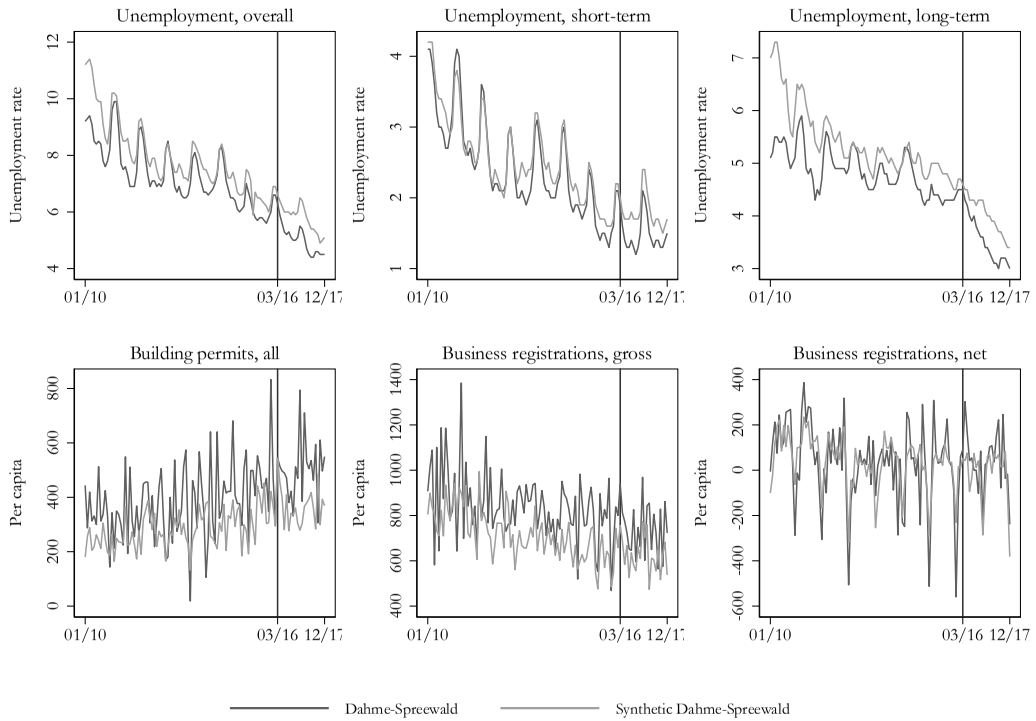
Appendix

Figure A5.1: Raw data plots



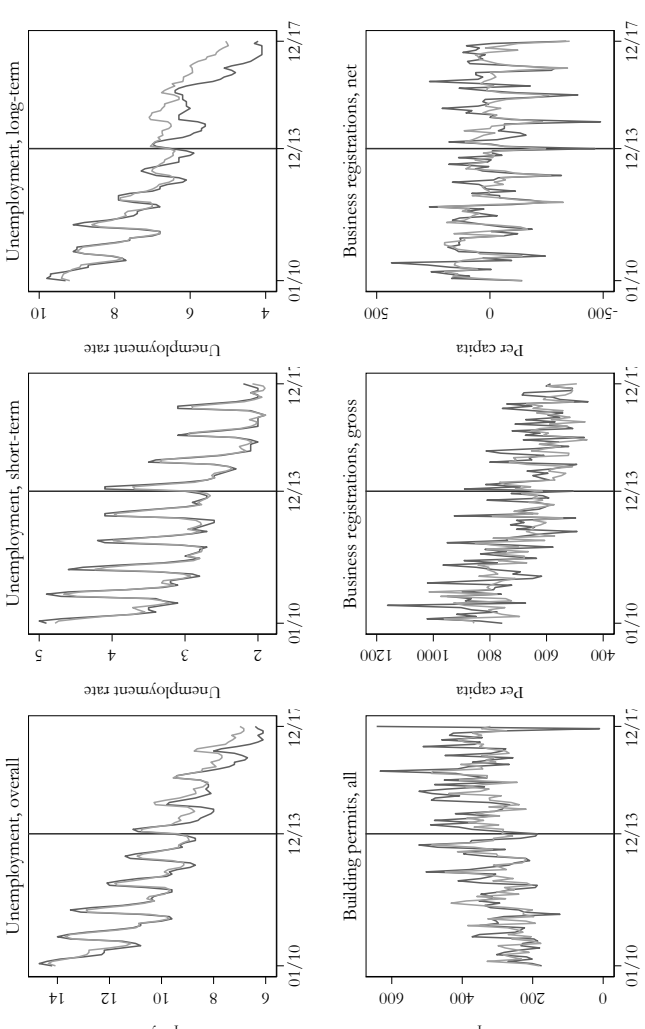
Notes: The figure shows how our six dependent variables evolve over the period 2009 to 2017 (annual data). The 14 counties of the German state of Brandenburg are our units of observation. Black dashed lines represent the average over all counties, solid lines represent individual counties. Gray lines show counties where the direct elections where suspended because the 15% quorum was not reached and the council decided on the head of government. Black lines show counties with "successful" direct elections where votes for the winning candidate represent at least 15% of the eligible voters. Data on building permits and business registration are per million capita.

Figure A5.2: Synthetic control method, Dahme-Spreewald



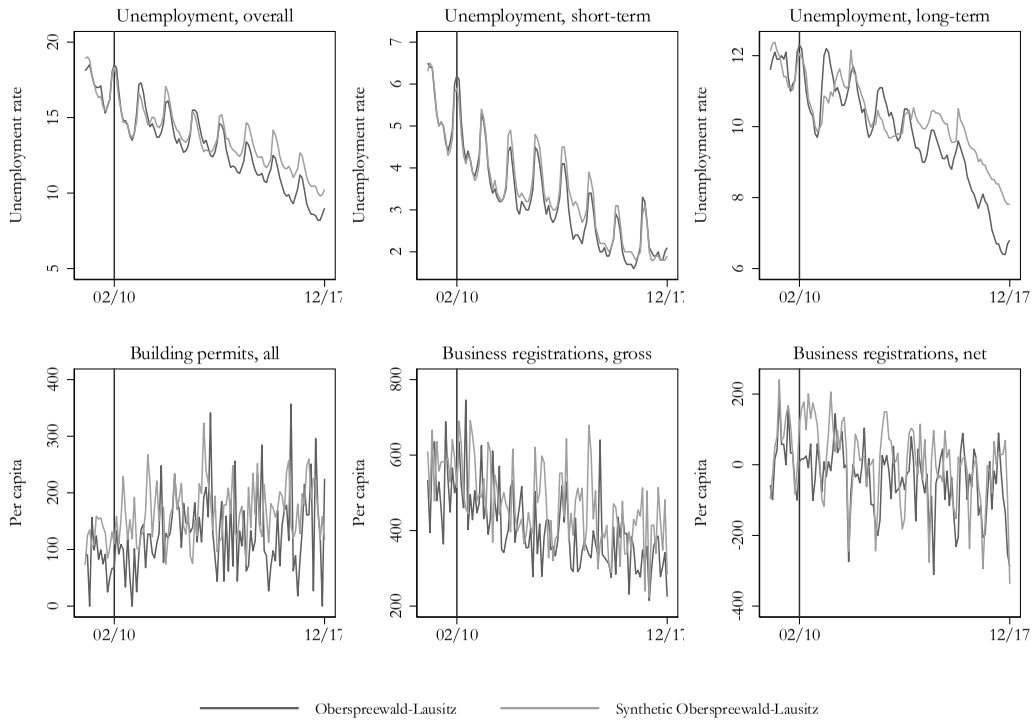
Notes: The figure shows our six dependent variables in the county Dahme–Spreewald and its synthetic counterpart (monthly data in levels). The vertical lines represents the month of inauguration of the directly elected head of local government. The donor pool consists of the counties in the German state of Brandenburg where the head of local government was never "successfully" elected and always appointed by the council. Data on building permits and business registration are per million capita.

Figure A5.3: Synthetic control method, Maerkisch-Oderland



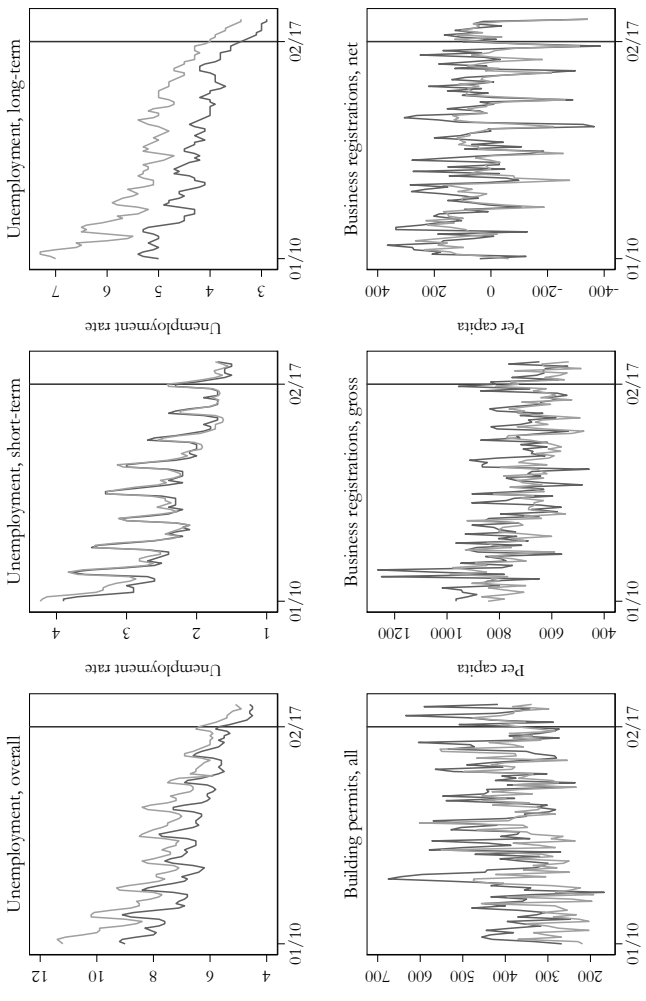
Notes: The figure shows our six dependent variables in the county Maerkisch–Oderland and its synthetic counterpart (monthly data in levels). The vertical lines represents the month of inauguration of the directly elected head of local government. The donor pool consists of the counties in the German state of Brandenburg where the head of local government was never "successfully" elected and always appointed by the council. Data on building permits and business registration are per million capita.

Figure A5.4: Synthetic control method, Oberspreewald-Lausitz



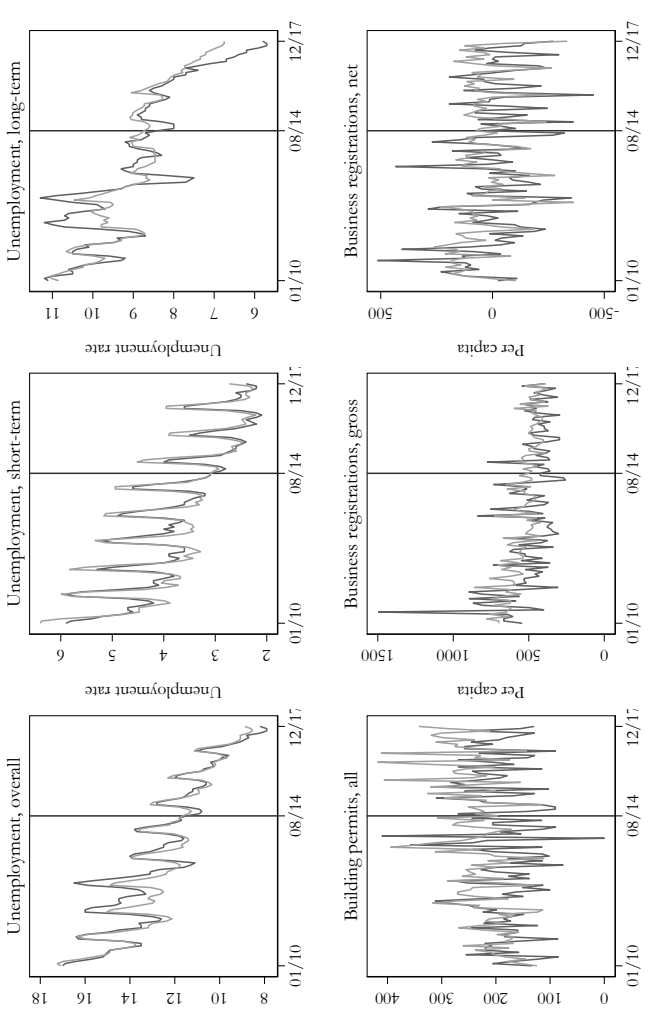
Notes: The figure shows our six dependent variables in the county Oberspreewald–Lausitz and its synthetic counterpart (monthly data in levels). The vertical lines represents the month of inauguration of the directly elected head of local government. The donor pool consists of the counties in the German state of Brandenburg where the head of local government was never "successfully" elected and always appointed by the council. Data on building permits and business registration are per million capita.

Figure A5.5: Synthetic control method, Potsdam-Mittelmark



Notes: The figure shows our six dependent variables in the county Potsdam–Mittelmark and its synthetic counterpart (monthly data in levels). The vertical lines represents the month of inauguration of the directly elected head of local government. The donor pool consists of the counties in the German state of Brandenburg where the head of local government was never "successfully" elected and always appointed by the council. Data on building permits and business registration are per million capita.

Figure A5.6: Synthetic control method, Prignitz



Notes: The figure shows our six dependent variables in the county Prignitz and its synthetic counterpart (monthly data in levels). The vertical lines represents the month of inauguration of the directly elected head of local government. The donor pool consists of the counties in the German state of Brandenburg where the head of local government was never "successfully" elected and always appointed by the council. Data on building permits and business registration are per million capita.

Table A5.1: Unit roots tests

A: Levels	Unemployment rate			Building permits	Business registration	
	(1) Overall	(2) Short-term	(3) Long-term	(4) All	(5) Gross	(6) Net
P–value (Breitung test)	1.00	1.00	1.00	0.55	0.99	0.09
P–value (Breitung test, time trend)	0.38	0.44	0.99	0.18	0.26	0.58
B: First differences	Δ Unemployment rate			Δ Building permits	Δ Business registration	
	(1) Overall	(2) Short-term	(3) Long-term	(4) All	(5) Gross	(6) Net
P–value (Breitung test)	0.00	0.00	0.00	0.00	0.00	0.00

Notes: The table shows results of the Breitung test. The null hypothesis is that all panels have a unit root. The 14 counties of the German state of Brandenburg are our units of observation. We use annual data over the period 2009 to 2017. The first panel (A) shows p-values of the Breitung test using level data and excluding and including a time trend to consider trend stationarity; the second panel (B) shows the p-value of the Breitung test using data in first differences (to the previous year).

6 Administrative areas and regional identity formation: The case of East Germany[1]

Abstract

I use an historical quasi experiment to examine sub-national and national identity formation. Under the governance of the GDR, states were dissolved and replaced by districts. In the course of German reunification states had to be re-established. As borders of states and districts did not coincide, state affiliation of some counties was not clear and referendums were conducted to draw new state borders. I use voter turnout in state and federal elections to measure revealed regional identity at different layers. Using difference-in-difference estimate I show that in counties where it was not clear to which state they would belong, regional identity with the state is lower (voter turnout decreased by around 3.6 percentage points), while national identity is similar to other counties. Changing administrative areas does on average not give rise to identity formation with the new region, even after 30 years.

[1] I thank Klaus Gruendler, Florian Neumeier, Niklas Potrafke, Felix Roesel, and Julia Rose for helpful comments.

6.1 Introduction

In a globalized world ties to regional entities like the nation state or a sub-national region seem to become more important. Prominent examples are the Brexit, where citizens apparently do not identify with the European Union any more but rather with the United Kingdom, or Catalonia, where the identity with the Catalan region is stronger than with the nation state Spain. Attempts for secession are often driven and motivated by deep regional ties or "roots".[2] Moreover, political parties who are critical towards the European Union, advocate the nation state and national identity are gaining electoral success in recent years.[3] But in the current debate it is often neglected that citizens posses more than one regional identity. One may well identify oneself as a European, a German, a Bavarian and a citizen of Munich simultaneously. Individual regional identities do not always exclude one another nor do they have to be substitutes.

I use a quasi-experiment to examine how changing administrative areas influences regional identity in the long-run. Regional identity is a form of group identity, which is describes as a key determinant for collective action (Paasi, 2003; Donatella and Diani, 1999), preferences (Costa-Font and Tremosa-Balcells, 2008), and a prerequisite for functioning societies and states. Nevertheless, it is yet poorly understood. Regional identity is affected by repressions and threats, but also by shared experiences (Dehdari and Gehring, 2019; Fouka, 2020; Gehring, 2020; Depetris-Chauvin *et al.*, 2020).

I investigate identity formation within new administrative regions and how regional identities at different layers are connected. I exploit the historical quasi-experiment of re-establishing the five states of Brandenburg, Mecklenburg-Western Pomerania, Saxony, Saxony-Anhalt, and Thuringia in the course of German reunification. Under the governance of the GDR (German Democratic Republic) states were dissolved and replaced by districts in 1952. The borders of the districts did not coincide with the borders of the old states. During reunification in 1990 states had to be re-established under enormous time pressure. In areas where borders of districts deviated from old state borders, it was not clear to which new state the counties should belong. Driven by economic, compatriot and historical desires, some citizens in the counties in question favored the old state borders while others favored the district borders. To democratically legitimize the construction of the states, referendums among the citizens were held in the 15 counties where the future state affiliation was not clear. This setting allows me to examine how regional identities are affected after borders are drawn and whether regional identity with the new (administrative) area develops over time.

[2] Other regions with a strong identity and tensions with the nation state include Corse, Scotland or the Basque region.

[3] For example the PIS in Poland, the AfD in Germany, the Front National in France or the Five Star Movement in Italy.

Regional identity is a subjective feeling and therefore not straightforward to measure. I investigate an revealed measure of national and sub-national (state) identity, namely voter turnout in federal and state elections.[4] Using data from the European Value Study, I show with an ordered probit model[5] that participation in local and federal elections in Germany is positively correlated with the regional identification at the respective layer (figure 6.1). Also Dehdari and Gehring (2019) show that in a real world setting outside the laboratory perceived identity measured within surveys is associated with revealed identity, measured for example with voting behavior. Citizens individual decision to vote is driven by several factors, such as cost-benefit considerations, norms, socialisations, or the institutional environment (Smets and Van Ham, 2013).[6] Cost-benefit considerations do not seem to suffciently explain voter turnout, giving the low probability to change the outcome (Owen and Grofman, 1984). The institutional environment is similar across German regions. After 40 years of the GDR regime also norms and socialization are not likely to differ within East German counties. However, citizens are also driven to the polls by a sense of civic duty (Riker and Ordeshook, 1968; Blais, 2000; Milbrath and Goel, 1977; Bonoldi *et al.*, 2020), to express an "attachment to the community" (Blais, 2000, p. 111; Blais and Galais, 2016, p. 61).[7] While civic duty is found to be an important driver of voter turnout (Blais, 2000; Huddy and Khatib, 2007; Verba *et al.*, 1995; Clarke *et al.*, 2004; François and Gergaud, 2019), an essential part of the civic duty to vote is the attachment to the community, wider region or country (Orviska and Hudson, 2003; Blais and Galais, 2016). Hence, voter turnout also reflects the attachment and loyalty to ones community and can therefore be considered as a revealed measure of regional identity. Using an revealed measure rather than surveys might also circumvent reporting issues and better mirror true preferences (Milbrath and Goel, 1977, p.52; Luttmer and Singhal, 2011, p. 173). Voter turnout allows me to proxy regional identity with different layers at a local level, and to measure a causal impact.[8] Nevertheless, I show in section 6.5.2 that other measures of regional identity, the subscription to regional newspapers and survey data, support my results in a cross section.

[4] This chapter therefore also adds to the literature on change of administrative areas by mergers of local governments and voter turnout (Roesel, 2017; Koch and Rochat, 2017; Fritz and Feld, 2020).

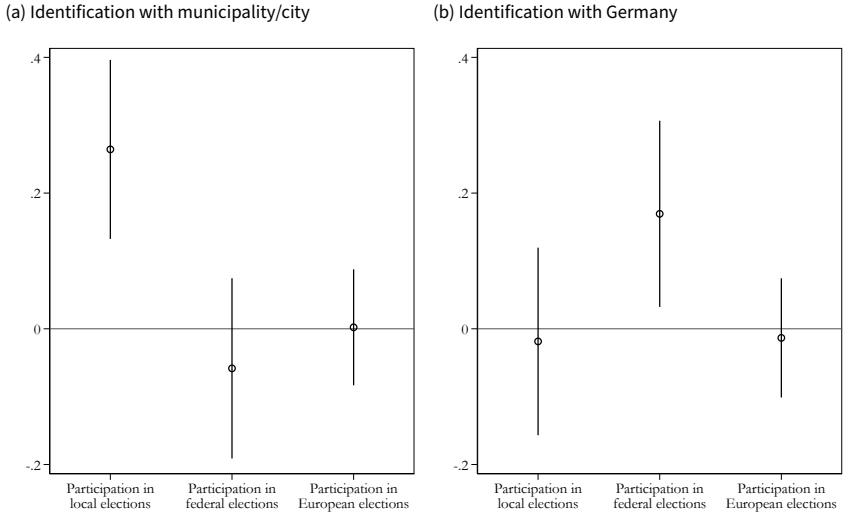
[5] I estimate an ordered probit model (answers in the European Value Study are not metric) that takes the following form: $Pr(I_j) = \gamma_l E_j^l + \gamma_f E_j^f + \gamma_e E_j^e + X_j'\lambda + \epsilon_j$. I_j is respondent j's regional identification (municipality/city, Germany) and E_j^l, E_j^f, E_j^e are respondent j's participation in elections (local (l), federal (f), European (e)). $X_j'\lambda$ is a vector of control variables which includes sex, age, size of the town the respondent is living in, marital status, employment status and state fixed effects. Standard errors are robust to heteroskedasticity. The European Value Study does not ask about identification with the state nor participation in state elections.

[6] For meta-analysis explaining voter turnout at the aggregate and individual level, see Cancela and Geys (2016); Smets and Van Ham (2013).

[7] Blais (2000) distinguishes two different motivations behind the sense of civic duty for voting: (I) according to a *personal sense* of duty one should vote because of a believe in democracy, (II) according to a *social sense* of duty one should vote because of the connection to and a caring about ones community (p. 111).

[8] Hillman *et al.* (2015) show that group identity based on religion also influences voter turnout.

Figure 6.1: Perceived identity correlates with participation in elections



Notes: This figure shows regression results of two ordered probit regressions using German data from the European Value Study, wave 2017 (EVS, 2019). Dependent variables are (a) identification with ones municipality/city and (b) identification with Germany. Circles represent point estimates, vertical bars the 90% confidence intervals. Controls include age, sex, town size, employment status, marital status and state fixed effects. Standard errors are robust to heteroskedasticity. Table A6.1 in the appendix shows the regression output.

Using difference-in-differences estimates in a pooled OLS and a panel framework I show that counties which had a referendum (treatment) have a lower participation in elections for the state assembly, compared to counties without a referendum (control), while the participation in elections for the federal assembly is not altered. This implies that in counties, where it was not clear to which state they would belong, regional identity with the state is lower while national identity is similar to other counties. These findings are in line with the theoretical model of multiple identities by Dehdari and Gehring (2019). Estimating difference-in-difference per period shows that the effect for state elections becomes smaller over time but remains significant, even in the elections in 2019, nearly three decades after the states were defined. The pure fact that citizens live in a different state than they revealed their preferences for does on the aggregate not induce identity formation with the new state. The results are robust to many specifications. The findings suggest that changing or setting administrative borders might have long-lasting implications for citizens regional identity. A decline in the electorate in counties with a referendum compared to counties without a referendum, for example, indicate that these counties are less attractive for citizens.

The establishment of the states in East Germany in 1990 constitutes an ideal setting to examine regional identity for four reasons. First, the counties belonged under the regime of the GDR to the same historical region and shared the same institutions. Second, states were dissolved and counties were forced into artificial districts for 38 years. Third, treatment is exogenous to the citizens as it concerned only counties that are located in the intersection of old state and district borders drawn in 1952. Fourth, counties belong to the same country and share the same institutional environment today.

This chapter contributes to the emerging economic literature on the origins of group identity.[9] Outside labratory experiments, causal evidence on the aspects shaping group identity, especially regional identity, is scarce.[10] Dehdari and Gehring (2019) show that the shock of occupation and repression for many years strengthens regional identity persistently, while there is no clear impact on national identity. The change in regional identity also has important

[9] My study also adds to the literature on the persistence of identities and values (Bisin *et al.*, 2011; Bisin and Verdier, 2011; Giuliano and Nunn, 2017) and to the literature on policies affecting identities (Dell and Querubin, 2017; Carvalho and Koyama, 2016).

[10] Studies using correlations and surveys find that the degree of regional identity seems to vary with socioeconomic factors, property, parenthood, mobility, municipal size (Bühlmann, 2010), municipal structure (Anton and Lawrence, 2014), municipal amalgamations (Terlouw, 2016), economic development (Ahlerup *et al.*, 2017; Fitjar, 2010), and the presence of regional languages and regionalized party systems (Fitjar, 2010). Regional identity further seems to trigger involvement in the local community (Foertsch *et al.*, 2019), attitudes towards the Euro (Meier-Pesti and Kirchler, 2003), secession movements and sometimes even violence (Michalopoulos and Papaioannou, 2016). In a geographical context Paasi (1986) identifies in a theoretical model four phases of regional identity formation: (I) territorial awareness/constitution of the territory, (II) symbolic shape (region's name, regional symbols are accepted and adopted), (III) institutional shape (regional institutions, e.g. in education, politics, or more informal ones like sport clubs, are established), and (IV) establishment of the region in the spatial structure/socio-spatial consciousness (Raagmaa, 2002; Zimmerbauer, 2011).

policy implications: preferences for regional decision making increase. Fouka (2020) shows how repressing policies targeted towards immigrant groups can strengthen their group identity. Gehring (2020) documents that external threats strengthen regional identity, which goes along with increased trust in common institutions and an increased support of common policies. Besides repression and threats also shared experiences in form of national sport events can strengthen national identity and reduce the potential for violence and conflicts (Depetris-Chauvin *et al.*, 2020).

Even though the concept of identity is well established in other disciplines such as philosophy, psychology, sociology and geography and, what is more, is unarguably an important part of human behavior and individual interaction, it is relatively new to economics. In their seminal work Akerlof and Kranton (2000, 2005, 2010) and Kranton (2016) incorporate identity in a utility function and show in a theoretical framework that economic action depends not only on monetary values but also on identity.[11] Kranton (2016) discusses several important economical and political implications of identity. Laboratory experiments show that individuals take group identity into account. They favor ingroup members and show discriminating behavior towards outgroup members (Chen and Li, 2009; Ockenfels and Werner, 2014; Fershtman and Gneezy, 2001). Group identity also fosters contributions to public goods (Charness *et al.*, 2014) and decreases free-riding (Chowdhury *et al.*, 2016).

Regional identity is based on a "imaginary" community. Figuratively speaking, members feel to be in the same boat and are emotionally connected, even though they know only a fraction of the other group members (Anderson, 2006; Foertsch *et al.*, 2019). This social cohesion is mainly based on a shared language, values, culture or mentality. But also national flags, national colors, national anthems, national sports teams, and, at a more local level, traditional garbs, dialects, festivities or a special cuisine strengthen the feelings of solidarity and trust. Regional identity is also formed through socialization based on shared history and territory (Raagmaa, 2002). In the European Value Study 2017 41% of German citizens stated to feel very close to their town or city, 45% feel very close to their country Germany and 26% feel very close to Europe.[12] Regional identity is an important factor for social and political actions and for the growth of social capital. Only if citizens see themselves as part of the community they will take interest and participate in politics and society (Lipset *et al.*, 1954; Bolan, 1997; Raagmaa, 2002; Zimmerbauer, 2011). Regional identity is even described as a basic requirement for a functioning democracy (Bühlmann, 2010). Especially patriotism or nationalism, as a form of regional identity, has shown in history to be a "powerful collective mobilization factor" (Raagmaa, 2002, p. 60).

[11] Other theoretical work incorporating identity, self-image and social norms in the field of economics are for example Bénabou and Tirole (2006, 2011); Köszegi (2006); Bisin *et al.* (2011).
[12] Additional 46% feel close to their town or city, 47% feel close to Germany and 51% feel close to Europe.

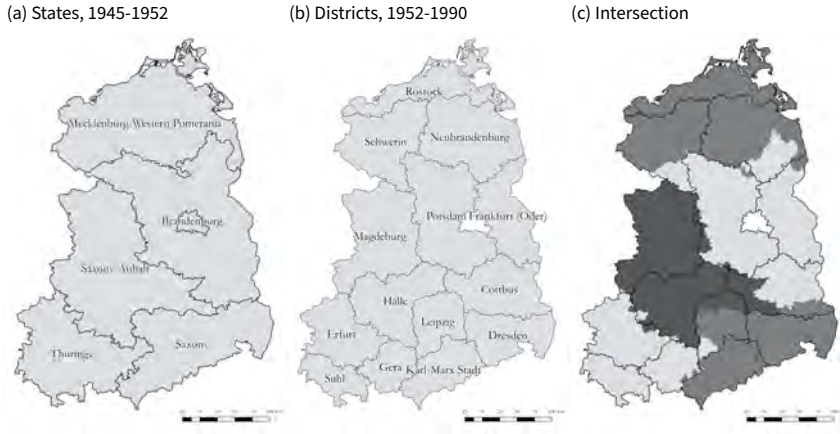
6.2 Historical background and timing of events

After the Second World War the Soviet Military Administration in Germany established in July 1945 in the Soviet Occupation Zone the three states Mecklenburg-Western Pomerania, Saxony and Thuringia, and the two Prussian Provinces Brandenburg and Saxony-Anhalt by merging historical grown areas (figure 6.2a; for a short historical overview of the East German states see the appendix and figure A6.1; for an overview of the timing of events between 1945 and 1990 see figure 6.3). In February 1947 the Prussian Provinces also became states. Between the end of 1946 and the beginning of 1947 the fives sates got their own constitutions. In October 1949 the GDR, a communist and socialist dictatorship with a centrally planed economy and governed by the SED (Socialist Unity Party of Germany)[13] in a one-party system, was officially established. Even though the constitution of the GDR was referring to the states, the existence of state authorities interfered with the intention of the SED to gain influence and control in every administrative unit (Bundesrat, 2010). In July 1952 the GDR restructured their regional layers by law.[14] The number of counties was increased from 132 to 217 (Wollmann, 1999) and the counties were grouped into 14 districts (see figure 6.2b). The borders of the new districts did not follow the borders of the states and divided historical grown areas (see figure 6.2c). Districts were instead designed to meet economic and security policy aspects, sociocultural factors were not considered (Bundesrat, 2010). As state assemblies were dissolved and replaced by district assemblies, this law de facto dissolved the five states. The historical irrelevant districts should help to enforce political conformity and centralism within the SED regime. Expressions of local identity, for example local dialects or traditions, were not well-received within the ideology of the SED. In 1989 citizens started to express their dissatisfaction with the government in demonstrations and protests. In the course of the peaceful revolution citizens showed flags of their states at demonstrations, even though they had not existed for almost 40 years. Identity with the states had endured the dictatorship and repression by the SED. Not only old-age citizens who had known the states in their youth, but also young citizens of the GDR started to express their regional affiliations and "roots" (Bundesrat, 2010; Blaschke, 1992). In November 1989 the wall, separating East and West Germany, finally came down, marking the near end of the GDR.

During the process of the German reunification in 1990 states had to be reestablished in East Germany for mainly two reasons. First, it was the will of the East German citizens expressed at the demonstrations. Citizens identified themselves more with the old states than with the GDR districts. Second, the constitution of the Federal Republic of Germany stated a federalist structure. Hence, for reunification, East Germany needed a federalist structure including states (Buchhofer, 1992; Blaschke, 1992). In March 1990 citizens of the GDR had for the first time the possibility to elect the *Volkskammer*, the parliament of the GDR, in a free and competitive

[13] The SED (*Sozialistische Einheitspartei Deutschlands*), was the East German communist and ruling party in the one-party-state GDR.
[14] *Gesetz über die weitere Demokratisierung des Aufbaus und der Arbeitsweise der staatlichen Organe in den Ländern der Deutschen Demokratischen Republik*.

Figure 6.2: Regional layers in the GDR, 1945-1990



Notes: This figure shows the regional layers of the GDR between 1945 and 1990. Figure 6.2a shows the borders of the five states Brandenburg, Mecklenburg-Western Pomarania, Saxony, Saxony-Anhalt and Thuringia between 1945 and 1952. The states were dissolved in 1952 and replaced by 14 districts (*Bezirke*) shown in figure 6.2b. Figure 6.2c shows the intersection of the states and districts. All figures exclude Berlin.

Figure 6.3: Timing of events (I), 1945-1990

Notes: This figure shows the development of the regional structure in East Germany, free elections, and major historical events between 1945 and 1990. For a timing of the referendums and decisions of the county assemblies see table 6.1.

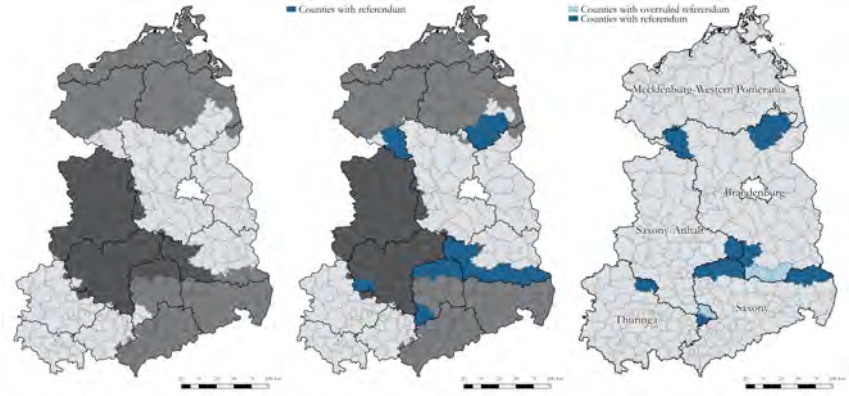
election. The new government discussed several possibilities of a new federal structure in East Germany (Bundesrat, 2010). But a profound restructuring, which normally takes years, was out of scope. The whole process of reunification was executed under enormous time pressure (Buchhofer, 1992; Blaschke, 1992). In the end, it was an obvious solution to either reconstruct the states of 1952 or to merge the GDR districts in pairs of two or three. The first solution, reconstructing the states of 1952, would have required to reform the county structure (Richter, 2002; Buchhofer, 1992; Blaschke, 1992). Because of the comprehensive county restructuring in 1952, along with the establishment of the districts, the old states could not be reconstructed exactly as their borders would have divided existing counties (see figure 6.4a). Reintroducing the county borders of 1952 would have led to a reduction in the number of counties, and therefore a dissolution of county administrations and unemployment among thousands of administrative employees, which might have led to unrest (Blaschke, 1992). For such a broad administrative reform was not enough time, nor enough political power. In the overwhelming process of reunification, a reform of county borders was also not the most important task (Buchhofer, 1992). The second solution, merging the GDR districts to roughly form the old states, was not popular among the citizens who perceived the districts as part of the SED regime. Merging the districts also involved another major problem: Some counties would become part of another state than they had belonged to until 1952 and feel historical and cultural related with (Richter, 2002; Buchhofer, 1992; Blaschke, 1992). Following this suggestion, the state of Mecklenburg-Western Pomerania, for example, would have been formed by merging the districts of Rostock, Schwerin and Neubrandenburg (see figure 6.2b). But areas in the south, who belonged historically to Brandenburg until 1952, would now be part of Mecklenburg-Western Pomerania, because they were part of the districts Schwerin and Neubrandenburg between 1952 and 1990. These problems arose at any point were the borders of districts did not follow the borders of the old states (see figure 6.2c and figure 6.4a).

On the second of May 1990, the government decided to reestablish the five states of Brandenburg, Mecklenburg-Western Pomerania, Saxony, Saxony-Anhalt, and Thuringia along existing county borders. In 15 counties future state affiliation was not clear, because old state and district borders diverged. Binding plebiscites among the citizens should be held in these counties to ensure acceptance of the new state borders and democratically legitimize the decisions (Richter, 2002; Buchhofer, 1992; Blaschke, 1992). Figure 6.4b highlights the 15 counties were the citizens should be asked about their preferred state affiliation. On the sixth of June these binding plebiscites were replaced by non-binding referendums, which had to be approved by the county assemblies. The power of decision about future state affiliation therefore would not lie by the citizens, as primary intended, but by the county assemblies. The government did not want to deprive the right of decision about the county's state affiliation from the respective county assemblies, who were elected for the first time in free elections on the sixth of May (Buchhofer, 1992; Richter, 2002).[15] Table 6.1 shows the counties, time of the referendums, voter turnout, results of the referendums and decisions by the county assemblies. The 15 counties were referendums were held accounted for

[15] On May 31, 1990 the district assemblies were dissolved.

Figure 6.4: The path to new state borders, 1990

(a) County borders (b) Counties with referendum (c) New state borders



Notes: Figure 6.4a shows the counties in the GDR (thin grey boundaries) in 1990 within the districts (thick black boundaries). Figure 6.4b shows the 15 counties which had referendums (dark blue) because they are located in the intersection of districts (thick black boundaries) and former states. Figure 6.4c shows the resulting new German states (thick black boundaries), the counties with referendum (dark blue) and the counties with overruled referendum (ligth blue). All figures exclude Berlin.

approximately one million citizen and an area of 10.000 square kilometers. The referendums were, even though it was somehow regulated by the resolution from June 6, not conducted in a standardized manner. In Prenzlau and Artern it was linked to the elections of the county assembly, in Hoyerswerda, Bad Liebenwerda, Perleberg, Delitzsch, Altenburg and Schmölln it was conducted as a postal vote, which might explain lower voter turnout. Also the timing of the referendums was diverse and ranged between February and July 1990. In most of the counties they were held after the government decided over the non-binding referendums on the sixth of June, but in some counties they were held under no legal basis at all or under the legal basis of binding plebiscites. In the respective counties a wide public debate about future state affiliation was held prior to the referendums. Homeland initiatives[16], public opinions of local politicians[17] as well as regional and local newspapers printing articles dealing with historic roots, opinions and debates of reader's letters played an important role during the process of public opinion forming in the times of the referendums (Buchhofer, 1992).

In the referendums citizens could decide between two states, an exception is Bad Liebenwerda were citizens could choose between Brandenburg, Saxony and Saxony-Anhalt. Voter participation ranged between 55.3% in Altenburg and 78.3% in Artern. Citizens decided in three cases against Mecklenburg-Western Pomerania, Saxony-Anhalt lost in six cases, only the citizens of Jessen decided in favor of this state. Thuringia lost in Altenburg but won in Schmölln and Artern. Brandenburg lost in five and won in four counties. In nine counties citizens could choose between Saxony and another state, and voted in eight cases in favor of Saxony (see table 6.1). After these non-binding referendums were held and the majority will of the citizens became public, county assemblies decided over the future state affiliation. In twelve counties the county assembly followed the citizen's majority will. But in three counties, Altenburg, Schmölln and Bad Liebenwerda, county assemblies decided against a state affiliation with Saxony, even though it was the declared will of the majority (see figure 6.4c). In Bad Liebenwerda, were 53.1 % of the citizens voted in favor of Saxony and only 25.5% in favor of Brandenburg, the county assembly decided the county would become part of Brandenburg, even though it never belonged to Brandenburg before (Richter, 2002). But otherwise, Saxony, which already dominated in terms of population and economy, would have become even more powerful compared to the other East German states. The decisions of the citizens and politicians were not only driven by historical, but also economic and political motives (Buchhofer, 1992). Some members of the county assemblies justified the decision with low voter turnout. They claimed that the (potential) will of non-voters has to be respected (Richter, 2002).

[16] For example *Heimatinitiative Westprignitz* in Perleberg or *Bürgerinitiative Pro Brandenburg* in Lauchhammer (Buchhofer, 1992).

[17] The heads of the county administration in Weißwasser and Bad Liebenwerder, for example, asked their citizens to vote in the referendum in favor or against Saxony (Buchhofer, 1992).

Table 6.1: Results of referendums and county assembly decisions

County	Time of referendum (1990)	Turnout (%)	Referendum (%)			Decision of county assembly (1990)	
			Brandenburg	Mecklenburg-W. Pomerania			
Prenzlau	February 2 - May 6	77.2	93.60	5.10		June 27	Brandenburg
Templin	March 18	62.3	96.09	3.90		June 13	Brandenburg
Perleberg	July	62.5	78.48	21.51		July 21	Brandenburg
			Saxony	Thuringia			
Altenburg	July 13	55.3	53.81	46.19		July 18	Thuringia
Schmölln	June 20 - July 13	60.7	18.08	81.92		July 16	Thuringia
			Saxony	Brandenburg			
Hoyerswerda	April	57.1	87.80	12.20		June 18	Saxony
Senftenberg	May, June	61.7	54.10	45.90		July 19	Brandenburg
Weißwasser	July	69.4	82.20	17.80		July 21	Saxony
			Saxony-Anhalt	Thuringia			
Artern	May 6	78.3	11.35	64.03			Thuringia
			Saxony-Anhalt	Saxony			
Delitzsch	July 19	78.3	10.74	89.26		July 20	Saxony
Eilenburg	June 20 - July 20	74.8	10.35	89.64		July 21	Saxony
Torgau	July 20 - July 21	56.5	6.26	93.74		July 21	Saxony
			Saxony-Anhalt	Brandenburg			
Herzberg	June 20 - July 13	71.5	38.60	61.40		July 19	Brandenburg
Jessen	July 2 - July 13	67.9	64.92	35.00		July 19	Saxony-Anhalt
			Saxony	Brandenburg	Saxony-Anhalt		
Bad Liebenwerda	June 11 - June 20	58.5	53.10	25.50	21.40	July 21	Brandenburg

Source: Bundesminister für Innerdeutsche Beziehungen (1990); Buchhofer (1992).
Notes: This tables shows the 15 counties which had a referendum in 1990 with their alternatives to choose from, the results of the (non-binding) referendum and the (binding) decision of the county assembly. In three counties (in italic) the county assembly did not follow the results of the referendum (see figure 6.4c). In Artern vote shares of the referendum do not sum to 100%, because 25% voted for a dissolution and division of the county into Saxony-Anhalt and Thuringia following the borders from 1952.

On the 22th of July, the *Volkskammer* passed the law[18] to determine the new state's borders, accepting the votes of the county assemblies. The law, and therefore the new borders, became effective in October 1990.[19] That county assemblies and government did not follow the will of the majority led to dissatisfaction and unrest among the voters.[20] But also local politicians were irritated by the procedure to ask citizens first, and then ignore their decisions.[21] Apparently, during the referendum it was not clear for citizens whether it was binding or not. Preiß, minister for Regional and Municipal Affairs in the GDR government, said that the new state borders were a compromise between consideration of the will of citizens and a quick procedure to establish the new states as a requirement of reunification. Roland Becker, chairman of the Committee on the Constitution and Administration, said he well understands the confusion, but the decisions of the first time elected county assemblies should be regarded es legitimate and legal. In any event, having a different decissions in direct and representative democracy and ignoring the will of citizens in the process of transition from a dictatorship to a

[18] *Ländereinführungsgesetz*.

[19] Originally the states should have been established legally on October 14, 1990, but this was changed to October 3, the day of German reunification.

[20] In the respective counties politicians got threatened with death, citizens organized demonstrations and blocked the highway between Dresden and Berlin on July 22, 1990 (Buchhofer, 1992).

[21] Dieter Gleisberg (Bund freier Demokraten): "Do you think it is democratic when referendums are held and then ignored by the county assembly?" (*"Halten Sie es für demokratisch, wenn Volksbefragungen durchgeführt werden und dann von den Kreistagen ignoriert werden?"*); Sabine Fache (PDS): "Many citizens asked me the question: why were referendums carried out first, when citizens were unaware whether they were binding or not, and why did they end up ignoring the outcome of the referendums?" (*"Viele Bürger stellten mir dazu die Frage, wieso man zunächst Befragungen durchführte, bei denen es den Bürgern nicht bewusst war, ob es sich nur um eine Befragung oder um eine Entscheidungsfindung handelt, um dann am Ende das Ergebnis dieser Befragung nicht zu beachten?"*); Volker Schemmel (SPD): "Of course, we were not happy with these results. [...] The Committee has determined that the procedure is legal. And what's legal should be democratic as well." (*"Wir waren selbstverständlich über diese Ergebnisse nicht glücklich. [...] Der Ausschuss hat festgelegt, dass das Verfahren rechtens ist. Und was rechtens ist, dürfte wohl auch demokratisch sein."*); cited from Mitteldeutscher Rundfunk, June 17, 2011, "Mein Land – Dein Land: Streifälle bei Ländergründung, `https://www.mdr.de/zeitreise/stoebern/damals/artikel98802.html`; Rainer Pietsch (Bündnis 90/Grüne): "[It is] very critical that the decisions of the county assemblies are opposing the vote of citizens in such a way" (*"[Es sei] sehr bedenklich, dass die Entscheidung des Kreistages dergestalt gegen das Votum der Bürger [gehe]"*); Lothar Bisky (PDS): "dilettantish way of surveying" (*"dilettantische Art und Weise der Befragung"*); cited from Richter (2002), p 57.

democracy may give rise to deteriorating trust in the system of a representative democracy.[22] It was not clear why there was such an effort to ask all citizens and ignore the results afterwards (Richter, 2002).

With German reunification on the third of October, 1990, the states of Brandenburg, Mecklenburg-Western Pomerania, Saxony, Saxony-Anhalt, and Thuringia were legally established. Two weeks later, on October 14, state assembly elections were held for the first time in East Germany. On December 12, 1990 the twelfth German Bundestag (federal assembly) was elected, for the first time in East and West Germany together. State and federal assemblies were then elected periodically by the citizens in the five new German states (see figure A6.2 in the appendix for the exact dates of the elections).

6.3 Empirical Strategy

6.3.1 Hypotheses

I examine regional identity in counties with referendum (treatment) compared with counties without referendum (control). In the counties with referendum there was a wide public debate about future state affiliation. Citizens got informed about their local history by homeland initiatives and local media. Even if the county assembly followed the majority will, part of the citizens revealed their preferences for another state, the referendum created losers in that matter. If these citizens do not identify themselves with the new state, aggregate identification with the state is expected to be lower, compared to states without referendum. In the long-run, these differences could vanish, as citizens adjust and develop an regional identity with the new state. Identitification with Germany, however, should not be altered. The hypotheses are in line with Dehdari and Gehring (2019). They develop a theoretical model

[22] This became obvious in readers letters to newspapers and local politicians: "back to old times, when the appearance was spread to the outside world that the interests of the people were represented, but these were trampled underfoot" (*"zurückversetzt in alte Zeiten, wo nach außen hin der Anschein verbreitet wurde, die Interessen des Volkes zu vertreten, diese jedoch mit Füßen getreten wurden"*), "Dulling the electorate and obtaining posts by fraud" (*"Verdummung der Wähler und Erschleichen von Posten"*),"in the old manner and with SED strategy" (*"in alter Manier und mit SED-Strategie"*), "With your decision, you have fully placed yourself at the same level as the 40-year-old SED regime, which was believed to be past. You are God knows no better than Honecker and Co" (*"Mit Ihrer Entscheidung haben Sie sich voll auf die gleiche Höhe placiert, wie das vergangen geglaubte 40-jährige SED-Regime. Sie sind weiß Gott keinen Deut besser als Honecker und Co."*), and with statements from local politicians: H. Uhlemann, member of the county assembly in Altenburg: "Working style of a central committee" (*"Arbeitsstil eines Zentralkomitees"*); Johannes Ungvári, major in Altenburg said citizens need an explanation for the diverging decision of the county assembly and "if these enlightening words are not forthcoming, they have done great damage to our newly awakened and not yet consolidated democratic consciousness, which will continue to have an effect for a long time to come" (*"Bleiben diese aufklärenden Worte aus, haben Sie unserem erst neu erwachten und noch nicht gefestigten Demokratiebewusstsein großen Schaden zugefügt, der noch lange nachwirken wird"*); all cited from Richter (2002), pp. 65f.

where individuals posses multiple identities which are not necessarily substitutes. Using this model they illustrate how a temporary shock can influence sub-national identity persistently, while national identity is unaffected.

The aggregate effect on regional identity might differ, depending on whether the county assembly followed the will of the citizens or overruled the referendum. I therefore conduct a heterogeneity analysis and further divide the treatment group into counties where the county assembly followed the referendum (successful referendum) and counties where the county assembly overruled the referendum. These counties only differ in the county assembly decision. All are located in the intersection of old state and county borders, had a public debate about state affiliation and had a referendum. It is expected that the aggregate impact on state identification is even larger in counties were the county assembly did not follow the majority will of the citizens.

6.3.2 Data and sample

My dataset covers 215 counties in the East German states of Brandenburg, Mecklenburg-Western Pomerania, Saxony, Saxony-Anhalt, and Thuringia. 15 counties had a referendum. In three counties the result of the referendum was overruled by the county assembly. As dependent variable I use voter turnout in state and federal elections, a revealed measure of regional identity. Regional identity is described as an essential factor for social and political participation. If one feels to be part of a community and identifies with the region, one may well have more interest in influencing regional affairs through electoral support.

My dataset includes 17 elections. These are the election for the *Volkskammer* and county assembly in March and May 1990 before establishment of the new states, and eight elections for the federal assembly (*Bundestagswahl*, BW) as well as seven elections for the state assembly (*Landtagswahl*, LW) after establishment of the new states between 1990 and 2019 (for the exact dates of the elections see figure 6.3 for elections in 1990 and figure A6.2 in the appendix for elections after 1990). Data for the elections in 1990 is obtained at the county level. Due to large municipal and county restructuring in the years after reunification, the counties from 1990 did not exist any more when the next federal and state elections were held in 1994. Therefore, for all elections after 1990, I collected data at the municipal level and reconstruct the counties from 1990 by adding electorate and passed votes in the respective municipalities.[23] Table 6.2 shows descriptive statistics. Voter turnout ranges between 33% and 97% with a mean of

[23] Municipalities were enabled to become part of the state they belonged to in 1952 by a treaty between the respective states if it is the majority will of the citizens and approved by the local representatives as long as there are no ex- or enclaves (§2 Ländereinführungsgesetz). Some municipalities changed from the state they became part of in 1990 to the state they belonged to until 1952. For example 1992 nine municipalities changed from Thuringia to Saxony, 1992 16 municipalities changed from Mecklenburg-Western Pomerania to Brandenburg, 1993 eight municipalities changed from Mecklenburg-Western Pomerania to Lower Saxony (Amt Neuhaus belonged to Hannover (Lower Saxony) and became part of Mecklenburg-Western Pomerania not until 1945). I exclude these municipalities from my sample.

63%. But this is quite diverse for the different elections. In the election for the *Volkskammer* in March 1990, which was the first free election for citizens in East Germany for some decades, turnout was on average 94% with a minimum of 90%. In May 1990, voter participation for the county assemblies was on average 77%, ranging between 61% and 87%. Voter turnout in federal and state elections is slightly lower with an average of 64% and 56%. Figure A6.3 in the appendix shows a raw data plot on voter turnout. The data does not include postal votes. Information on postal votes is only available at electoral districts and cannot be assigned to individual municipalities.[24]

Table 6.2: Descriptive statistics

	Obs.	Mean	SD	Min	Max
Dependent variable					
Voter turnout (%)	3,655	63.20	12.48	32.60	97.31
Referendum					
Referendum (yes = 1)	3,655	0.07	0.25	0.00	1.00
Successful referendum (yes = 1)	3,655	0.06	0.23	0.00	1.00
Overruled referendum (yes = 1)	3,655	0.01	0.12	0.00	1.00
Elections					
State assembly (LW) (yes = 1)	3,655	0.41	0.49	0.00	1.00
Federal assembly (BW) (yes = 1)	3,655	0.47	0.50	0.00	1.00
Control variables					
Electorate (log)	3,655	10.65	0.57	9.24	13.58
Urban counties	3,655	0.12	0.33	0.00	1.00
Longitude	3,655	51.90	1.11	50.33	54.47
Latitude	3,655	12.52	1.14	10.14	14.96
Altitude (SD)	3,655	41.23	35.46	5.01	180.63
Distance to inner German border (km)	3,655	92.35	62.85	2.15	234.35
Distance to nearest transmitter mast (km)	3,655	95.23	39.93	15.47	203.83

Notes: 215 counties in East Germany are the units of observation.

6.3.3 Identification and regression specifications

Counties with referendum constitute my treatment group, counties without referendum are the control group. A causal interpretation relies on three main assumptions. First, *sorting into treatment* was exogenous. This assumption should be fulfilled comparing counties with referendum to counties without referendum. Only in counties where future state affiliation was not clear, because they were located between diverging old state and district borders, referendums were conducted. Table 6.3 also shows t-test results for pre-treatment characteristics at the county level. Counties with and without referendum are not statistically different in electoral and demographic outcomes (panel A). Panel A of table A6.2 in the appendix also shows that

[24] Differences in the electoral system in elections for the state assemblies, for instance different age restrictions, are covered with fixed effects. Urban districts often form an electoral district, therefore votes passed with a ballot paper, like postal votes, are included in the data in urban district. In any event, inferences do not change when including a dummy for urban districts or excluding urban districts altogether (see section 6.4 and table A6.6, panel A in the appendix).

party vote shares in counties with and without referendum did not differ in the election for the *Volkskammer* and county assembly conducted in 1990. Moreover, socio-demographic regional differences in the GDR should be small because of harmonizing policies of the government. The GDR was centrally organized with a centralized education system and a centrally planed economy. School was compulsory, while access to tertiary education was restricted. Also occupational choices were constrained and vacant positions were assigned by the central planing committee. Following the socialist ideals of equality, payment should be rather equal across occupations and everyone was employed, i.e. officially, there was no unemployment (Friehe *et al.*, 2019). Table A6.3 in the appendix shows further evidence along this line on the district level. The survey *Welfare East* conducted by the *Zentralinstitut für Jugendforschung* asked adults in East Germany between October and December 1990 about their individual impression of conflicts between politicians and citizens, their membership in unions, parties or citizens' initiatives, their current sense of live, and satisfaction with ones neighborhood. Answers in districts including counties with referendums[25] are not statistically different from answers in districts were there was no referendum (panel A of table A6.3). Also responses regarding political interest, voting behavior, attitudes and expectations towards reunification, identification with the GDR or FRG (Federal Republic of Germany, West Germany), and the household income in the survey *Politbarometer Ost*, conducted by the *Forschungsgruppe Wahlen* between March and December 1990 among citizens in East Germany are not statistical different in districts with counties having a referendum and districts without counties having referendums (panel B of table A6.3).[26]

When I consider counties with overruled referendum and counties with successful referendum separately, the exogeneity assumption should also be fulfilled. The decisions of the county assemblies overruling the referendum were mainly driven by economic and political reasons and not by the results of the referendum per se. Politicians in Senftenberg expected a huge financial support from West Germany for their brown coal field, which Senftenberg would only receive if the county belongs to Brandenburg (Richter, 2002; Buchhofer, 1992). In Bad Liebenwerda the county assembly decided for Brandenburg because it had strong economic relationships within the old district of Cottbus, which would become part of Brandenburg. Additionally, it was feared that the county would merge with the county of Risa if it would become part of Saxony, resulting in a closure of the county assembly in Bad Liebenwerda. Members of the county assembly in Altenburg voted in favour of Thuringia because they did not want to separate Altenburg and its neighbouring county Schmölln in different states, as the counties belonged historically together[27] and are economically strongly connected. Additionally, it was argued that Altenburg fits better into the middle-class-oriented state of Thuringia (Richter, 2002). After the deviating decisions of the county assemblies in Senftenberg, Bad Liebenwerda, and Altenburg there were rumors circulating that the decisions against Saxony were "dictated

[25] These are Neubrandenburg, Schwerin, Cottbus, Halle, and Leipzig.

[26] Data from the surveys *Welfare East* and *Politbarometer Ost* are not available at the county level, only at the district level.

[27] Since 1554 in the Herzogtum Sachsen-Altenburg. Citizens and county assembly in Schmölln decided for Thuringia, before the county assembly in Altenburg decided about the future state affiliation.

by Berlin". Otherwise, Saxony would have dominated the other East German states in terms of population and economic power even more (Richter, 2002). Comparing pre-treatment characteristics in counties with overruled and successful referendum shows that counties were the referendum was overruled are more populated (table 6.3, panel B). The differences in electorate, population, female population and population density are significant. Among the counties with referendum, three counties with a population over 100,000 citizens in 1989 are by far the largest counties.[28] These are Hoyerswerda and Senftenberg, with their brown coal fields and coal-fired power station[29], one of the most important energy producers in the GDR, and Altenburg. As in two out of three of these populous counties the county assembly overruled the referendum, the significant results of the t-test are not surprising. Nevertheless, there is no evidence that county assembly decisions were influenced by population numbers, but rather by economic and political prospects the new states had to offer.[30] Regarding voter turnout and party vote shares there are no significant differences between the two groups of counties (panel B of table 6.3 and table A6.2 in the appendix).[31]

Second, the the *common trend assumption* has to be fulfilled. In absence of treatment, counties with referendum should have evolved similar to counties without referendum. Because there were no free federal or state elections before treatment (and no states to elect a state assembly for), I take the two free elections in the GDR before the new state borders were set to evaluate the common trend assumption. These are the election for the *Volkskammer* in March 1990, and the election for the county assemblies in May 1990 (see figure 6.3). Figure 6.5 shows the development of voter turnout in the different elections. Figure 6.5a divides the sample into counties with and without referendum. The first part (I) shows voter turnout in elections for the *Volkskammer* (VK) and county assemblies (*Kreistagswahl*, KW). Average voter turnout was very high in the election for the *Volkskammer* with 94% and decreased to 77% in elections for the county assemblies. In both elections voter turnout was quite similar in counties with and without referendum, albeit slightly smaller in the election for the county assemblies in counties with referendum. Figure 6.5b further splits the sample into counties without referendum, counties with referendum, which was confirmed by the county assembly (successful referendum), and counties with referendum, which was overruled by the county assembly. Again, voter turnout in the two elections before the new states were established is quite similar across the three groups. In table 6.3, which presents t-test results of county characteristics in 1989 and 1990, I show that the small differences in voter turnout in county

[28] Senftenberg: 114,000; Hoyerswerda: 110,000; Altenburg: 102,000. The next largest county among the counties with a referendum was Perleberg with 73.000 citizens.

[29] The brown coal fields and power station in Hoyerswerda and Senftenberg formed in the GDR the *Gaskombinat "Schwarze Pumpe"*. Both counties thought they would receive the the *Gaskombinat "Schwarze Pumpe"* en bloc, but instead the area was split (Richter, 2002). The state border of Saxony and Brandenburg even runs through buildings of the power station.

[30] However, number of population and potential economic power might influence voter turnout (Cancela and Geys, 2016). But it is shown in section 6.4 and figure 6.5b that counties with overruled and successful referendum have similar voter turnout in elections for the federal, state and county assembly and *Volkskammer*. Size of the population in counties with overruled referendum, therefore, does not seem to be a problem.

[31] With one exception, party vote shares for the CDU is significant different at the 10% level.

Table 6.3: Pre-treatment characteristics

A: Referendum	Mean referendum	Mean no referendum	Diff.	SD	Obs.
Turnout *Volkskammer*	94.00	94.18	0.18	0.38	215
Turnout county assembly	75.57	76.88	1.31	1.29	215
Electorate *Volkskammer*	45,106.33	53,849.81	8,743.48	12,456.33	215
Electorate county assembly	45,252.20	53,822.63	8,570.43	12,515.94	215
Population (1989)	60,538.73	71,232.51	10,693.78	16,368.88	215
Female population (1989)	31,083.73	37,119.83	6,036.10	8,684.42	215
Population density (1989)	114.27	321.20	206.94	148.64	215
B: Overruled vs. successful referendum	Mean overruled referendum	Mean successful referendum	Diff.	SD	Obs.
Turnout *Volkskammer*	93.49	94.13	0.64	0.85	15
Turnout county assembly	74.35	75.87	1.52	3.33	15
Electorate *Volkskammer*	67,579.67	39,488.00	-28091.67**	11,006.08	15
Electorate county assembly	67,997.33	39,565.92	-28431.42**	11,134.60	15
Population (1989)	89,590.33	53,275.83	-36314.50**	15,269.41	15
Female population (1989)	46,318.00	27,275.17	-19042.83**	7,689.57	15
Population density (1989)	191.33	95.00	-96.33**	35.96	15

Notes: This table shows a t-test for electoral and demographic variables in 1989 and 1990. Panel A compares counties which had a referendum to counties without an referendum. Panel B compares counties where the referendum was overruled by the county assembly to counties where the county assembly followed the referendum (successful referendum). Significance levels: *** 0.01, ** 0.05, * 0.10.

assembly elections does not turn out to be statistically significant. The same holds if I estimate difference-in-differences regressions with a dummy for each election period (figure 6.6 and A6.5 in the appendix). Therefore, the common trend assumption seems to be met.

Third, there was no *compound treatment* affecting the counties under consideration, i.e. despite the referendum no other treatment occurred. The year 1990 is an exceptional year with radical changes in the political and social system in East Germany. These changes, however, affected all counties in East Germany in a similar manner. The only difference between the treatment and control counties are the referendums. Therefore, compound treatment did not play a role and does not bias the results.

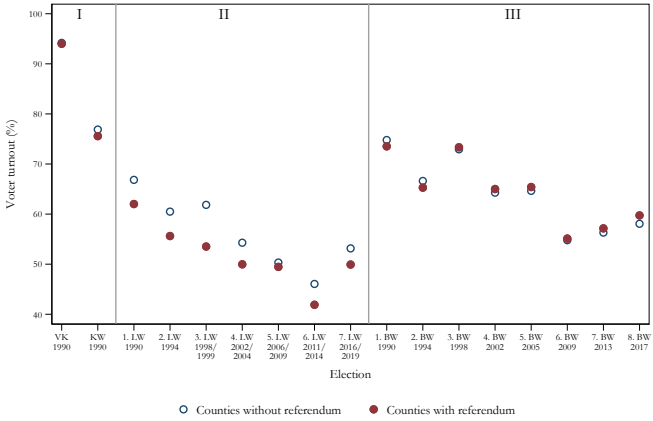
To estimate the effect on regional identity I use a difference-in-difference model in a pooled OLS and a panel framework, which I estimate separately for state and federal elections.[32] Standard errors are clustered at the county level.[33] The pooled OLS model takes the following form:

$$Y_{ct} = \alpha_s + \theta Treatment_c + \gamma Election_t + \beta(Treatment_c \cdot Election_t) + X'_{ct}\lambda + \epsilon_{ct} \quad (6.1)$$

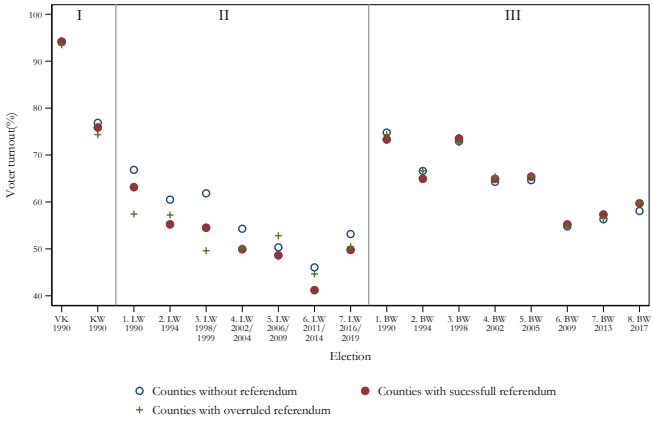
[32] Including both, federal and state elections together in the regression does not change the results (table A6.7 in the appendix).
[33] Using standard errors robust to heteroskedasticity or jackknife standard errors does not change the results (table A6.4 in the appendix).

Figure 6.5: Development of voter turnout, 1990-2019

(a) Referendum



(b) Successful vs. overruled referendum



Notes: This figure shows the development of voter turnout in different elections in East German counties. Grey vertical bars divide the figures in three parts. The first part (I) shows elections for the *Volkskammer* (VK) and county assemblies (*Kreistagswahl*, KW) in 1990 before the new states were established. The second and third part show elections between 1990 and 2019 after the new states were established. The second part (II) shows elections for the state assemblies (*Landtagswahl*, LW) and the third part (III) shows elections for the federal assembly (*Bundestagswahl*, BW). Figure 6.5a shows outcomes for counties with and without referendum while figure 6.5b further differentiates the sample into counties with an successful and an overruled referendum. For the exact dates of the elections see figures 6.3 and A6.2 in the appendix.

where Y_{ct} describes voter turnout in county c and election period t. $Treatment_c$ is a dummy variable indicating the treatment group. $Election_t$ is a dummy indicating federal or state elections after the new states were established. $Treatment_c \cdot Election_t$ is the interaction term and β describes the difference-in-differences estimate measuring the direct effect of modified administrative areas on regional identity. As control variables, included in the vector X'_{ct}, I use the electorate in logs and geographical controls. It migth be that regional identity differs in the mountainous south from the rather flat north. Also the distance to West Germany might influence the outlast of identity with the dissolved states during the suppression of the GDR regime. Therefore, I control for longitude, latitude, altitude[34], and the distance to the inner German border. Additionally, I include the distance to the next transmitter mast in West Germany and West Berlin as control variable. Friehe *et al.* (2019) show that the exposure to West German TV during the GDR regime influences voting behavior. Moreover, I include a dummy for urban city counties. α_s are state fixed effects.

The fixed effects panel model takes the following form:

$$Y_{ct} = \alpha_c + \mu_t + \beta(Treatment_c \cdot Election_t) + X'_{ct}\lambda + \epsilon_{ct} \tag{6.2}$$

Similar to equation (6.1), Y_{ct} describes voter turnout, X'_{ct} includes the electorate as a control variable, $Treatment_c$ and $Election_t$ are dummy variables denoting treated counties and elections for state or federal assemblies, and β estimates the difference-in-differences effect. State fixed effects are replaced by county fixed effects α_c and election period fixed effects μ_t.[35]

In a first step, I use equations (6.1) and (6.2) to estimate the short-term impact of changing administrative areas. Therefore, I examine the four elections taking place in 1990. In March and May, living in the GDR districts, citizens elected the *Volkskammer* and county assemblies, in October and December of the same year, living in the newly established states, citizens elected the state and federal assembly. In a second step, I use equations (6.1) and (6.2) to estimate possible persistent long-term impacts on regional identity and include federal and state elections until the year 2019.

Equations (6.1) and (6.2) estimate an average effect between 1990 and 2019. This might mask temporal development, if citizens start to identify with the newly established states. To examine possible identity formation with the new state in more detail, I also estimate a difference-in-difference model with dummies for each election period. Therefore, I replace the $Treatment_c \cdot Election_t$ interaction from equation (6.2) with a vector of dummies measuring the elections before and after the new state borders were set. The election for the *Volkskammer* serves as the base category. This allows me to estimate a treatment effect for each election period until 2019 separately. The results allow me to detect a possible fading out of the effect. Moreover, it also enables me to examine the validity of the common trend assumption. The

[34] Altitude is measured as the standard deviation of the elevation in each county.

[35] State elections take place at different election dates across the states. To control for different election periods I include election period fixed effects. For the timing of elections see figures 6.3 and A6.2 in the appendix. Baseline results do not change if estimated with election date fixed effects.

model takes the following form:

$$Y_{ct} = \alpha_c + \mu_t + \sum_{j=b}^{B} \beta_j (Treatment_c \cdot Election_t^j) + X'_{ct}\lambda + \epsilon_{ct} \tag{6.3}$$

where Y_{ct}, α_c, μ_t, $Treatment_c$, $Election_t$, X'_{ct} and ϵ_{ct} are similar to equation (6.2). The vector of coefficients of interest is described by $\sum_{j=b}^{B} \beta_j$. $Treatment_c \cdot Election_t$ takes on the value of one for treated counties during federal or state elections in county c in $(t + j)$ elections and zero otherwise. j ranges from $c = -1$ to $C = 7$ for state elections, and $C = 8$ for federal elections, excluding -2 as the base category.

6.4 Results

6.4.1 Baseline results

Table 6.4, panel A shows the results of the short-term impact, considering only the elections in 1990. Results move gradually from a pooled OLS model (POLS, equation (6.1)) to a fixed effects model (FE, equation (6.2)). In counties with referendum citizens participate less in state assembly elections, compared to counties without referendum: voter turnout in state assembly elections is about 4.0 percentage points lower (panel A, columns 1-3). Participation in federal elections, however, is similar in both groups of counties (panel A, columns 4-6). The regression results are supported by graphical evidence in figure 6.5a. Part II and III of figure 6.5 show voter turnout in state elections (*Landtagswahl*, LW) and federal elections (*Bundestagswahl*, BW). Voter turnout in counties with referendum is lower in state, but similar in federal elections in 1990, compared to counties without referendum. This result is remarkable. All four considered elections take place in 1990. Voter turnout in elections for the *Volkskammer* in March and for the county assemblies in May are similar in both groups, diverge for state elections in October and are again similar in federal elections in December.

Figure 6.5a shows that the difference in voter turnout in state elections is persistent until the elections for the seventh state assembly in 2016/2019, whereas voter turnout in federal elections is quite similar. This is confirmed by the regression results in table 6.4, panel B. Voter turnout in counties with referendum is, even in the long-run, lower than in counties without referendum (panel B, columns 1-3). The effect is around 3.6 percentage points. Including election period and county fixed effects within the panel framework does not change the results. Again, there is no significant difference in voter turnout in federal elections using pooled OLS. Including county and election period fixed effects the effect becomes significant at the 10% level with a point estimate of 1.1 percentage points, indicating that aggregate voter participation somewhat increased in federal elections in counties which had a referendum.

It may well be that the effect of lower voter turnout in counties with referendum during state elections fades out over time if citizens start to identify with the new state. I therefore estimate the difference-in-difference model with dummies for each election period. Figure 6.6 shows

Table 6.4: Baseline results

	State assembly (LW)			Federal assembly (BW)		
	(1) POLS	(2) POLS	(3) FE	(4) POLS	(5) POLS	(6) FE
A: Short-term effect (1990)						
Referendum · election	-4.08***	-4.07***	-3.97***	-0.52	-0.52	-0.41
	(0.74)	(0.75)	(0.74)	(0.48)	(0.49)	(0.51)
Within R^2	0.55	0.61	0.97	0.29	0.37	0.96
Number of counties	215	215	215	215	215	215
Observations	645	645	645	645	645	645
B: Long-term effect (1990-2019)						
Referendum · election	-3.63***	-3.62***	-3.56***	1.01	1.01	1.08*
	(0.68)	(0.68)	(0.69)	(0.62)	(0.62)	(0.63)
Within R^2	0.64	0.65	0.88	0.53	0.56	0.97
Number of counties	215	215	215	215	215	215
Observations	1,935	1,935	1,935	2,150	2,150	2,150
Electorate (log)	No	Yes	Yes	No	Yes	Yes
Geographical controls	No	Yes	No	No	Yes	No
State fixed effects	No	Yes	No	No	Yes	No
Election period fixed effects	No	No	Yes	No	No	Yes
County fixed effects	No	No	Yes	No	No	Yes

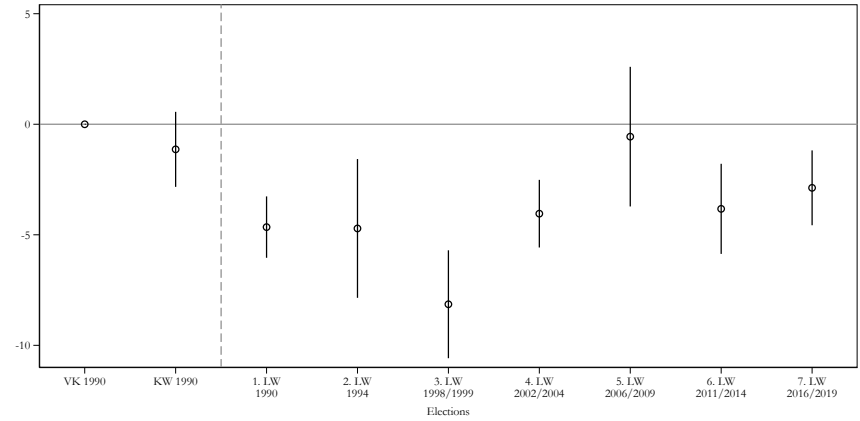
Notes: This tables shows results of difference-in-differences regressions using pooled OLS (POLS) and panel fixed effects (FE) models where voter turnout (%) is the dependent variable, comparing counties which had a referendum (treated) to counties without an referendum (control). Panel (A) shows short-term effects, data covers elections in the year 1990. Panel B shows long-term effects, data covers elections in the years between 1990 and 2019. The variable of interest *Referendum · election* takes on a value of one for federal/state elections in counties that had an referendum and zero otherwise. Control variables include the electorate in logs, a dummy denoting rural counties, longitude, latitude, altitude, the distance to the inner German border and closest West German transmitter mast, state fixed effects, election period fixed effects and county fixed effects. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10.

the results for counties with and without referendum. In state assembly elections, voter turnout is around 5 percentage points lower in 1990. The effect fluctuates but stays significant and negative in state elections between 1994 and 2019 (with one exception for the elections in 2006/2009, where the coefficient becomes insignificant). This confirms the results of table 6.4. Even three decades after the administrative areas were set, voter turnout remains lower in state elections. The effect in federal elections, on the other hand, tends to be positive.

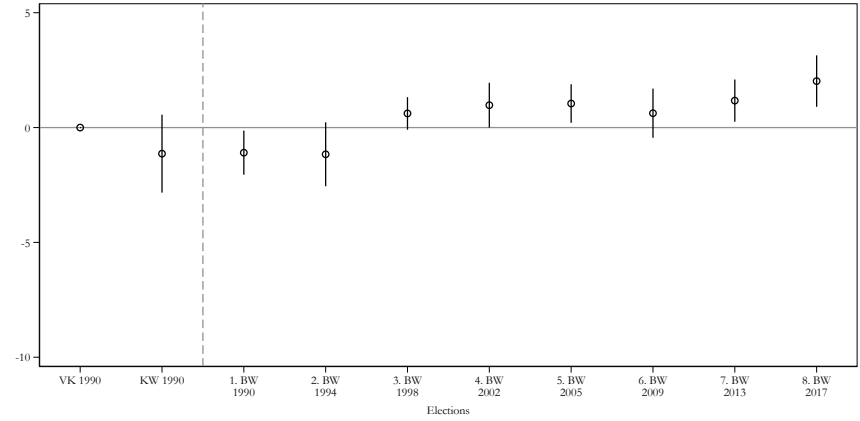
I conduct several robustness checks. I showed that the results are robust to different estimation methods like pooled OLS, fixed effects and difference-in-difference estimations separately for each period. My baseline results also hold when estimated with standard errors robust to heteroskedasticity (table A6.4, panel A in the appendix). To account for the low number of observations in my sample I also compute jackknife inferences (table A6.4, panel B in the appendix). Inferences do not change. As rainfall at the election day might affect

Figure 6.6: Period difference-in-differences results

(a) State assembly (LW)



(b) Federal assemly (BW)



Notes: This figure shows the results of difference-in-difference estimations for each election period using a panel fixed effects model where voter turnout (%) is the dependent variable. The figure shows regression results of counties which had a referendum (treated) compared to counties without an referendum (control). Vertical dashed lines represent the establishment of the new East German states. The left side of the dashed lines shows elections for the *Volkskammer* (VK) and county assemblies (*Kreistagswahl*, KW) in 1990 before the new states were established. The right side of the dashed lines shows elections for the state assemblies (*Landtagswahl*, LW) and the federal assembly (*Bundestagswahl*, BW), respectively. Circles are point estimates, black lines represent the 90% confidence interval. The electorate in logs, election period and county fixed effects are included as control variables. Standard errors are clustered at the county level.

voter participation[36] I add precipitation to the control variables (table A6.5 in the appendix), which hardly changes the results. Tables A6.6 in the appendix shows further robustness tests modifying the control group. As all counties which had a referendum are rural counties, panel A of table A6.6 excludes all urban counties from the control group.[37] The results are quite similar to the baseline results from table 6.4. Panel B of table A6.6 considers only counties at a "inner East German state border" in the control group (see figure A6.4 in the appendix). It may well be that citizens in counties with a referendum identify less with their state, because they border another state. Therefore, counties at a "inner East German state border" could constitute a better control group than all counties. The baseline results from table 6.4 are fairly reproduced, albeit the point estimate is slightly decreased. Also the number of observations is decreased severely. Applying the specifications of table A6.6 the effect on voter turnout in federal elections turns out to be statistically significant using pooled OLS and fixed effects regression. In table A6.7 in the appendix I do not estimate the effect for state and federal elections separately, but rather include both dummies. The baseline results hardly change. Having a referendum reduces voter turnout in state elections by 3.6 percentage points, while there is no robust significant impact an voter turnout in federal elections.

6.4.2 Successful vs. overruled referendum

The impact on state identity is likely to differ depending on whether the county assembly followed the majority will or decided contrary. Table 6.5 reproduces table 6.4 but further divides the counties with referendum into counties where the count assembly followed the majority will (successful referendum) and counties where the county assembly overruled the results of the referendum. Panel A shows the short-term effect considering only elections in 1990. Counties with a overruled referendum have a 7.7 percentage point lower voter participation in state elections compared to counties without referendum. For counties with an successful referendum the effect is smaller and amounts to around 3 percentage points (panel A, columns 1-3). A t-test shows that the coefficients for successful and overruled referendum are statistically different from each other. In federal elections, in contrast, voter turnout seems to be higher in counties with overruled referendum while the effect is negative for counties with successful referendum and becomes statistically not significant ones county and election period fixed effects are included (panel A, columns 4-6). The divergent result for counties with successful and overruled referendum becomes obvious in figure 6.5b. Voter turnout in counties with overruled referendum is lower in state, but similar in federal elections, compared to counties with successful referendum.

[36] Fujiwara *et al.* (2016) and Artés (2014), for example, find a negative impact of rain at the election day on voter turnout, while Persson *et al.* (2014) do not find a robust negative effect. On spurious effects of rain at election days on voter turnout, see Lind (2019).
[37] This also accounts for the fact that postal votes are sometimes considered differently in urban counties than in municipalities.

Turning to long-term effects until 2019, figure 6.5b shows that in both groups of counties, with successful and overruled referendum, voter turnout in state elections stays smaller than voter turnout in counties without referendum, even 30 years after the state borders where defined. On the other hand, it is not obvious whether counties with overruled referendum have lower voter participation than counties where the county assembly followed the referendum, as one would expect. Also there does not seem to be a clear impact on voter participation in federal elections. Panel B of table 6.5 confirms that having a referendum affects voter turnout in state elections in both groups persistently and similarly, a t-test cannot reject the equality of the coefficients at any conventional significance level. While in the short-run having an overruled referendum decreases turnout even further, in the long-run having had a referendum matters, but whether it was successful or overruled does not make a difference. In the 1990 state election directly following the ignored referendum citizen might be enraged by large about rejecting their declared will, but afterwards their voting participation is not different to those counties, where the county assembly followed the referendum. In federal elections there is an positive effect on voter turnout in counties with successful referendum, but the difference between the two coefficients of successful and overruled referendum does not turn out to be statistically significant according to the t-test.

Figure A6.5 in the appendix shows the long-run results for counties with successful and overruled referendum, estimated separately for each election period. There is a significant reduction in voter turnout in the state election in 1990, which is twice as large in counties with overruled referendum than in counties with successful referendum. But in the state elections between 1994 and 2019 the difference between counties with successful and overruled referendums becomes smaller. In federal elections the effect is smaller in magnitude, similar across both groups of counties and positive.

6.5 Discussion

6.5.1 Implications of reduced regional identity

The results show that there is an persistent negative effect from having a referendum on voter turnout in state elections, whereas there is no (robust) evidence for an impact on voter turnout in federal elections. Within the group of counties with referendum, having it overruled or followed by the county assembly does not make a difference in the long-run, neither in state nor in federal elections. Therefore, having a debate and referendum about state affiliation by itself leads to reduced state identification among citizens, while identification with the federal state is by and large not affected. The results are in line with the theoretical model and empirical findings by Dehdari and Gehring (2019). Changing borders of administrative areas can influence sub-national identity persistently, without necessarily influencing national identity.

In consequence of the county restructuring in 1952, county borders in the GDR did not follow old state borders. All counties located at the intersection of old state and district borders conducted a referendum. Regardless of the result of this referendum, some areas of the counties would belong to a different state than they had belonged to before 1952. In the referendum parts of the citizens revealed their identification with a different state than the majority, or the county assembly, decided for. This might explain why it does not make a (significant) difference whether the county assembly followed the will of the majority or not, but whether the county had, because its boundaries intersecting the boundaries of the old states, an unclear state affiliation. The debate about regional affiliation led to an engagement with the topic and to awareness and decision making among the citizens, while the referendum "produced losers". The pure fact that citizens now live in a different state than they voted for does on the aggregate not induce identity formation with the new state. The aggregate level of identification with the new administrative region does not adjust to the same level as in non-affected areas over time, even after 30 years.

In areas with secession movements typically not all inhabitants share the same regional identity and opinion about regional affiliation. A quite extreme example is the Brexit referendum in 2016. Prior to the referendum there was a huge medial debate about pro and contra of being a member of the European Union. Interest groups, political parties and politicians gave voting recommendations. The potential Brexit was a severe and wide spread issue, causing citizens to get engaged with the topic and potentially forming their mind. In the referendum a slim majority of 52% voted against a the European Union, with a voter participation of 72%. My results point to the direction that citizens who voted in favor of the European Union might on the aggregate not fully adjust their regional identity to be a "no-European-Union-member", even years after the Brexit, which leaves the population divided in that matter. If (administrative) regions are dissolved, either in a process of regional restructuring, a change in boundary lines, or by leaving the European Union, identification with this old regions might not disappear but rather remain strong, irrespective the loss of administrative status, especially if the new regions are established in a top-down fashion (Zimmerbauer, 2011). Nevertheless, regional identity with the new area might develop in the very long-run, but a 30 years window does not seam to be sufficient.

Reduced regional identity can have several implications. If citizens identify less with their region, they might be more eager to move as the region becomes less attractive. In table A6.8 in the appendix I replicate my baseline fixed effects regression (equation (6.2)) with the electorate (log) as dependent variable. The results show that in counties with referendum, where identification with the state is reduced, the electorate decreased between 1990 and 2019. In the sample of overruled vs. successful referendum, where there was no significant difference for state identification between both groups of counties, results for the development of the electorate also does not turn out to be statistically different from each other. Again, not whether the referendum was overruled or not matters, but whether there was an unclear state affiliation in the first place. Using cross-sectional data from the Life in Transition Survey

conducted between 2015 and 2016, I find further suggestive evidence regarding long-term implications of reduced regional identity. Table A6.9 in the appendix indicates that respondents living in a county with overruled referendum have lower trust in the government and politics, while trust in the neighborhood, people in general and foreigners is similar to respondents living in counties without referendum.[38] Dehdari and Gehring (2019) and Gehring (2020) also find that regional identity affects policy preferences. Citizens with stronger sub-national identity favor the transfer of competences from the federal to the sub-national level and favor more regional autonomy (Dehdari and Gehring, 2019), while citizens with increased supranational identity show increased trust in common institutions and an increased support of common policies (Gehring, 2020).

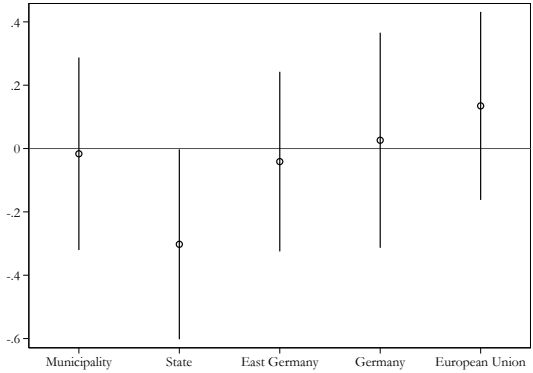
6.5.2 Survey results and regional newspapers

Figure 6.7 provides further suggestive evidence that the change in administrative areas in 1990 has persistent effects on identity. In an survey conducted 2014 in Thuringia citizens where asked about their regional identification. In the electoral district of Greiz and Altenburger Land[39], comprising the counties (as of 1990) Altenburg (overruled referendum, 53.81% in favor of Saxony), Schmölln (successful referendum, 18.08% in favor of Saxony) and the city of Greiz (no referendum), citizens reported on average to identify less with their state Thuringia, compared to citizens in other electoral districts, while identification with their municipality, East Germany, Germany and the European Union is similar. In the county Altenburger Land (former counties of Altenburg and Schmölln) in Thuringia the head of county administration started in 2011 again the discussion whether the county should not better become a part of Saxony after all.[40]

There are also some noticeable patters in the availability of regional newspapers in the counties where the referendum was overruled. The sales territory of the Saxon regional issues of the *Bild* newspaper, *Bild Leipzig* and *Bild Dresden*, reach across the borders of Saxony. In some municipalities in the counties Altenburger Land in Thuringia as well as Elbe-Elster and Oberspreewald-Lausitz in Brandenburg the *Bild Leipzig* and *Bild Dresden* are spread, but not the *Bild Thuringia* and *Bild Berlin-Brandenburg* (sales territory of 2018/2019; ZMG Mediaservice, 2020). These municipalities lie in the areas of the three former counties where the citizens' decision in favor of Saxony was overruled by the county assemblies in 1990. In the areas of these former counties where the decision in favor of Saxony was overruled households subscribed in 2018/2019 also for other regional newspapers from Saxony: the newspaper *Morgenpost für Sachsen* with its regional issue *Dresdner Morgenpost* in the coun-

[38] Out of 346 respondents interviewed in East Germany 21 are living in Starkenberg. Starkenberg is located in the county of Altenburg which had a overruled referendum in 1990.
[39] Survey results are only available at the electoral district level, not at the county or municipality level.
[40] Sächsische Zeitung, "Das Altenburger Land will nach Sachsen", 27.01.2011, `https://www.saechsische.de/das-altenburger-land-will-nach-sachsen-349781.html`; Ostthüringer Zeitung, "Der zweite Versuch zum Altenburger Länderwechsel", 04.02.2011, `https://www.otz.de/politik/der-zweite-versuch-zum-altenburger-laenderwechsel-id217801003.html`).

Figure 6.7: Regional identity in Thuringia, 2014



Notes: This figure shows regional identity in the electoral district of Greiz & Altenburger Land compared to the rest of Thuringia. Results are derived with an ordered probit regressions using data from the survey *Langfrist-Online-Tracking zur Landtagswahl Thüringen 2014* (Rattinger *et al.*, 2015). The survey is conducted at the electoral district level. The electoral district *Greiz & Altenburger Land* comprises the counties (as of 1990) Altenburg (referendum, 53.81% in favor of Saxony), Schmölln (referendum, 18.08% in favor of Saxony) and the city of Greiz (no referendum). Circles represent point estimates, vertical bars the 90% confidence intervals. Controls include sex, age, education level, employment status and income. Standard errors are robust to heteroskedasticity. Table A6.10 in the appendix shows the regression output.

ties of Elbe-Elster and Oberspreewald-Lausitz in Brandenburg[41] and the newspaper *Leipziger Volkszeitung* with its regional issue *OVZ Osterländer Volkszeitung* in the county Altenburger Land in Thuringia (ZMG Mediaservice, 2020). In addition to the results using voter turnout, this suggestive evidence using survey results and the sales territory of regional newspapers indicates that changing administrative areas might influence regional identity, even in the long-run.

6.5.3 Alternative explanations: Lower trust or punishment of local politicians

Rather than an expression of regional identity, the motive behind lower voter participation in my setting could also be a sign of reduced trust in the political system or a form of punishment. Table A6.9 in the appendix indicates that citizens living in counties with overruled referendum might distrust government and politics. However, since voter turnout in federal assembly elections is by and large not negatively affected in counties with referendum nor in counties

[41] With some subscriptions in the counties of Cottbus and Spree-Neiße.

with overruled referendum it indicates that having a referendum and ignoring the will of the citizens did not lead, as prophesied, to a loss of trust in the representative democratic system in general.

If citizens want to punish politicians for ignoring their declared will, than voter turnout in county assembly elections should be decreased as well. After all, the county assemblies were the administrative unit deciding in 1990 independently about future state affiliation. Table 6.6 shows long-term results similar to panel B of table 6.4 for county, state and federal assembly elections using fixed effects panel regressions (equation (6.2)). Consistent local election data for the five states is available for the county assembly elections between 2007 and 2019. For a better comparability of results table 6.6 considers only federal and state elections taking place in the same time frame.[42] The results show that overall there is no significant impact of having a referendum on voter turnout in county assembly elections with a point estimate close to zero, while the effect for state elections is, also in the shorter sample, negative with 1.9 percentage points.[43] No long-term effect on voter participation in county assembly elections but a negative impact on voter participation in state assembly elections is in contradiction to the hypothesis that the results are an expression of punishment. Changing state borders has the biggest influence on voter turnout in state elections. This indicates that reduced regional identity with the state is crucial.

6.5.4 Thread to identification: Migration

Long term effects on identity, however, might be influenced by migration. East Germany experienced a huge out migration after reunification. If citizens in counties with referendum are less attached to their state and migrate more, as shown in table A6.8 in the appendix, this should bias against my results. In any event, short term effects presented in panel A of tables 6.4 and 6.5 should not be affected by migration. Before reunification, also mobility in the GDR was severely restricted and was subject to the central planing. Citizens had to apply for new housing (and new occupation) and moved across municipality borders on average only once every 40 years, mobility across county borders was even less (Friehe *et al.*, 2019; Grundmann, 2013).

[42] These are the federal elections in 2009, 2013 and 2017, and the state elections in 2006/2009, 2011/2014 and 2016/2019.

[43] While interpreting these results one has to keep in mind that there where severe county mergers in the beginnings of the 1990s and that the counties from 1990, who followed or overruled the referendum in 1990, did not exist any more. Again, data comes from municipal election results and are aggregated to reconstruct the counties from 1990.

6.6 Conclusion

I examine a unique setting in East Germany and contribute to the emerging economic literature on aspects shaping group identities with two main results. First, administrative borders influence regional identity and second, regional identity with a new administrative region is not developed within a 30 year time window. The GDR replaced historical grown states with districts in 1952. During the process of German reunification in 1990 states had to be re-established. Because the GDR also restructured counties in 1952, it was not possible to construct the old states along county borders. 15 counties were located between old state and district borders and thus, state affiliation was not clear. To decide about future state affiliation, referendums were held. In three counties the county assembly did not follow the majority will but overruled the referendum.

As an important measure of revealed regional identity I consider voter turnout in state and federal election. My sample includes 215 counties between 1990 and 2019. Applying a difference-in-differences approach the results show that counties which had a referendum have a significant lower participation in elections for the state assembly, compared to counties without a referendum. On the other hand, participation in elections for the federal assembly is, by and large, not altered. The effect for state elections becomes smaller over time, but remains significant, even nearly three decades after the state borders were defined. Within the group of counties with referendum, having it overruled or followed by the county assembly does not make a difference in the long-run. This implies that in counties, where it was not clear to which state they would belong, regional identity with the state is persistently lower while national identity is not affected. Since in the long-run also voter turnout in county assembly elections is not affected in counties with referendum, it is likely that the reduced voter turnout in state elections is not a form of punishment of local politicians for ignoring the majority will, but rather an expression of reduced state identity. Administrative areas are important determinants of common regional identity. Changing them does on the aggregate not lead to a formation of regional identity with the new region, which might have long-lasting implications if it affects preferences. A significant decline in the electorate in counties with a referendum compared to counties without a referendum indicate that these counties are less attractive for citizens.

Table 6.5: Successful vs. overruled referendum

	State assembly (LW)			Federal assembly (BW)		
	(1) POLS	(2) POLS	(3) FE	(4) POLS	(5) POLS	(6) FE
A: Short-term effect (1990)						
Successful referendum · election	-3.15***	-3.14***	-3.05***	-0.95*	-0.94*	-0.84
	(0.63)	(0.64)	(0.63)	(0.51)	(0.52)	(0.55)
Overruled referendum · election	-7.79***	-7.79***	-7.67***	1.19***	1.19***	1.28***
	(1.08)	(1.09)	(1.06)	(0.34)	(0.35)	(0.33)
t-test (successful = overruled referendum)	0.00	0.00	0.00	0.00	0.00	0.00
Within R^2	0.55	0.61	0.97	0.29	0.37	0.96
Number of counties	215	215	215	215	215	215
Observations	645	645	645	645	645	645
B: Long-term effect (1990-2019)						
Successful referendum · election	-3.84***	-3.84***	-3.84***	0.76	0.76	0.76
	(0.77)	(0.77)	(0.77)	(0.73)	(0.73)	(0.73)
Overruled referendum · election	-2.76***	-2.76***	-2.76***	2.01***	2.00***	2.01***
	(0.98)	(0.99)	(0.99)	(0.73)	(0.73)	(0.73)
t-test (successful = overruled referendum)	0.36	0.37	0.36	0.22	0.22	0.22
Within R^2	0.64	0.65	0.88	0.53	0.56	0.97
Number of counties	215	215	215	215	215	215
Observations	1,935	1,935	1,935	2,150	2,150	2,150
Electorate (log)	No	Yes	Yes	No	Yes	Yes
Geographical controls	No	Yes	No	No	Yes	No
State fixed effects	No	Yes	No	No	Yes	No
Election period fixed effects	No	No	Yes	No	No	Yes
County fixed effects	No	No	Yes	No	No	Yes

Notes: This tables shows results of difference-in-differences regressions using pooled OLS (POLS) and panel fixed effects (FE) models where voter turnout (%) is the dependent variable, comparing counties which had a successful/overruled referendum (treated) to counties without an referendum (control). Panel (A) shows short-term effects, data covers elections in the year 1990. Panel B shows long-term effects, data covers elections in the years between 1990 and 2019. The variables of interest *Successful/Overruled referendum · election* take on a value of one for federal/state elections in counties where the referendum was successful/overruled and zero otherwise. A t-test tests the equality of the two coefficients. Control variables include the electorate in logs, a dummy denoting rural counties, longitude, latitude, altitude, the distance to the inner German border and closest West German transmitter mast, state fixed effects, election period fixed effects and county fixed effects. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10.

Table 6.6: Elections for the county assembly

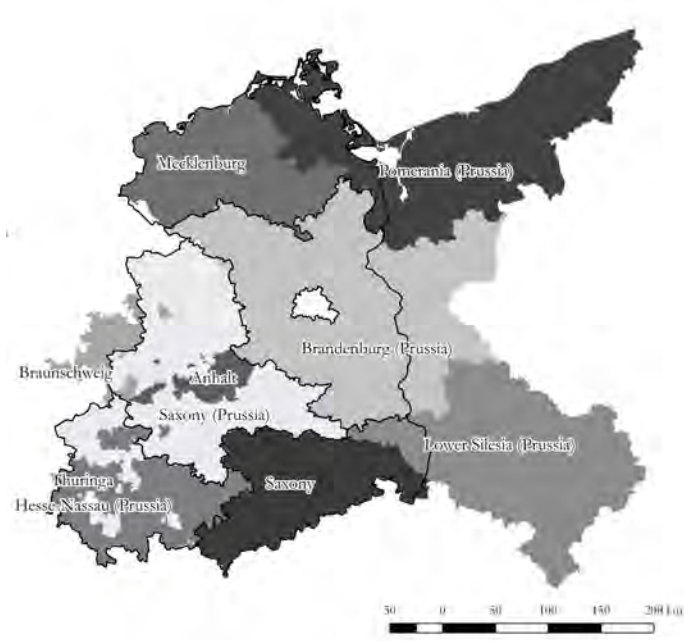
	Election for the ... assembly		
	(1) County (KW)	(2) State (LW)	(3) Federal (BW)
Referendum · election	-0.40	-1.90**	1.86**
	(1.20)	(0.90)	(0.80)
Electorate (log)	Yes	Yes	Yes
Election date fixed effects	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes
Within R^2	0.96	0.98	0.98
Number of counties	215	215	215
Observations	1,014	1,075	1,075

Notes: This table reproduces table 6.4 (panel B) including results for voter turnout in elections for the county assembly using fixed effects panel regressions. Due to data availability in local elections data covers elections between 2006 and 2019. The variable of interest *Referendum · election* takes on a value of one for federal/state elections in counties that had an referendum and zero otherwise. Control variables include the electorate in logs, election period fixed effects and county fixed effects. Significance levels (standard errors clustered at the county level in bracket): *** 0.01, ** 0.05, * 0.10.

Appendix

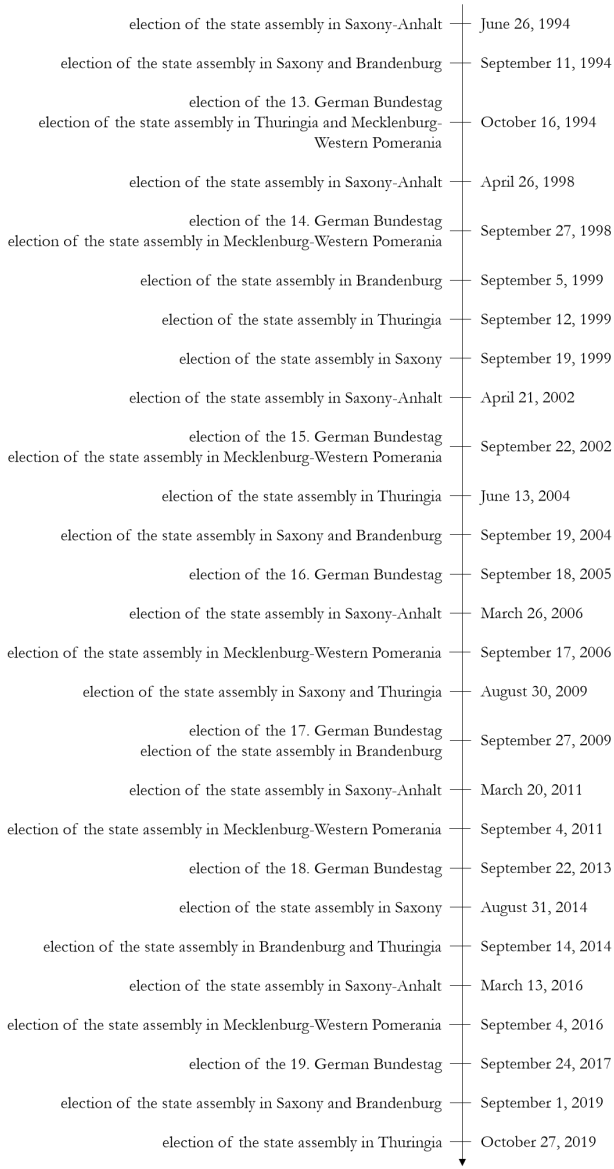
Figures and tables

Figure A6.1: Historical states and provinces, 1815-1945



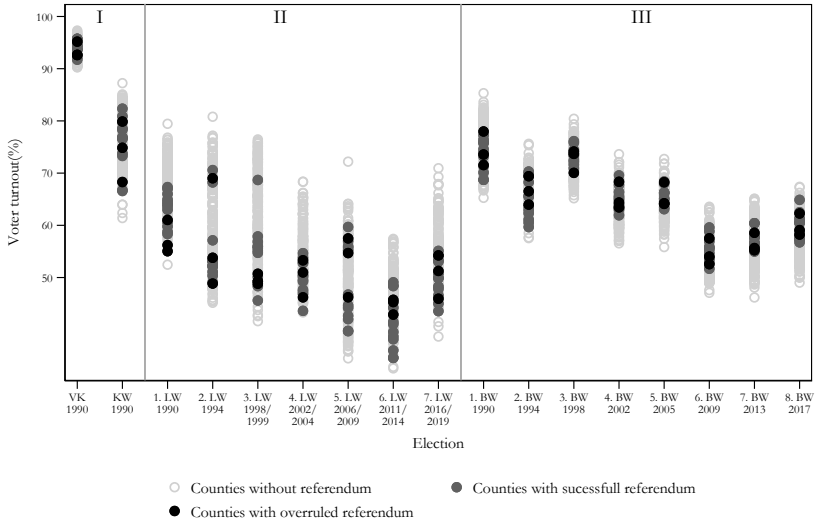
Notes: This figure shows in black lines the borders of the three states Mecklenburg-Western Pomerania, Saxony and Thuringia, and the two Provinces Brandenburg and Saxony-Anhalt (states since 1947) established in 1945, and the historical areas they are based on. These are the states of Mecklenburg, Anhalt, Thuringia, Saxony, enclaves of the state Braunschweig, and parts of the Prussian Provinces Pomerania, Brandenburg, Lower Silesia, Saxony, and an enclave of Hesse-Nausau (the map only shows parts of the provinces Braunschweig and Hesse-Nassau). The border to Poland in the East was established 1945 along the rivers Oder and Neiße which divide the provinces Pomerania, Brandenburg and Lower Silesia as well as the state Saxony. The map excludes Berlin.

Figure A6.2: Timing of events (II), 1994-2019



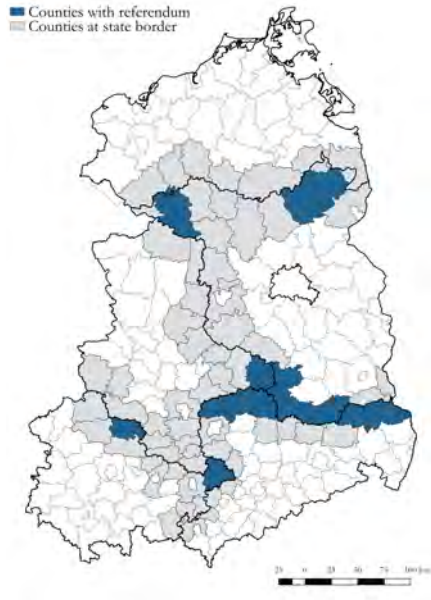
Notes: This figure shows the timing of different elections between 1994 and 2019.

Figure A6.3: Raw data plot



Notes: This figure shows turnout for each county in the elections between 1990 and 2019.

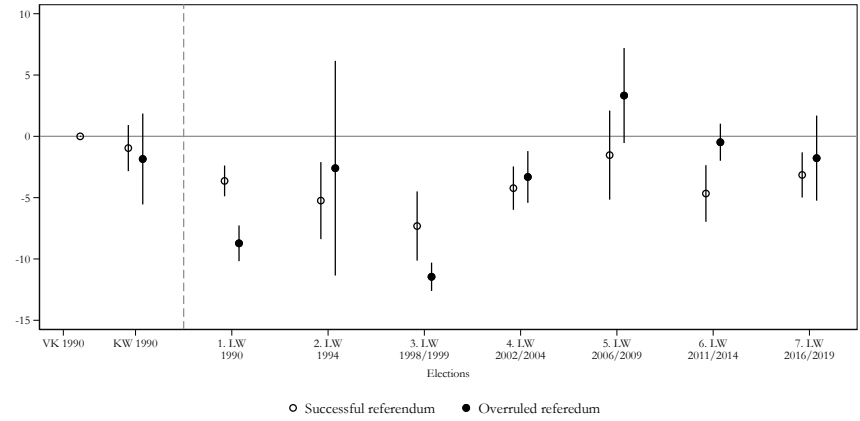
Figure A6.4: Counties at state borders between East German states



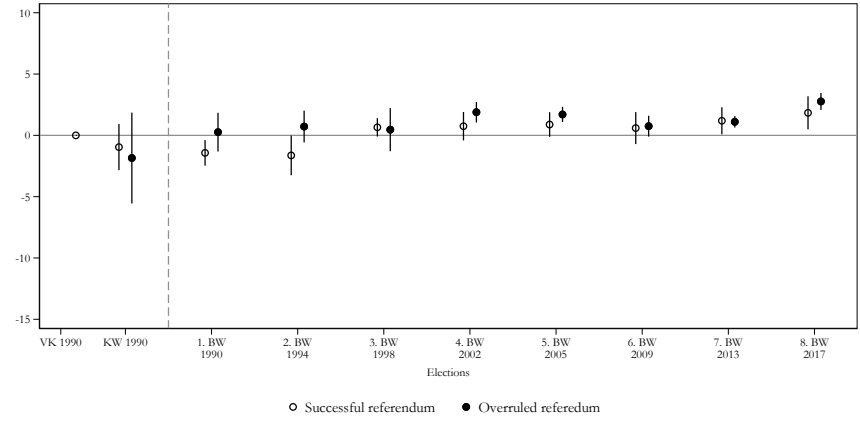
Notes: This figure shows in light gray all counties that lie at a state border of an East German state and are used as a robustness test in table A6.6, panel B. Blue counties had a referendum.

Figure A6.5: Period difference-in-differences results - Succesful vs. overruled referendum

(a) State assembly (LW)



(b) Federal assemly (BW)



Notes: This figure shows the results of difference-in-difference estimations for each election period using a panel fixed effects model (similar to table 6.5, column 3 and 6) where voter turnout (%) is the dependent variable. It shows regression results of counties which had a successful and overruled referendum (treated) compared to counties without an referendum (control). Vertical dashed lines represent the establishment of the new East German states. The left side of the dashed lines shows elections for the *Volkskammer* (VK) and county assemblies (*Kreistagswahl*, KW) in 1990 before the new states were established. The right side of the dashed lines shows elections for the state assemblies (*Landtagswahl*, LW) and the federal assembly (*Bundestagswahl*, BW), respectively. Circles are point estimates, black lines represent the 90% confidence interval. The electorate in logs, election period and county fixed effects are included as control variables. Standard errors are clustered at the county level.

Table A6.1: Perceived identity correlates with participation in elections

	Indentification with	
	(1) municipality/city	(2) Germany
Participating in local elections	0.26***	-0.02
	(0.07)	(0.07)
Participating in federal elections	-0.06	0.17**
	(0.07)	(0.07)
Participating in European elections	0.00	-0.01
	(0.04)	(0.04)
State fixed effects	Yes	Yes
Additional controls	Yes	Yes
Pseudo R^2	0.02	0.04
Observations	2,338	2,331

Notes: This table shows regression results of an ordered probit regression using German data from the European Value Study, wave 2017 (EVS, 2019). Controls include age, sex, town size, employment status, marital status and state fixed effects. Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

Table A6.2: Pre-treatment characteristics – Party vote shares

A: Referendum	Mean referendum	Mean no referendum	Diff.	SD	Obs.
Volkskammer					
CDU	44.65	44.98	0.33	2.61	215
SPD	20.87	20.58	-0.29	2.00	215
FDP	5.62	5.52	-0.10	0.55	215
PDS	15.58	14.84	-0.74	1.49	215
B90/Gr	3.71	4.21	0.50	0.40	215
County assembly					
CDU	35.78	36.17	0.40	2.64	215
SPD	18.77	19.07	0.30	2.22	215
FDP	9.10	8.23	-0.87	1.19	215
PDS	13.46	13.04	-0.42	1.28	215
B90/Gr	5.22	6.09	0.87	1.25	215
B: Overruled vs. successful referendum	Mean overruled referendum	Mean successful referendum	Diff.	SD	Obs.
Volkskammer					
CDU	49.84	43.35	-6.49	5.59	15
SPD	17.13	21.81	4.68	4.39	15
FDP	5.57	5.63	0.05	0.75	15
PDS	13.81	16.03	2.22	2.54	15
B90/Gr	3.71	3.71	-0.00	0.45	15
County assembly					
CDU	42.29	34.15	-8.14*	4.55	15
SPD	16.38	19.36	2.98	3.23	15
FDP	8.23	9.31	1.08	2.02	15
PDS	12.74	13.64	0.90	2.32	15
B90/Gr	4.63	5.37	0.74	3.20	15

Notes: This table shows t-tests for party vote shares in 1990. Panel A compares counties which had a referendum to counties without an referendum. Panel B compares counties where the referendum was overruled by the county assembly to counties where the county assembly followed the referendum (successful referendum). *CDU*: Association of conservative parties – Christian Democratic Union (CDU), Bürgerbewegung "Demokratischer Aufbruch - sozial + ökologisch" (DA); *SPD*: Social Democratic Party (SPD); *FDP*:Association of liberal parties – Free Democratic Party (FDP), Liberale, National-Demokratische Partei Deutschlands (NDPD), Bund Freier Demokraten (BFD), Liberale Demokratische Partei (LDP); *PDS*: Party of Democratic Socialism (PDS); *B90/Gr*: Association of green parties – Bündnis 90, GRÜNE-UFV, Neues Forum (NF), Demokratie Jetzt (DJ), Initiative Frieden und Menschenrechte (IFM), Grüne Partei, Grüne Liga, Grüne Listen, Unabhängiger Frauenverband (UFV). Significance levels: *** 0.01, ** 0.05, * 0.10.

Table A6.3: Surveys at district level, 1990

A: Welfare East	(1) Conflict between politicians and citizens	(2) Union membership	(3) Party membership	(4) Citizens' initiative membership	(5) Current sense of live	(6) Satisfaction with neighborhood
District including counties with referendum	0.14 (0.25)	-0.40 (0.30)	0.42 (0.42)	0.49 (0.63)	-0.17 (0.30)	0.30 (0.23)
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	0.03	0.07	0.05	0.17	0.02	0.01
Observations	668	682	659	650	685	687
B: Politbarometer Ost	(1) Political interest	(2) Voting if there is an election next sunday	(3) In favor of reunification	(4) Reunification brings long-term advantages	(5) Identity with GDR (rather than FDR)	(6) Household income
District including counties with referendum	0.12 (0.19)	-0.05 (0.14)	-0.10 (0.20)	-0.27 (0.19)	0.70 (0.47)	-0.05 (0.09)
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	0.06	0.02	0.05	0.02	0.13	0.11
Observations	808	4,956	3,934	2,148	678	4,853

Notes: This table shows ordered probit regressions using in panel A data from the survey *Welfare East* conducted by the *Zentralinstitut für Jugendforschung* between October and December 1990 and in panel B data from the survey *Politbarometer Ost* conducted by the *Forschungsgruppe Wahlen* between March and December 1990 among adults in East Germany (Zapf and Noll, 1993; Forschungsgruppe Wahlen, Mannheim *et al.*, 1991). Districts including counties with a referendum takes on the value of one for the districts of Neubrandenburg, Schwerin, Cottbus, Halle, and Leipzig, and zero otherwise. Additional controls include age, sex, town size, education level, and size of household. Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10 (no significant values to report).

Table A6.4: Robustness (I) – Standard errors

	State assembly (LW)			Federal assembly (BW)		
	(1) POLS	(2) POLS	(3) FE	(4) POLS	(5) POLS	(6) FE
A: Standard errors robust to heteroskedasticity						
Referendum · election	-3.63*	-3.62*	-3.56***	1.01	1.01	1.08*
	(2.03)	(2.03)	(0.69)	(1.97)	(1.98)	(0.63)
Within R^2	0.64	0.65	0.88	0.53	0.56	0.97
B: Jackknife standard errors						
Referendum · election	-3.63***	-3.62***	-3.56***	1.01	1.01	1.08
	(0.71)	(0.71)	(0.73)	(0.66)	(0.66)	(0.67)
Within R^2	0.64	0.65	0.88	0.53	0.56	0.97
Electorate (log)	No	Yes	Yes	No	Yes	Yes
Geographical controls	No	Yes	No	No	Yes	No
State fixed effects	No	Yes	No	No	Yes	No
Election period fixed effects	No	No	Yes	No	No	Yes
County fixed effects	No	No	Yes	No	No	Yes
Number of counties	215	215	215	215	215	215
Observations	1,935	1,935	1,935	2,150	2,150	2,150

Notes: This table reproduces the long term effects of table 6.4 (panel B) with standard errors robust to heteroskedasticity (panel A) and jackknife standard errors (panel B). Data covers elections between 1990 and 2019. The variable of interest *Referendum · election* takes on a value of one for federal/state elections in counties that had an referendum and zero otherwise. Control variables include the electorate in logs, a dummy denoting rural counties, longitude, latitude, altitude, the distance to the inner German border and closest West German transmitter mast, state fixed effects, election period fixed effects and county fixed effects. Significance levels (standard errors in brackets): *** 0.01, ** 0.05, * 0.10.

Table A6.5: Robustness (II) – Controlling for precipitation

	State assembly (LW)			Federal assembly (BW)		
	(1) POLS	(2) POLS	(3) FE	(4) POLS	(5) POLS	(6) FE
Referendum · election	-4.13***	-4.23***	-3.72***	0.92	0.94	1.10*
	(0.79)	(0.80)	(0.70)	(0.66)	(0.66)	(0.62)
Electorate (log)	No	Yes	Yes	No	Yes	Yes
Geographical controls	No	Yes	No	No	Yes	No
Precipitation	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	No	Yes	No	No	Yes	No
Election period fixed effects	No	No	Yes	No	No	Yes
County fixed effects	No	No	Yes	No	No	Yes
Within R^2	0.66	0.68	0.89	0.53	0.56	0.97
Number of counties	213	213	213	213	213	213
Observations	1,883	1,883	1,883	2,093	2,093	2,093

Notes: This table reproduces the long term effects of table 6.4 (panel B) controlling for precipitation at the election day. Data covers elections between 1990 and 2019. The variable of interest *Referendum · election* takes on a value of one for federal/state elections in counties that had an referendum and zero otherwise. Control variables include the electorate in logs, a dummy denoting rural counties, longitude, latitude, altitude, the distance to the inner German border and closest West German transmitter mast, precipitation, state fixed effects, election period fixed effects and county fixed effects. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10.

Table A6.6: Robustness (III) – Control group

	State assembly (LW)			Federal assembly (BW)		
	(1) POLS	(2) POLS	(3) FE	(4) POLS	(5) POLS	(6) FE
A: Without urban counties						
Referendum · election	-2.93***	-2.96***	-2.84***	1.27**	1.26**	1.36**
	(0.66)	(0.66)	(0.68)	(0.62)	(0.62)	(0.64)
Within R^2	0.65	0.67	0.90	0.55	0.57	0.97
Number of counties	189	189	189	189	189	189
Observations	1,701	1,701	1,701	1,890	1,890	1,890
B: Counties at a "inner East German state border"						
Referendum · election	-1.90**	-1.91**	-1.97***	1.75***	1.75***	1.75***
	(0.73)	(0.73)	(0.69)	(0.65)	(0.65)	(0.64)
Within R^2	0.68	0.70	0.90	0.56	0.57	0.97
Number of counties	75	75	75	75	75	75
Observations	675	675	675	750	750	750
Electorate (log)	No	Yes	Yes	No	Yes	Yes
Geographical controls	No	Yes	No	No	Yes	No
State fixed effects	No	Yes	No	No	Yes	No
Election period fixed effects	No	No	Yes	No	No	Yes
County fixed effects	No	No	Yes	No	No	Yes

Notes: This table reproduces the long term effects of table 6.4 (panel B) considering in the control group in panel A only rural counties and in Panel B only counties at a "inner East German state border" (see figure A6.4). Data covers elections between 1990 and 2019. The variable of interest *Referendum · election* takes on a value of one for federal/state elections in counties that had an referendum and zero otherwise. Control variables include the electorate in logs, longitude, latitude, altitude, the distance to the inner German border and closest West German transmitter mast, state fixed effects, election period fixed effects and county fixed effects. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10.

Table A6.7: Robustness (IV) – Federal and state elections together

	(1)	(2)	(3)
Referendum · BW	1.01	1.02	1.09*
	(0.62)	(0.62)	(0.65)
Referendum · LW	-3.63***	-3.62***	-3.55***
	(0.68)	(0.68)	(0.71)
Electorate (log)	No	Yes	Yes
Geographical controls	No	Yes	No
State fixed effects	No	Yes	No
Election period fixed effects	No	No	Yes
County fixed effects	No	No	Yes
Within R^2	0.52	0.54	0.89
Number of counties	215	215	215
Observations	3,655	3,655	3,655

Notes: This tables shows results of difference-in-differences regressions using pooled OLS. Data covers elections between 1990 and 2019. The variable of interest *Referendum · LW/BW* takes on a value of one for federal/state elections in counties that had an referendum and zero otherwise. Control variables include the electorate in logs, a dummy denoting rural counties, longitude, latitude, altitude, the distance to the inner German border and closest West German transmitter mast, state fixed effects, election period fixed effects and county fixed effects. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10.

Table A6.8: Development of the electorate

	Referendum vs. no referendum	Overruled vs. successful referendum
	(1) FE	(2) FE
Referendum · election	-0.05***	
	(0.02)	
Successful referendum · election		-0.05**
		(0.02)
Overruled referendum · election		-0.08*
		(0.04)
Year fixed effects	Yes	Yes
County fixed effects	Yes	Yes
t-test (successful = overruled referendum)		0.51
Within R^2	0.27	0.27
Number of counties	215	215
Observations	2,503	2,503

Notes: This tables shows results of difference-in-differences regressions using panel fixed effects (FE) models where the electorate (log) per election year between 1990 and 2019 is the dependent variables. Column 1 shows regression results of counties which had a referendum (treated) compared to counties without an referendum (control). The variable of interest *Referendum · election* takes on a value of one for federal/state elections in counties that had an referendum and zero otherwise. Column 2 divides the treatment group into counties where the county assembly followed the referendum (successful referendum) and counties where the referendum was over-ruled by the county assembly. The variables of interest *Successful/Overruled referendum · election* take on a value of one for federal/state elections in counties where the referendum was successful/overruled and zero otherwise. A t-test tests the equality of the two estimates. Control variables include year fixed effects and county fixed effects. Significance levels (standard errors clustered at the county level in brackets): *** 0.01, ** 0.05, * 0.10.

Table A6.9: Life in transition survey, 2015/2016

	Trust in						
	(1) the government/ cabinet of ministers	(2) regional government	(3) local government	(4) political parties	(5) the neighborhood	(6) people	(7) foreigners
Referendum (overruled)	-0.46*	-0.67***	-0.56**	-0.61**	-0.06	-0.13	-0.22
	(0.25)	(0.24)	(0.24)	(0.24)	(0.24)	(0.26)	(0.29)
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	0.02	0.04	0.04	0.02	0.01	0.02	0.03
Observations	346	344	344	343	346	340	344

Notes: This table shows ordered probit regressions using data from the Life in Transition Survey (LiTS III) conducted between the end of 2015 and the beginning of 2016 (European Bank for Reconstruction and Development, 2016). Household interviews in East Germany where conducted in Brandenburg an der Havel, Chemnitz, Döllstädt, Dresden, Forst, Freiberg, Glauchau, Havelberg, Jena, Lambrechtshagen, Luckau, Magdeburg, Spremberg, Starkenberg, Tanna, Tharandt and Wolgast. The variable of interest *Referendum (overruled)* takes on the value of one for interviews conducted in Starkenberg. Starkenberg is located in the county of Altenburg which had a (overruled) referendum in 1990 (21 responses). Additional controls include age, sex, urbanity status, state, education level, and marital status. Significance levels (standard errors robust to heteroskedasticity): *** 0.01, ** 0.05, * 0.10.

Table A6.10: Regional identity in Thuringia, 2014

	Indentification with				
	(1) Municipality	(2) State	(3) East Germany	(4) Germany	(5) European Union
Greiz & Altenburger Land	-0.02	-0.30*	-0.04	0.03	0.13
	(0.18)	(0.18)	(0.17)	(0.21)	(0.18)
Additional controls	Yes	Yes	Yes	Yes	Yes
Pseudo R^2	0.02	0.02	0.02	0.01	0.02
Observations	480	479	475	478	477

Notes: This table shows ordered probit regressions using data from the survey *Langfrist-Online-Tracking zur Landtagswahl Thüringen 2014* (Rattinger *et al.*, 2015). The survey is conducted at the electoral district level. The electoral district *Greiz & Altenburger Land* comprises the counties (as of 1990) Altenburg (referendum, 53.81% in favor of Saxony), Schölln (referendum, 18.08% in favor of Saxony) and the city of Greiz (no referendum). Controls include sex, age, education level, employment status and income. Significance levels (standard errors robust to heteroskedasticity in brackets): *** 0.01, ** 0.05, * 0.10.

Short history of the East German states

Important determinants for the course of East German state borders in 1952 are the Congress of Vienna in 1815 and the new border to Poland in 1945, along the rivers Oder and Neiße. Nevertheless, also before 1815 borders shifted several times.

The Margrave of Brandenburg, founded in the 12th century, became an electorate in the 14th century. The borders of 1952 trace back to the Congress of Vienna in 1815, when the electorate of Brandenburg became a Prussian Province. In 1945 the areas to the East of the rivers Oder and Neiße became part of Poland.

The state of Mecklenburg-Western Pomariana was established in 1945 based on the state Mecklenburg and the western parts of the Prussian Province Pomerania, the areas to the east of the river Oder became part of Poland. Mecklenburg was first mentioned in 995, Pomerania in 1046. The dukedom Pomerania became a Prussian Province in 1815. The dukedoms of Mecklenburg-Schwerin and Mecklenburg-Strelitz (states since 1918/1919) formed the state of Mecklenburg in 1934.

Saxony has its precursor in 929 and became over the centuries an dukedom, electorate and in 1806 a kingdom. Similarly to Brandenburg the borders of 1952 trace back, by and large, to 1815, where the Kingdom of Saxony lost in the Congress of Vienna around two thirds of its area to Prussia. When the Soviet Military Administration established Saxony in 1945 they extended the former kingdom by parts of the Prussian Province Lower Silesia, while areas to the east of the river Neiße became part of Poland.

Saxony-Anhalt in its borders of 1952 is mostly based on the Prussian Province Saxony (some parts of the Province Saxony became a part of Thuringia) and the state Anhalt (as well as parts of Braunschweig and Thuringia, mostly enclaves of both states). The Prussian Province Saxony was founded in 1815, based on Prussian areas along the river Elbe and the parts of the Kingdom Saxony, that became Prussian in the course of the Congress of Vienna. The princedom Anhalt was established in the 13th century, but the estate was divided into smaller princedoms/dukedoms. In the 19th century the dukedoms of Anhalt-Dessau, Anhalt-Köthen and Anhalt-Bernburg formed the dukedom of Anhalt, which became a state in 1918.

The name Thuringia was first time mentioned in 395. After the Middle Age the state of Thuringia fall into different small states. In 1920 seven small states founded the state of Thuringia. In 1945 the Soviet Military Administration established Thuringia based on the state founded in 1920, extended by parts of the Prussian Province Saxony (Erfurt and North Thuringia) and an enclave of the Prussian Province Hesse-Nassau (for a map see figure A6.1 in the appendix).

7 Conclusion

Reducing regional disparities to align economic perspectives across regions and counteract the feeling of being left behind has become a primary political goal. To design carefully targeted policy measures, it is essential to investigate their possible far reaching consequences. In this thesis I thus elaborate on the impacts of different policy measures. Particularly, I investigate how infrastructure, institutions and identity influence outcomes at the local level which are closely tied to economic development.

The results regarding the economic impact of infrastructure are mixed. While my coauthors and I show in chapter 2 that airport infrastructure in a rather rural area fosters tourism, indicating positive impulses on economic development, results from chapter 3 are ambiguous. Highway infrastructure influences public finances, population and economic activity, the direction of the impact, however, varies according to the location of a region. My coauthor and I find that peripheral municipalities, once they get accessible via a highway, increase their property tax factors to keep tax revenues stable. Central municipalities, on the other hand, do not seem to adjust their tax policy. Examining possible drivers of this effect shows that central municipalities profit from increased accessibility in terms of population and economic activity, but they do so at the expanse of peripheral areas. The results imply that infrastructure investment are not an panacea for regional development, as it may induce opposite effects. Policy makers should adjust infrastructure investments to the specific circumstances. While a regional airport is well suited to promote regional development via tourism in an touristic region like the Allgäu, new infrastructure might also strengthen the movement from the periphery to the core.

In chapters 4 and 5 my coauthors and I investigate two institutions at the local level: the public accounting system and the local election system. At the first glance they seem not as closely tied to economic development as are infrastructure investments. Nevertheless, they might well unfold effects on economic outcomes. Especially the change in public accounting systems, from traditional cash-based to business-like accrual accounting is motivated by the promise of enhanced budget transparency, which is expected to increase accountability, efficiency, and sustainable budgeting. Sustainable public finances, in turn, are a prerequisite for regional development. The chapter provides evidence for the current debate about public accounting standards in Europe. The harmonized European Public Sector Accounting Standard (EPSAS), based on accrual accounting, is advocated, despite there being hardly any research proving its merits. Our findings do not show that accrual accounting in the public sector has kept its promise. Public finances, government efficiency, and accountability did not develop differently after the new accounting system was implemented, while costs to run the administration increase. However, the electoral system seems to map into economic

outcomes. Chapter 5 shows that the mode how local politicians are elected, directly by the electorate or appointed by the council, influences their economic performance. While directly elected politicians do not seem to attract more businesses or speed up administrative tasks, the public employment service becomes somewhat more effective, as a consequence the long-term unemployment rate decreases.

The development of a region may also be shaped by its inhabitants identity with the area they are living in. Regional identity, the local connection to ones community, is an important underlying factor of social cohesion and civic participation. I show in chapter 6 that regional identity is influenced by a change of administrative areas. Moreover, identification with the new area does on the aggregate not develop within a time window of 30 years. Chapters 5 and 6 provide evidence that policy measures not directly intended to influence regional development may also influence local outcomes. Policy makers should therefore pay attention to possible "side effects" of public interventions. A policy measure aimed at increasing voter turnout might turn out to reduce long-term unemployment, while administrative reforms, usually performed to reduce costs and increase performance of the public administration, might have long lasting implications for citizens regional identity and their attachment to the region.

The results of this thesis show that policy measures shape regional development via various channels. While infrastructure investments to foster regional development are an obvious public intervention, less apparent measures like regional identity should not be disregarded. The results reveal some important insights for policy makers, nevertheless, one has to keep in mind that they are based on the example of Germany, a federalistic high-income country. Further research should pursue to investigate implications of policy measures to target funds of regional cohesion policies precisely. Whether the same policy measure unfolds different consequences for peripheral and central or rural and urban areas is of particular importance.

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