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## Empirical Studies in Public Economics and Political Economy

*Florian Dorn*

ifo INSTITUTE
Leibniz Institute for Economic Research
at the University of Munich

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# Preface

Florian Dorn prepared this study during his doctoral studies at the Ludwig-Maximilians-University of Munich (LMU). The study was completed in September 2020 and accepted as doctoral thesis by the Department of Economics.

This dissertation contributes to the empirics of public economics and political economy. All chapters are self-contained research articles and can be read independently. However, the chapters are mutually related and contribute to the discussion on how economic inequality, institutions, and economic and infrastructure policies can influence welfare and political stability. Chapter 2 examines how relative economic deprivation influences the support for radical parties. Chapter 3 examines how trade openness influences income inequality. Chapter 4 uses a case study to discuss how infrastructure policies can affect regional economic development. Chapters 5 and 6 contribute to the debate on the effect of political and fiscal institutions on budgeting, accountability, and government efficiency.

Empirical identification strategies, endogeneity problems, and remaining caveats regarding causal inferences are discussed in detail in all chapters of this dissertation. The study employs state-of-the-art empirical techniques to infer causal effects including instrumental variables, difference-in-differences, event study, and synthetic control estimations.

The addendum contains extended abstracts of three further research projects during the Ph.D. phase of Florian Dorn: (I) Globalization, government ideology, and top income shares: Evidence from OECD countries; (II) Political institutions and health expenditure; (III) The common interest of health and the economy: Evidence from Covid-19 containment policies. These papers are also related to the empirics on public economics and political economy and contribute well to his overall research agenda.

Keywords: Public economics, public finance, political economy, applied econometrics, economic deprivation, income inequality, top income shares, political polarization, radical voting, trade openness, globalization, development, development levels, transition economies, economic policy, infrastructure, transportation infrastructure, infrastructure policy, airports, tourism, regional development, regional studies, Germany, municipalities, local government, institutions, fiscal rules, public accounting, budget transparency, sustainability, government efficiency, accountability, electoral cycles, elections, public goods, public services, health expenditure, health protection, government ideology, partisan theory, optimal policy, integrated simulations, real-time analysis, stochastic frontier analysis (sfa), data envelopment analysis (dea), panel data, econometrics, ordinary least squares (OLS), instrumental variable (IV), synthetic control method, difference-in-differences, event study

JEL-No: C23, C26, D02, D31, D63, D72, D73, H11, H20, H51, H72, H80, H83, F02, F60, F62, I15, I18, I32, L93, O18, P50, Z38

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I would also like to thank Clemens Fuest for co-supervising my thesis and his valuable advice and exchange on research projects and policy reports. His work and ideas inspire to think about the policy relevance of our daily work and research. As president of the ifo Institute, Clemens Fuest also motivated me to do sound economic research to foster evidence-based policies and advice, and to engage in public debate. I am grateful for having the opportunity to work for and with him, and to learn from him. Many thanks are also due to Andreas Haufler for joining my supervision comittee. I gained from Andreas Haufler's knowledge and lectures on Public Economics and Economic Policy during my studies at the University of Munich (LMU).

I am indebted to the ifo Institute for providing me with an excellent environment and infrastructure for economic research, professional development, and completing my thesis. The ifo Institute also financed several conference participations around the world, and my visit at the summer school seminars on *Quantitative Methods for Public Policy Evaluation* and *Dynamic and Non-linear Panel Data Models* at the Barcelona Graduate School of Economics. Next to Clemens Fuest and Niklas Potrafke, I would like to thank Stephanie Dittmer and Meinhard Knoche representative for the great support of the executive board as well as the administrative and technical staff at the ifo Institute, and colleagues of all ifo research centers. Among many others, I would also like to thank Hans-Werner Sinn for the inspiring collaboration during my first year at the ifo Institute, and Klaus Gründler and Felix Rösel for their valuable expertise and helpful comments on my research. For the productive and enouraging collaborations I am also very grateful to the co-authors of my research papers during the last four years.

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I am thankful for the excellent education I received as student at the University of Munich (LMU), the University of Mannheim, and as visiting Ph.D. student at the Economics Department and Goldman School of Public Policy at the University of California, Berkeley (USA). I am grateful to Frank Cowell for inviting me to a valuable research stay at the London School of Economics and Political Science (LSE) in UK during my doctoral studies. In addition, I also thank the Bavarian Graduate Program in Economics for inviting me to participate in the graduate course on *Regional and Urban Economics*.

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I owe a great deal to my parents, who always believed in me. I am more than thankful for their love and support throughout my life. The diligence and values of my parents have always been a model for me. I also thank my parents-in-law for their great support. My dear son Rafael had to do without me a lot before the completion of my doctoral thesis. This phase was a tough time for us — especially during the corona crisis and the nursery school closures. I love him and I am very grateful to him for how well he has mastered this phase.

Finally and most importantly, I am very grateful to my wonderful wife Ingrid for her tremendous support and unconditional love throughout my academic career. She made all this possible. Thank you!

Florian Dorn
September 2020

*For Ingrid*

# Inequality, Infrastructure, and Institutions –

## Empirical Studies in Public Economics and Political Economy

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| | Prof. Dr. Andreas Haufler |

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# 1 Introduction

This dissertation contributes to the empirics of public economics and political economy. Chapters 2 - 6[1] are self-contained research articles and can be read independently. All chapters, however, are mutually related and contribute to the discussion on how economic inequality, institutions, and economic and infrastructure policies can influence welfare and political stability.[2]

The introduction gives an outline of the individual chapters and reveals how they are related. I provide new empirical evidence on causes and consequences of economic inequality, how economic and infrastructure policies influence economic development, and how institutions and political forces affect public budgeting and government efficiency.

## Causes and consequences of inequality

Many economies around the world experienced a rise in economic inequality over the past decades (see Atkinson and Piketty, 2007; Atkinson *et al.*, 2011). The United States (US), for example, is widely seen as the country that has experienced the most pronounced increase in inequality. Today, about $1/5$ of the national income belongs to the richest percentile (top 1 %) in the USA, whereas the bottom 50 % in the US income distribution only earn half as much as the rich. Figure 1.1 illustrates how incomes of the top percentile (top 1 %) and the bottom 50 % percentiles evolved as share of the national income in selected industrialized countries. In the US and UK, the rich doubled their income shares since the beginning of the 1980s. During the same period, the income share of the bottom 50 % almost halved in the US. Several other industrialized countries also report growing divergence between rich and poor (before taxes and transfers; see Figure 1.1).

The trend of growing economic inequality has triggered heated public debates about its causes and consequences. One of the major concerns is that the rise in economic inequality could be perceived as unfair and jeopardizes social cohesion and political stability. Populist movements address economic concerns on fairness and emphasize the view of a corrupt elite to fuel

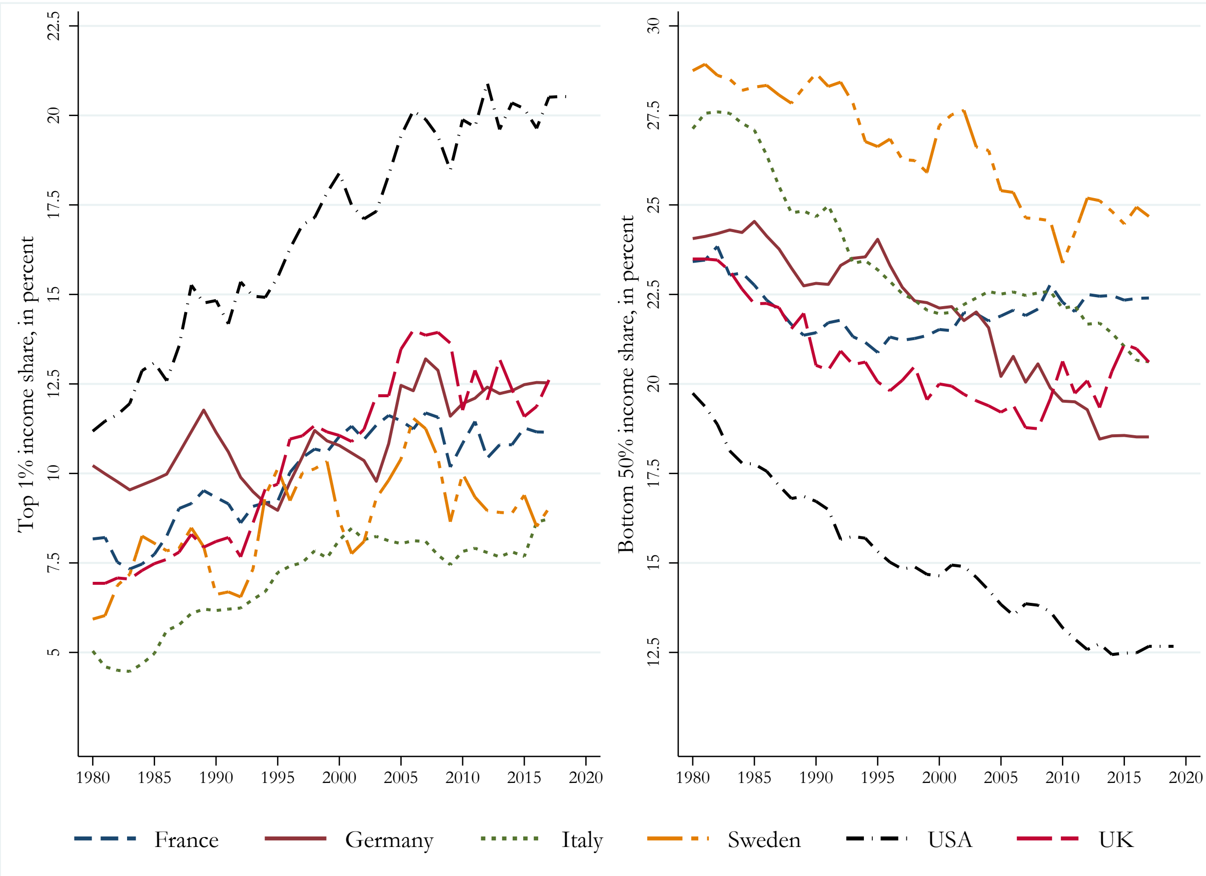
[1] Chapters 2 – 6 are based on five self-contained research contributions which I have conducted during my Ph.D. More information on co-authors and publication process are described at the starting page of each individual chapter.

[2] I am co-author of three further research projects during my Ph.D. phase: (I) Globalization, government ideology, and top income shares: Evidence from OECD countries (Dorn and Schinke, 2018); (II) Political institutions and health expenditure (Blum *et al.*, 2021); (III) Health protection and the economy: Evidence from Covid-19 containment policies (Dorn *et al.*, 2020b). These papers are also related to the empirics on public economics and political economy and well contribute to my research agenda. I provide an extended abstract of each paper and more information on co-authors and publication process in the addendum of this dissertation.

resentments against the "establishment" and the political order itself. Populists emphasize a perceived state of political and economic crisis, and try to appeal to voters by posing as their advocates and discrediting mainstream political parties and political institutions (Moffitt, 2016; Mudde, 2007).

Radical parties in many Western European countries experienced increasing power and vote shares. Referring to the countries in Figure 1.1, *Lega* and *MoVimento-5-Stelle* (5-Star-Movement) in Italy, *Front National* in France, the *Sverigedemokraterna* (Sweden Democrats) in Sweden, the *Alternative für Deutschland* (Alternative for Germany, AfD) and *Die Linke* (The Left Party) in Germany, or the *UK Independence Party* (UKIP) in the United Kingdom are a few prominent examples of the last decade. The Brexit referendum in UK in 2016 and the victory of Donald Trump in the USA in 2016 are also widely seen as reflecting the growing anger of the left behind.

Figure 1.1 : Income shares of the top 1% and bottom 50% (*pre-tax/transfer*)




*Data:* World Inequality Database (WID).

**Chapter 2**, which is joint work with Clemens Fuest, Lea Immel and Florian Neumeier (Dorn *et al.*, 2020a), contributes to the debate by investigating how relative economic deprivation influences the support for radical parties. We use a unique dataset covering different indicators of economic deprivation as well as federal election outcomes at the county level in Germany from 1998 to 2017. The results show that economic deprivation has a sizeable effect on vote shares for parties at both ends of the political spectrum, the radical left and the radical right. The higher a county's poverty rate, and the average shortfall from the national median income or poverty line, the higher the support for radical parties. Our results also show that regional variation in economic deprivation in Germany gave rise to the electoral success of the populist right-wing party AfD in the federal election of 2017.

Our indicators of economic deprivation follow a concept of relative deprivation suggesting that the individual support for populist or radical views results from an unfavorable comparison with other members of society. An unfavorable social comparison or the fear of social and economic decline, in turn, are believed to trigger perceived economic threat and feelings of anxiety – people believe that they are not getting what they are entitled to. Scholars have shown that those feelings may indeed foster resentments against the political mainstream and the political system itself (Algan *et al.*, 2017; Dal Bó *et al.*, 2018; Mutz, 2018). Overall, our findings support this view and provide evidence that the prevalence of relative economic deprivation and economic inequality is an important driver of political polarization and electoral success of radical parties, which may ultimately threaten democracy and political stability.

How should economic policy respond to the development of economic inequality? The answer should be based on a sound understanding of the key factors driving economic inequality trends. Clearly, various factors are likely to play a role. These include skill biased technological change, economic reforms such as deregulation in financial markets, rolling back the welfare state or reforms of the tax system, the growing role of the mass media, and many more. Several pundits, however, emphasize the role of economic globalization for the relationship of inequality and political polarization (e.g., Autor *et al.*, 2020; Dippel *et al.*, 2018; Algan *et al.*, 2017; Malgouyres, 2017).

Populists around the world pick up the anti-globalization argument and link the fight against inequality and for social justice with the fight against globalization and trade openness. That way, populist politicians try to appeal voters who feel themselves as losers from economic globalization (see Mudde, 2007; Acemoglu *et al.*, 2013; Inglehart and Norris, 2017). US president Donald Trump, for example, pursues protectionism and claims that his "America First" policy makes America – and thus the relative economic losers – great again. In a similar vein, advocates of UK's exit from the EU as well as protest movements against the Transatlantic Trade and Investment Partnership (TTIP) during the last decade were also influenced by the perception that gains from international trade and globalization are distributed unevenly.

An important question therefore is whether trade openness increases economic inequality. Some studies use microdata within single countries and show how trade shocks affect incomes and jobs in specific industries and regions. Autor *et al.* (2013), for example, find how import competition from China has destroyed jobs for medium and low skilled manufacturing workers and thus contributed to the rise of economic inequality in the United States. By contrast, Dauth *et al.* (2014, 2017) show that Germany's trade integration retained or even increased total manufacturing jobs. These studies are useful to investigate effects within single countries and to understand causal mechanisms, but cannot predict external validity to the overall effect of trade openness on income inequality.

**Chapter 3** therefore examines how trade openness influences income inequality within countries by using a sample of 139 countries over the period 1970-2014. In joint work with Clemens Fuest and Niklas Potrafke (Dorn *et al.*, 2021a, 2018), we show that the effect of trade openness on income inequality differs across countries. Our findings suggest that trade openness tends to disproportionately benefit relative income shares of the poor in emerging and developing economies, and income shares of higher income groups in advanced economies. The positive effect of trade openness on income inequality in the sample of advanced economies, however, is driven by outliers.

We, moreover, find a strong effect of trade openness on income inequality within transition countries including Eastern Europe and China. These countries have experienced a particularly fast trade integration process while public income insurance and labor market institutions in these countries were less developed than in several advanced welfare states in the rest of the world — particularly Western Europe. Overall, we conclude that the effect of trade openness on income inequality seems to be context specific and may also depend on the institutional framework. The widespread view in the public debate that trade openness has adverse effects on economic inequality needs to be reconsidered.

## Economic policy and regional development

Economic inequality may also increase because of growing regional disparities within countries. Regional disparities can increase if, for example, economic crisis, trade shocks or structural and technological changes affect regions differently. Trade integration may likewise increase employment and incomes in regions specialized on export-oriented industries, but may destroy import-competing industries and jobs in other regions (see Dauth *et al.*, 2014). In a similar vein, climate change policies may increase employment in regions specialized on new technologies, but destroy fossil-specialized industries (e.g., coal production) and jobs in often otherwise structural weaker regions (see Oei *et al.*, 2020; Pittel and Ragnitz, 2019). Municipalities in rural areas are also concerned about adverse economic effects of closures or relocation of large plants or military bases.[3] Regional economic and structural decline may well translate into perceived economic threat, and fear of social decline. If citizens in relative poorer regions feel left behind, growing regional disparities might trigger support for populists and radical parties.

Governments implement economic policies to help regions lagging behind, and to reduce regional disparities. The European Commission, for example, has spent 355 billion Euros for European regional development and cohesion policies in the EU 2020 strategy, which was about $1/3$ of the overall EU budget in the period 2014-2020 (European Commission, 2020).[4] Regional policies often target on public infrastructure investments. Since reunification, Germany has spent about 250-300 billion Euros in the infrastructure of East German regions to pursue regional cohesion (Böick and Lorke, 2019).[5]

Transportation infrastructure plays an important role in infrastructure policies.[6] Scholars have shown how transportation infrastructure increases regional economic development (see Ahlfeldt and Feddersen, 2018; Donaldson, 2018; Donaldson and Hornbeck, 2016; Duranton and Turner, 2012; Gibbons *et al.*, 2019). Investments in roads, railroads and airports connect regions and reduce transportation costs, and help to attract new businesses, production plants, jobs and tourists. Tourism, in particular, can be an important cross-sector industry for many rural and structural weaker regions (BMWi 2017; StMWi 2019). When transport infrastructure facilitates convenient and low-cost journeys, tourists may well travel to rural regions and endorse economic development by local expenditures on accommodation, amenities, restaurants, or amusement parks etc.

**Chapter 4** contributes to the discussion on how transport infrastructure policies affect regional economic development. In joint work with Luisa Dörr, Stefanie Gäbler, and Niklas Potrafke (Doerr *et al.*, 2020), we examine how new airport infrastructure influences tourism in a rural region in the German state of Bavaria. Our identification is based on the conversion of a military airbase into the regional commercial airport Memmingen. The airbase was closed by a decision of the federal government in 2003, which was exogenous to the regional touristic sector. The converted airport opened in 2007 and promotes travelling to the touristic region Allgäu close

[3] See Augsburger Allgemeine (2020): "Trotz und Trauer wegen Trump: Kommunen fürchten Abzug von US-Soldaten." BR (2020): "US-Truppenabzug: ‚Schock' für Zivilbeschäftigte in der Oberpfalz." The Mayor (2020): "German communities face economic uncertainty amid US troop withdrawal." Scholars discuss the regional economic impact of large plant and military base closures (e.g., Hooker and Knetter, 2001; Calia *et al.*, 2020; Paloyo *et al.*, 2010; Jofre-Monseny *et al.*, 2017).

[4] The EU Commission's regional policy strategy aims at supporting job creation, business competitiveness, economic growth, sustainable development, and improving citizens' quality of life.

[5] Ensuring equivalent living conditions plays an important political guideline in Germany and is established by constitutional law (see Art 72 Abs 2 GG, *Grundgesetz*). Guidelines of the "Regional Planning Act" (*Raumordnungsgesetz*, ROG) state that policies should provide "a long-term competitive and spatially balanced economic structure and business-oriented infrastructure as well as (...) a sufficient (...) supply of jobs" (§2 Abs. 2 Nr. 4 ROG).

[6] See Gaebler (2020) for a related discussion on the relevance of infrastructure policies for regional development.

to the Bavarian mountains and the city of Munich. We show that the new commercial airport increased tourism in the Allgäu region over the period 2008-2016. The positive effect is driven by attracting new tourists from abroad and especially pronounced in the county where the airport is located. In combination with a passenger survey of incoming passengers conducted at the new regional airport — covering expenditures, place of stay, etc. — our findings show a positive example how airport infrastructure can promote regional economic development. We also contribute to the discussion on preconditions for a sustainable conversion of military airbases into commercial airports (e.g., Cidell, 2003; Die Zeit, 2020).

## Institutions and public-sector performance

Economic development and regional disparities are also related to public-sector performance and sustainable public finances. A prosperous economic development increases the taxable capacity and thus the public budget for expenditures. Public expenditure and an efficient use of public resources can, in turn, ensure sustainable economic development. Chapters 5 and 6 contribute to the discussion on the role of fiscal and political institutions for a sustainable economic, social, and political development.

Fiscal and democratic institutions which increase accountability of politicians and encourage political involvement of citizens are expected to enhance governments' efficiency in the use of public resources (e.g., Ostrom and Ostrom, 1971). Populist and radical parties, however, are used to criticize the political order and argue that established parties do not act in the interest of the citizens. Populists describe themselves as anti-establishment and representatives of the silent majority. They emphasize the antagonism between the pure and ordinary people and the dishonest and corrupt elite of the established political order (Mudde, 2007; Acemoglu *et al.*, 2013; Inglehart and Norris, 2017). Voters' feelings of betrayal by established parties and politicians may well give rise to the political support of populists (see Di Tella and Rotemberg, 2018; Krause and Méndez, 2009).

Do established politicians systematically manipulate and confirm the narrative of populists? The theory on electoral cycles suggests that incumbent politicians seeking reelection manipulate economic policies and increase voter-friendly spending before elections (Nordhaus, 1975; Rogoff, 1990; Rogoff and Sibert, 1988). Clearly, election-motivated politicians are expected to allocate public resources in a manner to gain electoral advantage. It is, however, a pending question whether this allocation comes at the cost of wasteful spending and gives rise to welfare losses.

In **Chapter 5**, I examine whether elections influence the efficiency of governments in the provision of public goods and services (Dorn, 2021). I likewise contribute to the understanding of democratic institutions (in particular elections), its role for pubic-sector performance, and whether reelection incentives give rise to (in)efficient policies. I use a panel of more than 2,000 municipalities in the German state of Bavaria over the period 2007-2017 and examine the effect of electoral cycles on efficiency at the local government level. My findings do not suggest that incumbents increase public spending in a wasteful manner before elections. By contrast, electoral cycles rather increase cost efficiency in election and pre-election years.

My results challenge the literature on electoral cycles and give rise to the question on institutional preconditions. Governance at the municipal layer in Bavaria is characterized by direct elections, a high decentralization of responsibilities, great fiscal autonomy, balanced budget rules, and high transparency. Empirical evidence suggests that these institutional characteristics endorse government accountability, efficiency, and fiscal performance (e.g., Barankay and Lockwood, 2007; Benito and Bastida, 2009; Alt and Lassen, 2006a,b; Asatryan *et al.*, 2018; Akhmedov and Zhuravskaya, 2004; Guillamón and Cuadrado-Ballesteros, 2020; Seabright, 1996).

Many pundits argue that an enforcement of fiscal transparency by stricter audit and financial reporting requirements would improve accountability, efficiency, and fiscal performance of decision makers (e.g., Brender, 2003). International organizations such as the OECD, the International Monetary Fund (IMF), and the European Union (EU) have encouraged governments to switch from traditional cash-based to – business-like – public-sector accrual accounting, on the presumption that long-run benefits from higher fiscal transparency may outweigh implementation and operating costs.

**Chapter 6** evaluates whether switching accounting standards pays off. In joint work with Stefanie Gäbler and Felix Rösel (Dorn *et al.*, 2021b), we use a quasi-experimental setting at the county-level in the German state of Bavaria to examine whether changing public sector accounting standards influences budgeting, accountability, or government efficiency. We investigate the 96 Bavarian counties over the period 1995 to 2016. Our results do not find that county governments perform better after switching to accrual accounting. By contrast, costs to run the administration increase under accrual accounting. Not all improvements in financial reporting and fiscal transparency seem to directly map into a positive benefit-cost-ratio.

## Empirical methods

Policy implications should be based on a sound understanding of causal links. Economists therefore aim at identifying causal effects of a treatment variable (e.g., a policy change) on a dependent variable (e.g., income inequality). The key for causal inference is to control for any confounding factor which may otherwise bias the estimated relationship of interest. The gold standard clearly would be an experimental setting with randomized control trials (RCT) of treated and non-treated groups, for example by a policy measure (see Angrist and Pischke, 2009, pp. 11 ff.). In the absence of an experimental RCT environment, however, estimating causal effects is challenging in economics and social sciences.

I employ quantitative empirical methods in large panel data sets to estimate size and direction of effects. In all chapters, I use ordinary-least-squares (OLS) estimation approaches and include many observable confounding factors as control variables, that is, factors which may influence both the dependent variable and the treatment variable. Omitted confounding factors in regression models give rise to biased estimates. The panel data – that is repeated observations on the same unit (country, county or municipality) that I employ in the individual chapters of my dissertation – allow to control for unobserved but time-invariant omitted variables. Year fixed effects, moreover, control for shocks which affect all units simultaneously in a year, for example economic crisis, epidemics, or policy changes of higher governmental layers. Panel fixed effects models would produce parallel worlds and allow to interpret the coefficient in a causal way in case we can assume that unobservables are not time-variant and when no further endogeneity issues arise (see Angrist and Pischke, 2009, pp. 221 ff.).

Several sources of endogeneity may, however, still lead to an over- or underestimation of the "true" causal effect in OLS regression models. First, all chapters examine relationships in real world settings. Further time-varying unobservable variables or model misspecifications can therefore be hardly excluded and may yield biased estimates. Second, measurement errors may cause biased estimates when the treatment variable is measured with error and is therefore correlated with the error term of the regression model. Finally, reverse causality may occur and give rise to false interpretations regarding size and direction of effects. For example, Chapter 3 investigates the effect of trade openness on income inequality. Changes in income inequality, however, likely influence elections and government policies which may also affect trade openness.

I employ further state-of-the-art empirical techniques to address endogeneity concerns and to infer causal effects. In Chapters 2, 3, and 5, I employ instrumental variables in two-stage-least squares (2SLS) estimation approaches (see Angrist and Pischke, 2009, pp. 113 ff.). Strong and valid instruments use external information (*not part of the original model*) to explain variation in the treatment variable, but are unrelated to the dependent variable. Instruments can thus produce unbiased results by fulfilling the identifying assumptions to be relevant and exogenous. Chapter 2 employs an instrument for county-specific economic deprivation measures that only captures changes that are driven by national trends — the instrument is by design not affected by any county-specific development. Chapter 3 uses predicted openness based on a gravity equation using a time-varying interaction of geography and exogenous large-scale natural disasters as an instrument for trade openness. In Chapter 5, I employ pension eligibility rules, which are set exogenously by state law, to instrument the politicians' decision to run for office.

Chapters 4, 5, and 6 employ difference-in-differences, synthetic control, and event study methods. These methods are useful techniques to examine effects of (exogenous) policy treatments given a common trends assumption (see Angrist and Pischke, 2009, pp. 227 ff.). The identifying common trends assumption states that the treated units (e.g., counties or municipalities) would have evolved in the same manner as their non-treated counterfactuals (control group) in a hypothetical world without policy change. Effects can be interpreted in a causal way if the policy treatment causes deviations from the common trend — given that selection into treatment can be controlled or excluded. Chapter 4 uses an exogenous positive infrastructure shock as policy treatment for the regional tourism sector. In Chapter 5, I use local election dates as treatment which are set exogenously by state law, and Chapter 6 exploits the gradual and partial shift in accounting standards at the county level after an exogenously stated new state law.

All chapters thoroughly discuss potential endogeneity problems, empirical identification strategies, and remaining caveats regarding causal interpretation.

# 2 Economic deprivation and radical voting: Evidence from Germany[1]

## Abstract

This chapter studies the impact of economic deprivation on radical voting. Using instrumental variable estimation and a unique dataset covering different indicators of economic deprivation as well as federal election outcomes at the county-level in Germany for the period 1998–2017, we examine whether economic deprivation influences the share of votes for radical right and left-wing parties. Our results suggest that an increase in economic deprivation has a sizeable effect on the support for radical parties at both ends of the political spectrum. The higher a county's rate of relative poverty, and the average shortfall from the national poverty line or median income, the higher the vote share of radical right-wing and left-wing parties. We also provide evidence that regional variation in economic deprivation gave rise to the electoral success of the populist right-wing party AfD in the federal election of 2017. Our findings thus indicate that a rise in relative economic deprivation may undermine moderate political forces and be a threat to political stability.

[1] This chapter is joint work with Clemens Fuest, Lea Immel, and Florian Neumeier. It is based on our paper "Economic deprivation and radical voting: Evidence from Germany", *ifo Working Paper* No. 336, 2020.
We thank participants of the meeting of the European Public Choice Society (EPCS) in Rome (2018), the annual congress of the International Institute of Public Finance (IIPF) in Tampere (2018), the annual conference of the German Economic Association (*Verein für Socialpolitik*, VfS) in Freiburg (2018), the PEARL workshop – Public Economics at the Regional and Local Level – in Zermatt (2018), a conference on regional inequalities held at the University of Marburg (2018), the MACIE research seminar at the University of Marburg (2018), the Doctoral workshop of the Hanns-Seidel-Foundation at the Banz monastery (2019), the annual congress of the European Economic Association (EEA) in Manchester (2019), the Canadian Economics Association (CEA) annual conference in Banff (2019), and the meeting of the Society for the Study of Economic Inequality (ECINEQ) in Paris (2019) for helpful comments.

## 2.1 Introduction

Over the past decades, economic inequality as well as the share of people suffering from (relative) economic deprivation has increased in many industrialized countries. This trend has not only spurred research into the underlying causes and economic consequences, but also triggered heated public debates about its political and social implications. One of the major concerns is that the rise in economic deprivation jeopardizes social cohesion and nourishes radical and populist political movements. The economic pressure experienced by certain groups in society is widely believed to fuel resentment against mainstream political parties as well as the political order itself. Many pundits link the increase in economic deprivation to the emergence of populist movements and the surge in public support for radical parties in Europe and other parts of the world: *Syriza* in Greece, *Podemos* in Spain, *MoVimento-5-Stelle* (5-Star-Movement) and *Lega* in Italy, *Front National* in France, *Fidesz* in Hungary, the *Sverigedemokraterna* (Sweden Democrats) in Sweden, or the *Alternative für Deutschland* (Alternative for Germany; AfD) in Germany are only a few examples of parties at the far left and far right of the political spectrum that capitalize on growing economic insecurity and deprivation. Moreover, the rise in economic deprivation is believed to be one of the major sources of what has been labelled neo-nationalism — a political leaning that promotes nativism, opposition to immigration, and protectionism.

The available empirical evidence suggests that, in general, economic deprivation and support for radical views and parties are indeed correlated. Evidence on the causal relationship is scarce, though. The chapter contributes to the literature by examining the causal effect of economic deprivation on support for radical parties in Germany. We exploit regional variation in election outcomes as well as the prevalence and intensity of economic deprivation. More precisely, we estimate regressions linking the share of radical left-wing and right-wing votes to regional indicators of economic deprivation. We measure economic deprivation of regions' citizens relative to the national average (not inequality or relative deprivation within regions). To identify causal effects, we follow Boustan *et al.* (2013) and construct instruments for region-specific measures of economic deprivation that are exogenous to asymmetric economic developments, endogenous political reactions to the rise in the support for radical parties, as well as endogenous sorting of individuals into regions.

Our analysis is conducted at the county-level, corresponding to NUTS-3. In the main part of our analysis we use data for the period from 1998 to 2017. In an extension, we restrict our focus to the federal elections held in 2017 and the vote share of the AfD, which is interesting for at least two reasons. First, the AfD is the first nationalist party represented in the German federal parliament with significant size since World War II. Second, survey evidence indicates that AfD supporters — unlike supporters of other radical right-wing parties in Germany — do not differ in their socioeconomic characteristics from supporters of parties at the center of the political spectrum, like the Christian Democratic Party (CDU) or the Social Democratic Party (SPD), in terms of income, education, or employment status (e.g., Bergmann *et al.*, 2017; Hansen and Olsen, 2019; Goerres *et al.*, 2018).

Germany is particularly well-suited to study the effect of regional economic deprivation on the support for radical parties. The multi-party system in Germany covers parties from the entire political spectrum, including far left-wing and far right-wing parties. Arguably, this constitutes an important advantage over studies that focus on countries where only few parties compete in elections, like the United States (US) or United Kingdom (UK), as it facilitates the measurement of political polarization. Moreover, by using data on election outcomes, we observe the electorate's revealed support for radical parties. This is an advantage over studies that rely on survey data, which only include stated preferences, not real voting behavior.

Our findings suggest that regional economic deprivation has a statistically and economically significant effect on the vote share of radical parties. The higher the intensity of economic deprivation in a county — measured by the average shortfall from the national median income (median gap), the poverty line (poverty gap), as well as the poverty rate — the more successful are radical parties at the polls. For instance, if the poverty gap (median gap) increases by one percentage point (pp), the share of radical right-wing party votes rises, on average, by 1.2 (0.7) pp. This effect is even more pronounced when focusing on the AfD votes at the 2017 federal election. Here, a one pp increase in the poverty gap (median gap) leads to a rise in the AfD vote share by 4.9 (1.9) pp. This effect is more pronounced in East Germany compared to West Germany. Our results thus indicate that prevalence of economic deprivation is an important determinant of the electoral success of radical right-wing parties in Germany. In contrast, our results for radical left-wing parties are more ambiguous in that they are sensitive to the definition of radical parties, and whether East or West German counties are examined.

How can these results be reconciled with the observation from survey evidence that AfD voters are not poorer, on average, than other voters (Hansen and Olsen, 2019; Bergmann *et al.*, 2017; Goerres *et al.*, 2018)? An explanation is that middle or even upper class voters in counties with a high degree of deprivation vote for AfD because they perceive higher economic threat and fear for their status, not because they are poor (see Dal Bó *et al.*, 2018; Mutz, 2018; Goerres *et al.*, 2018).

## 2.2 Related literature, hypotheses, and contribution

### 2.2.1 The economics of radical voting

Economic conditions matter at the polls. In fact, among the various determinants of voting behavior scholars have been analyzing, economic circumstances are typically considered to be among the most important ones (e.g. Fair, 1978; Lewis-Beck, 1990; Lewis-Beck and Stegmaier, 2000, 2013). Consequently, in an attempt to explain the increase in political polarization as well as the rising support for radical parties — especially nationalist ones — various Western countries have been experiencing over the past few years, many scholars focus on economic factors. Recent empirical studies have linked the rise in political radicalism and nationalist (including anti-immigration) sentiments to major macroeconomic trends and events, particularly economic globalization and its adverse consequences (Malgouyres, 2017; Colantone and Stanig, 2018; Dippel *et al.*, 2018; Autor *et al.*, 2020), growing economic insecurity (Algan *et al.*, 2017; Dal Bó *et al.*, 2018; Guiso *et al.*, 2017), the economic strains resulting from the financial and economic crisis (Mian *et al.*, 2014; Funke *et al.*, 2016), as well as rising economic inequality (Duca and Saving, 2016; Garand, 2010; Jesuit *et al.*, 2009; McCarty *et al.*, 2016; Voorheis *et al.*, 2015; Winkler, 2019).[2]

Most approaches linking radical voting to inequality and economic deprivation emphasize the importance of *relative deprivation*. The concept of relative deprivation suggests that individual support for radical (political) views results from an unfavorable comparison with other members of society (Runciman, 1966; Runciman and Bagley, 1969). Plainly speaking, people tend to be more concerned about their relative standing in a society's income distribution than their absolute level of income. An unfavorable social comparison or the fear of social decline are believed to trigger feelings of anxiety and frustration — people are convinced that they are not getting what they are entitled to. Those feelings, in turn, may foster resentments against the political mainstream as well as the political system itself (Algan *et al.*, 2017; Dal Bó *et al.*, 2018; Mutz, 2018). An inclination toward such sentiments seems to make the economically deprived particularly responsive to the messages of radical political parties and movements. Radical and populist politicians try to appeal to voters experiencing relative economic deprivation by posing as their advocates and discrediting mainstream political parties and political institutions (Mudde, 2007).

The traditional view is that economic deprivation translates into greater support for left-wing parties as they advocate redistributive policies and cater to the needs of those at the bottom of the income distribution (Romer, 1975; Meltzer and Richard, 1981). However, recent studies point out that economic deprivation can increase the popularity of right-wing parties as well. Aggeborn and Persson (2017) develop a theoretical model to explain why low-income voters are prone to support right-wing (populist) parties. They argue that low-income voters are particularly vulnerable to economic insecurity and depend more heavily on basic public services. In contrast to left-wing parties, right-wing parties oppose spending on global goods such as generous refugee support systems, foreign aid, and environmental protection in favor of basic public services that mainly benefit the domestic population.

Other scholars emphasize that in a highly globalized world, the welfare state is constrained in its ability to redistribute resources and to raise taxes due to the danger of capital flight (Antràs *et al.*, 2017; Sinn, 2003). When redistribution becomes prohibitively costly, protectionist views

[2] A related literature strand links economic strain to anti-immigrations sentiments as well as right-wing extremist crime. See, for example, Becker *et al.* (2017), Guiso *et al.* (2017), Davis and Deole (2015), Billiet *et al.* (2014), Falk *et al.* (2011), Facchini and Mayda (2009), and Mayda (2006).

and hostile attitudes toward globalization may become particularly popular among voters suffering from economic deprivation. As Colantone and Stanig (2018, p.3) put it: "As the losers (of globalization; authors' note) realize that effective redistribution policies are not feasible, the demand for protection emerges as an alternative. This breeds the success of economic nationalism." Consequently, in a country that is highly integrated into the world economy, radical right-wing parties may have a particularly great appeal to voters suffering from economic deprivation.

### 2.2.2 Empirical evidence on the association between deprivation and polarization

Existing empirical evidence appears to support the conjecture that indicators related to economic deprivation such as unemployment, a low income level, and economic inequality are positively related to political polarization and the support for radical parties.[3] Duca and Saving (2016), Garand (2010), and McCarty *et al.* (2016) for the US, Guiso *et al.* (2017) and Jesuit *et al.* (2009) for samples of European countries, Lubbers and Scheepers (2001) and Garmann and Potrafke (2019) for Germany, as well as Dal Bó *et al.* (2018) and Rydgren and Ruth (2011) for Sweden are just a few of the studies that document such an empirical relationship.

However, the bulk of the empirical literature analyses statistical correlations or provides survey evidence. Causal evidence on the effect of economic deprivation on political polarization or radical voting is scarce. To the best of our knowledge, the only studies that employ a credible identification strategy to estimate the causal impact of indicators of economic deprivation on the support for radical parties and political polarization are Voorheis *et al.* (2015), Algan *et al.* (2017), and Winkler (2019).

Voorheis *et al.* (2015) and Winkler (2019) adopt the instrumental variable approach proposed by Boustan *et al.* (2013) that is also used in the present paper and explained in detail below. Voorheis *et al.* (2015) use data on the degree of political polarization in U.S. state legislatures and state-level data on income inequality covering the years from 2005 to 2011. The authors report a positive effect of income inequality on political polarization. Winkler (2019) uses survey data from different European countries aggregated at different NUTS levels covering the period from 2002 and 2014. The evidence he provides suggests that an increase in inequality within a region increases the share of people supporting extreme left-wing parties. In contrast, an increase in inequality increases the support for extreme right-wing parties only among older voters. Algan *et al.* (2017) use data from European countries at the NUTS-2 level for the period from 2000 to 2016 and examine the effect of crises-driven increases in regional unemployment on vote shares for anti-establishment parties. The authors use regional variation in the pre-

[3] Some scholars argue that unemployed people, lower skilled workers and the 'old middle class' are particularly affected by economic insecurity and perceptions of relative economic deprivation (Rydgren, 2007; Dal Bó *et al.*, 2018; Inglehart and Norris, 2017).

crisis share of real estate and housing construction as instrument for regional unemployment. Their estimates suggest that a crisis-induced rise in unemployment increases vote shares of anti-establishment parties, especially populist ones.

Our paper contributes in several ways. First, by focusing on German counties (corresponding to the NUTS-3 level), this paper uses data collected at a much more granular regional level than the literature cited above. In Germany, there are currently more than 400 counties with, on average, roughly 170,000 inhabitants. Exploiting variation at such a highly disaggregated regional level increases both our sample size as well as the variation in our measures of economic deprivation and, thus, the power of the statistical tests we perform. Second, most of the studies listed above use survey data to study the association between economic deprivation and political polarization. In contrast, we assess the support for radical parties using data on election outcomes and, thus, capture the electorate's revealed (and not stated) political preferences. Third, many studies use data from the US. Due to its two-party system, it is rather tedious to measure the degree of political polarization in the US. The multi-party system in Germany covers parties from the entire political spectrum, including parties at the far right and the far left. This facilitates the measurement of political polarization.[4] Fourth, our sample period covers two decades and, thus, a considerably larger time span than the studies discussed above. This is particularly important because the degree of economic deprivation typically changes only slowly over time. Finally, in our empirical analysis, we employ different measures of regional economic deprivation, that is, the poverty rate, the poverty gap, as well as the median gap, which has not been done before.

## 2.3 Data

To study the influence of economic deprivation on electoral outcomes, we construct a unique panel dataset covering more than 400 counties in Germany. Our dataset combines county-specific measures of economic deprivation and outcomes of federal elections that took place between 1998 and 2017. During this period, federal elections were held six times; in 1998, 2002, 2005, 2009, 2013, and 2017. Due to territorial reforms, the number of counties varies across our sample period. Therefore, our panel dataset is slightly unbalanced. To construct our variables of main interest, we mainly rely on two sources. Regional measures of economic deprivation are constructed based on microdata from the German Microcensus (*Mikrozensus*). Federal election outcomes at the county-level are provided by the Federal Returning Officer (*Bundeswahlleiter*).

[4] Studies with a focus on the US typically rely on DW-nominate scores to measure the degree political polarization within US politics. DW-nominate scores represent measures of the distance between legislators. These scores indicate how similar or different, respectively, the voting records of legislators are. DW-nominate scores are not without criticism. Only recently, the political science journal *Studies in American Political Development* has devoted a special issue on the advantages and disadvantaged of the DW-nominate scores. See *Studies in American Political Development*, Vol. 30, Issue 2, 2016.

### 2.3.1 The German microcensus

The Microcensus is a household survey carried out annually since 1957 by the statistical offices of the German states (*Statistische Landesämter*) and administered by the Federal Statistical Office (*Statistisches Bundesamt*). It comprises a representative one percent-sample of the German population, resulting in a sample size of more than 800,000 persons in almost 400,000 households per year. The sample is representative at the regional level. The Microcensus contains information on various demographic characteristics, including the county of residence, employment status, household size, the age of all household members, and household income. For our analysis, we use the waves from 1991 to 2017.

Besides the large number of variables, one major advantage of the Microcensus is its large sample size, which allows us to construct indicators of economic deprivation at the regional level. Moreover, the Microcensus is administered by a federal agency and there is a legal obligation to answer the questions. Hence, item-non-response is not an issue. Also, answers must be truthful and complete. This makes the Microcensus well-suited to study economic deprivation at the county-level in Germany.

To construct our measures of economic deprivation, we use information on monthly net household income. To account for differences in household size, we compute equivalized household incomes using the OECD equivalence scale. In addition, we adjust the income figures for changes in prices using the consumer price index for Germany. Note that the income variable in the Microcensus dataset is interval-censored, i.e., respondents are asked to indicate in which income class they are. However, the width of the income classes are rather narrow and the number of income classes is large, varying between 18 and 24, depending on the survey year. To obtain continuous household income figures, we apply an imputation approach. We estimate a continuous income figure for each household based on information on a household's income class as well as various socio-demographic characteristics using interval regressions. This imputation technique ensures that the empirical distribution of the continuous income variable fits the shape of the distribution of the income classes and that the income figure computed for each household lies within the borders of the income household's income class (see Royston, 2007).

### 2.3.2 Indicators of economic deprivation

A large literature suggests that concerns about personal economic well-being determine preferences for redistribution and protectionism and thereby voting behavior (Section 2.2). When focusing on federal elections, we thus expect that an individual's position in the national income distribution is decisive for her vote. This implies that a regionally aggregated measure of economic deprivation should indicate how residents residing in a county compare to the national average.

In our empirical analysis, we employ three different indicators of economic deprivation that account for the relative economic well-being of a county's citizens compared to the national average. Our first indicator is the poverty rate, i.e., the share of households in a county with an income below the national poverty line $z^{nat}_{pov,t}$. As it is common, we set the poverty line equal to 60 percent of the national median income $z^{nat}_{50,t}$, so that $z^{nat}_{pov,t} = 0.6 \times z^{nat}_{50,t}$.

Our second indicator of economic deprivation is the poverty gap, which is defined as the average shortfall from the national poverty line:

$$\textit{Poverty gap}_{it} = 100 \frac{1}{n_{i,t}} \sum_{j=1}^{q} \frac{z^{nat}_{pov,t} - y_{ijt}}{z^{nat}_{pov,t}} \tag{2.1}$$

Here, $n_{it}$ is the number of households in county $i$ and year $t$ that are included in the Microcensus data, $q$ is the number of households with an income below the poverty line, and $y_{ijt}$ is the income of household $j$.

Our third measure of relative economic deprivation is constructed in a similar fashion, but measures the average shortfall from the national median income (instead of the poverty line). We refer to this measure as the median gap. It is constructed as follows:

$$\textit{Median gap}_{it} = 100 \frac{1}{n_{it}} \sum_{j=1}^{r} \frac{z^{nat}_{50,t} - y_{ijt}}{z^{nat}_{50,t}} \tag{2.2}$$

$r$ refers to the number of households in a county with an income below the national median income, while the other variables in equation (2.2) are defined as above.

### 2.3.3 The German electoral system and the definition of radical parties

The electoral system in Germany is based on proportional representation and multiple parties run for elections. Since those parties cover the entire political spectrum from the far left to the far right, Germany is a particularly interesting country to study the association between economic deprivation and support for radical parties. At federal elections in Germany, voters have two votes: The first vote (*Erststimme*) is for a local candidate whom voters would like to see in parliament, the second vote (*Zweitstimme*) is for one of the political parties running for election.[5] In our analysis, we focus on the second votes since they determine the number of seats parties receive in parliament, provided a party passes the five percent election threshold.[6]

We are mainly interested in the vote shares of radical left-wing and radical right-wing parties in the federal elections held between 1998 and 2017. We consider parties to be radical in case the party or a subgroup of party members have been under surveillance of the German Federal Office for the Protection of the Constitution (*Bundesverfassungsschutz*) or its state-level equivalents (*Landesverfassungsschutz*).[7] Parties or party members are put under surveillance if they impose an imminent threat to the free democratic basic order. Table 2.1 provides a list of parties that we label radical right-wing and radical left-wing, respectively. The marks indicate in which federal elections the parties ran.

Our list of radical left-wing parties includes five parties. The Left Party (*Die Linke*), which was founded in 2007 when the Party of Democratic Socialism (PDS)[8] and the Electoral Alternative for Labour and Social Justice (WASG) merged, is the most popular leftist party in Germany and regularly represented in the German federal parliament (*Deutscher Bundestag*).[9] Besides the Left Party (*Die Linke*), there are several small radical left-wing parties, but none of those has ever passed the five percent election threshold during our sample period. Small radical parties on the far left are communist parties such as the German Communist Party (DKP), the Communist Party of Germany (KPD), the Marxist-Leninist Party of Germany (MLPD), and the Trotzkyist oriented Party for Socialist Equality (SGP).

On the far right, twelve parties ran in German federal elections since 1998. The populist party Alternative for Germany (*Alternative für Deutschland*, AfD) is the most successful radical right-wing party in Germany since 1945. The AfD started to run for elections in 2013 and entered the European parliament one year later, i.e., in 2014. However, despite its Euro-skepticism, the AfD was not a radical right-wing party in its early years, but rather a conservative, market-liberal party (see Arzheimer, 2015; Schmitt-Beck, 2017). Since 2015, however, the AfD became more and more radical after several leading moderate politicians left the party. The nationalist and radical fraction took over power and clearly favored anti-immigration policies, emphasized

[5] The candidate who receives the majority of first votes in an election district is directly elected to the parliament. The distribution of seats in the parliament is, however, solely determined by the share of second votes a party receives.

[6] Note that the five percent threshold is not binding if a party wins at least three election districts directly by the first vote. In all federal elections in Germany since 1990, this occurred only once in 1994, when four candidates of the leftist Party of Democratic Socialism (PDS) received the majorities of first votes in their election districts. As result, the party got in total 30 seats in parliament, corresponding to its 4.4 percent vote share of second votes.

[7] We also define parties as radical if they cooperate in elections with other parties that are monitored by the German Federal Office for the Protection of the Constitution or its state-level equivalents.

[8] The PDS was founded in 1990 and is the successor of the Socialist Unity Party of Germany (SED), the communist party governing the German Democratic Republic (GDR) between 1949 and 1989.

[9] In the first unified German federal elections in 1990, the Left Party received only 2.4 percent of the second votes. However, the party was represented in the parliament with 17 seats because of a one-time exception that was made for parties that won at least five percent of all votes in the former German Democratic Republic.

Table 2.1 : Radical parties at federal elections in Germany, 1998–2017

| | Federal elections in Germany | | | | | |
|---|---|---|---|---|---|---|
| | 1998 | 2002 | 2005 | 2009 | 2013 | 2017 |
| *Radical right-wing parties* | | | | | | |
| ADM | | | | X | | |
| AfD | | | | | X | X |
| BfB* | X | | | | | |
| Büso | X | X | X | X | X | X |
| Die RECHTE* | | | | | X | X |
| DM | | | | | | X |
| DVU* | X | | | X | | |
| NPD* | X | X | X | X | X | X |
| Pro Deutschland* | | | | | X | |
| REP (Republikaner)* | X | X | X | X | X | |
| Volksabstimmung* | X | | X | X | X | X |
| 50plus | | | X | | | |
| *Radical left-wing parties* | | | | | | |
| Die LINKE (PDS) | X | X | X | X | X | X |
| DKP* | | | | X | | X |
| KPD* | | X | | | | |
| MLPD* | X | | X | X | X | X |
| SGP* | X | | X | X | X | X |

*Notes:* *indicates parties also included in the narrow definition. The narrow definition labels parties in case the party as a whole is under surveillance of the German Office for the Protection of the Constitution.
*Abbr.:* ADM (Allianz der Mitte), AfD (Alternative für Deutschland), BfB (Bund freier Bürger), Büso (Bürgerrechtsbewegung Solidarität), DM (Deutsche Mitte), DVU (Deutsche Volksunion), NPD (Nationaldemokratische Partei Deutschlands), PDS (Partei des Demokratischen Sozialismus), DKP (Deutsche Kommunistische Partei), KPD (Kommunistische Partei Deutschlands), MLPD (Marxistisch-Leninistische Partei Deutschlands), SGP (Sozialistische Gleichheitspartei).

German nationalism, and provoked distrust in the political order. This new radical right-wing party was successful in several state elections held in 2015 and 2016. In 2017, the AfD entered the German federal parliament for the first time. The AfD received a vote share of 12.6 percent and became the third largest party in parliament.

Besides the AfD, there are eleven other radical right-wing parties, the most prominent ones being the National Democratic Party of Germany (NPD), the German People's Union (DVU; merged with NPD in 2011), and the Republicans (REP). While none of these parties was ever represented in the federal parliament, they do have regional strongholds and entered some state parliaments in the past. Moreover, the NPD has won a seat in the European parliament in 2014, after the three percent threshold was removed by the Federal Constitutional Court of Germany. Besides AfD, NPD, DVU, and REP, there is a number of other radical right-wing parties that ran for federal elections during our sample period, such as the nationalist Union of Free Citizens (BfB), the Right Party (*Die Rechte*), Pro Germany (*Pro Deutschland*), the party Popular Referendum (*Volksabstimmung*), and the Civil Rights Movements Solidarity (BüSo).[10]

To test the sensitivity of our results with regard to the definition of radical parties, we also employ a narrow definition. In the narrow definition, we only label a party radical in case the party as a whole is under surveillance of the Office for the Protection of the Constitution. This reduces the number of radical right-wing parties from twelve to seven and the number of radical left-wing parties from five to four. Note that the two largest radical parties, i.e., the Left Party (*Die Linke*) and the AfD, are excluded from the narrow definition.

As a further robustness test, we also estimate the impact of relative economic deprivation on the vote shares of established parties. Our definition of established parties includes the Social Democratic Party (SPD), the Green Party (*Bündnis90/Die Grünen*), the Christian Democratic Party (CDU/CSU), and the Free Democratic Party (FDP). During our sample period, each of these four parties was a coalition member of the federal government for at least one legislative period.

### 2.3.4 Control variables

In our empirical analysis, we include several control variables describing the demographic and economic situation in a county. We control for the population share of different age groups, population density, the unemployment rate, the share of recipients of social transfers, the shares of graduates from different schooling tracks (no degree (reference category), lower secondary degree (*Hauptschule*), intermediate secondary degree (*Realschule*), higher secondary degree (*Gymnasium*)), and the share of foreigners. Population density figures are provided by the Federal Institute for Research on Building, Urban Affairs and Spatial Developments (*Bundesinstitut für Bau , Stadt-, und Raumforschung, BBSR*). The share of foreigners is taken from the German Regional Database (*Regionaldatenbank Deutschland*) as well as the statistical offices of the German states (*Statistische Landesämter*). Information on school graduates comes from the Federal Statistical Office (*Statistisches Bundesamt*). The remaining control variables are calculated based on individual responses from the German Microcensus (see Section 2.3.1).

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[10] Note that many scholars studying right-wing extremism in Germany only include the AfD, NPD, DVU, and REP to their lists of radical right-wing parties, as they are the largest ones.

## 2.4 Descriptive statistics

### 2.4.1 Regional variation in economic deprivation

Figure 2.1 illustrates how the average realizations of the economic deprivation indicators developed over the past 20 years. Between 1998 and 2017, the average degree of relative economic deprivation at the county-level in Germany increased slightly. The share of households with an income below the poverty line grew from 14.7 percent in 1998 to 16.7 percent in 2017. Similarly, the average shortfall from the poverty line (median income), that is, the poverty gap (median gap), rose from 3.8 (15.3) percent to 4.1 (15.8) percent.

Figure 2.1 : Economic deprivation over time

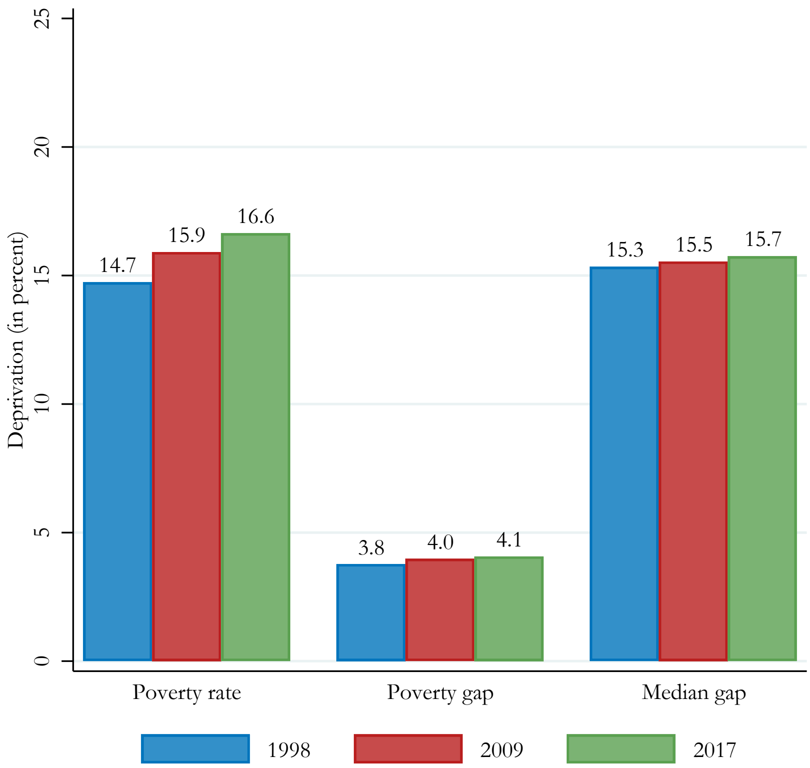
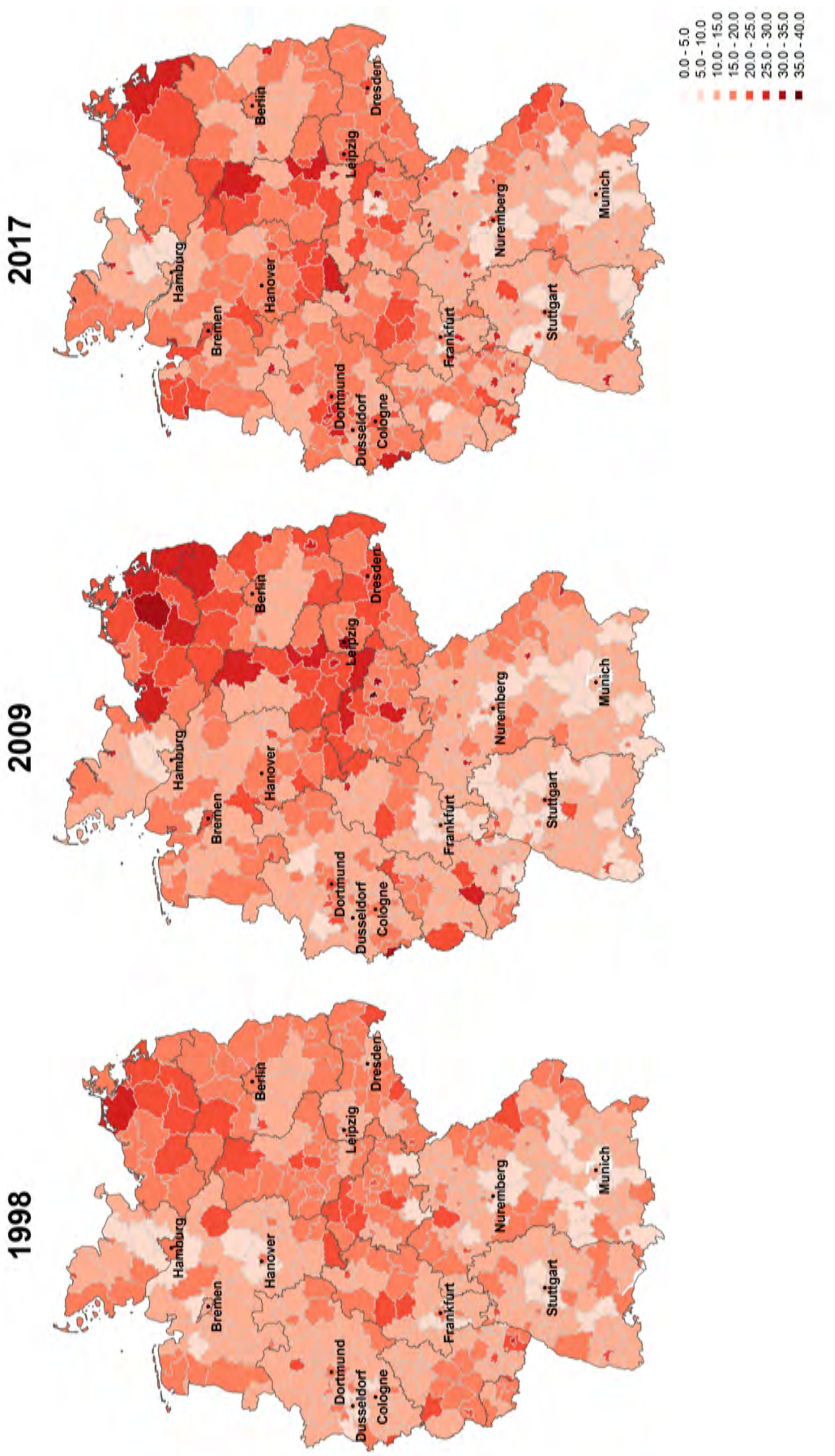



Figure 2.2 shows the realizations of the poverty rate in 1998, 2009, and 2017 at the county-level. The figure reveals that the extent of economic deprivation varies considerably across regions. Particularly pronounced are the differences between West and East German counties as well as between North and South. Interestingly, it appears that the differences between West and East Germany became smaller over time, while the North/South divide grew.

Figure 2.2 : Poverty rate of German counties between 1998 and 2017

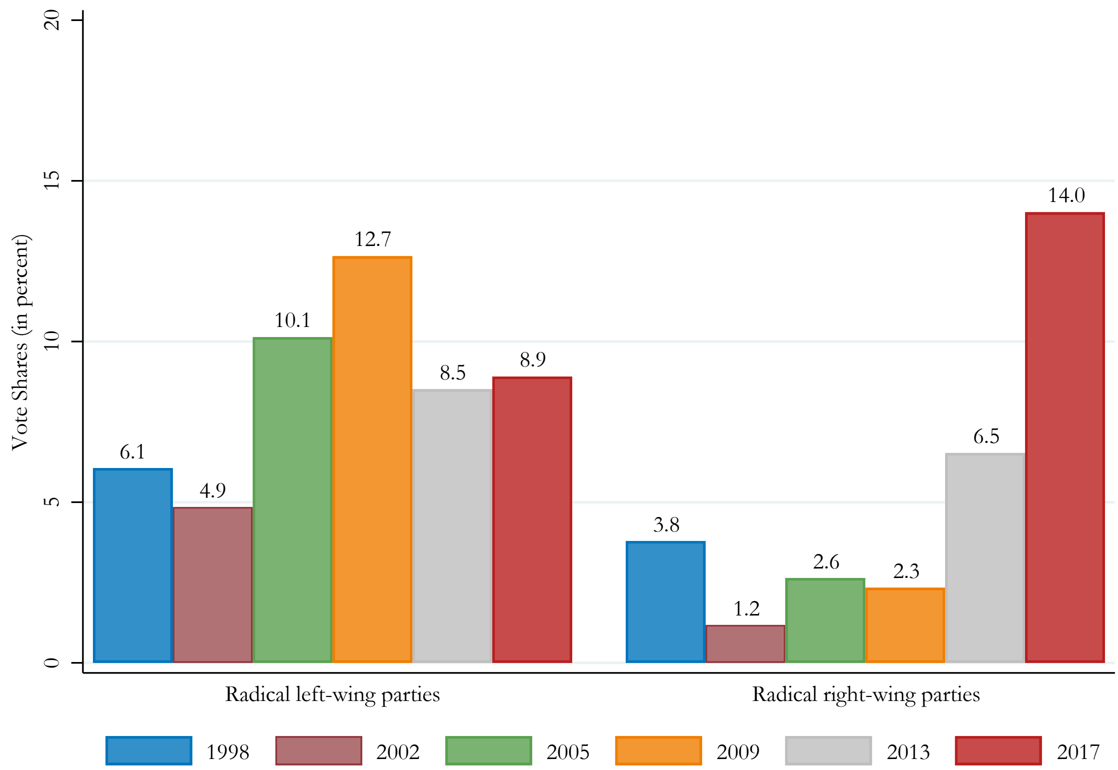



*Notes:* This figure shows the poverty rate across counties for 1998, 2009, and 2017. The poverty rate is measured in percent.

### 2.4.2 Support for radical parties

Figure 2.3 shows the average vote shares of radical right-wing and left-wing parties at the federal elections held between 1998 and 2017. Until 2017, radical left-wing parties have consistently been more successful at the polls than radical right-wing parties. This is mainly due to the popularity of the socialist Left Party and its predecessor, the PDS, in East Germany, where these parties have managed to always receive roughly one fifth of the votes. Many pundits link the noticeable jump in the average vote share of radical left-wing parties at the 2005 federal election to the so-called Hartz reforms, which led to a liberalization of the German labor market and were implemented by the left-wing coalition government consisting of the SPD and the Green Party. This resulted in many voters turning away from the SPD and Green Party and turning to the Left Party.

Figure 2.3 : Average vote shares in German counties




In 2013, however, there has been a notable rise in the share of votes for radical right-wing parties, which is entirely driven by the success of the newly founded right-wing populist party AfD. The AfD was founded in April 2013 to oppose German federal policies concerning the eurozone crisis and just missed the five percent election threshold in 2013. In 2017, the AfD received 12.6 percent of the votes and became the third-largest party in the federal parliament, having completed the turn from a Eurosceptical conservative party to a radical right-wing party favoring anti-immigration policies.

Whereas in 1998 the combined county vote shares of radical right- und left-wing parties was on average 9.9 percent, it more than doubled to 22.9 percent in 2017. These averages conceal substantial differences in voting outcomes between East and West Germany. East German counties exhibit considerably larger vote shares for radical parties. This is not only due to the success of the Left Party (*Die Linke*), but also the AfD enjoys greater popularity in the East than in the West. In 2017, the average vote share of radical left-wing (right-wing) parties was 17.2 (23.4) percent in East German counties and 7.0 (11.8) percent in West German counties (see Appendix, Figure A2.1).

## 2.5 Empirical strategy

To study the association between economic deprivation and support for radical parties, we estimate the following empirical panel data model:

$$Y_{it} = \alpha_i + \beta \, \textit{Deprivation}_{it} + \gamma \, X_{it} + \delta_t + \varepsilon_{it} \tag{2.3}$$

Index $i$ refers to the county and index t to the year of the federal election. Our sample covers six federal elections: 1998, 2002, 2005, 2009, 2013, and 2017. We use two dependent variables in our empirical model (see Section 2.3.3): the vote share of radical right-wing parties and the vote share of radical left-wing parties. $Deprivation_{it}$ is a measure of regional economic deprivation. We consecutively employ three deprivation measures: (i) the poverty rate, (ii) the poverty gap and (iii) the median gap (see Section 2.3.2). The vector $X_{it}$ includes the control variables described in Section 2.3.4 Finally, $\alpha_i$ is a county-fixed effect that is included to account for time-invariant regional-specific factors related to economic conditions and $\delta_t$ is a year-fixed effect included to capture the effect of nation-wide events.

Identifying the causal effect of economic deprivation on voting behavior is challenging since there are several confounding factors that are correlated with both election outcomes and regional economic conditions. First, households may sort into regions depending on their socio-demographic characteristics as well as political preferences. For example, households may prefer to live among people who are similar to them with regard to lifestyle and political views. Spatial segregation of households based on their economic situation may also occur due to regional differences in labor market conditions, housing prices, and costs of living. All those factors could also be related to election outcomes, implying that omitting them from the regression would lead to biased estimates when using OLS to identify the parameters of Equation (2.3). Unfortunately, the data we would need to control for those factors are typically not available at the county-level, and neither are suitable proxy variables. Furthermore, there are a number of regional characteristics that are potentially correlated with both regional economic deprivation and voting behavior such as, for example, factors related to labor supply in a county, household structure, geographic features, etc. While some important variables can be controlled for, we cannot exclude the possibility that there are other relevant variables we cannot observe.

To address concerns regarding biased OLS estimates due to the endogeneity of our covariates, we construct instrument variables for our deprivation measures that are similar to the instrument proposed by Boustan *et al.* (2013). The construction proceeds in four steps. In step one, we compute the average household income for each income percentile of the national income distribution and for all survey years (i.e., 1991-2017). In the second step, we compute percentile-specific annual national income growth rates for each survey year. In step three, we focus on household incomes in a base year, determine to which percentile of the national income distribution each household in that base year belongs, and multiply each household's income with the percentile-specific annual national income growth rates. That way, we obtain a time-series of hypothetical incomes for each household that we observe in the base year. In the final step, we use these hypothetical incomes to compute counterfactual economic deprivation measures which we then use as instruments for the actual realizations of the regional deprivation measures.

The counterfactual deprivation measures indicate how regional economic deprivation would have developed in the absence of inward and outward migration and if each household's income would have changed over time in accordance with the percentile-specific national average. Consequently, our instruments only capture changes in the regional income distribution that are driven by national trends and cannot, by design, be influenced by county-specific trends such as mobility into and out of regions or asymmetric economic and political developments (Boustan *et al.*, 2013). The cross-sectional variation in our instruments stems entirely from the variation in the base year's income distribution, whereas the time-variation comes from the percentile-specific income growth rate at the national level.

The results of the first-stage in the two-stage-least-squares (2SLS) regressions demonstrate that the instruments are highly relevant. The coefficients of all instrumental variables are highly significant with coefficient estimates that are close to unity.[11] The relevance of our instruments is further indicated by the Cragg-Donald F statistics for exclusion restriction tests, which are far larger than the critical values proposed by Stock and Yogo (2005) (see Section 2.6.2).

An additional challenge specific to the use of county-level data in Germany is that the number of counties in East Germany has changed considerably after German unification due to various administrative-territorial reforms. For example, from 1990 to 1996, the number of counties in East Germany (excluding East-Berlin) decreased from 215 to 111. For this reason, we are

[11] Results available on request.

forced to use 1997 as our base year for the construction of our instruments for East German counties. For West Germany, our base year for the construction of the instrumental variables is 1991.

## 2.6 Results

### 2.6.1 Baseline results

We start with the OLS estimation results, which are presented in Table 2.2. The left panel shows the results for radical left-wing parties, the right panel for radical right-wing parties.

The estimates reveal a statistically significant relationship between the level of economic deprivation in a county and the vote share of radical left-wing parties. The estimated effects are of modest size, though. The coefficient estimates suggest that a one percentage point (pp) increase in the poverty rate is associated with an increase in the share of votes for radical left-wing parties of 0.06 pp. In relation to the sample mean, this is equivalent to an increase in the vote share of 0.7 percent. For the poverty gap (median gap), the estimated effect of a one pp increase is 0.15 (0.12) pp, implying a 1.8 (1.5) percent increase in votes compared to the sample mean. In contrast, for radical right-wing parties, we do not detect any significant association between the share of votes these parties receive and our deprivation measures.

A glance at the coefficient estimates of the control variables reveals some interesting findings. An increase in the county's unemployment rate as well as population density is associated with an increase in the vote share of radical left-wing parties, but a decrease in the vote share of radical right-wing parties. The latter result suggests that right-wing parties are more popular in rural areas, which is well in line with anecdotal evidence. Radical right-wing (left-wing) parties, moreover, are less (more) successful in counties with higher prevalence of older people, as suggested by the decrease (increase) in the magnitudes of the corresponding coefficient estimates. Lower and intermediate levels of formal education show stronger support for radical right-wing parties in a county, whereas higher shares of highly educated people in a county give rise to higher vote shares of radical left-wing parties. The share of foreigners is positively related to the vote share of radical left-wing parties, but not significantly related to the share of votes for radical right-wing parties.

The OLS estimates should be interpreted with caution, though, as we cannot rule out that they are affected by confounding factors. Table 2.3 reports the results of the 2SLS estimation where we instrument the actual realizations of our deprivation measures by measures that are computed based on counterfactual incomes. Again, the left panel shows the results for the share of votes for left-wing parties, the right panel for right-wing parties.

Table 2.2 : Support for radical parties (OLS)

| | Radical left-wing parties | | | Radical right-wing parties | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Poverty rate | 0.062*** | | | 0.013 | | |
| | (0.023) | | | (0.017) | | |
| Poverty gap | | 0.152** | | | -0.008 | |
| | | (0.063) | | | (0.043) | |
| Median gap | | | 0.123*** | | | -0.022 |
| | | | (0.038) | | | (0.028) |
| Unemployment | 0.317*** | 0.325*** | 0.303*** | -0.429*** | -0.423*** | -0.417*** |
| | (0.062) | (0.062) | (0.062) | (0.056) | (0.055) | (0.055) |
| Transfer recipients | -0.002 | 0.007 | -0.005 | 0.003 | 0.009 | 0.014 |
| | (0.048) | (0.047) | (0.048) | (0.033) | (0.034) | (0.034) |
| Population density | 7.242*** | 7.118*** | 7.180*** | -7.336*** | -7.435*** | -7.482*** |
| | (1.647) | (1.648) | (1.635) | (1.451) | (1.448) | (1.446) |
| Age 15 - 24 | 0.237*** | 0.232*** | 0.236*** | -0.420*** | -0.417*** | -0.416*** |
| | (0.049) | (0.049) | (0.049) | (0.040) | (0.040) | (0.040) |
| Age 25 - 34 | 0.202*** | 0.197*** | 0.206*** | -0.271*** | -0.268*** | -0.268*** |
| | (0.057) | (0.056) | (0.057) | (0.050) | (0.050) | (0.050) |
| Age 35 - 44 | 0.178*** | 0.169*** | 0.183*** | -0.257*** | -0.259*** | -0.262*** |
| | (0.064) | (0.063) | (0.064) | (0.053) | (0.052) | (0.053) |
| Age 45 - 54 | 0.167*** | 0.160*** | 0.179*** | -0.228*** | -0.230*** | -0.234*** |
| | (0.051) | (0.050) | (0.051) | (0.043) | (0.043) | (0.044) |
| Age 55 - 64 | 0.077* | 0.069* | 0.085** | -0.161*** | -0.164*** | -0.168*** |
| | (0.042) | (0.042) | (0.043) | (0.039) | (0.039) | (0.040) |
| Age 65+ | 0.110*** | 0.107*** | 0.111*** | -0.207*** | -0.207*** | -0.207*** |
| | (0.041) | (0.040) | (0.041) | (0.036) | (0.036) | (0.036) |
| Schooling lowest track | 0.049 | 0.053 | 0.051 | 0.104*** | 0.105*** | 0.105*** |
| | (0.039) | (0.039) | (0.039) | (0.030) | (0.029) | (0.029) |
| Schooling interm. track | -0.005 | -0.004 | -0.002 | 0.066** | 0.065** | 0.065** |
| | (0.034) | (0.034) | (0.034) | (0.027) | (0.027) | (0.027) |
| Schooling highest track | 0.151*** | 0.151*** | 0.152*** | 0.033 | 0.033 | 0.033 |
| | (0.038) | (0.038) | (0.038) | (0.030) | (0.030) | (0.030) |
| Foreigners | 0.185*** | 0.184** | 0.185*** | 0.028 | 0.030 | 0.031 |
| | (0.071) | (0.071) | (0.071) | (0.084) | (0.085) | (0.084) |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean dep. variable | 8.51 | 8.51 | 8.51 | 4.95 | 4.95 | 4.95 |
| $R^2$ | 0.958 | 0.958 | 0.958 | 0.911 | 0.911 | 0.911 |
| N | 2510 | 2510 | 2510 | 2510 | 2510 | 2510 |

*Notes:* Broad definition of radical parties. Baseline OLS estimates. Robust standard errors in parentheses and clustered at the county-level. Significance levels: *p $<$ 0.1, **p $<$ 0.05, ***p $<$ 0.01.

Table 2.3 : Support for radical parties (2SLS)

| | Radical left-wing parties | | | Radical right-wing parties | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Poverty rate | -0.261** | | | 0.496*** | | |
| | (0.126) | | | (0.119) | | |
| Poverty gap | | 0.213 | | | 1.243*** | |
| | | (0.218) | | | (0.215) | |
| Median gap | | | 0.050 | | | 0.683*** |
| | | | (0.175) | | | (0.233) |
| Economic controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Demogr. controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Foreigners | Yes | Yes | Yes | Yes | Yes | Yes |
| Education | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean dep. variable | 8.51 | 8.51 | 8.51 | 4.95 | 4.95 | 4.95 |
| N | 2510 | 2510 | 2510 | 2510 | 2510 | 2510 |
| Cragg-Donald | 56.37 | 98.48 | 44.98 | 56.37 | 98.48 | 44.48 |
| Kleibergen-Paap | 42.25 | 54.33 | 5.64 | 42.25 | 54.33 | 5.64 |

*Notes:* Broad definition of radical parties. 2SLS estimates. Robust standard errors in parentheses and clustered at the county-level. Significance levels: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Comparing the 2SLS estimates to the OLS estimates suggests that the OLS estimates are indeed severely biased. With regard to the vote share of radical left-wing parties, the results we obtain based on 2SLS estimation are very different to the OLS results. We detect a significantly negative effect of the poverty rate on the vote share of radical left-wing parties. The effect is not huge, but not negligible either. A rise in the share of households with an income below the poverty line decreases the vote share of radical left-wing parties by 0.26 pp or about three percent of the sample mean, respectively. However, the coefficient estimates of the other two deprivation measures, that is, the poverty gap and the median gap, are not statistically different from zero. Note that it is unlikely that the insignificance of these deprivation measures is due to inefficient estimation, as the Cragg-Donald F statistics are far above the critical values of the weak instrument test by Stock and Yogo (2005).[12]

In contrast, the 2SLS estimates indicate that economic deprivation has a positive impact on the vote share of radical right-wing parties. The estimated effects are statistically significant even at the one percent level of significance and of relevant magnitude. According to the estimates, a one pp increase in the poverty rate leads to a rise in the vote share of radical right-wing parties by 0.5 pp. In relation to the sample mean, this implies an increase in the vote share by ten percent. The effects of an increase in the poverty gap and median gap are

[12] The critical values for the Stock-Yogo weak IV F-test are 16.38 (10 percent maximal IV size), 8.96 (15 percent), 6.66 (20 percent), and 5.53 (25 percent).

even larger. Here, a one pp increase leads to 1.24 and 0.68 pp higher vote shares, implying a 25 percent and 14 percent increase in votes, respectively. The fact that a change in the average shortfall from the poverty line has a larger effect on the share of radical right-wing votes than a change in the average shortfall from the median income suggests that people are more prone to support radical right-wing parties the higher the prevalence of economic deprivation in their county.

### 2.6.2 Extensions and robustness checks

To test the robustness of our results, we modify our empirical specification in several ways. In a first robustness test, we apply a narrow definition of radical parties that includes only those parties that are entirely under the Office for the Protection of the Constitution's surveillance (see Section 2.3.3). With regard to radical left-wing parties, the only party included in the broad definition, but excluded from the narrow definition, is the Left Party. Of the radical right-wing parties, five out of twelve do not meet the narrow definition, among them the AfD. The 2SLS results using the IV approach are presented in Table A2.1 in the Appendix.

For left-wing radical parties, we detect a positive effect of all three economic deprivation measures that is statistically significant. It thus appears that in the baseline specification, the negative coefficient estimate for the poverty rate and the insignificant estimates for the poverty gap and median gap are entirely driven by the Left Party. The coefficient estimates indicate that a one pp increase in the poverty rate/poverty gap/median gap increases the share of radical left-wing votes by 0.03/0.12/0.06 pp, which implies an increase in the vote share by 50/200/100 percent. However, in light of the small vote share radical left-wing parties other than the Left Party received in federal elections, the effects are too small to describe a meaningful effect. In contrast, the results we obtain for radical right-wing parties remain qualitatively unchanged when changing the definition of radical parties. The fact that the coefficient estimates become notably smaller compared to the baseline results is most likely due to the exclusion of five out of twelve parties when moving from the broad to the narrow definition, among them the AfD, the most popular right-wing party in recent years.

Second, we investigate how changes in economic deprivation affect the share of votes of established parties. The results are presented in Table A2.2 in the Appendix. We detect a significantly negative effect of the poverty gap on the share of votes for established parties. The coefficient estimate of the median gap is negative as well, but just above the ten percent level of significance. The gain in votes for radical parties in response to an increase in economic deprivation seems to come at the expense of established parties.[13]

[13] Further analyses suggest that the reduction in the combined vote share of established parties is primarily due to a reduction in the votes for the Social Democratic Party (SPD) and the Green Party, which both lean to the left. The results are available upon request.

Third, we examine whether the effect of economic deprivation differs across West and East Germany. In Section 2.4, we highlighted that economic deprivation is much more prevalent in East Germany, although the West-East divide appears to have decreased over the past decades. At the same time, radical parties at both ends of the political spectrum enjoy greater popularity in East Germany than in West Germany. It is thus interesting to check whether the effect economic deprivation has on the vote share of radical parties varies across the two regions. We estimate separate coefficients for our deprivation measures across West and East German counties by including two dummy variables, i.e., one dummy that is equal to one for West German counties and one dummy that is equal to one for East German counties, and interacting these dummies with the deprivation measures. The results of the 2SLS estimation are presented in Table 2.4.

According to our estimates, an increase in the poverty gap has a somewhat stronger effect on the support for radical right-wing parties in West Germany than in East Germany. In West German counties, a one pp increase in the poverty gap leads to a 1.6 pp increase in the vote

Table 2.4 : Support for radical parties in West and East Germany (2SLS)

| | Radical left-wing parties | | | Radical right-wing parties | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| East × Poverty rate | -0.556* | | | 0.708*** | | |
| | (0.290) | | | (0.184) | | |
| West × Poverty rate | 1.269 | | | -0.605 | | |
| | (2.407) | | | (1.505) | | |
| East × Poverty gap | | -0.113 | | | 1.084*** | |
| | | (0.258) | | | (0.260) | |
| West × Poverty gap | | 0.920 | | | 1.590** | |
| | | (0.758) | | | (0.774) | |
| East × Median gap | | | -2.373 | | | 0.912* |
| | | | (1.537) | | | (0.485) |
| West × Median gap | | | 3.689 | | | 0.339 |
| | | | (3.399) | | | (1.282) |
| Economic controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Demogr. controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Foreigners | Yes | Yes | Yes | Yes | Yes | Yes |
| Education | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean dep. variable | 8.51 | 8.51 | 8.51 | 4.95 | 4.95 | 4.95 |
| N | 2510 | 2510 | 2510 | 2510 | 2510 | 2510 |
| Cragg-Donald | 0.36 | 5.15 | 0.93 | 0.36 | 5.15 | 0.93 |
| Kleibergen-Paap | 0.20 | 3.21 | 0.49 | 0.20 | 3.21 | 0.49 |

*Notes:* Broad definition of radical parties. 2SLS estimates. Robust standard errors in parentheses and clustered at the county-level. Significance levels: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

share for radical right-wing parties, compared to 1.1 pp in East German counties. However, for the poverty rate and the median gap, we only find significant coefficient estimates for East Germany.

For the radical left, the effect rather diverges between both regions. While economic deprivation decreases vote shares of radical left-wing parties in East German counties, the relationship is positive in West Germany. The difference between both regions is mainly driven bei the Left Party, which is seen as radical left-wing alternative in the West, but might rather be seen as established party in East Germany — as successor of the former governing communist party in the GDR.[14] Coefficient estimates of the interaction terms, however, lack statistical significance in most specifications (see Table 2.4, columns 1-3).

Finally, we investigate whether the effect of deprivation on the support for radical parties varies across urban and rural areas. It is often argued that people living in rural areas are more prone to support radical parties, especially nationalist ones. As before, we estimate separate coefficients by interacting the deprivation measures with two dummy variables, taking the value of one for urban or rural counties, respectively.[15] Our results do not support the conjecture that the effect economic deprivation has on the support for radial parties varies across urban and rural areas (see Table 2.5).

## 2.7 The 2017 election and the rise of the AfD

The federal election of 2017 marked a new era for the Federal Republic of Germany. For the first time since its foundation in 1949, a radical right-wing party with a nationalist and xenophobic platform entered the federal parliament. Yet, the vote shares of the AfD were not distributed evenly across German regions. Figure 2.4 illustratess the regional distribution of AfD vote shares at the 2017 federal election.

The differences across German counties are quite remarkable: vote shares range from 4.9 percent in Münster (Northrhine-Westphalia) to 35.5 percent in Sächsische Schweiz — Osterzgebirge (Saxony). Most striking are the differences in vote shares between East and West German counties. Whereas the population weighted county average in West Germany is 10.7 percent, it is 22.5 percent, i.e. about twice as high, in East Germany. Additionally, one can also discern regional discrepancies within East and West. In East Germany, vote shares are particularly high along the Polish and Czech border. In West Germany, vote shares are somewhat higher in the South than in the North; but, again, largest in economically weaker regions.

[14] Estimation results in West and East Germany using the narrow definition of parties are provided upon request.
[15] The classification of urban counties and rural counties is taken from the Federal Institute for Research on Building, Urban Affairs and Spatial Developments. Basis for the classification is the population density.

Table 2.5 : Support for radical parties in urban and rural counties (2SLS)

| | Radical left-wing parties | | | Radical right-wing parties | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Rural × Poverty rate | -0.282** | | | 0.504*** | | |
| | (0.128) | | | (0.119) | | |
| Urban × Poverty rate | -0.220* | | | 0.479*** | | |
| | (0.132) | | | (0.124) | | |
| Rural × Poverty gap | | 0.119 | | | 1.287*** | |
| | | (0.217) | | | (0.216) | |
| Urban × Poverty gap | | 0.342 | | | 1.184*** | |
| | | (0.230) | | | (0.222) | |
| Rural × Median gap | | | 0.024 | | | 0.693*** |
| | | | (0.178) | | | (0.234) |
| Urban × Median gap | | | 0.084 | | | 0.670*** |
| | | | (0.180) | | | (0.236) |
| Economic controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Demogr. controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Foreigners | Yes | Yes | Yes | Yes | Yes | Yes |
| Education | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean dep. variable | 8.51 | 8.51 | 8.51 | 4.95 | 4.95 | 4.95 |
| N | 2510 | 2510 | 2510 | 2510 | 2510 | 2510 |
| Cragg-Donald | 28.16 | 49.33 | 22.50 | 28.16 | 49.33 | 22.50 |
| Kleibergen-Paap | 21.19 | 27.06 | 2.81 | 21.19 | 27.06 | 2.81 |

*Notes:* Broad definition of radical parties. 2SLS estimates. Robust standard errors in parentheses and clustered at the county-level. Significance levels: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

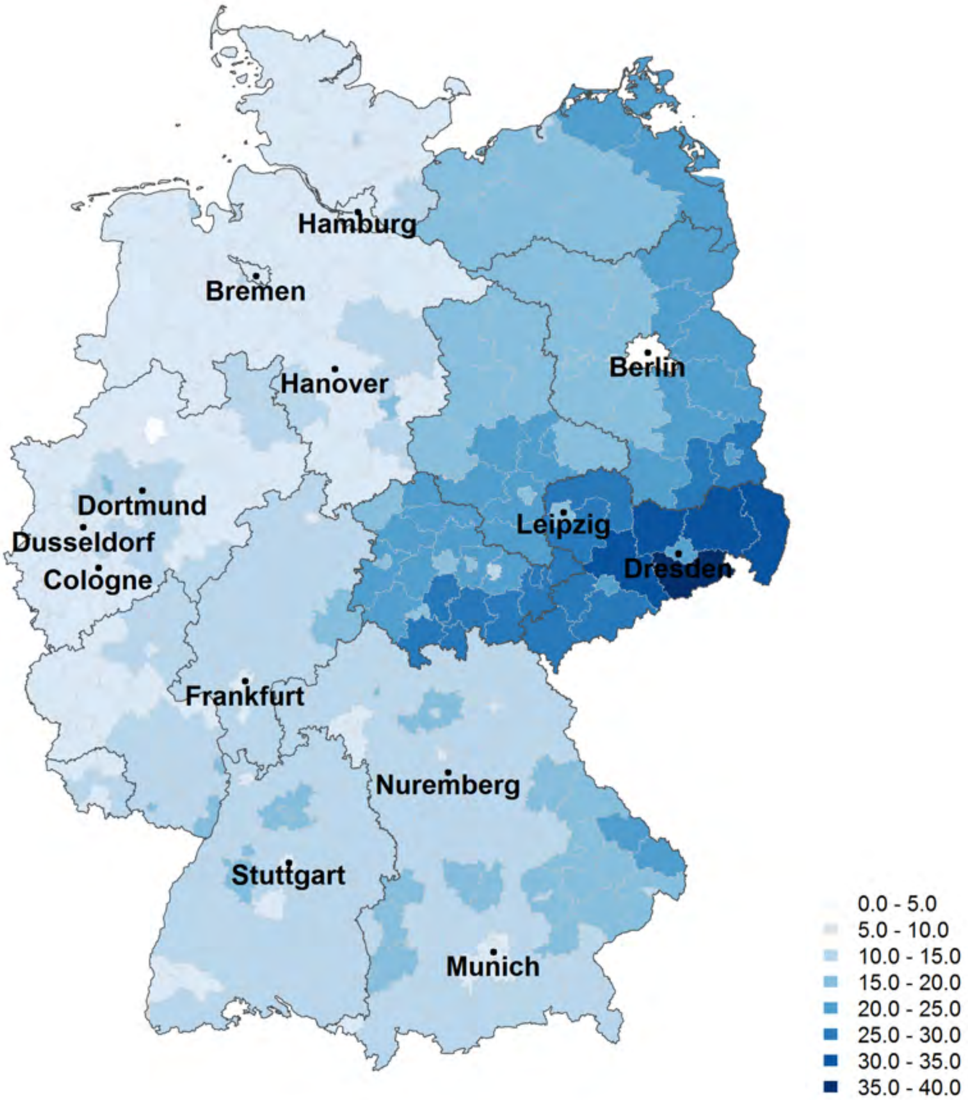
We examine whether and to what extent economic deprivation can explain the observed regional differences in AfD vote shares. For this purpose, we re-estimate our baseline empirical model, but employ the AfD vote share as the dependent variable and only use data from the federal election of 2017:

$$Y_{i2017} = \alpha_i + \beta \, \textit{Deprivation}_{i2017} + \gamma \, X_{i2017} + \varepsilon_{i2017} \tag{2.4}$$

Table 2.6 shows the 2SLS estimates. The results indicate that regional variation in economic deprivation influences the electoral success of the AfD in a statistically significant and sizeable way.

According to our estimates, a one pp increase in the poverty rate leads, on average, to an increase in the AfD vote share by about 2.0 pp, which is equivalent to a 15 percent increase in votes in relation to the sample mean. An increase in the poverty gap has an even larger effect. If the poverty gap increases by one pp, the AfD vote share increases by almost 5.0 pp, which

Figure 2.4 : AfD vote shares in 2017




implies a 37 percent increase in votes. Thus, the effect of economic deprivation on the vote share of the AfD in the 2017 election is three to four times higher than the general effect of economic deprivation on voting for radical right-wing parties in all federal elections between 1998 and 2017 (see Section 2.6).

As before, we also estimate separate effects for West vs. East Germany and for urban vs. rural areas. The results suggest that the average effect conceals important regional differences. We find that the effect of economic deprivation on the AfD vote share is about three times larger in East German counties than in West German counties (Appendix, Table A2.3). In contrast, the effect of economic deprivation on vote shares of all radical right-wing parties is more similar between East and West German counties (see Section 2.6). We again do not detect any heterogenous effects between rural and urban counties (Appendix, Table A2.4).

Our results using 2SLS estimation are in line with the view that the AfD is particularly successful in economically weaker regions (e.g. Bergmann *et al.*, 2017; Garmann and Potrafke, 2019). But how can these findings be reconciled with evidence suggesting that AfD supporters do not differ from supporters of established parties in terms of income and other socio-demographic characteristics (Hansen and Olsen, 2019; Goerres *et al.*, 2018)? One possible explanation is that the extent of economic deprivation in a region also strengthens the AfD's popularity among voters from middle and high-income groups. There are at least two potential reasons for such a relationship. First, a high level of economic deprivation in close regional proximity may increase economic anxiety among middle and high-income earners, as well as the perceived risk of social decline. Economic anxiety, in turn, is found to be an important determinant of the popularity of populist parties (Algan *et al.*, 2017; Guiso *et al.*, 2017). Survey evidence, for example, suggests that the support for AfD is not driven by unemployed or respondents receiving social assistance, but by voters which expect less prospective economic situation and perceive a larger self-reported risk of poverty and unemployment (Goerres *et al.*, 2018; Hansen and Olsen, 2019). Second, middle and high-income earners may not only care about their own economic situation, but also about the economic conditions in the region in which they are living. A high level of regional economic deprivation may thus increase dissatisfaction with the political mainstream and make middle and high-income earner more prone to support the populist platform on which the AfD runs.

Table 2.6 : AfD vote shares in German counties (2SLS)

| | AfD vote shares | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Poverty rate | 1.974*** | | |
| | (0.484) | | |
| Poverty gap | | 4.868*** | |
| | | (1.639) | |
| Median gap | | | 1.943*** |
| | | | (0.329) |
| Economic controls | Yes | Yes | Yes |
| Demogr. controls | Yes | Yes | Yes |
| Foreigners | Yes | Yes | Yes |
| Education | Yes | Yes | Yes |
| Mean dep. variable | 13.41 | 13.41 | 13.41 |
| N | 396 | 396 | 396 |
| Cragg-Donald | 25.63 | 13.98 | 70.31 |
| Kleibergen-Paap | 21.43 | 12.18 | 54.33 |

*Notes:* 2SLS estimates. Robust standard errors in parentheses. Significance levels: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

## 2.8 Conclusion

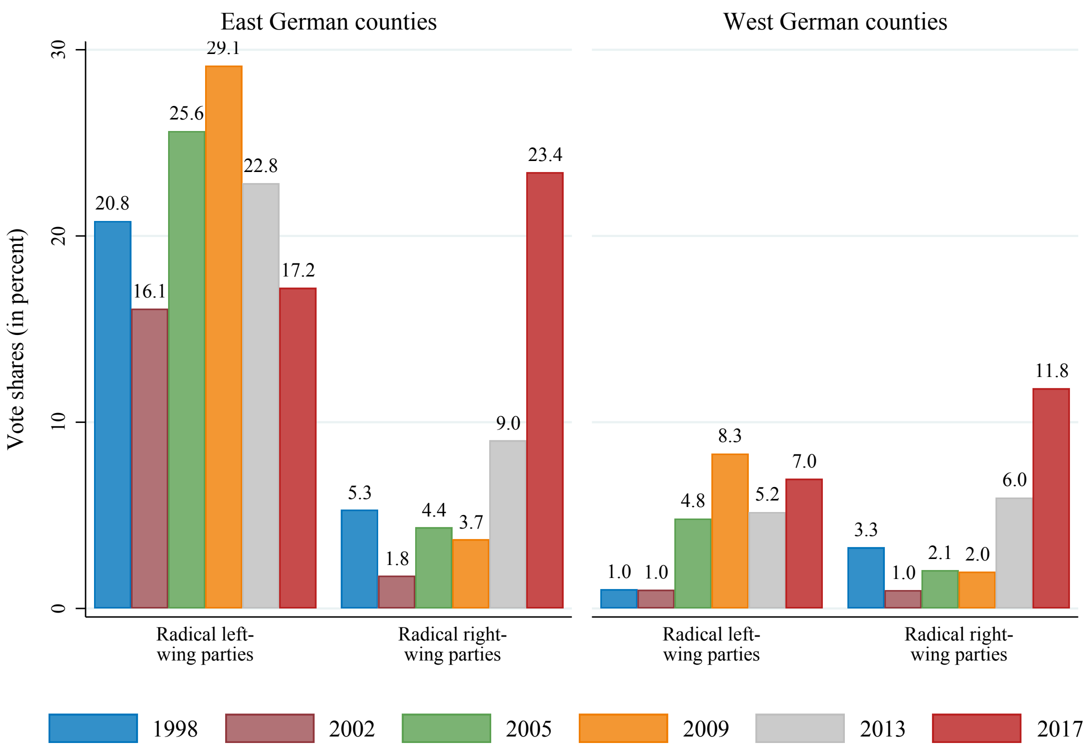
Arguably, two of the major challenges many industrialized countries have been facing over the past few years are the increase in relative economic deprivation and growing political polarization. Many observers argue that these two phenomena are closely linked, blaming the relative economic deprivation many people experience to be a main factor driving the increasing popularity of radical parties and movements around the world. This paper explores whether economic deprivation influences the support for radical parties in a causal way. Using data from Germany, we employ instrumental variable estimation to study the effect of economic deprivation on the share of votes radical left-wing and right-wing parties received in federal elections. Our analysis is conducted at the county-level (NUTS-3) and covers six federal elections held between 1998 and 2017.

The empirical results suggest that regional economic deprivation has a causal and sizeable effect on vote shares of radical parties. This effect is particularly pronounced for radical right-wing parties. The greater the prevalence of (relative) poverty, the greater the success of nationalist parties at the polls. Moreover, our results suggest that relative economic deprivation was an important determinant of the electoral success of the AfD (*Alternative für Deutschland*), the new nationalist party in Germany, in the federal election of 2017. All in all, our findings provide evidence that the prevalence of relative economic deprivation is an important driver of political polarization, the rise of radical parties and populist movements, and may thus undermine moderate political forces and ultimately threaten political stability.

# Appendix

## Figures

Figure A2.1 : Radical vote shares in East and West German counties




*Notes:* Average vote shares across counties.

## Tables

Table A2.1 : Support for radical parties – narrow definition (2SLS)

| | Radical left-wing parties | | | Radical right-wing parties | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Poverty rate | 0.032*** | | | 0.039 | | |
| | (0.006) | | | (0.040) | | |
| Poverty gap | | 0.116*** | | | 0.178** | |
| | | (0.015) | | | (0.076) | |
| Median gap | | | 0.064*** | | | 0.189*** |
| | | | (0.009) | | | (0.056) |
| Economic controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Demogr. controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Foreigners | Yes | Yes | Yes | Yes | Yes | Yes |
| Education | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean dep. variable | 0.06 | 0.06 | 0.06 | 2.02 | 2.02 | 2.02 |
| N | 2510 | 2510 | 2510 | 2510 | 2510 | 2510 |
| Cragg-Donald | 56.37 | 98.48 | 44.98 | 56.37 | 98.48 | 44.98 |
| Kleibergen-Paap | 42.25 | 54.33 | 5.64 | 42.25 | 54.33 | 5.64 |

*Notes:* Narrow definition of radical parties. 2SLS estimates. Robust standard errors in parentheses and clustered at the county-level. Significance levels: *p $<$ 0.1, **p $<$ 0.05, ***p $<$ 0.01.

Table A2.2 : Established parties (2SLS)

| | Established parties | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Poverty rate | 0.076 | | |
| | (0.133) | | |
| Poverty gap | | -0.810*** | |
| | | (0.233) | |
| Median gap | | | -0.234 |
| | | | (0.147) |
| Economic controls | Yes | Yes | Yes |
| Demogr. controls | Yes | Yes | Yes |
| Foreigners | Yes | Yes | Yes |
| Education | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| County FE | Yes | Yes | Yes |
| Mean dep. variable | 83.45 | 83.45 | 83.45 |
| N | 2510 | 2510 | 2510 |
| Cragg-Donald | 56.37 | 98.48 | 44.98 |
| Kleibergen-Paap | 42.25 | 54.33 | 5.64 |

*Notes:* 2SLS estimates for established parties. Considered as established parties are CDU/CSU, SPD, FDP, and the Greens. Robust standard errors in parentheses and clustered at the county-level. Significance levels: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A2.3 : AfD vote shares in East and West German counties (2SLS)

| | AfD vote shares | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| East × Poverty rate | 1.030*** | | |
| | (0.210) | | |
| West × Poverty rate | 0.390* | | |
| | (0.200) | | |
| East × Poverty gap | | 3.811*** | |
| | | (0.969) | |
| West × Poverty gap | | 1.238 | |
| | | (0.927) | |
| East × Median gap | | | 0.946*** |
| | | | (0.190) |
| West × Median gap | | | 0.291 |
| | | | (0.197) |
| Economic Controls | Yes | Yes | Yes |
| Demogr. Controls | Yes | Yes | Yes |
| Foreigners | Yes | Yes | Yes |
| Education | Yes | Yes | Yes |
| Mean dep. variable | 13.41 | 13.41 | 13.41 |
| N | 396 | 396 | 396 |
| Cragg-Donald | 17.75 | 7.36 | 37.35 |
| Kleibergen-Paap | 14.03 | 5.47 | 26.91 |

*Notes:* 2SLS estimates for East and West German counties. Robust standard errors in parentheses. Significance levels: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A2.4 : AfD vote shares in urban and rural German counties (2SLS)

| | AfD vote shares | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Rural × Poverty rate | 2.025*** | | |
| | (0.504) | | |
| Urban × Poverty rate | 2.104*** | | |
| | (0.534) | | |
| Rural × Poverty gap | | 4.875*** | |
| | | (1.653) | |
| Urban × Poverty gap | | 4.922*** | |
| | | (1.743) | |
| Rural × Median gap | | | 1.987*** |
| | | | (0.340) |
| Urban × Median gap | | | 2.050*** |
| | | | (0.358) |
| Economic controls | Yes | Yes | Yes |
| Demogr. controls | Yes | Yes | Yes |
| Foreigners | Yes | Yes | Yes |
| Education | Yes | Yes | Yes |
| Mean dep. variable | 13.41 | 13.41 | 13.41 |
| N | 396 | 396 | 396 |
| Cragg-Donald | 11.44 | 6.54 | 32.06 |
| Kleibergen-Paap | 10.63 | 6.05 | 26.76 |

*Notes:* 2SLS estimates for urban and rural German counties. Robust standard errors in parentheses. Significance levels: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

# 3 Trade openness and income inequality: New empirical evidence[1]

## Abstract

In this chapter, we examine how trade openness influences income inequality within countries. The sample includes 139 countries over the period 1970-2014. We employ predicted openness as the instrumental variable to deal with the endogeneity of trade openness. The results do not show that trade openness influences income inequality in the full sample. The effect of trade openness on income inequality differs across countries. Trade openness tends to disproportionately benefit the relative income shares of the poor, but not necessarily all poor, in the sample of emerging and developing economies. In most advanced economies, trade openness increased income inequality, an effect that is, however, driven by outliers. The positive effect of trade openness on income inequality in our benchmark country sample is driven by China and transition countries from Central and Eastern Europe.

[1] This chapter is joint work with Clemens Fuest and Niklas Potrafke. It is based on our paper "Trade openness and income inequality: New empirical evidence", a revised version of this chapter is published in *Economic Inquiry, forthcoming*, 2021. Previous Working Paper versions were prepared with the title "Globalization and income inequality - revisited" as *CESifo Working Paper* No. 6859, 2018, as *ifo Working Paper* No. 247, 2018, and as *European Commission Discussion Paper* No. 056, 2017, in the context of the European Commission DG ECFIN's fellowship initiative 2016/17.

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## 3.1 Introduction

How trade openness relates to income inequality has been examined in many empirical studies in the mid-1990s (e.g. Wood, 1995; Cragg and Epelbaum, 1996; Feenstra and Hanson, 1996; Borjas *et al.*, 1997; Leamer, 1998)[2] and has been revisited in the last decade (e.g. Meschi and Vivarelli, 2009; Jaumotte *et al.*, 2013; Roser and Cuaresma, 2016). The empirical evidence is mixed. The results by Jaumotte *et al.* (2013) suggest that trade openness is associated with lower income inequality, a result that is based on a sample of 51 developing and developed countries. Roser and Cuaresma (2016) use data for 32 developed countries and show that — in line with the Stolper-Samuelson theorem — imports from developing countries are positively correlated with income inequality. The results by Meschi and Vivarelli (2009) suggest, by contrast, that trade is positively associated with income inequality in 65 developing countries. These studies use macrodata at the country level and hardly report causal effects.[3] We therefore investigate how trade openness influences income inequality by employing a new identification strategy and considering heterogeneity across countries. The sample includes up to 139 countries over the period 1970-2014. The study provides several contributions to the empirics on how trade openness influences income inequality within countries:[4]

First, we deal with the endogeneity problem of trade openness. Examining the causal effect of trade openness on income inequality is challenging. We control for many variables, but other unobserved omitted variables may still cause biased estimates by influencing both, trade openness and income inequality. Moreover, reverse causality may occur because changes in income inequality are likely to influence policies which, in turn, affect trade openness. Previous cross-country studies do little to deal with the endogeneity of trade openness and therefore mostly provide descriptive evidence on the link between trade openness and inequality. This descriptive evidence is useful but it is important to ask whether there is a causal effect running from trade openness to inequality. We use an instrumental variable (IV) approach to identify causal links between trade openness and inequality. Our IV is predicted openness based on a gravity equation using a time-varying interaction of geography and exogenous large-scale natural disasters as proposed by Felbermayr and Gröschl (2013). Predicted openness has been used as an IV for trade openness in the trade-growth-nexus (Frankel and Romer, 1999;

2 Winters *et al.* (2004) review early empirical studies of the trade-inequality nexus and conclude that "there can be no simple general conclusion about the relationship between trade liberalization and poverty" (p.106).

3 Other new studies use microdata to identify how trade openness influences local incomes across regions and workers within individual countries (see Autor et al. 2013). These studies are useful to understand mechanisms of the effect but cannot predict external validity with respect to the overall effect of trade openness on inequality. Other studies review country case studies in the developing world (Goldberg and Pavcnik, 2007; Pavcnik, 2017).

4 Trade openness is an aspect of globalization. Globalization is often measured by the KOF globalization index (Dreher and Gaston, 2008; Bergh and Nilsson, 2010; Dorn and Schinke, 2018; Dorn *et al.*, 2018; Lang and Tavares, 2018; Sturm *et al.*, 2019; Bergh *et al.*, 2020). New studies made progress in identifying causal effects. For example, Lang and Tavares (2018) use instrumental variables based on the geographical distribution of globalization. The disadvantage is that there is no encompassing theory describing how overall (economic) globalization influences income inequality. Scholars often use trade-based theories to describe how globalization influences income inequality (see Section 3.2).

Felbermayr and Gröschl, 2013). OLS and 2SLS results, however, both suggest that overall trade openness and income inequality are hardly correlated in the full country sample. The benchmark sample excludes countries, where the available data is often poor and estimates may be biased. Within our benchmark sample, our 2SLS results suggest that trade openness influences income inequality. We find that the upper deciles disproportionately gain from trade openness.

Second, our 2SLS results show how the effect of trade openness on income inequality differs across countries. In emerging and developing countries, our results suggest that trade openness disproportionately benefits the very poor (not necessarily all poor), as predicted by the Stolper-Samuelson theorem (Stolper and Samuelson, 1941). This finding is in line with empirical findings indicating that globalization reduces inequality and poverty in developing countries (see Winters *et al.*, 2004; Bergh and Nilsson, 2014). Trade openness increased income inequality in advanced economies as predicted by the Stolper-Samuelson theorem. 2SLS results within the sample of most advanced economies suggest that upper deciles disproportionately gain from trade openness at the expense of the income shares of the bottom deciles in the income distribution. The relationship, however, is driven by outliers.

Third, we find that the average positive effect of trade openness on income inequality in our benchmark sample is driven by trade openness and rising income inequality in China and transition countries from Central and Eastern Europe. Our results suggest a strong effect of trade openness on inequality within transition countries. These countries have experienced a particularly fast change towards trade openness accompanied by large-scale market-oriented reforms and an economic transition process in our period of observation. The market-oriented reforms likewise promoted integration in the global market and increased income inequality. The impact on income distribution during the transition period was hardly cushioned by either labor market institutions or welfare states, which characterize many advanced economies (see Milanovic, 1999; Myant and Drahokoupil, 2010; Perugini and Pompei, 2015b).

Governments are likely to influence market outcomes by designing taxation and social policies to redistribute income from the rich to the poor. There are two competing views on the relationship between globalization, welfare state policies and the impact on income inequality: the race to the bottom theory (e.g. Sinn, 2003) expects that globalization gives rise to less redistribution, whereas the compensation hypothesis (Rodrik, 1998) rather suggests that welfare activities would increase. We use the difference between Gini market and net outcomes as indicator for redistribution and do not conclude that trade openness affects income inequality before or after redistribution in different manners.

Our findings suggest that the effect of trade openness on income inequality is rather context specific (see Pavcnik, 2017). The widespread view in the public debate that globalization – especially trade openness – has adverse effects on the income distribution within countries needs to be reconsidered.

## 3.2 Theoretical predictions

The classical theoretical framework for analyzing the relationship between trade openness and distributional market outcomes is the Heckscher-Ohlin (HO) model (Ohlin, 1933). It explains the inequality effect of trade openness as a result of productivity differences and the relative factor endowment of countries, and the extent to which individuals depend on labor or capital income. Countries specialize in production within their relatively abundant factor and export these goods when they open up to trade. The Stolper-Samuelson theorem (Stolper and Samuelson, 1941) shows that the subsequent trade-induced relative changes in product prices increase the real return to the factors used intensively in the production of the factor-abundant export goods and decrease the returns to the other factors. As a consequence, the country's abundant production factors gain from openness, while scarce factors lose. Most theories distinguish between the production factors labor and capital, or between unskilled and skilled labor. Because capital and skilled labor are relatively abundant in advanced economies, income inequality and income concentration towards the top incomes is expected to increase. In developing countries, unskilled labor, which is intensively used in local production, would benefit from economic openness by increasing wages. In developing countries, income inequality is therefore expected to decrease. Based on the HO-model assumptions, how trade openness influences income inequality depends on a country's development level.

Since the 1990s, many studies have pointed to limitations of the standard HO model implications and suggested different ways in which trade openness may affect income inequality. For instance, the Heckscher-Ohlin model relies on between sector reallocations and neglects within-sector shifts in production and vertical specializations across countries. While offshoring and outsourcing of less-skilled production within a sector decrease the wages and bargaining power of less skilled workers in advanced economies, the offshored and outsourced activities along the value chain may be relatively skill-intensive from the perspective of the developing countries (see Feenstra and Hanson, 1996, 1999). Along the same lines Feenstra and Hanson (1997), for example, describe that foreign direct investments (FDI) increase the relative demand for skilled labor and the skill premium due to capital-skill-complementarities in the developing world. In addition, as a response to the rising exposure to import competition, occupations in traded sectors of the developing world may become more skill-intensive so that relative wages of low-skilled workers decline (Cragg and Epelbaum, 1996). Income inequality may also rise because of heterogeneous firms within sectors and countries and resulting wage premia for workers in firms that participate in international trade. Exporting firms are more productive than non-exporting firms and pay higher wages to hire higher-skilled labor (see Manasse and Turrini, 2001; Yeaple, 2005; Munch and Skaksen, 2008; Verhoogen, 2008; Egger and Kreickemeier, 2009; Frías *et al.*, 2012; Egger *et al.*, 2013; Sampson, 2014). Helpman *et al.* (2010, 2017) predict a non-monotonic relationship between trade openness and wage inequality, where trade liberalization at first raises and later reduces wage inequality.

Skill biased technological change is discussed as one of the main alternative explanations of the rising skill premium and income inequality within countries. Many studies discuss how innovations and new labor-saving technologies have eliminated low-skilled jobs through automation or by upgrading the required skill levels (see Berman *et al.*, 1994, 1998; Machin and Van Reenen, 1998; Acemoglu, 1998, 2002; Krusell *et al.*, 2000; Card and DiNardo, 2002). While technological innovations primarily occur in advanced economies, trade openness may facilitate technology transfer across borders, so that skill biased technological change also takes place in less developed countries (see Berman and Machin, 2000; Burstein *et al.*, 2013). Rising import competition may also induce investments in new technologies and accelerate technological shifts which decrease employment of relatively unskilled workers (Bloom *et al.*, 2016).

Governments are likely to influence market outcomes by setting agreements, regulations and tariffs; and design taxation and social policies to redistribute income from the rich to the poor. There are two competing views on the relationship between globalization, welfare state policies and the impact on income inequality: the race to the bottom hypothesis and the compensation hypothesis. The “race-to-the-bottom” theory (e.g., Sinn, 2003) describes that globalization puts a downward pressure on tax rates and regulations for mobile factors such as tax rates on capital. Large welfare states, moreover, attract unskilled and poor immigrants who want to benefit from redistribution. This together gives rise to lower public spending and less redistribution. Globalization is thus expected to increase income inequality after taxes and transfers. Experts emphasizing the ‘dark side of globalization’ such as Stiglitz (2002) believe that globalization is responsible for diminishing redistribution activities and shrinking social security systems. The “race-to-the-bottom” theory would expect that trade openness decreases redistribution and that trade openness would affect income inequality after tax and transfers even to a larger extent than income inequality before redistribution. In contrast, the compensation hypothesis (Rodrik, 1998) predicts an expansion of the welfare state, providing insurance against growing risks associated with globalization and international trade competition. A variant of this argument is that losers from globalization and trade openness may demand compensation. This theory predicts that globalization will increase the size and scope of government. In a similar vein, Gozgor and Ranjan (2017) suggest that when globalization raises market income inequality, policymakers who are interested in maximizing the sum of welfare of all agents would increase redistribution. Meltzer and Richard (1981) describe that higher inequality tends to increase redistribution, because the median voter would favor more redistribution. Because taxes and transfers are often designed to mitigate income inequality, as suggested by the compensation hypothesis, we expect that trade openness increases redistribution and influences market income inequality to a larger extent than income inequality after taxes and transfer (for related discussion see, for example, Uusitalo, 1985; Bergh, 2005; Brady and Sosnaud, 2010).[5]

[5] The empirical evidence on the globalization-welfare state nexus is mixed (e.g. Schulze and Ursprung, 1999; Milanovic, 2000; Ursprung, 2008; Meinhard and Potrafke, 2012; Kauder and Potrafke, 2015; Potrafke, 2015, 2019b; Pleninger and Sturm, 2020; Bergh *et al.*, 2020).

## 3.3 Data

We use an unbalanced panel for up to 139 countries over the period 1970-2014. The data are averaged over five years in nine periods between 1970 and 2014. We follow related literature and use five-year averages to reduce the possibility of outliers, measurement errors, missing observations in individual years and short-term movements in the business cycle influencing the inferences (see Bergh and Nilsson, 2010; Felbermayr and Gröschl, 2013; Lang and Tavares, 2018).[6]

### 3.3.1 Variables

**Income inequality**[7]
We use the Gini index as the primary measure of income inequality. Gini indices are often based on different sources and welfare definitons, and are therefore calculated in manifold ways (see Dorn, 2016). Secondary source datasets combine several data sources and data quality to achieve a higher coverage.[8] Scholars who use secondary source datasets often apply constant adjustment procedures to standardize different Gini measures. Differences of Gini measures are likely to vary across countries and within countries over time depending on the extent of taxation and transfer policies, patterns of consumption and savings, family structure, and other factors. Constant adjustment procedures are therefore likely to produce systematic errors in the data and estimation results. On the one hand, secondary source datasets have a high coverage at the expense of comparability; on the other hand, harmonized microdata sets such as the Luxembourg Income Study (LIS) are more comparable, but at the expense of coverage over time and countries: this reflects the trade-off between greater comparability and broader coverage of income inequality datasets.

We use the Gini household income inequality indices of Solt's (2016) Standardized World Income Inequality Database (SWIID, v5.1).[9] SWIID provides standardized Gini income inequality measures for market and net outcomes based on the same concept, and thus allows the comparison of income inequality before and after redistribution by taxation and transfers over time. We use both the market and net income Gini indices. We use the difference between the Gini market and Gini net index to measure the level of redistribution.

The high coverage across countries and time and the adjustment procedure for achieving possible comparability is the major reason for preferring SWIID to other secondary source datasets. SWIID uses the LIS series as baseline. To predict missing observations in the LIS series, data from other secondary data sources and statistical offices are standardized to LIS by using systematic relationships of different Gini types and model-based multiple imputation

6 Using 3-year averaged data works equally well in our robustness tests (see Section 3.6.4).
7 Table A3.2 in the Appendix describes summary statistics and data sources of all variables.
8 The World Income Inequality Database (WIID) of UNU-WIDER and Branko Milanovic's All-the-Ginis (ATG) database are, for example, large collections of secondary data sources and are often used in empirical research.
9 SWIID has been used in several empircal studies before (see Bergh and Nilsson, 2010; Acemoglu *et al.*, 2015).

estimates.[10] When estimating missing observations, Solt (2016) considers that adjustments cannot be constant across countries and time by relying on information from proximate years in the same country as the best solution, and on information on countries in the same region and with similar development level as second-best solution. There are, however, concerns over the reliability of SWIID's imputed estimates in data-poor regions (Ferreira *et al.*, 2015; Jenkins, 2015). We address these concerns in our benchmark sample selection (see Section 3.3.2).

A shortcoming of Gini indices is that they do not show which parts of a country's income distribution disproportionately gain or lose and cause changes in the Gini index. We therefore also employ the released data on relative net income shares of the Global Consumption and Income Project (GCIP) by Lahoti *et al.* (2016) as a measure of post tax and transfer income inequality. In a similar vein as SWIID, they estimate standardized measures based on the available data sources to increase comparability across countries and time, and increase the coverage of the data by using interpolation methods for missing country-year observations.

**Trade openness and covariates**

We measure trade openness by the sum of imports and exports as a share of GDP. Trade data are taken from the World Development Indicators (World Bank, 2017). We follow previous studies by including the following control variables (see Bergh and Nilsson, 2010; Lang and Tavares, 2018; Bergh *et al.*, 2020): real GDP per capita[11] of the Penn-World-Table version 9.0 by Feenstra *et al.* (2015), to control for any distributional effect due to different income levels. Studies show that economic growth and the GDP per capita level are related to globalization (see Dreher, 2006; Dreher *et al.*, 2008; Gygli *et al.*, 2019) and to the development of the income distribution over time (see Berg *et al.*, 2012). Demographic changes and shifts in the size of population are also likely to influence both international trade and the income distribution (OECD, 2008). We therefore add the age dependency ratio by the World Development Indicators (World Bank, 2017) and the logarithm of total population of the Penn-World-Table (Feenstra *et al.*, 2015). The dependency ratio measures the proportion of dependents per 100 of the working age population, where citizens younger than 15 or older than 64 are defined as the dependent (typically non-productive) part. A higher share of dependent citizens is usually associated with higher income inequality and higher redistribution activities within countries. Shifts in the size of the population affect the dependency ratio as well as a country's labor and skill endowment. Trade openness is likely to be correlated with other indicators of globalization such as FDIs, migration or political globalization. Other globalization indicators might also influence inequality within countries (Borjas *et al.*, 1997; Bergh and Nilsson, 2010; Jaumotte

[10] The ratios of different Gini types are estimated by systematic relationships on the basis of eleven different combinations of welfare definitions and income scales (see Solt, 2016).

[11] We use the expenditure-side real GDP at chained PPPs to compare relative living standards across countries and over time.

*et al.*, 2013; Dorn *et al.*, 2018; Lang and Tavares, 2018). We therefore use the KOF globalization subindices for political and social[12] globalization as well as an index for FDIs as additional controls in our baseline models (Dreher, 2006, update KOF 2016).

Our instrument *predicted openness* is constructed by using a gravity model including exogenous large scale natural disasters in other countries. Natural disasters themselves are shown to influence trade openness and the per capita income level of countries (see Felbermayr and Gröschl, 2013, 2014). Some natural disasters are registered across borders. Natural disasters registered in the home country might have a direct impact on the home country's income distribution. To make sure that our estimated relationship between trade and inequality is not driven by the correlation between disasters registered in the home country and income inequality, we directly control for the effect of large-scale natural disasters on the income distribution within countries. We included the one-period lagged large scale natural disasters as baseline control variable.

### 3.3.2 Country subsamples

**Full and benchmark samples**

Next to our *full sample* of 139 countries, we also use a sample for high and upper middle income countries as our *benchmark sample*. High and upper middle income countries are classified by the criterion of the World Bank as of 2015 and include 82 countries having a gross national income (GNI) per capita of USD 4,126 or more. The 57 countries in our dataset below the GNI per capita threshold of USD 4,126 are classified as low income and lower middle income countries (lower income countries). Lower income countries are more likely than high and middle income countries to have few period observations per country due to a lack of data availability. Data in lower income countries are, moreover, more likely to be subject to measurement errors. There are serious concerns about the quality of the income inequality data from less developed countries.[13] Jenkins (2015), for example, shows that source data on inequality of high quality, in which the income concept and the survey can be verified, are rare in less developed and in particular in sub-Saharan African countries. The lack of data quality is also reflected in the imputed Gini estimates in SWIID, as the imputation variability of imputed country-period observations is large in some countries, especially in lower income countries

[12] The stock of international migrants as share of population is included in the index of social globalization. We include migration as single control variable in our robustness tests (see Section 3.6.4).

[13] There are several reasons for poor inequality and poverty measures in low-income countries. On the one hand, official statistical data of good quality about the income distribution are often rare in developing countries as they have high shares of informal working participants and self-employed persons in business and agriculture. On the other hand, reliable survey data on income or consumption are also rare. Surveys in developing countries might have a sample bias when some parts of the population are systematically not surveyed, for example unskilled people because of literacy problems or people who live in rural regions. Respondents, moreover, might not report the truth as they might fear that information is provided to government authorities, for example tax institutions. The lack of political will, unskilled staff, and high turnover in statistical offices are also reasons why data are not collected consistently and continually (Deaton, 2005).

(Ferreira *et al.*, 2015; Jenkins, 2015). To address potential biases in the estimates because of measurement error, our benchmark sample excludes the 57 lower income countries that are in the full sample. 29 of the 57 excluded countries are sub-Saharan African countries.[14]

**Development levels**

Some theories predict different outcomes of the effect of trade openness on income inequality depending on the development level of countries (Section 3.2). Next to our full and benchmark sample of our baseline regressions we therefore use subsamples for the most *advanced economies* as well as *emerging markets & developing economies (EMD)*.[15] To distinguish between advanced economies and emerging markets and developing economies we apply the classification of the International Monetary Fund (IMF, 2016). The IMF classification is based on per capita income levels, export diversification and the degree of integration into the global financial system.[16] The 34 countries fulfilling the criterion of the advanced economies sample are also included in our benchmark sample (high and upper middle income countries). The subsample of emerging markets and developing economies includes 105 countries taken from both income groups, the full set of lower income countries and the countries of the benchmark sample, which are not classified as advanced economies.

**Transition economies**

Transition economies are another important country sample when examining the trade openness-inequality nexus. Transition economies have experienced a large shift in trade openness since the fall of the Iron Curtain. The globalization shock for transition countries was, however, hardly cushioned by either labor market institutions, education systems or welfare states, which characterize many advanced economies in the rest of the world. The transition countries had limited capabilities in the education system and higher labor market frictions at the beginning of their transition. The transition to an open and competitive market economy, FDI-induced new technologies and equipment, and the overall skill-biased technological shift in the 1990s suddenly required other skills than the working age population and the education systems were prepared for (see Aghion and Commander, 1999). During the simultaneous period, transition countries also experienced many structural and institutional changes in political institutions and their economy, such as privatizations of state-owned enterprises, deindustrialization, price liberalizations, financial development, labor and product market deregulation, new models of corporate governance, or shrinking and reforming of the public sector during their transformation from centrally planned to market-based economies. One of the most visible outcomes of the systematic change and complex interplay of several forces is a remarkable increase in income inequality (see Perugini and Pompei, 2015a). The market-oriented reforms, moreover, promoted the inflow of FDI and the integration in the

[14] The benchmark sample includes four Subsaharan African countries: Angola, Gabon, Mauritius, and South Africa.

[15] See Appendix for the list of countries by development levels.

[16] Several oil exporters that have high per capita GDP, for example, would not make the advanced classification because around 70 % of its exports are oil.

global market. The transition toward market economies might therefore be an omitted driver of trade openness and inequality in transition countries. The systemic change and restructuring of the economy and governance has likely influenced the speed of globalization and the rise of income inequality (Milanovic, 1999; Milanovic and Ersado, 2011; Aristei and Perugini, 2014).

We use a sample of the (new) European Union member states from Central and Eastern Europe (East EU) and China.[17] These countries have already been shown to contribute to a large extent to changes in the global income distribution since the fall of the Berlin Wall (see Lakner and Milanovic, 2016).[18]

## 3.4 Descriptive statistics

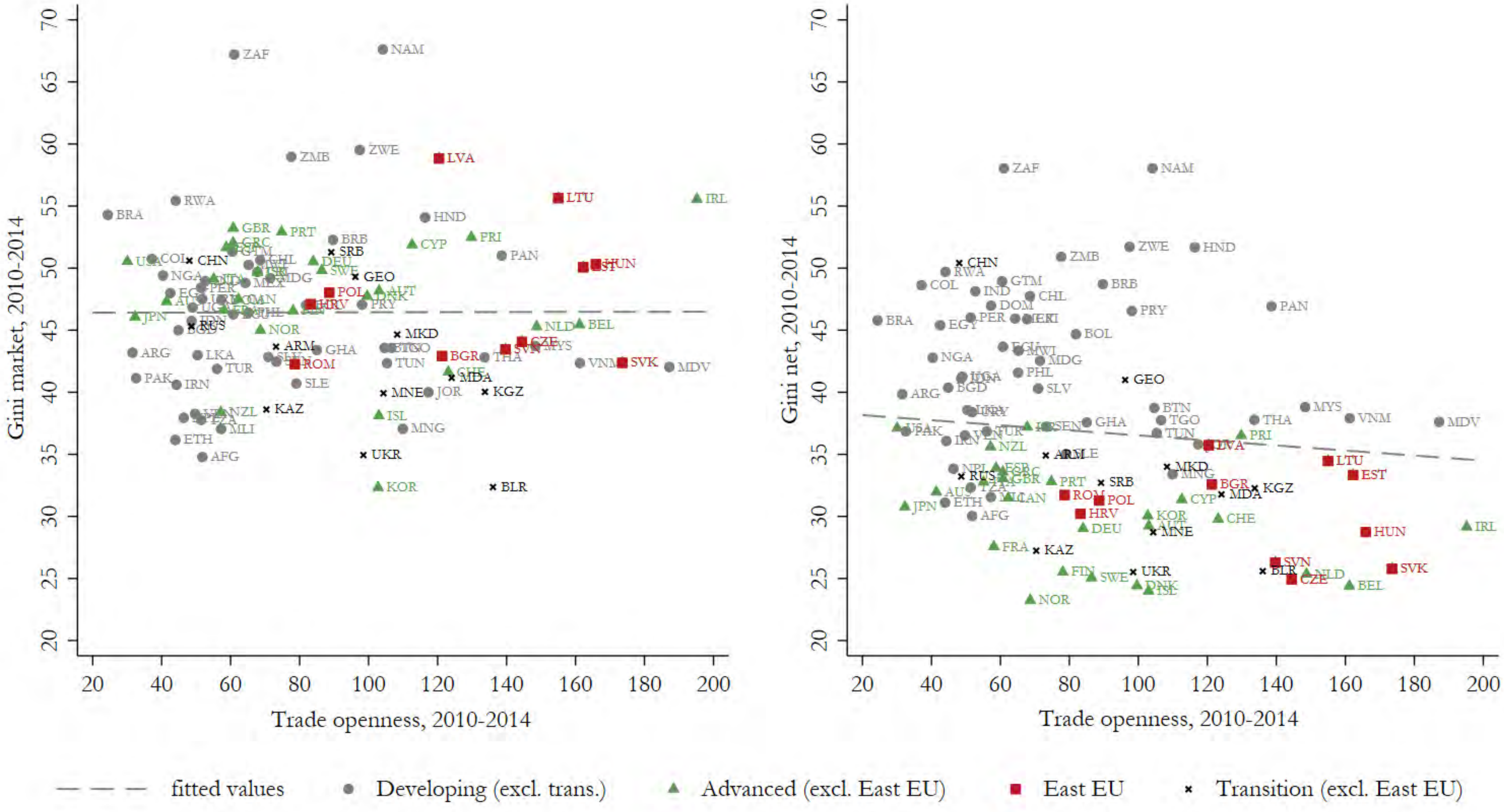
### 3.4.1 Trade openness and income inequality across countries

We examine the correlation between trade openness and income inequality across countries in the most recent five-year period of observation, 2010-2014: Income inequality before taxes and transfers is hardly correlated with trade openness (see Figure 3.1). The coefficient of correlation is 0.01.

The Gini index after tax and transfers is on average 9.8 index points lower than the Gini index value before redistribution in the period 2010-14. Net income inequality in open countries is, however, lower than in less open countries. The correlation coefficient between trade openness and the Gini net index is -0.17, indicating that more developed and open countries have larger welfare states. EU member states and other advanced economies are among the most open countries and have the world's lowest levels of income inequality after redistribution. The mean value of absolute redistribution in Gini index points (as measured by the difference between the Gini market and Gini net indices) is 17.32 in the sample of advanced economies compared to 5.93 in the sample of emerging and developing economies in the period 2010-14. This suggests why there is a negative relationship between trade openness and after tax/transfer income inequality across countries (see Figure 3.1). The role of the welfare state, however, varies within the group of advanced economies, for example with larger redistribution in EU15 countries (mean value of 20.53) than advanced non-EU countries (mean value of 14.78) such as the United States.

[17] We also test for further transition countries including former Soviet member states or satellite countries.
[18] The systematic change towards market economies and the rise of globalization in transition countries is discussed in more detail in Dorn *et al.* (2018).

Figure 3.1 : Trade openness and Gini income inequality, 2010-2014




*Source:* SWIID 5.1, World Bank (2017), own calculations.

*Notes:* Figure 3.1 relates to the full country sample within the period 2010-2014. The figure excludes Luxembourg and Singapore as outliers. Transition (excl. East EU) relate to former members of the Soviet Union (FSU, non-EU), Western Balkan (non-EU) states, and China. Unconditional correlations: $\beta_{market} = 0.005$; $\beta_{net} = -0.171^{*}$ ($^{*}p < 0.1$).

### 3.4.2 Trends across samples and countries

Trade openness and income inequality both increased quite rapidly between the late 1980s and the late 1990s; that is the first decade after the fall of the Berlin Wall in 1989 (Figure 3.2). There was a further increase in trade openness around the world in the 2000s.[19] The pre tax/transfer and post tax/transfer Gini indices, however, decreased since the early 2000s in EMD economies.[20] In a similar vein, income inequality has also not increased on average in the benchmark samples since its peak in the late 1990s. The pre tax/transfer Gini is around an index value of 47 since 2000. The post tax/transfer Gini index has even decreased since 2000. In the period 2010-2014, the Gini net indices in the benchmark sample (35.5) is about the same as in the period 20 years before. In advanced economies, the Gini net index has been around 31 since 2000, while market income inequality has increased in the same period of time. The differing trends in the mean values of the Gini indices before and after taxation and transfers indicate a rise of redistribution in the sample of advanced economies since the early 2000s. The mean level of redistribution increased by 1.5 Gini index points between the late 1980s and 2000, and again by about 1.7 points ever since. In the 2010-14 period, the mean level of redistribution are 17 points in advanced economies. Redistribution also increased in emerging and developing economies by about 1.3 points since the late 1980s, but the total level of redistribution (on average 5.2 points) is much lower in EMD economies than in advanced economies. Before taxation and transfers, income inequality is at a similar level in advanced and EMD economies. After taxation and transfers, inequality is much lower in advanced economies than in the emerging and developing world.[21]

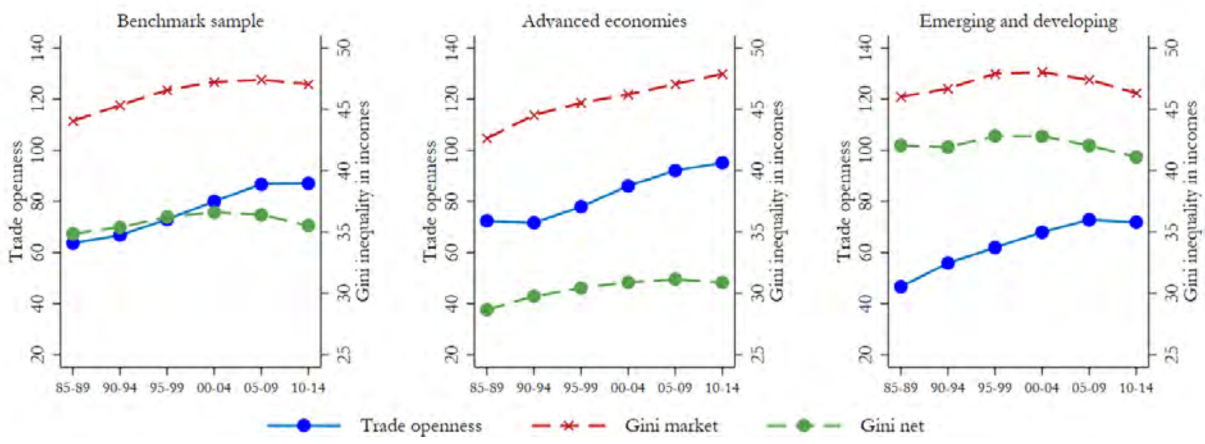
In Figure 3.3 we focus on changes in income inequality and trade openness in individual countries of our benchmark sample between the periods 1990-1994 and 2005-2009 (based on 69 countries from the benchmark sample having observations in both periods 1990-1994 and 2005-2009). The unconditional correlation between the changes in trade openness and the market and net income inequality is positive. The coefficients of correlation are 0.025 and 0.023. There are, however, two groups of countries that are the key drivers of the linear relationship between the late 1980s and late 2000s: First, Hong Kong, Luxembourg and Singapore are outliers regarding trade openness. Second, the transition countries in Eastern Europe and China experienced a huge opening process (globalization shift) and a huge rise

[19] Between the late 1980s and the period 2010-14, the average level of trade openness increased by 23 percentage points in the benchmark sample, by 25 points in advanced economies, and by 25 points in EMD economies.

[20] In the period 2010-2014, both the mean values of Gini market (46.3) and Gini net (41.1) are even lower in EMD economies than the mean values (46.7 and 41.9) of the period 1990-1994.

[21] In the EU15, post tax/transfer inequality is lower and redistribution higher than in other advanced regions such as the western offshores. The trends in inequality reflect the fact that countries of the western offshores such as the United States do have more market-oriented economic systems and less generous welfare states than their Scandinavian and continental European counterparts (see Fuest *et al.*, 2010; Doerrenberg and Peichl, 2014; Dorn and Schinke, 2018). Empirical research has shown how inequality dynamics differ among advanced economies during the last wave of globalization, with larger increases in income inequality in Anglo-Saxon countries such as the United States and less pronounced trends in Continental Europe (see Atkinson and Piketty, 2007; Dorn, 2016; Dorn and Schinke, 2018).

Figure 3.2 : Global trends in trade openness and Gini income inequality

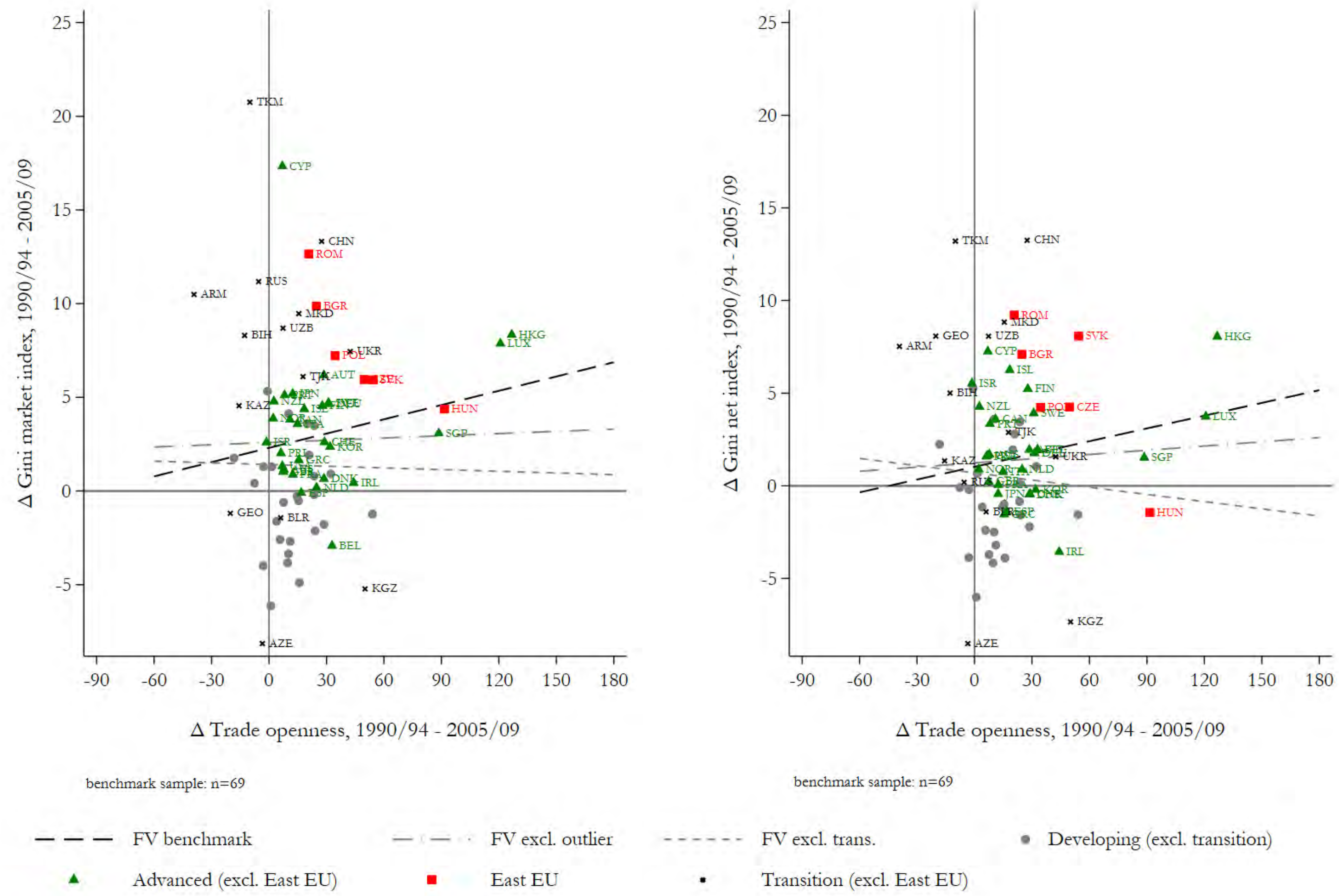



*Source:* SWIID 5.1, World Bank (2017), own calculations.
*Notes:* Trends between the periods 1985-1989 and 2010-2014. Unweighted mean of balanced samples. In the full sample, 63 of 140 countries have observations in all six periods, in the benchmark sample 47 of 82 countries, 24 of 34 countries within the sample of advanced economies, and 39 of 106 countries in the sample of emerging and developing economies (EMD).

in income inequality during that time.[22] The other countries from the benchmark sample also enjoyed rapidly increasing trade openness but experienced less pronounced increases in income inequality than Eastern European countries and China. When we exclude the outliers Hong Kong, Luxembourg and Singapore, the unconditional correlation between the change in trade openness and income inequality is almost zero (the coefficients are 0.004 and 0.008). After excluding outliers and transition countries, the unconditional correlation between the change in trade openness and income inequality is negative instead. The coefficients of correlation are -0.003 and -0.013 when we exclude transition countries and outliers from the benchmark sample. Within the sample of advanced economies, the changes in trade openness and income inequality outcomes are hardly correlated between the periods 1990-1994 and 2005-2009. The coefficients of correlation are 0.027 and 0.024. After excluding transition countries and outliers, the relationship between trade and the Gini inequality indices turns out to be negative. The coefficients of correlation are -0.072 and -0.078 in the remainder sample of advanced economies.

[22] Post-communist countries from Central and Eastern Europe (East EU) and the former Soviet Union (FSU) had relatively low levels of trade openness and income inequality before 1990. During their first stage of transition from centrally planned to market-based economies in the 1990s, both groups experienced a large rise in trade openness and income inequality (see Dorn *et al.*, 2018). While trade openness increased in both groups during the 2000s, inequality increased in new EU member countries from Central and Eastern Europe but decreased in the other countries of the former Soviet Union such as the Russian Federation (see Gorodnichenko *et al.*, 2010; Aristei and Perugini, 2014).

Figure 3.3 : Changes in trade openness and Gini income inequality, between 1990/94 and 2005/09




*Source:* SWIID 5.1, World Bank (2017), own calculations.
*Notes:* Figure 3.3 describes countries within the benchmark sample including high and middle income countries having observations in periods 1990-1994 and 2005-2009. Transition (excl. EU) captures former members of the Soviet Union, Western Balkan (Non-EU) states, and China. The balanced benchmark sample includes 69 countries. Hongkong, Luxembourg and Singapore are extreme outliers. After excluding outliers and transition countries, the balanced sample consists of 52 countries. Unconditional correlations in the benchmark sample: $\beta_{market} = 0.025, \beta_{net} = 0.023$, after excluding outliers $\beta_{market} = 0.004, \beta_{net} = 0.008$, and after excluding outliers and (EU and Non-EU) transition economies $\beta_{market} = -0.003, \beta_{net} = -0.013$; significance level: $^{*}p < 0.1$.

## 3.5 Empirical strategy

### 3.5.1 OLS panel fixed effects model

We estimate the baseline panel model by ordinary least squares (OLS), where countries are described by i and five-year periods by $\tau$:

$$Y_{i,\tau} = \beta \times TRADE_{i,\tau} + \mathbf{\Theta}' \times \boldsymbol{\chi_{i,\tau}} + \upsilon_i + \upsilon_\tau + \epsilon_{i,\tau} \tag{3.1}$$

$Y_{i,\tau}$ describes the measure of income inequality (Gini index, or relative income share by decile) of country i in period $\tau$. The explanatory variable $TRADE_{i,\tau}$ describes the trade openness of country i in period $\tau$. The vector $\chi_{i,\tau}$ includes control variables as described in Section 3.3.1, $\upsilon_i$ describes the country fixed effects, $\upsilon_\tau$ describes the fixed period effects, and $\epsilon_{i,\tau}$ is the error term. All variables are included as averages in each of the nine periods (t = 1,...,9).

By estimating OLS in a fixed effects (FE) model we exploit the within-country variation over time, eliminating any observable and unobservable country-specific time-invariant effects. We also include fixed time effects to control for other confounding factors (e.g., period-specific shocks) that influence multiple countries simultaneously. We use standard errors robust to heteroscedasticity clustered at the country level.

### 3.5.2 2SLS panel IV model

**Endogeneity problem and IV approach**
There are two reasons for potential endogeneity of trade openness in our model: omitted variable bias and reverse causality.

We included many control variables, but other unobserved omitted variables may give rise to biased estimates. The omitted variable bias indicates that there is still a third (or more) variable(s), which influence(s) both trade openness and income inequality. For example, increasing mobility may induce countries to reduce (capital) taxes and cut welfare benefits, which in turn, will influence disposable income and probably also employment. If competition from countries with cheap labor induces companies in high income countries to specialize in the production of high-tech goods and services, which requires highly skilled labor, this will have an impact on the skill premium. It is difficult to disentangle these effects from the 'direct' influence of trade openness on income inequality, that is the influence of trade openness, given other factors.

Second, reverse causality may occur because changes in income inequality are likely to influence policies that affect trade openness. The debate on the Transatlantic Trade and Investment Partnership (TTIP), for instance, is also influenced by the perception that gains from trade may be distributed rather unevenly. Shifts in the income distribution within a country may also have direct effects on the trade openness level of the country, for example if more people are able to travel, to buy more expensive import goods or to make international investments and savings.

To deal with the endogeneity problem of trade openness, we use predicted openness based on a gravity equation as an IV. Frankel and Romer (1999) apply predicted openness in a cross-sectional approach. We want to exploit exogenous time variation in predicted openness using the IV in a panel model and controlling for unobserved country effects (see Feyrer, 2009; Felbermayr and Gröschl, 2013). We employ the exogenous component of variations in openness predicted by geography and time-varying natural disasters in foreign countries, as proposed by Felbermayr and Gröschl (2013) for a panel data model, as an IV for trade openness. Based on a modified gravity framework, Felbermayr and Gröschl (2013) show that the incidence of natural disasters such as earthquakes, hurricanes or volcanic eruptions in one country influences the openness of its trading partners, depending on the two countries' geographic proximity.[23] An earthquake hitting Mexico, for example, will increase international trade of other countries with Mexico. The rise in a country's trade openness level will be larger, the closer a country is located to Mexico.

**Instrument construction**

The predicted openness by Felbermayr and Gröschl (2013) is constructed in two steps: First, exogeneous natural disasters are included in a gravity model to predict bilateral trade openness. Bilateral openness $\hat{\omega}_t^{i,j}$ describes trade flows between country i and country j in year t and is predicted by a reduced[24] gravity model using a Poisson Pseudo Maximum Likelihood (PPML) estimation to account for zero trade flows and standard errors clustered by country pairs.

Bilateral openness $\hat{\omega}_t^{i,j}$ is regressed on variables exogenous to income inequality such as large-scale natural disasters in foreign countries j, interactions of the incidence of natural disasters in foreign countries j and bilateral geographic variables, or population. Felbermayr and Gröschl (2013) estimate

$$\hat{\omega}_t^{i,j} = exp[\delta \times D_t^j + \boldsymbol{\gamma}' \times \boldsymbol{Z_t^{i,j}} + \boldsymbol{\lambda}' \times (\boldsymbol{\Phi_t^{i,j}} \times D_t^j) + \upsilon^i + \upsilon^j + \upsilon_t + \epsilon_t^{i,j}] \quad (3.2)$$

where $Z_t^{i,j}$ = $[lnPOP_t^i; lnPOP_t^j; lnDIST^{i,j}; BOR^{i,j}]$ includes exogenous controls such as population ($POP$) in countries i and j in year t, and the bilateral geographic variables distance $DIST$, and a common border dummy $BOR$, based on Frankel and Romer (1999). $D_t^j$ denotes exogenous large-scale natural disasters in country j, while $\Phi_t^{i,j}$ = $[lnFINDIST_t^j; lnAREA^j;$

[23] For example, the effect of an earthquake in Mexico will be stronger for trade flows of Honduras or the United States than those of India.

[24] The reduced form of the gravity model differs from standard (trade) gravity models by excluding variables that would be correlated to income inequality such as GDP per capita.

$lnPOP_t^j; BOR^{i,j}]$ describes the exogenous variables interacting with $D_t^j$, such as the international financial remoteness $FINDIST$, the surface area $AREA$, or population $POP$ of country j.[25] Country and time fixed effects[26] are captured by $\upsilon^i$, $\upsilon^j$, $\upsilon_t$, while $\epsilon_t^{i,j}$ accounts for the idiosyncratic error. The bilateral openness equation (2) is designed to maximize conditional correlation between observed trade openness and the constructed instrument (see relevance of the instrument below).

We follow the approach preferred by Felbermayr and Gröschl (2013) and use truly exogenous "large" scale natural disasters (as $D_t^j$) to make sure that a disaster is of a sufficiently large dimension and caused not by local determinants or the development level of the country but rather by exogenous global phenomena. This classification of natural disasters includes "large" earthquakes, droughts, storms, storm floods, and volcanic erruptions that (i) caused 1,000 or more deaths; or (ii) injured 1,000 or more people; or (iii) affected 100,000 or more people. In our robustness checks, we use alternative definitions of disasters to construct the instrument, such as a broader specification of disasters that includes all kinds of natural disasters[27] or counting all sizes of disasters (Section 3.6.4). Felbermayr and Gröschl (2013) use data on natural disasters taken from the Emergency Events database (EM-DAT).

In the second step of constructing the IV, Felbermayr and Gröschl (2013) use an exogenous proxy for multilateral openness $\Omega_{i,t}$ by aggregating the obtained predicted bilateral openness values $\hat{\omega}_t^{i,j}$ of country i over all bilateral country pairs and years t:[28]

$$\Omega_{i,t} = \sum\nolimits_{i \neq j} \hat{\omega}_t^{i,j} \tag{3.3}$$

Based on our underlying data, we obtain values for all years from 1966 to 2008. Averaging over nine periods $\tau$ and using one-period lags of predicted openness $\Omega_{i,\tau-1}$, we obtain our instrument for $TRADE_{i,\tau}$ in equation (3.1).

#### Relevance of the instrument

The relevance of the IV predicted openness $\Omega_{i,\tau-1}$ depends on its conditional correlation with trade openness $TRADE_{i,\tau}$. The first stage regression has the following form:

[25] As large-scale natural disasters may hit both bordering countries, the interaction of disasters and the common border dummy is included. Interactions of the disaster variable with surface area and population in country j consider the fact that economic and population density matters for the aggregate damage caused by large-scale natural disasters. The interaction of disasters with financial remoteness is motivated by related literature (Felbermayr and Gröschl, 2013).

[26] Time fixed effects also account for improved reporting of natural disasters and its consequences over time (Felbermayr and Gröschl, 2013).

[27] Natural disasters caused by extreme temperature, floods, (mud)slides, or wildfires are also included in this extended definition of natural disasters. Epidemics are not included in any of our classifications.

[28] The instrument in equation 3.3 is constructed based on all available trade partners in the raw data following Felbermayr and Gröschl (2013). The sample includes more countries than our full sample of 139 countries.

$$TRADE_{i,\tau} = \alpha \times \Omega_{i,\tau-1} + \boldsymbol{\varphi}' \times \boldsymbol{\chi_{i,\tau}} + \upsilon^j + \upsilon_t + \epsilon_t^{i,j} \tag{3.4}$$

The model is estimated by applying the FE estimator, controlling for any time-invariant country characteristics, and using robust standard errors clustered at the country level. The first stage also includes all control variables $\chi_{i,\tau}$ as in equation (3.1) and period dummies to control for common period effects.

The first stage regression results show that the IV is relevant (see Appendix, Table A3.1). Our predicted openness variable is qualitatively good and correlates positively with trade openness ($TRADE$). The relationship is statistically significant at the 1 % level in the full sample, the benchmark sample and in the sample of advanced economies. In the sample of developing economies, the statistical significance is at the 10 % level. The Cragg-Donald Wald F-statistics on the excluded instrument are well above the 10 % critical value ($F \geq 16.38$) of the weak instrument test by Stock and Yogo (2005). The partial $R^2$ of lagged predicted openness ranges between 2.4 % in the sample of developing economies and 23.3 % in the sample of advanced economies.

**Exclusion restriction**

Income inequality does not influence predicted openness because the instrument is constructed from exogenous components, such as large-scale natural disasters and bilateral geographic components. We do not believe that predicted openness influences income inequality directly or through other explanatory variables that we did not include in our model. Predicted openness is an arguably excludable instrument. Foreign natural disasters are expected to have no effect on income inequality other than through the extent of trade openness or other indicators of globalization, e.g., international transactions and migration. We control for other globalization indicators such as FDIs and social and political globalization in our regression models. Migration is included in the social globalization index and we control for migration as an individual variable in our robustness tests.[29]

Large-scale natural disasters may give rise to changes in the income distribution. Felbermayr and Gröschl (2013, 2014), for example, have shown that natural disasters influence overall per capita income. Some natural disasters are registered across borders. Natural disasters

[29] One may want to maintain that the exclusion restriction is not fulfilled because natural disasters that occur in the trading partner countries (which are often direct geographical neighbors) give rise to migration. For example, when a natural disaster occurs in Mexico, especially poor Mexican citizens are likely to leave Mexico and migrate to a neighboring country such as Honduras. If this is true, the natural disaster that hit Mexico (and gave rise to the exogenous variation in our instrumental variable predicted openness) influenced trade openness and income inequality in Honduras. Empirical studies show, however, that natural disasters hardly give hardly rise to international migration in the medium and long term (see Gröschl and Steinwachs, 2017).

registered in the home country might have a direct impact on the home country's income distribution. To mitigate any potential omitted variable bias because of cross-border natural disasters we directly control for the effect of large-scale natural disasters in the home country.[30]

## 3.6 Results

### 3.6.1 Baseline results

We examine the average effect of trade openness on Gini income inequality and redistribution in our full and benchmark sample. Our results in Table 3.1 do not suggest a statistically significant relationship between trade openness and income inequality in the full sample and benchmark sample — estimating the models by OLS (columns 1-6) and 2SLS (columns 7-12) notwithstanding. The coefficients of trade openness have a negative sign in any specification when we use redistribution as dependent variable, but again lack statistical significance. Overall, our baseline specifications do not confirm that trade openness influences inequality within countries when we use large country samples.

The baseline results in Table 3.1 also show the coefficients of correlation between inequality and our control variables. FDIs and large-scale natural disasters increase income inequality both before and after redistribution. The Gini market index and redistribution increases when the share of dependents increases. Redistribution is also significantly higher in richer countries, but is decreasing when the size of population is increasing. Population and inequality are negatively correlated before tax and transfers.

Table 3.2 shows the baseline 2SLS results when we use the relative net income shares (by deciles) as the dependent variables. The results in Table 3.2 corroborate our baseline results when using the Gini index as the dependent variable in the full sample (panel a), indicating that the relationship between trade openness and income inequality lacks statistical significance. The relationship between trade openness and relative income shares in the benchmark sample is more pronounced (panel b). The coefficient estimate of trade openness is negative when the relative income shares of the lower income deciles 1 to 7 are used as dependent variables and positive when the relative income shares of the three highest income share deciles are used as dependent variables. But the coefficient estimates are rather small. The effect of trade openness, however, is only statistically significant for the upper middle class in the 9th decile (column 9 of Table 3.2). The coefficient is significant at the 5 % level and indicates that the income share of decile (9) increased by 0.12 percentage points when trade openness increased by ten percentage points.

[30] The gravity model also includes population growth to construct predicted openness. We control for population growth as baseline control in the OLS and IV regressions.

Table 3.1 : Trade openness and income inequality – baseline results (OLS and 2SLS)

| | OLS | | | | | | 2SLS | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Full sample | | | Benchmark sample | | | Full sample | | | Benchmark sample | | |
| | (1) Gini market | (2) Gini net | (3) Redistr. | (4) Gini market | (5) Gini net | (6) Redistr. | (7) Gini market | (8) Gini net | (9) Redistr. | (10) Gini market | (11) Gini net | (12) Redistr. |
| Trade openness | 0.00817 | 0.0110 | -0.00280 | -0.00872 | -0.00107 | -0.00764 | -0.0658 | -0.0276 | -0.0382 | 0.000943 | 0.0232 | -0.0223 |
| | (0.0133) | (0.0106) | (0.00760) | (0.0153) | (0.0113) | (0.0105) | (0.0692) | (0.0563) | (0.0261) | (0.0418) | (0.0339) | (0.0230) |
| GDP p.c. | 0.0952 | 0.0235 | 0.0717*** | 0.0955 | 0.00434 | 0.0912*** | 0.150 | 0.0522 | 0.0981** | 0.0880 | -0.0146 | 0.103** |
| | (0.0590) | (0.0500) | (0.0269) | (0.0575) | (0.0460) | (0.0293) | (0.0916) | (0.0673) | (0.0415) | (0.0743) | (0.0517) | (0.0418) |
| Population (log) | -5.322* | -2.298 | -3.024** | -2.873 | 1.146 | -4.019* | -5.964** | -2.633 | -3.331** | -2.660 | 1.682 | -4.342* |
| | (2.835) | (2.203) | (1.465) | (3.969) | (3.075) | (2.310) | (2.842) | (2.204) | (1.507) | (3.858) | (3.186) | (2.294) |
| Age dependency | 0.129** | 0.0666 | 0.0625*** | 0.193*** | 0.140** | 0.0529* | 0.101 | 0.0518 | 0.0489** | 0.197*** | 0.151*** | 0.0466* |
| | (0.0509) | (0.0436) | (0.0189) | (0.0692) | (0.0597) | (0.0278) | (0.0613) | (0.0513) | (0.0198) | (0.0683) | (0.0578) | (0.0279) |
| Social glob. | 0.0618 | 0.0252 | 0.0365 | 0.0431 | 0.000700 | 0.0424 | 0.0604 | 0.0245 | 0.0359 | 0.0435 | 0.00156 | 0.0419 |
| | (0.0507) | (0.0400) | (0.0309) | (0.0522) | (0.0399) | (0.0387) | (0.0498) | (0.0375) | (0.0323) | (0.0508) | (0.0407) | (0.0382) |
| Political glob. | -0.0346 | -0.0173 | -0.0173 | -0.0102 | 0.00560 | -0.0158 | -0.0212 | -0.0103 | -0.0109 | -0.0131 | -0.00176 | -0.0113 |
| | (0.0369) | (0.0303) | (0.0191) | (0.0464) | (0.0379) | (0.0287) | (0.0395) | (0.0312) | (0.0203) | (0.0467) | (0.0377) | (0.0290) |
| FDI | 0.0695*** | 0.0426*** | 0.0269** | 0.0777*** | 0.0437*** | 0.0341* | 0.0783*** | 0.0472*** | 0.0311*** | 0.0777*** | 0.0437*** | 0.0341** |
| | (0.0208) | (0.0154) | (0.0116) | (0.0260) | (0.0162) | (0.0176) | (0.0222) | (0.0168) | (0.0117) | (0.0254) | (0.0159) | (0.0174) |
| Nat. disasters (t-1) | 2.103*** | 2.115*** | -0.0120 | 2.390*** | 2.450*** | -0.0607 | 2.255*** | 2.194*** | 0.0605 | 2.377*** | 2.419*** | -0.0416 |
| | (0.377) | (0.478) | (0.192) | (0.315) | (0.392) | (0.226) | (0.345) | (0.445) | (0.215) | (0.317) | (0.403) | (0.227) |
| *Fixed effects* | | | | | | | | | | | | |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Period FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Countries | 139 | 139 | 139 | 82 | 82 | 82 | 139 | 139 | 139 | 82 | 82 | 82 |
| Observations | 794 | 794 | 794 | 516 | 516 | 516 | 794 | 794 | 794 | 516 | 516 | 516 |
| Partial $R^2$ | | | | | | | | 0.067 | | | 0.131 | |
| F Test, weak ID | | | | | | | | 45.573 | | | 62.899 | |
| F Test, p-value | | | | | | | | 0.000 | | | 0.000 | |

*Notes:* OLS and 2SLS panel fixed effects estimations based on nine periods using 5-year averages between 1970 and 2014. Clustered robust standard errors in parentheses. Weak ID test using Cragg-Donald Wald F-statistic. Stock and Yogo (2005) weak ID critical value: 16.38 (10 %). Significance levels: ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Table 3.2 : Trade openness and income inequality – subsample results (2SLS)

| | (1) D1 | (2) D2 | (3) D3 | (4) D4 | (5) D5 | (6) D6 | (7) D7 | (8) D8 | (9) D9 | (10) D10 | (11) Gini market | (12) Gini net | (13) Redistr. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| ***(a) Full sample*** | | | | | | | | | | | | | |
| Trade openness | -0.000703 | -0.00229 | -0.00168 | -0.000640 | 0.000542 | 0.00174 | 0.00288 | 0.00384 | 0.00442 | -0.00835 | -0.0666 | -0.0284 | -0.0383 |
| | (-0.08) | (-0.29) | (-0.23) | (-0.09) | (0.08) | (0.30) | (0.57) | (0.78) | (0.51) | (-0.20) | (-0.97) | (-0.51) | (-1.46) |
| Countries | 136 | 136 | 136 | 136 | 136 | 136 | 136 | 136 | 136 | 136 | 136 | 136 | 136 |
| Observations | 783 | 783 | 783 | 783 | 783 | 783 | 783 | 783 | 783 | 783 | 783 | 783 | 783 |
| Partial $R^2$ | 0.067 | | | | | | | | | | | | |
| F Test, weak ID | 45.593 | | | | | | | | | | | | |
| F Test pvalue | 0.000 | | | | | | | | | | | | |
| ***(b) Benchmark sample*** | | | | | | | | | | | | | |
| Trade openness | -0.00707 | -0.00861 | -0.00826 | -0.00735 | -0.00600 | -0.00411 | -0.00137 | 0.00302 | 0.0117** | 0.0280 | 0.000222 | 0.0228 | -0.0225 |
| | (-1.02) | (-1.41) | (-1.47) | (-1.42) | (-1.28) | (-0.99) | (-0.37) | (0.81) | (2.00) | (0.89) | (0.01) | (0.68) | (-0.99) |
| Countries | 81 | 81 | 81 | 81 | 81 | 81 | 81 | 81 | 81 | 81 | 81 | 81 | 81 |
| Observations | 513 | 513 | 513 | 513 | 513 | 513 | 513 | 513 | 513 | 513 | 513 | 513 | 513 |
| Partial $R^2$ | 0.134 | | | | | | | | | | | | |
| F Test, weak ID | 64.572 | | | | | | | | | | | | |
| F Test pvalue | 0.000 | | | | | | | | | | | | |
| ***(c) Advanced econ.*** | | | | | | | | | | | | | |
| Trade openness | -0.00867** | -0.00779* | -0.00667 | -0.00537 | -0.00382 | -0.00191 | 0.000522 | 0.00389 | 0.00942*** | 0.0204 | -0.0287 | 0.00350 | -0.0321 |
| | (-2.12) | (-1.94) | (-1.51) | (-1.14) | (-0.81) | (-0.42) | (0.13) | (1.28) | (2.88) | (0.69) | (-0.79) | (0.16) | (-1.00) |
| Countries | 34 | 34 | 34 | 34 | 34 | 34 | 34 | 34 | 34 | 34 | 34 | 34 | 34 |
| Observations | 244 | 244 | 244 | 244 | 244 | 244 | 244 | 244 | 244 | 244 | 244 | 244 | 244 |
| Partial $R^2$ | 0.233 | | | | | | | | | | | | |
| F Test, weak ID | 58.875 | | | | | | | | | | | | |
| F Test pvalue | 0.000 | | | | | | | | | | | | |
| ***(d) Developing econ.*** | | | | | | | | | | | | | |
| Trade openness | 0.0317* | 0.0240 | 0.0219 | 0.0203 | 0.0183 | 0.0148 | 0.00900 | -0.00151 | -0.0231 | -0.117 | -0.235 | -0.212 | -0.0233 |
| | (1.71) | (1.41) | (1.25) | (1.13) | (1.03) | (0.88) | (0.61) | (-0.12) | (-1.07) | (-1.04) | (-1.09) | (-1.14) | (-0.50) |
| Countries | 102 | 102 | 102 | 102 | 102 | 102 | 102 | 102 | 102 | 102 | 102 | 102 | 102 |
| Observations | 539 | 539 | 539 | 539 | 539 | 539 | 539 | 539 | 539 | 539 | 539 | 539 | 539 |
| Partial $R^2$ | 0.025 | | | | | | | | | | | | |
| F Test, weak ID | 10.821 | | | | | | | | | | | | |
| F Test pvalue | 0.072 | | | | | | | | | | | | |

*Notes:* 2SLS panel fixed effects estimations based on nine periods using 5-year averages between 1970 and 2014. T-statistics in parentheses. Robust standard errors clustered at the country level. All specifications include country and year fixed effects, and baseline control variables (see Table 1): GDP per capita, population(log), dependecy ratio, social globalization index, political globalization index, FDI index, and large scale natural disasters. Weak ID test using Cragg-Donald Wald F-statistic. Stock and Yogo (2005) weak ID critical values: 16.38 (10 %), 8.96 (15 %). Significance levels: ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

### 3.6.2 The role of development levels

The effect of trade openness on income inequality is likely to differ depending on the development level of countries. The classical Stolper-Samuelson-theorem (Stolper and Samuelson, 1941), for example, predicts that trade openness increases inequality in advanced economies, but favors relative income shares of the poor in developing countries. Empirical studies, however, have shown poor performance of the Stolper-Samuelson-theorem (e.g., Leamer, 1998). We therefore examine two subsamples depending on the development level of countries: the sample of 34 advanced economies and the sample of 102 emerging markets and developing economies (see Table 3.2, panel c and d). The instrument is relevant within both subsamples. The Cragg-Donald Wald F-statistic is above the 10 % and 15 % critical values.

We examine how trade openness influences Gini inequality and redistribution indices. 2SLS results in Table 3.2 do not show that trade openness influences income inequality or redistribution when we use Gini market and Gini net indices or Gini redistribution as dependent variables (columns 11-13), neither within the most advanced economies (panel c) nor within the sample of emerging and developing economies (panel d).

We also examine how trade openness influences the relative net income shares in Table 3.2 (columns 1-10). Within the advanced economies, the results suggest that trade openness increased income inequality. Table 3.2 shows that trade openness decreased the relative net income shares of the lowest income deciles and increased the relative net income shares of the upper middle class income deciles (panel c). The effect is negative and significant for the two lowest income deciles (panel c, columns 1-2) and positive and statistically significant for the 9th decile (panel c, column 9). The coefficient, however, indicates a rather small effect. The income share of the upper middle class (decile 9) increased by 0.09 percentage points when trade openness increased by 10 percentage points. Within the emerging and developing world, our results suggest that trade openness tends to decrease income inequality. Trade openness tends to decrease income shares of the upper deciles and to increase income shares of the poor and middle class within the emerging and developing economies. Trade openness, however, also lacks statistical significance in almost all specifications in Table 3.2, panel (d). The exception is the coefficient estimate in panel (d), column (1), suggesting a rather positive effect of trade openness on the relative income share of the poorest in the income distribution of emerging and developing countries. The coefficient indicates that the bottom 10 % income share (decile 1) increased by 0.3 percentage points when trade openness increased by 10 percentage points.

Our 2SLS results based on relative income shares as the dependent variable are in line with predictions of the Stolper-Samuelson theorem. Within developing economies our findings suggest that the poorest people disproportionately gain from trade openness at the expense of the relative income shares of higher income deciles. Within advanced economies our findings suggest that the upper middle class disproportionately gain from trade openness at the expense of the relative income shares of bottom deciles.

The findings suggest that trade openness influences income inequality, both within our benchmark country sample and within advanced economies. The benchmark sample includes the advanced economies sample and the 48 emerging economies having a per capita income level above a minimum threshold (not including developing countries having a GNI per capita below USD 4,126, as of 2015). As coefficient estimates of trade openness in the benchmark sample are larger than in the sample of advanced economies, and 41.5 percent of countries in the benchmark sample are advanced economies, other countries within the benchmark sample might be the main drivers of the significant positive effect of trade openness on income inequality.

### 3.6.3 Outliers and transition countries

The unconditional relationship between the change in trade openness and income inequality seems to be driven by outliers in trade openness and by Central and Eastern European transition countries (East EU) and China (see Section 3.4). We therefore examine the effect of trade openness on income inequality when we exclude outliers and transition countries. The results are shown in Table 3.3.

First, we exclude Singapore as an outlier in trade openness from the sample of advanced economies (Table 3.3, panel a) and the benchmark sample of high and upper middle income countries (Table 3.3, panel b). The results in Table 3.3 show that all coefficient estimates lack statistical significance after excluding Singapore, both in the advanced economies and in the benchmark sample. Within the remaining 33 advanced economies, the coefficient estimates for trade openness are positive for the effect on the bottom 70 % income share (panel a, columns 1-7) and negative for the effect on income shares of the upper 30 % (panel a, columns 8-10) after excluding the nine observations for Singapore. Within the remaining benchmark sample of 80 countries, the coefficient estimates for trade openness hare positive for the bottom 20 % and top 20 % income shares (panel b, columns 1-2 and 9-10) and negative for the deciles in the middle class (panel b, columns 3-8) after excluding observations for Singapore.

Second, we exclude China and the East EU transition countries from the benchmark sample of high and upper middle income countries (Table 3.3, panel c). The coefficients of the trade openness variables become smaller and do not turn out to be statistically significant when we exclude China and the East EU transition countries. After excluding China and transition economies, the coefficient estimate of trade openness on the income share of the 9th decile in column (9) is 0.008 and lacks statistical significance — it is 0.012 at the 5 % significance level when China and transition economies are included (Table 3.2, panel b). In a similar vein, the coefficient of trade openness for the effect on the top 10 % income share is 0.003 for the remainder benchmark sample (column 10). It is 0.028 when China and transition economies are included (Table 3.2, panel b). This effect suggests that trade openness especially

Table 3.3 : The role of outliers and transition economies (2SLS)

| | (1) D1 | (2) D2 | (3) D3 | (4) D4 | (5) D5 | (6) D6 | (7) D7 | (8) D8 | (9) D9 | (10) D10 | (11) Gini market | (12) Gini net | (13) Redistr. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| ***(a) Advanced, excl. outlier*** | | | | | | | | | | | | | |
| Trade openness | 0.00728 | 0.00503 | 0.00386 | 0.00304 | 0.00231 | 0.00150 | 0.000403 | -0.00131 | -0.00432 | -0.0178 | -0.0306 | -0.000956 | -0.0297 |
| | (0.95) | (0.72) | (0.55) | (0.44) | (0.36) | (0.26) | (0.08) | (-0.27) | (-0.60) | (-0.44) | (-0.49) | (-0.03) | (-0.50) |
| Countries | 33 | 33 | 33 | 33 | 33 | 33 | 33 | 33 | 33 | 33 | 33 | 33 | 33 |
| Observations | 235 | 235 | 235 | 235 | 235 | 235 | 235 | 235 | 235 | 235 | 235 | 235 | 235 |
| Partial $R^2$ | | | | | | | 0.186 | | | | | | |
| F Test, weak ID | | | | | | | 42.613 | | | | | | |
| F Test, p-value | | | | | | | 0.000 | | | | | | |
| ***(b) Benchmark, excl. outlier*** | | | | | | | | | | | | | |
| Trade openness | 0.00630 | 0.000605 | -0.00177 | -0.00316 | -0.00404 | -0.00452 | -0.00449 | -0.00348 | 0.000570 | 0.0140 | -0.0227 | -0.00786 | -0.0148 |
| | (1.26) | (0.11) | (-0.30) | (-0.51) | (-0.62) | (-0.68) | (-0.68) | (-0.55) | (0.09) | (0.31) | (-0.50) | (-0.21) | (-0.54) |
| Countries | 80 | 80 | 80 | 80 | 80 | 80 | 80 | 80 | 80 | 80 | 80 | 80 | 80 |
| Observations | 504 | 504 | 504 | 504 | 504 | 504 | 504 | 504 | 504 | 504 | 504 | 504 | 504 |
| Partial $R^2$ | | | | | | | 0.126 | | | | | | |
| F Test, weak ID | | | | | | | 58.531 | | | | | | |
| F Test, p-value | | | | | | | 0.000 | | | | | | |
| ***(c) Excl. transition econ.*** | | | | | | | | | | | | | |
| Trade openness | -0.00174 | -0.00316 | -0.00313 | -0.00274 | -0.00209 | -0.00111 | 0.000357 | 0.00279 | 0.00765 | 0.00317 | -0.0454 | -0.0120 | -0.0334 |
| | (-0.26) | (-0.54) | (-0.55) | (-0.50) | (-0.41) | (-0.24) | (0.09) | (0.75) | (1.33) | (0.09) | (-1.16) | (-0.39) | (-1.34) |
| Countries | 69 | 69 | 69 | 69 | 69 | 69 | 69 | 69 | 69 | 69 | 69 | 69 | 69 |
| Observations | 454 | 454 | 454 | 454 | 454 | 454 | 454 | 454 | 454 | 454 | 454 | 454 | 454 |
| Partial $R^2$ | | | | | | | 0.139 | | | | | | |
| F Test, weak ID | | | | | | | 59.729 | | | | | | |
| F Test, p-value | | | | | | | 0.000 | | | | | | |
| ***(d) Transition effect*** | | | | | | | | | | | | | |
| Trade openness | -0.00333 | -0.00538 | -0.00540 | -0.00485 | -0.00390 | -0.00251 | -0.000453 | 0.00286 | 0.00939 | 0.0136 | -0.0301 | -0.00195 | -0.0282 |
| | (-0.48) | (-0.89) | (-0.95) | (-0.90) | (-0.79) | (-0.56) | (-0.12) | (0.74) | (1.56) | (0.41) | (-0.76) | (-0.06) | (-1.15) |
| Trade*Transition | -0.0167* | -0.0145 | -0.0128 | -0.0112 | -0.00938 | -0.00716 | -0.00412 | 0.000701 | 0.0106 | 0.0646 | 0.136** | 0.111* | 0.0253 |
| | (-1.90) | (-1.58) | (-1.44) | (-1.34) | (-1.24) | (-1.07) | (-0.72) | (0.13) | (1.10) | (1.41) | (2.00) | (1.79) | (0.94) |
| Countries | 81 | 81 | 81 | 81 | 81 | 81 | 81 | 81 | 81 | 81 | 81 | 81 | 81 |
| Observations | 513 | 513 | 513 | 513 | 513 | 513 | 513 | 513 | 513 | 513 | 513 | 513 | 513 |
| Partial $R^2$ | | | | | | | 0.131 | | | | | | |
| F Test, weak ID | | | | | | | 30.608 | | | | | | |
| F Test, p-value | | | | | | | 0.001 | | | | | | |

*Notes:* Singapore excluded as outlier in panels (a) and (b). Transition economies include China and East-EU member states (Bulgaria, Croatia, Czech Republic, Estonia, Hungary, Latvia, Lithuania, Poland, Romania, Slovakia, Romania). 2SLS panel fixed effects estimations based on nine periods using 5-year averages between 1970 and 2014. T- statistics in parentheses. Robust standard errors clustered at the country level. All specifications include country and year fixed effects, and baseline control variables (see Table 1): GDP per capita, population(log), dependecy ratio, social globalization index, political globalization index, FDI index, and large scale natural disasters. Weak ID test using Cragg-Donald Wald F-statistic. Stock and Yogo (2005) weak ID critical values: 16.38 (10 %). Significance levels: ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

increased relative income shares of very rich citizens in China and Eastern European transition economies.[31] After excluding China and transition economies, the coefficients even turn negative when we use Gini indices as dependent variables.

The results suggest that China and the Eastern European countries drive the effect of trade openness on income inequality. We therefore include an interaction effect of trade openness and the sample of China and transition economies in Table 3.3, panel (d). The trade openness variable lacks statistical significance in any specification (columns 1-13). Trade openness, however, has a positive effect on Gini income inequality in transition countries (columns 11-12). The interaction effect is statistically significant at the 5 % and 10 % levels and suggests that Gini inequality in transition economies increases by an additional 1.4 index points (Gini market) and 1.1 points (Gini net) when trade openness increases by 10 percentage points. The negative interaction effect of trade openness on the income shares of the bottom 10 % of transition countries is also statistically significant (Table 3.3, panel d, column 1).[32]

### 3.6.4 Robustness checks

We tested the sensitivity of our baseline results in many ways. First, we replaced social globalization by the log of international migration (as share of population) as covariate (Appendix, Table A3.4). Inferences regarding the relationship of trade openness and income inequality do not change. The migration share lacks statistical significance. We follow related studies and use the ICT capital stock as a proxy to control for technological change (Jaumotte *et al.*, 2013). While the ICT capital stock is positively related to changes in the Gini inequality outcomes and negatively related to redistribution in all OLS and 2SLS models, inferences about the relationship between trade openness and income inequality do not change (Appendix, Table A3.5). ICT capital stock is only statistically significant in the full sample when we use Gini inequality after tax and transfers as dependent variable.

Second, we followed related literature and used periods with five-year averages in our baseline models. We tested the robustness of the results by including a larger frequency with shorter time periods of three-year averages (Appendix, Tables A3.6 and A3.7). Inferences do not change. Our results show that trade openness increased the income share of the 9th decile within the benchmark sample and the 8th and 9th decile within advanced economies. Within advanced economies, the negative effect on the poor is significant for the bottom 30 % of the income distribution. Within emerging and developing economies, the trade openness variable lacks statistical significance in any specification. The coefficient for the bottom 10 % is statistically significant when we use five-year averaged periods. The t-statistic of the coefficient estimate is 1.55 when we use three-year averaged periods (Appendix, Table A3.7, panel d). The estimates

[31] The coefficient estimates for the middle class income shares (deciles 3-8) are negative in the remainder country sample.

[32] We also examined the effect of trade openness on inequality within the sample of emerging and developing economies when we exclude transition countries (Appendix, Table A3.3). Estimates however suffer from a weak ID after excluding transition economies.

in the sample of emerging and developing economies suffer from a weak ID when we use three-year averaged periods. The Cragg-Donald Wald F-statistic is 10.88 and below the 10 % critical value by Stock and Yogo (2005). The p-value on the excluded instrument is above 0.1. Using five-year averaged periods is therefore preferred over three-year averaged periods to obtain valid IV results for the sample of emerging and developing economies.

Third, the descriptive statistics in Section 3.4 suggest that there are trends over time. We therefore used the trend rather than the period fixed effects in a robustness test (Appendix, Table A3.8). Inferences of our results on the trade-inequality nexus do not change. The trend variable shows that redistribution is increasing over time at a statistical significant level, in the full and benchmark sample and OLS or 2SLS models notwithstanding.

Fourth, inequality measures might be persistent across periods. We therefore included lagged dependent variables to allow for dynamics that give rise to serial correlation. Our main results, however, do not change.[33] Trade openness has a negative effect on redistribution in the full country sample when we include the lagged redistribution variable. In the benchmark sample, the trade openness coefficient turns out to have a positive and significant effect on Gini inequality after redistribution. These findings would support the predictions of the "race-to-the-bottom"-hypothesis. The pro poor effect in developing economies and the pro upper middle class effect in advanced economies are more pronounced when we include lagged dependent variables. The effects in the benchmark sample and sample of advanced economies are again driven by outliers and transition countries.

Fifth, the relationship between trade openness and income inequality might be non-monotonic, where inequality first rises and later declines when trade openness increases. This would follow Kuznet's (1955) hypothesis predicting a non-monotonic relationship where income inequality first increases and later decreases when the overall income level of a country increases. We examine whether the effect of trade openness on inequality changes at different levels of the trade openness process. We include trade openness in levels and squared trade openness in our baseline model.[34] We do not find evidence for a non-monotonic relationship (Appendix, Figure A3.1 for marginal trade openness effects on Gini indices depending on the level of trade openness).

Sixth, we used alternative definitions of natural disasters by constructing the instrument predicted openness in the panel model, such as broader specifications that include all kinds of natural disasters or counting all sizes of disasters (small and large), as suggested by Felbermayr and Gröschl (2013). Using the alternative instruments, inferences do not change (Appendix, Table A3.9).

[33] Robustness tables including lagged dependent variables upon request.

[34] The models are estimated by OLS. We also estimated the 2SLS and instrumented the squared globalization index by the squared instrument, but the instruments turn out to be weak. We therefore elaborate on the OLS estimates. Lang and Tavares (2018), who also elaborate on non-monotonic effects of globalization on income inequality, need to handle the weak instrument problem as well.

## 3.7 Conclusion

We examined how trade openness influences income inequality using predicted openness as an IV for trade openness. The baseline results do not show that trade openness influences income inequality and redistribution in the full country sample. The effect of trade openness on income inequality differs across countries. In particular, our results using relative income shares as the dependent variable are in line with predictions of the Stolper-Samuelson theorem: Trade openness tends to disproportionately benefit relative income shares of the poor in the sample of emerging and developing economies. In advanced economies, trade openness increased income inequality, an effect that is, however, driven by outliers. The positive effect of trade openness on income inequality in our benchmark country sample is driven by China and transition countries from Central and Eastern Europe.

Why is there a positive relationship between trade openness and inequality in the transition countries including China and the countries of Central and Eastern Europe but hardly so in the group of advanced economies?

The transition countries from Eastern Europe and China have experienced a rapid process of trade openness, while the welfare states and labor market institutions in these countries were less developed than in many advanced countries in the rest of the world -– in particular in Western Europe. Chinese reform programs were, for example, concentrated on economic growth that has not been moderated by large public education and redistribution programs. Participation in China's rise to a global economic power, therefore, is unequally distributed within the country (see Ravallion and Chen, 2007). Transition countries from Central and Eastern Europe have also experienced systematic structural and institutional changes towards market economies, which might be the drivers of rising trade openness levels and inequality outcomes in our results.

In the most advanced economies, established progressive tax and transfer systems, stable political and democratic institutions, and widely accessible opportunities for education may have moderated adverse effects of trade openness on income inequality. Our results do not suggest that trade openness had adverse effects on redistribution in advanced economies. The role of institutions and the coordination of the economy and welfare system, however, even seem to be relevant for different inequality rising effects of trade openness among advanced economies. The United States, for example, is widely seen as the country that has experienced the most pronounced increase in income inequality, partly because competition from emerging economies such as China has destroyed jobs for medium and low-skilled labor (see Autor *et al.*, 2013). Our descriptive statistics suggest that redistribution programs in EU15 countries reduce income inequality to a much larger amount than equivalent tax/transfer programs in other advanced economies. Future research should examine in more detail how institutions influence income inequality when countries are active in trading goods and services.

# Appendix

## List of countries

**Advanced Economies*:** Australia, Austria, Belgium, Canada, Cyprus, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Iceland, Ireland, Israel, Italy, Japan, Republic of Korea, Latvia, Lithuania, Luxembourg, Malta, Netherlands, New Zealand, Norway, Portugal, Singapore, Slovakia, Slovenia, Spain, Sweden, Switzerland, United Kingdom, United States

**Emerging and Developing Economies:** Albania*, Algeria*, Angola*, Argentina*, Armenia, Azerbaijan*, Bangladesh, Barbados*, Belarus*, Belize*, Benin, Bolivia, Bosnia and Herzegovina*, Brazil*, Bulgaria*, Burkina Faso, Burundi, Cambodia, Cameroon, Cape Verde, Central African Republic, Chad, Chile*, China*, Colombia*, Comoros, Costa Rica*, Cote d'Ivoire, Croatia*, Djibouti, Dominican Republic*, Ecuador*, Egypt, El Salvador, Ethiopia, Fiji*, Gabon*, Gambia, Georgia, Ghana, Guatemala, Guinea, Guinea-Bissau, Haiti, Honduras, Hungary*, India, Indonesia, Iran*, Jamaica*, Jordan*, Kazakhstan*, Kenya, Kyrgyz Republic, Lao, Lebanon*, Macedonia (FYR)*, Madagascar, Malawi, Malaysia*, Maldives*, Mali, Mauritania, Mauritius*, Mexico*, Moldova„ Mongolia*, Morocco, Mozambique, Nepal, Nicaragua, Niger, Nigeria, Pakistan, Panama*, Paraguay*, Peru*, Philippines, Poland*, Romania*, Russian Federation*, Rwanda, Senegal, Sierra Leone, South Africa*, Sri Lanka, St. Lucia*, Suriname*, Syria, Tajikistan, Tanzania, Thailand*, Togo, Trinidad and Tobago*, Tunisia*, Turkey*, Turkmenistan*, Uganda, Ukraine, Uruguay*, Uzbekistan, Venezuela*, Viet Nam, Republic of Yemen, Zambia, Zimbabwe

**Central and Eastern European EU Members*:** Bulgaria, Croatia, Czech Republic, Estonia, Hungary, Latvia, Lithuania, Poland, Romania, Slovakia, Slovenia

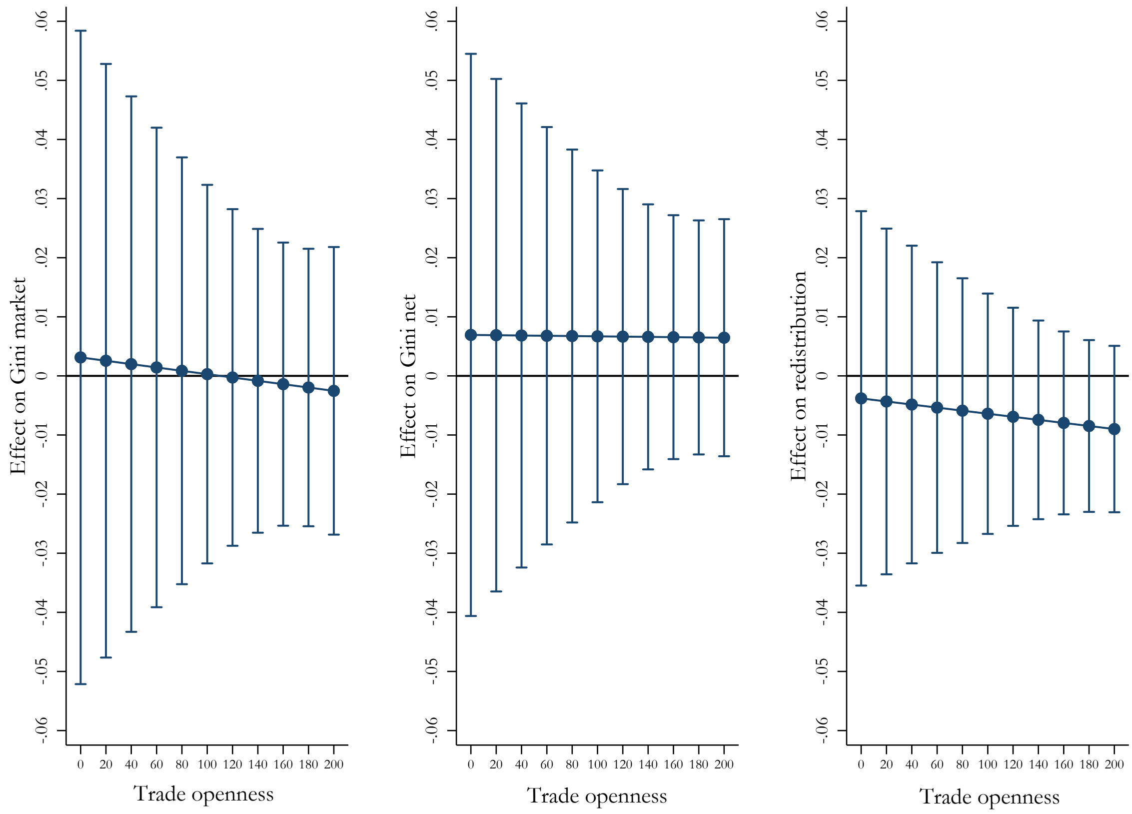
**Former Members of the Soviet Union:** Armenia, Azerbaijan*, Belarus*, Georgia, Kazakhstan*, Kyrgyz Republic, Moldova, Russian Federation*, Tajikistan, Turkmenistan*, Ukraine, Uzbekistan Western Balkan*: Albania, Bosnia and Herzegovina, Macedonia (FYR)

**EU 15*:** Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, Netherlands, Portugal, Spain, Sweden, United Kingdom

*Countries and samples marked with * are high and middle income countries and included in our benchmark sample. The World Bank (2015) classified countries having a GNI per capita of USD 4,126 or more as high and middle income countries.*

## Figures

Figure A3.1 : Robustness (VII) – Non-monotonic relationship




*Source:* SWIID 5.1, World Bank (2017), own calculations.
*Notes:* The figure shows marginal effects of trade openness on income inequality conditional on the level of trade openness. Estimates use an OLS panel fixed effects model that includes trade openness in levels and squared terms. The sample includes 82 countries and 517 observations based on nine periods using 5-year averages between 1970 and 2014. The model is estimated with robust standard errors clustered at the country level. The dots show the marginal effects of a one percentage point increase in trade openness on income inequality, at a given level of trade openness (x-axis). The lines indicate the 90 percent confidence intervals.

## Tables

Table A3.1 : First stage regression results (2SLS)

| *Panel sample* | (1) Full | (2) Benchmark | (3) Advanced | (4) Developing |
|---|---|---|---|---|
| $\Omega^{i}_{\tau-1}$ | 0.430*** | 0.548*** | 0.630*** | 0.332* |
| | (4.82) | (5.08) | (5.02) | (1.78) |
| Partial $R^2$ | 0.067 | 0.131 | 0.233 | 0.024 |
| F-Test, weak ID | 45.573 | 62.899 | 58.875 | 10.652 |
| F-Test, p-value | 0.000 | 0.000 | 0.000 | 0.079 |
| Controls | Yes | Yes | Yes | Yes |
| *Fixed Effects* | | | | |
| Country | Yes | Yes | Yes | Yes |
| Period | Yes | Yes | Yes | Yes |
| Countries | 139 | 82 | 34 | 105 |
| Observations | 794 | 516 | 244 | 550 |

*Notes:* T-statistics in parentheses. 2SLS panel fixed effects estimations based on nine periods using 5-year averages between 1970 and 2014. Robust standard errors clustered at the country level. All specifications include country and year fixed effects, and baseline control variables: GDP per capita, population(log), dependecy ratio, social globalization index, political globalization index, FDI index, and large scale natural disasters. Weak ID test using Cragg-Donald Wald F-statistic. Stock and Yogo (2005) weak ID critical values: 16.38 (10 %), 8.96 (15 %), 6.66 (20 %). Significance levels: ***p $<$ 0.01; **p $<$ 0.05; *p $<$ 0.1.

Table A3.2 : Summary statistics and data sources, 5-year averaged periods

| Variable | Mean | Std. Dev. | Min. | Max. | N | Source | Definition |
|---|---|---|---|---|---|---|---|
| *Dependent variables* | | | | | | | |
| Gini market | 45.86 | 7.09 | 23 | 74.46 | 808 | Solt (2016), SWIID v5.1 | Gini inc. inequality, pre tax & transfers |
| Gini net | 37.7 | 8.85 | 18.15 | 61.84 | 808 | | Gini inc. inequality, post tax & transfers |
| Redistribution (Gini) | 8.17 | 6.08 | -10.97 | 26.39 | 808 | | Redistribution (Gini market - Gini net) |
| Income shares (net) | | | | | | Lahoti et al. (2016), GCIP | Relative net income share by decile |
| Decile 1 | 1.91 | 1.06 | 0.18 | 5.95 | 795 | | |
| Decile 2 | 3.06 | 1.3 | 0.54 | 6.53 | 795 | | |
| Decile 3 | 4.06 | 1.44 | 0.94 | 7.21 | 795 | | |
| Decile 4 | 5.07 | 1.51 | 1.43 | 8.1 | 795 | | |
| Decile 5 | 6.16 | 1.51 | 2.07 | 8.95 | 795 | | |
| Decile 6 | 7.42 | 1.45 | 2.95 | 9.92 | 795 | | |
| Decile 7 | 9.02 | 1.32 | 4.27 | 11.99 | 795 | | |
| Decile 8 | 11.28 | 1.08 | 6.53 | 14.71 | 795 | | |
| Decile 9 | 15.26 | 0.94 | 10.85 | 18.15 | 795 | | |
| Decile 10 | 36.76 | 10.36 | 18.48 | 69.55 | 795 | | |
| *Trade variable* | | | | | | | |
| Trade openness | 75.23 | 49.53 | 6.79 | 410.25 | 808 | World Bank(2017), WDI | Exports & imports as share of GDP |
| *Instrument* | | | | | | | |
| $\Omega^i_\tau$ | 59.32 | 35.95 | 0.47 | 322.62 | 710 | Felbermayr & Gröschl (2013) | Predicted openness |
| $\Omega^i_{\tau-1}$ | 56.08 | 34.52 | 0.42 | 322.62 | 808 | | One period lag of predicted openness |
| *Baseline controls* | | | | | | | |
| GDP pc | 12.65 | 13 | 0.44 | 90.5 | 800 | Feenstra et al.(2015), | Real GDP per capita, in billions USD |
| ln POP | 2.53 | 1.63 | -1.89 | 7.21 | 800 | PWT v9.0 | Log of total population, in millions |
| Dependency | 65.87 | 18.76 | 34.96 | 112.84 | 808 | World Bank (2017), WDI | Age dependency ratio, young & old |
| Social GLOB | 45.67 | 22.84 | 6.55 | 93 | 802 | update KOF 2016 | KOF index of social globalization 2016 |
| Political GLOB | 65.90 | 20.05 | 18.55 | 97.67 | 802 | | KOF index of political globalization 2016 |
| FDI | 59.75 | 23.77 | 5 | 100 | 806 | | KOF index of total FDI-to-GDP ratio 2016 |
| $L.Disaster_{\tau-1}$ | 0.2 | 0.63 | 0 | 7.8 | 808 | | One period lag of large scale natural disasters |
| *Robustness test controls* | | | | | | | |
| ln migrants | 12.64 | 1.69 | 8.19 | 17.6 | 804 | World Bank(2017), WDI | Log of international migrant stock as share of population |
| ICT capital stock | 0.19 | 0.81 | 0 | 12.71 | 571 | Jorgenson and Vu (2017) | ICT capital stock, in 100'000 USD |

Table A3.3 : Robustness (I) – EMD economies excluding transition countries (2SLS)

| | (1) D1 | (2) D2 | (3) D3 | (4) D4 | (5) D5 | (6) D6 | (7) D7 | (8) D8 | (9) D9 | (10) D10 | (11) Gini market | (12) Gini net | (13) Redistr. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Trade openness | 0.0476 | 0.0391 | 0.0366 | 0.0343 | 0.0309 | 0.0253 | 0.0158 | -0.00120 | -0.0368 | -0.193 | -0.366 | -0.301 | -0.0649 |
| | (1.47) | (1.32) | (1.23) | (1.16) | (1.09) | (1.00) | (0.78) | (-0.08) | (-1.06) | (-1.11) | (-1.02) | (-1.03) | (-0.87) |
| *Fixed effects* | | | | | | | | | | | | | |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | |
| Period FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | |
| Countries | 96 | 96 | 96 | 96 | 96 | 96 | 96 | 96 | 96 | 96 | 96 | 96 | 96 |
| Observations | 504 | 504 | 504 | 504 | 504 | 504 | 504 | 504 | 504 | 504 | 504 | 504 | 504 |
| Partial $R^2$ | | | | | | | 0.020 | | | | | | |
| F Test, weak ID | | | | | | | 8.135 | | | | | | |
| F Test, p-value | | | | | | | 0.130 | | | | | | |

*Notes:* Emerging and developing economies (EMD) excluding transition countries economies excluding transition countrieTransition economies include China and East-EU member states (Bulgaria, Croatia, Czech Republic, Estonia, Hungary, Latvia, Lithuania, Poland, Romania,Slovakia, Romania). 2SLS panel fixed effects estimations based on nine periods using 5-year averages between 1970 and 2014. T-statistics in parentheses. Robust standard errors clustered at the country level. All specifications include baseline control variables: GDP per capita, population(log), dependecy ratio, social globalization index, political globalization index, FDI index, and large scale natural disasters. Weak ID test using Cragg-Donald Wald F-statistic. Stock and Yogo (2005) weak ID critical values: 16.38 (10 %), 8.96 (15 %), 6.66 (20 %). Significance levels: ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Table A3.4 : Robustness (II) – Baseline including migration as control

| | OLS | | | | | | 2SLS | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Full sample | | | Benchmark sample | | | Full sample | | | Benchmark sample | | |
| | (1) Gini market | (2) Gini net | (3) Redistr. | (4) Gini market | (5) Gini net | (6) Redistr. | (7) Gini market | (8) Gini net | (9) Redistr. | (10) Gini market | (11) Gini net | (12) Redistr. |
| Trade openness | 0.00778 | 0.0107 | -0.00291 | -0.00754 | -0.000181 | -0.00736 | -0.0579 | -0.0273 | -0.0306 | 0.00564 | 0.0206 | -0.0150 |
| | (0.0135) | (0.0107) | (0.00776) | (0.0155) | (0.0117) | (0.0109) | (0.0696) | (0.0568) | (0.0249) | (0.0423) | (0.0342) | (0.0219) |
| Migrant share (log) | 0.548 | 0.374 | 0.174 | -0.643 | -0.556 | -0.0877 | 0.582 | 0.394 | 0.188 | -0.690 | -0.629 | -0.0608 |
| | (0.753) | (0.658) | (0.266) | (0.704) | (0.613) | (0.471) | (0.759) | (0.658) | (0.268) | (0.689) | (0.577) | (0.490) |
| *Fixed effects* | | | | | | | | | | | | |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Period FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Countries | 139 | 139 | 139 | 82 | 82 | 82 | 139 | 139 | 139 | 82 | 82 | 82 |
| Observations | 790 | 790 | 790 | 512 | 512 | 512 | 790 | 790 | 790 | 512 | 512 | 512 |
| Partial $R^2$ | | | | | | | | 0.067 | | | 0.128 | |
| F Test, weak ID | | | | | | | | 45.209 | | | 60.890 | |
| F Test, p-value | | | | | | | | 0.000 | | | 0.000 | |

*Notes:* This table includes log of migration(share of population) as control variable. Social globalization is excluded. All specifications include the following control variables: GDP per capita, population(log), dependecy ratio, political globalization index, FDI index, and large scale natural disasters. OLS and 2SLS panel fixed effects estimations based on nine periods using 5-year averages between 1970 and 2014. Clustered robust standard errors in parentheses. Weak ID test using Cragg-Donald Wald F-statistic. Stock and Yogo(2005) weak ID critical value: 16.38 (10 %). Significance levels: ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Table A3.5 : Robustness (III) – Baseline including ICT capital stock as control

| | OLS | | | | | | 2SLS | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Full sample | | | Benchmark sample | | | Full sample | | | Benchmark sample | | |
| | (1) Gini market | (2) Gini net | (3) Redistr. | (4) Gini market | (5) Gini net | (6) Redistr. | (7) Gini market | (8) Gini net | (9) Redistr. | (10) Gini market | (11) Gini net | (12) Redistr. |
| Trade openness | -0.000519 | 0.00289 | -0.00341 | -0.0145 | -0.00820 | -0.00631 | -0.0197 | 0.0161 | -0.0358 | -0.00228 | 0.0190 | -0.0213 |
| | (0.0155) | (0.0115) | (0.0104) | (0.0179) | (0.0115) | (0.0132) | (0.0578) | (0.0463) | (0.0311) | (0.0554) | (0.0450) | (0.0278) |
| ICT capital stock | 0.248 | 0.382* | -0.134 | 0.0463 | 0.149 | -0.103 | 0.219 | 0.403* | -0.184 | 0.0723 | 0.207 | -0.135 |
| | (0.277) | (0.204) | (0.122) | (0.215) | (0.122) | (0.149) | (0.282) | (0.211) | (0.128) | (0.250) | (0.167) | (0.149) |
| *Fixed effects* | | | | | | | | | | | | |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Period FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Countries | 90 | 90 | 90 | 64 | 64 | 64 | 90 | 90 | 90 | 64 | 64 | 64 |
| Observations | 569 | 569 | 569 | 428 | 428 | 428 | 569 | 569 | 569 | 428 | 428 | 428 |
| Partial $R^2$ | | | | | | | | 0.078 | | | 0.109 | |
| F Test, weak ID | | | | | | | | 39.170 | | | 42.419 | |
| F Test, p-value | | | | | | | | 0.002 | | | 0.002 | |

*Notes:* This table includes ICT capital stock (proxy for technological change) as control variable. All specifications include baseline control variables: GDP per capita, population(log), dependecy ratio, social globalization index, political globalization index, FDI index, and large scale natural disasters. OLS and 2SLS panel fixed effects estimations based on nine periods using 5-year averages between 1970 and 2014. Clustered robust standard errors in parentheses. Weak ID test using Cragg-Donald Wald F-statistic. Stock and Yogo (2005) weak ID critical value: 16.38 (10 %). Significance levels: ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Table A3.6 : Robustness (IV) – Baseline using 3-year periods

| | OLS | | | | | | 2SLS | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Full sample | | | Benchmark sample | | | Full sample | | | Benchmark sample | | |
| | (1) Gini market | (2) Gini net | (3) Redistr. | (4) Gini market | (5) Gini net | (6) Redistr. | (7) Gini market | (8) Gini net | (9) Redistr. | (10) Gini market | (11) Gini net | (12) Redistr. |
| Trade openness | 0.0124 | 0.0143 | -0.00195 | -0.0142 | -0.00418 | -0.0100 | -0.0334 | -0.00931 | -0.0241 | 0.0143 | 0.0214 | -0.00712 |
| | (0.0132) | (0.0110) | (0.00605) | (0.0145) | (0.0118) | (0.00852) | (0.0649) | (0.0535) | (0.0248) | (0.0382) | (0.0309) | (0.0211) |
| *Fixed effects* | | | | | | | | | | | | |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Period FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Countries | 140 | 140 | 140 | 82 | 82 | 82 | 140 | 140 | 140 | 82 | 82 | 82 |
| Observations | 1151 | 1151 | 1151 | 748 | 748 | 748 | 1151 | 1151 | 1151 | 748 | 748 | 748 |
| Partial $R^2$ | | | | | | | | 0.049 | | | 0.111 | |
| F Test, weak ID | | | | | | | | 50.877 | | | 80.871 | |
| F Test, p-value | | | | | | | | 0.000 | | | 0.000 | |

*Notes:* OLS and 2SLS panel fixed effects estimations based on 15 periods using 3-year averages between 1970 and 2014. All specifications include baseline control variables: GDP per capita, population(log), dependecy ratio, social globalization index, political globalization index, FDI index, and large scale natural disasters. Clustered robust standard errors in parentheses. Weak ID test using Cragg-Donald Wald F-statistic. Stock and Yogo (2005) weak ID critical value: 16.38 (10 %). Significance levels: ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Table A3.7 : Robustness (V) – Subsamples using 3-year periods (2SLS)

| | (1) D1 | (2) D2 | (3) D3 | (4) D4 | (5) D5 | (6) D6 | (7) D7 | (8) D8 | (9) D9 | (10) D10 | (11) Gini market | (12) Gini net | (13) Redistr. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| ***(a) Full sample*** | | | | | | | | | | | | | |
| Trade openness | -0.00389 | -0.00587 | -0.00608 | -0.00567 | -0.00490 | -0.00383 | -0.00240 | -0.000358 | 0.00347 | 0.0286 | -0.0360 | -0.0122 | -0.0238 |
| | (-0.40) | (-0.67) | (-0.75) | (-0.76) | (-0.73) | (-0.64) | (-0.44) | (-0.06) | (0.35) | (0.65) | (-0.56) | (-0.23) | (-0.97) |
| Countries | 137 | 137 | 137 | 137 | 137 | 137 | 137 | 137 | 137 | 137 | 137 | 137 | 137 |
| Observations | 1136 | 1136 | 1136 | 1136 | 1136 | 1136 | 1136 | 1136 | 1136 | 1136 | 1136 | 1136 | 1136 |
| Partial $R^2$ | | | | | | | 0.051 | | | | | | |
| F Test, weak ID | | | | | | | 52.197 | | | | | | |
| F Test pvalue | | | | | | | 0.000 | | | | | | |
| ***(b) Benchmark sample*** | | | | | | | | | | | | | |
| Trade openness | -0.00851 | -0.0101 | -0.0101 | -0.00934 | -0.00802 | -0.00597 | -0.00283 | 0.00239 | 0.0129* | 0.0395 | 0.0132 | 0.0207 | -0.00746 |
| | (-1.14) | (-1.48) | (-1.60) | (-1.64) | (-1.57) | (-1.33) | (-0.70) | (0.55) | (1.86) | (1.17) | (0.35) | (0.67) | (-0.36) |
| Countries | 81 | 81 | 81 | 81 | 81 | 81 | 81 | 81 | 81 | 81 | 81 | 81 | 81 |
| Observations | 745 | 745 | 745 | 745 | 745 | 745 | 745 | 745 | 745 | 745 | 745 | 745 | 745 |
| Partial $R^2$ | | | | | | | 0.113 | | | | | | |
| F Test, weak ID | | | | | | | 82.178 | | | | | | |
| F Test pvalue | | | | | | | 0.000 | | | | | | |
| ***(c) Advanced economies*** | | | | | | | | | | | | | |
| Trade openness | -0.0127** | -0.0114** | -0.00991* | -0.00811 | -0.00587 | -0.00304 | 0.000698 | 0.00603** | 0.0149*** | 0.0295 | -0.00381 | 0.00376 | -0.00757 |
| | (-2.23) | (-2.04) | (-1.70) | (-1.38) | (-1.04) | (-0.59) | (0.16) | (2.02) | (3.77) | (0.81) | (-0.11) | (0.16) | (-0.25) |
| Countries | 34 | 34 | 34 | 34 | 34 | 34 | 34 | 34 | 34 | 34 | 34 | 34 | 34 |
| Observations | 361 | 361 | 361 | 361 | 361 | 361 | 361 | 361 | 361 | 361 | 361 | 361 | 361 |
| Partial $R^2$ | | | | | | | 0.164 | | | | | | |
| F Test, weak ID | | | | | | | 59.871 | | | | | | |
| F Test pvalue | | | | | | | 0.000 | | | | | | |
| ***(d) Developing economies*** | | | | | | | | | | | | | |
| Trade openness | 0.0310 | 0.0228 | 0.0179 | 0.0135 | 0.00882 | 0.00310 | -0.00465 | -0.0162 | -0.0360 | -0.0405 | -0.224 | -0.209 | -0.0150 |
| | (1.55) | (1.31) | (1.10) | (0.86) | (0.57) | (0.20) | (-0.28) | (-0.80) | (-1.15) | (-0.40) | (-1.02) | (-1.07) | (-0.35) |
| Countries | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 | 103 |
| Observations | 775 | 775 | 775 | 775 | 775 | 775 | 775 | 775 | 775 | 775 | 775 | 775 | 775 |
| Partial $R^2$ | | | | | | | 0.016 | | | | | | |
| F Test, weak ID | | | | | | | 10.880 | | | | | | |
| F Test pvalue | | | | | | | 0.165 | | | | | | |

*Notes:* 2SLS panel fixed effects estimations based on 15 periods using 3- year averages between 1970 and 2014. T-statistics in parentheses. Robust standard errors clustered at the country level. All specifications include country and year fixed effects, and baseline control variables: GDP per capita, population(log), dependecy ratio, social globalization index, political globalization index, FDI index, and large scale natural disasters. Weak ID test using Cragg-Donald Wald F-statistic. Stock and Yogo (2005) weak ID critical values: 16.38 (10 %), 8.96 (15 %). Significance levels: ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Table A3.8 : Robustness (VI) – Baseline including time trend as control

| | OLS | | | | | | 2SLS | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Full sample | | | Benchmark sample | | | Full sample | | | Benchmark sample | | |
| | (1) Gini market | (2) Gini net | (3) Redistr. | (4) Gini market | (5) Gini net | (6) Redistr. | (7) Gini market | (8) Gini net | (9) Redistr. | (10) Gini market | (11) Gini net | (12) Redistr. |
| Trade openness | 0.00699 | 0.00969 | -0.00270 | -0.0104 | -0.00334 | -0.00702 | -0.0571 | -0.0201 | -0.0369 | 0.00200 | 0.0208 | -0.0188 |
| | (0.0132) | (0.0103) | (0.00759) | (0.0151) | (0.0108) | (0.0106) | (0.0633) | (0.0508) | (0.0241) | (0.0391) | (0.0326) | (0.0204) |
| Trend | 0.267 | -0.162 | 0.430** | 0.433 | 0.0146 | 0.419* | 0.308 | -0.143 | 0.451** | 0.412 | -0.0265 | 0.439* |
| | (0.420) | (0.325) | (0.201) | (0.420) | (0.298) | (0.234) | (0.410) | (0.318) | (0.200) | (0.414) | (0.307) | (0.224) |
| *Fixed effects* | | | | | | | | | | | | |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Countries | 139 | 139 | 139 | 82 | 82 | 82 | 139 | 139 | 139 | 82 | 82 | 82 |
| Observations | 794 | 794 | 794 | 516 | 516 | 516 | 794 | 794 | 794 | 516 | 516 | 516 |
| Partial $R^2$ | | | | | | | | 0.064 | | | 0.120 | |
| F Test, weak ID | | | | | | | | 43.848 | | | 58.143 | |
| F Test, p-value | | | | | | | | 0.000 | | | 0.000 | |

*Notes:* This table includes the trend within nine periods as control variable. Period fixed effects are excluded. All specifications include country and year fixed effects, and baseline control variables: GDP per capita, population(log), dependecy ratio, social globalization index, political globalization index, FDI index, and large scale natural disasters. OLS and 2SLS panel fixed effects estimations based on nine periods using 5-year averages between 1970 and 2014. Clustered robust standard errors in parentheses. Weak ID test using Cragg-Donald Wald F-statistic. Stock and Yogo (2005) weak ID critical value: 16.38 (10 %). Significance levels: ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Table A3.9 : Robustness (VIII) – Alternative definitions of natural disasters (2SLS)

| | all large scale disasters | | | all exogeneous disasters | | | all disasters | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) Gini market | (2) Gini net | (3) Redistr. | (4) Gini market | (5) Gini net | (6) Redistr. | (7) Gini market | (8) Gini net | (9) Redistr. |
| Trade openness | -0.00792 | 0.0180 | -0.0260 | -0.00109 | 0.0209 | -0.0220 | -0.00450 | 0.0182 | -0.0227 |
| | (0.0449) | (0.0360) | (0.0252) | (0.0417) | (0.0334) | (0.0238) | (0.0440) | (0.0352) | (0.0248) |
| *Fixed effects* | | | | | | | | | |
| Country FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Period FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Countries | 82 | 82 | 82 | 82 | 82 | 82 | 82 | 82 | 82 |
| Observations | 516 | 516 | 516 | 516 | 516 | 516 | 516 | 516 | 516 |
| Partial $R^2$ | | 0.115 | | | 0.133 | | | 0.122 | |
| F Test, weak ID | | 54.533 | | | 64.161 | | | 58.327 | |
| F Test, p-value | | 0.000 | | | 0.000 | | | 0.000 | |

*Notes:* Benchmark sample. The instrument (predicted openness) is constructed in a gravity model including (i) all large scale natural disasters (columns 1-3), (ii) all exogeneous natural disasters (columns 4-6), and (iii) all natural disasters (columns 7-9). All specifications include baseline controls. 2SLS panel fixed effects estimations based on nine periods using 5-year averages between 1970 and 2014. Robust standard errors are clustered at the country level. t-statistics in parentheses. Significance levels: ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

# 4 How new airport infrastructure promotes tourism: Evidence from 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 based on joint work with Luisa Dörr, Stefanie Gäbler 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*, 2020, 54(10), 1402-1412.
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## 4.1 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.

## 4.2 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 A4.1 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 A4.1 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 A4.2 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

## 4.3 Empirical strategy and data

### 4.3.1 Estimation strategy

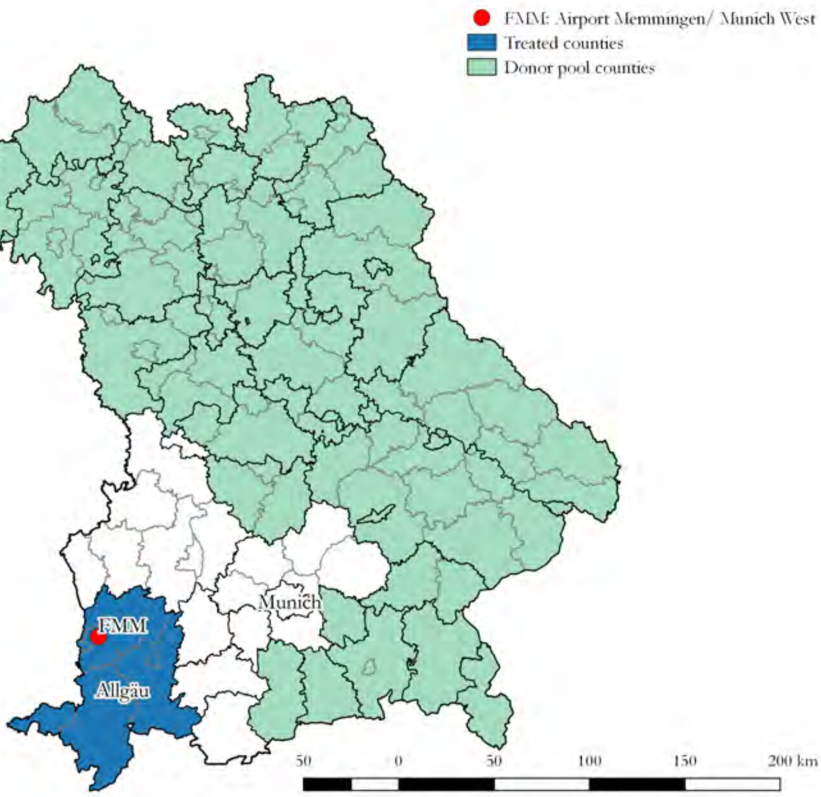
We compare the development of tourism across counties in the German federal state of Bavaria. A total of 96 Bavarian counties form 36 tourism regions (Figure 4.1), which merchandise as Bavarian touristic destinations. Therefore, the treatment and control areas (donor pool) are counties belonging to different 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 4.1). 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 A4.1 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 A4.1 and A4.2 in the Appendix).

Figure 4.1 : 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.*, 2021). 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 4.1). The difference in the outcome variable between

[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.

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]

### 4.3.2 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

[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.
[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 A4.3 in the Appendix.

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]

## 4.4 Results

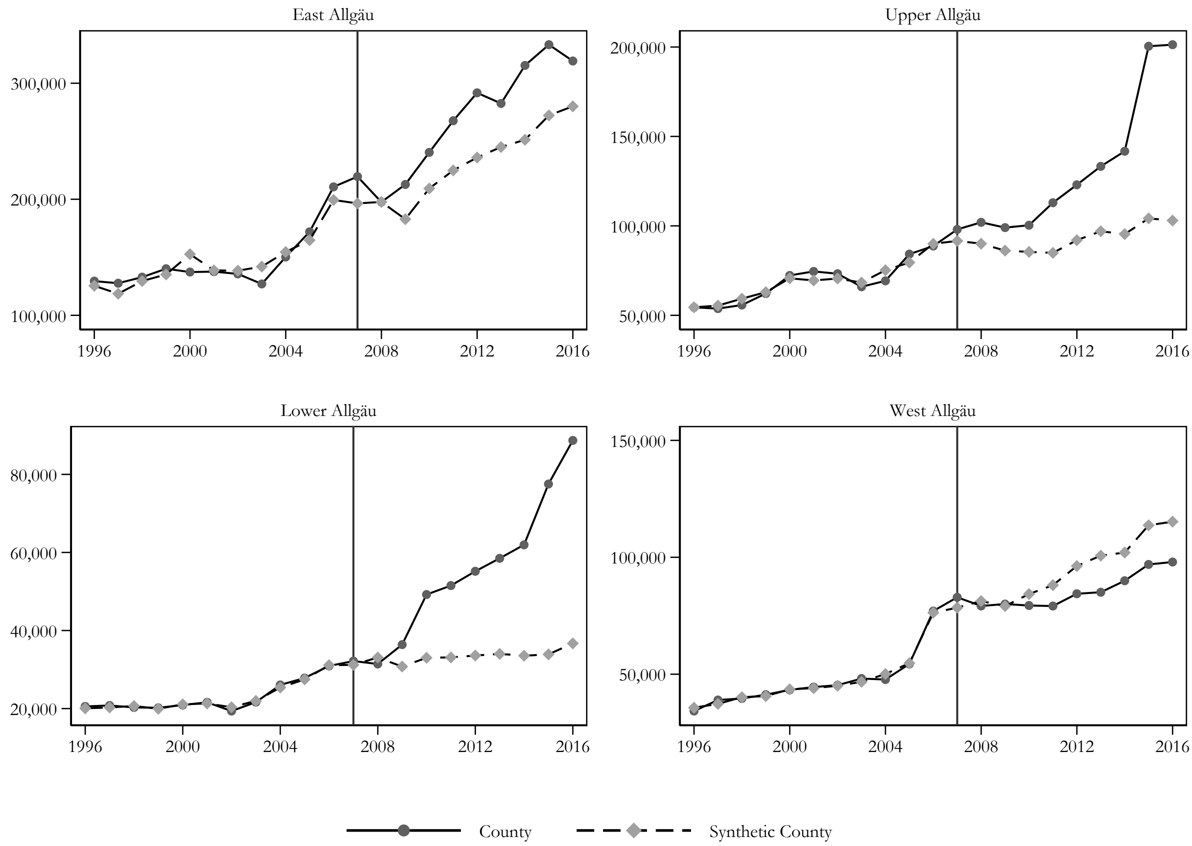
### 4.4.1 Baseline results

The results of the baseline synthetic control model are shown in Figure 4.2 and Table A4.2 in the Appendix. We report results for guest arrivals from abroad in the four regions of East, Lower, Upper and West Allgäu. Table A4.2 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 A4.2 in the Appendix). Figure 4.2 shows the pre-treatment matching trends graphically. Table A4.3 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 A4.2, column 2, in the Appendix). In West Allgäu, however, the results do not suggest that Memmingen Airport increased the number of arrivals from abroad.

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 A4.3 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 4.1). 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 4.1). 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.

[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 A4.4 in the Appendix.

Figure 4.2 : 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 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 A4.5 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 A4.4 in the Appendix). The ratio of real and synthetic total arrivals is 122% for Lower Allgäu over the treatment period 2008-2016 (Table A4.2 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

Table 4.1 : Difference-in-differences, arrivals from abroad (WLS)

| | 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 |

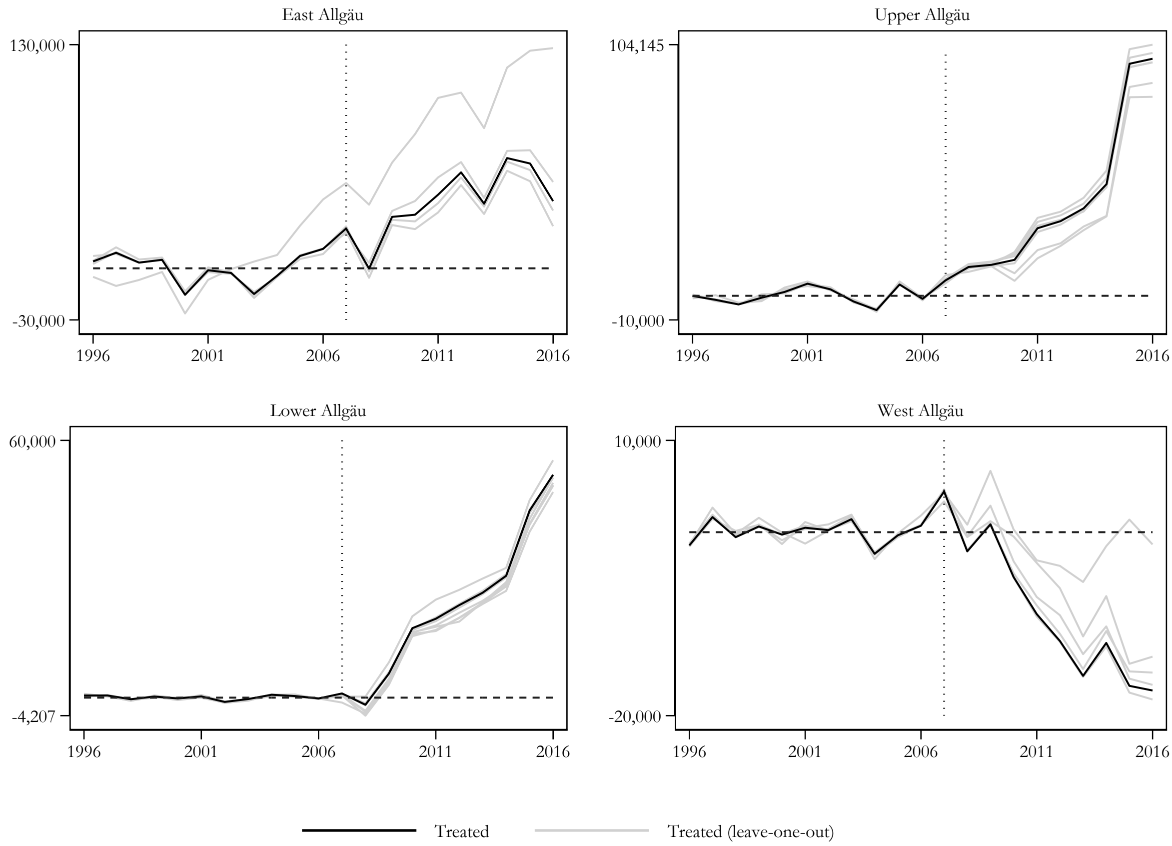
*Notes:* The Table compares results from the synthetic control approach to difference-in-differences results. The synthetic control approach results in panel A are calculated from Table A4.2 in the Appendix as the difference in before-after treatment differences of the treated regions and their synthetic counterparts. Panel B shows the results of difference-in-differences estimations using a weighted least squares (WLS) regression with weights derived from the synthetic control method (see Table A4.3 in the Appendix). We use yearly data over the period of 1996 to 2016 (without 2007). Significance levels (standard errors robust to heteroskedasticity in parentheses): ***p $< 0.01$, ** $< 0.05$, * $< 0.10$.

for the Upper Allgäu (Figure A4.3 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.

### 4.4.2 Robustness checks

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 4.3, which reproduces the baseline results (black line) from Figure 4.2 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 4.2. 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.

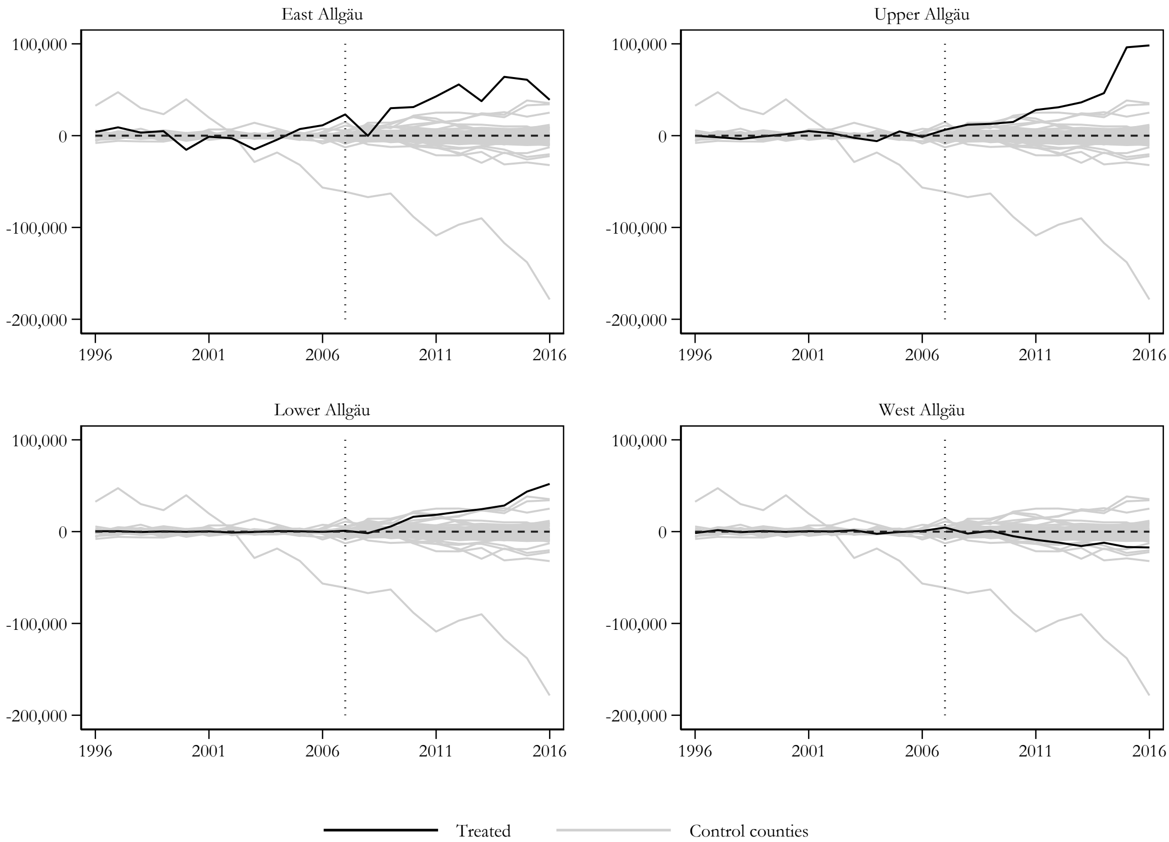
Figure 4.3 : Robustness (I) – Leave-one-out




*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.

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 4.4, 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 4.4 : 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 A4.5 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 A4.6 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 4.1). 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 A4.5 in the Appendix).

## 4.5 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 A4.2 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 generate revenues in the tourist industry via numerous channels: they spend, for example, on food and accommodation, go shopping, and demand local transport, amenities, spa and skiing, or cultural affairs etc. 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 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]

[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.

[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

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.* (2019) 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 that the new airport infrastructure helped to improve conditions regarding location and attracting specialist workers from abroad. Breidenbach (2020), 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.

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 airbase that operated until the year 2003 also had economic impacts on the Allgäu region. The former airbase 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).

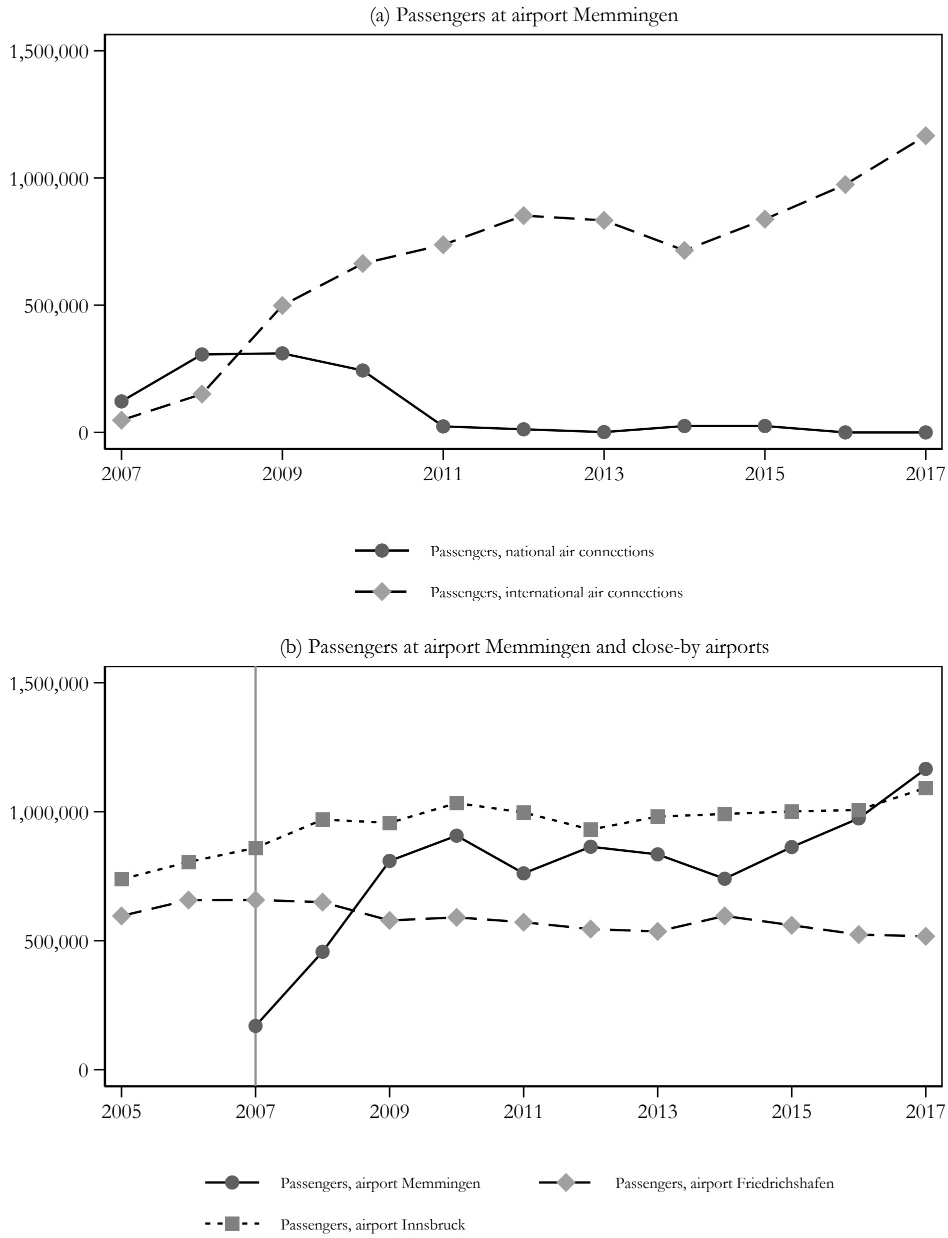
## 4.6 Conclusion

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

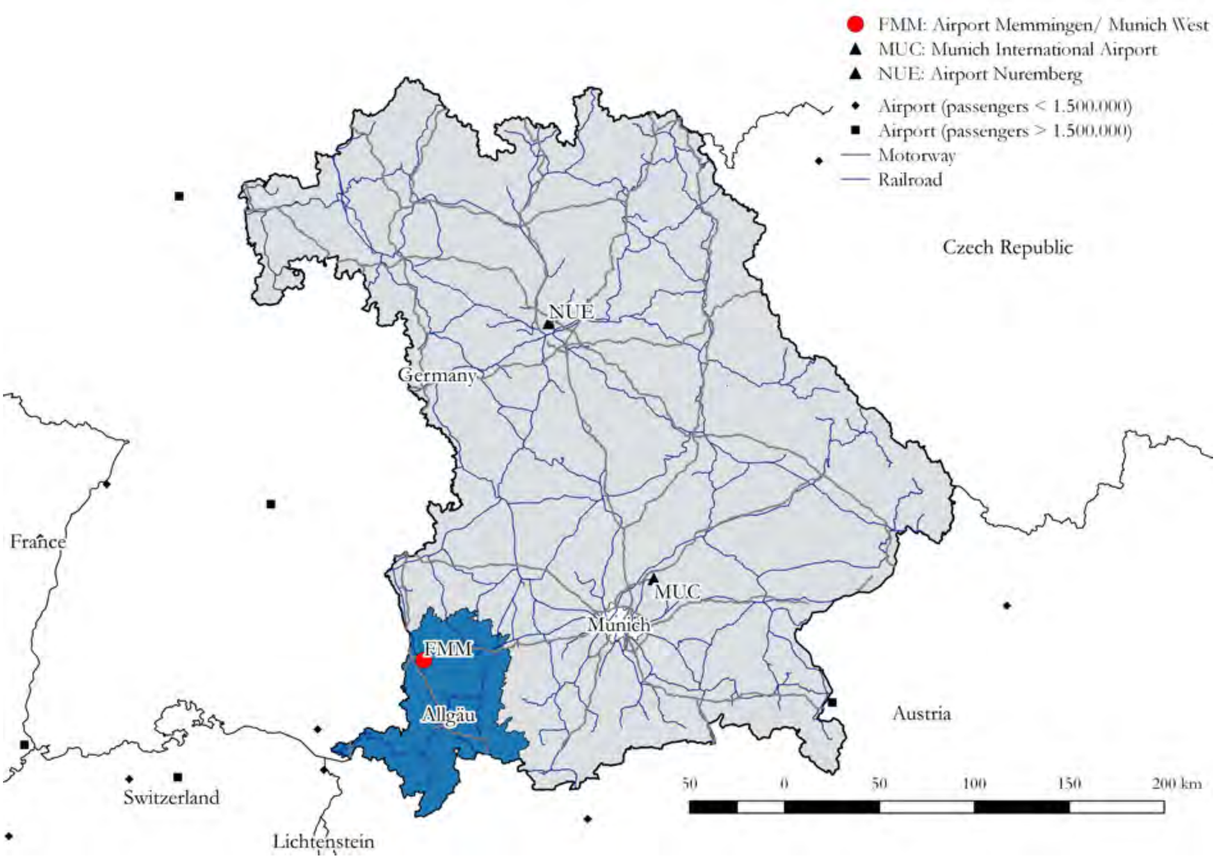
## Figures

Figure A4.1 : Passengers at Memmingen Airport and close-by airports



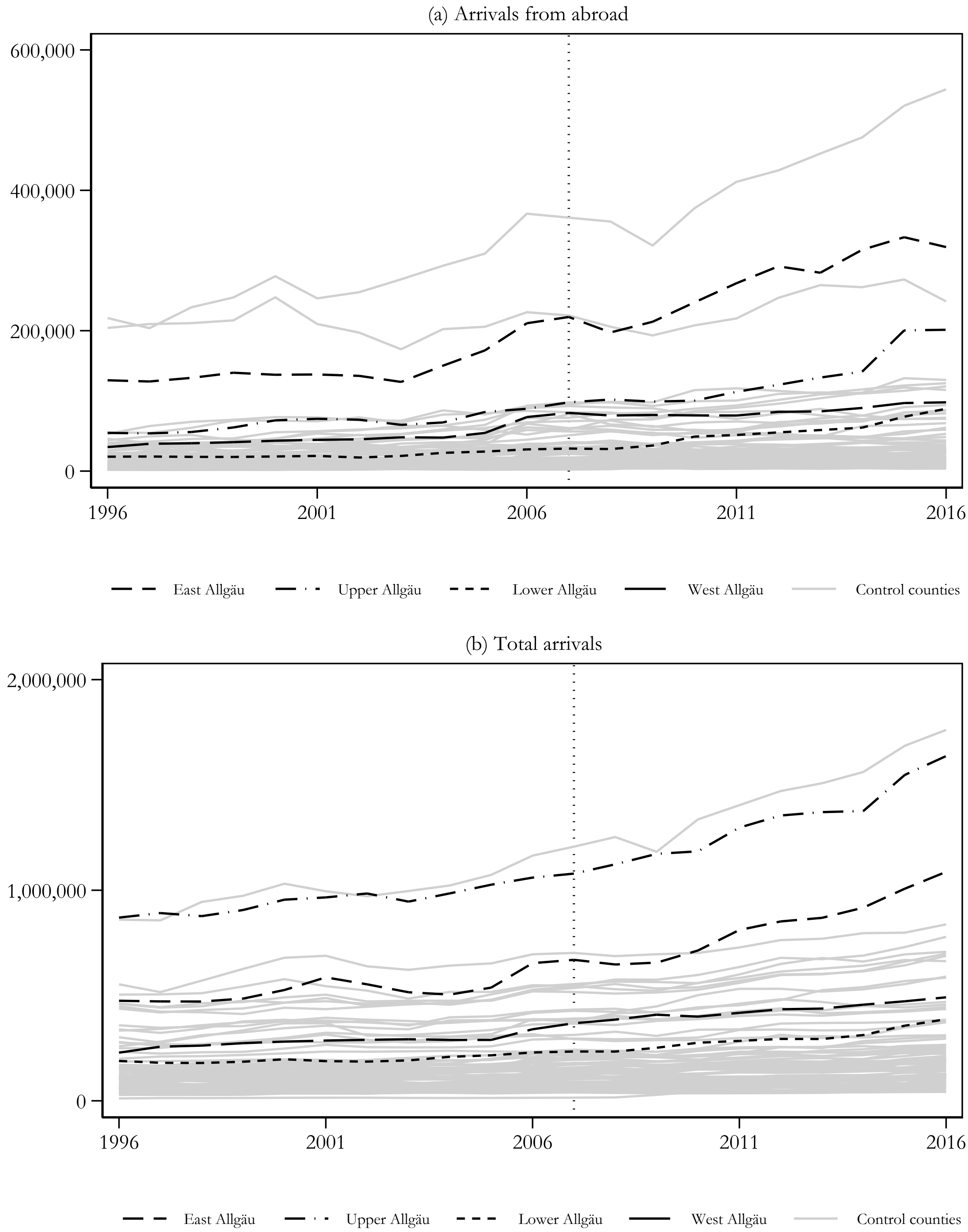

*Notes:* Figure A4.1a shows the development of passengers at national air connections (dark gray) and international air connections (light gray) at the Airport Memmingen. Figure A4.1b shows the development of passengers overall at the Airport Memmingen and the close-by airports Friedrichshafen and Innsbruck.

Figure A4.2 : Map of Bavaria



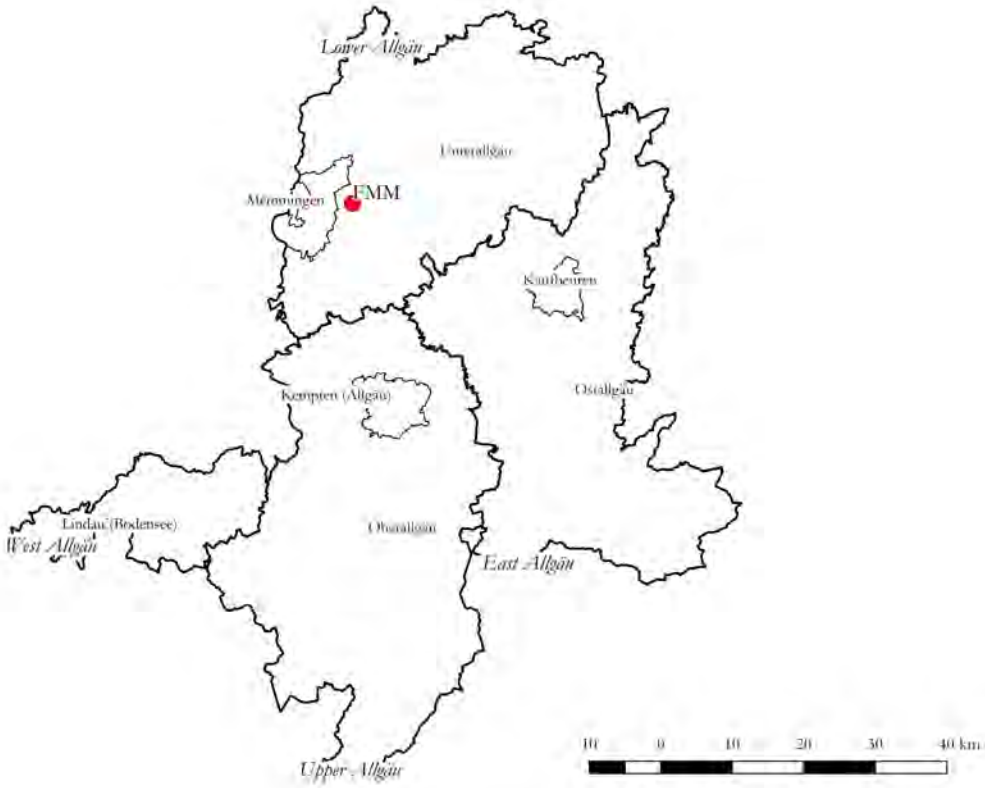

*Notes:* The map shows the federal state of Bavaria (light gray) with its two international airports in Munich and Nuremberg and the regional airport in Memmingen (red circle). Gray lines show the motorway network, blue lines the railroad network in Bavaria. The blue region (Allgäu) is our treatment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

Figure A4.3 : Raw data plots



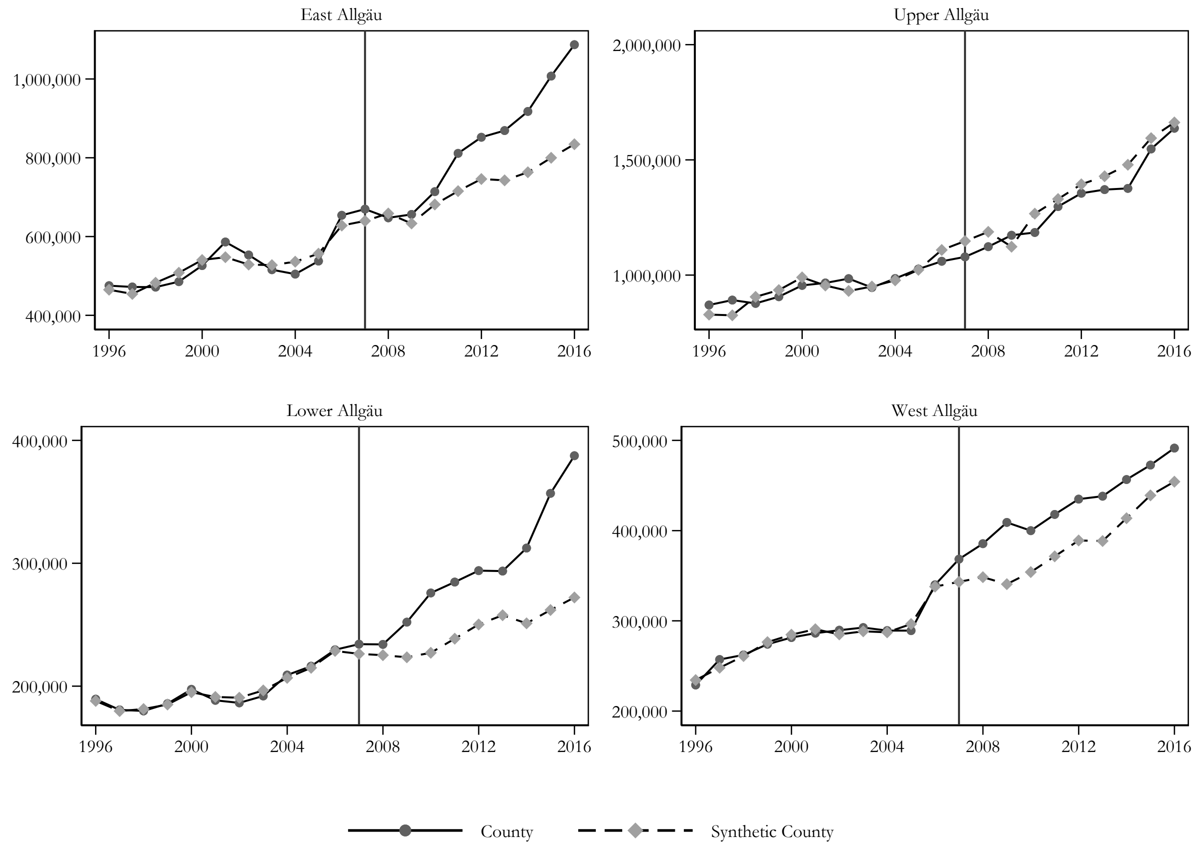

*Notes:* This figure shows how the two dependent variables evolve over our period of investigation. Black lines represent treated counties, light gray lines control counties (see Figure 4.1).

Figure A4.4 : Treatment regions



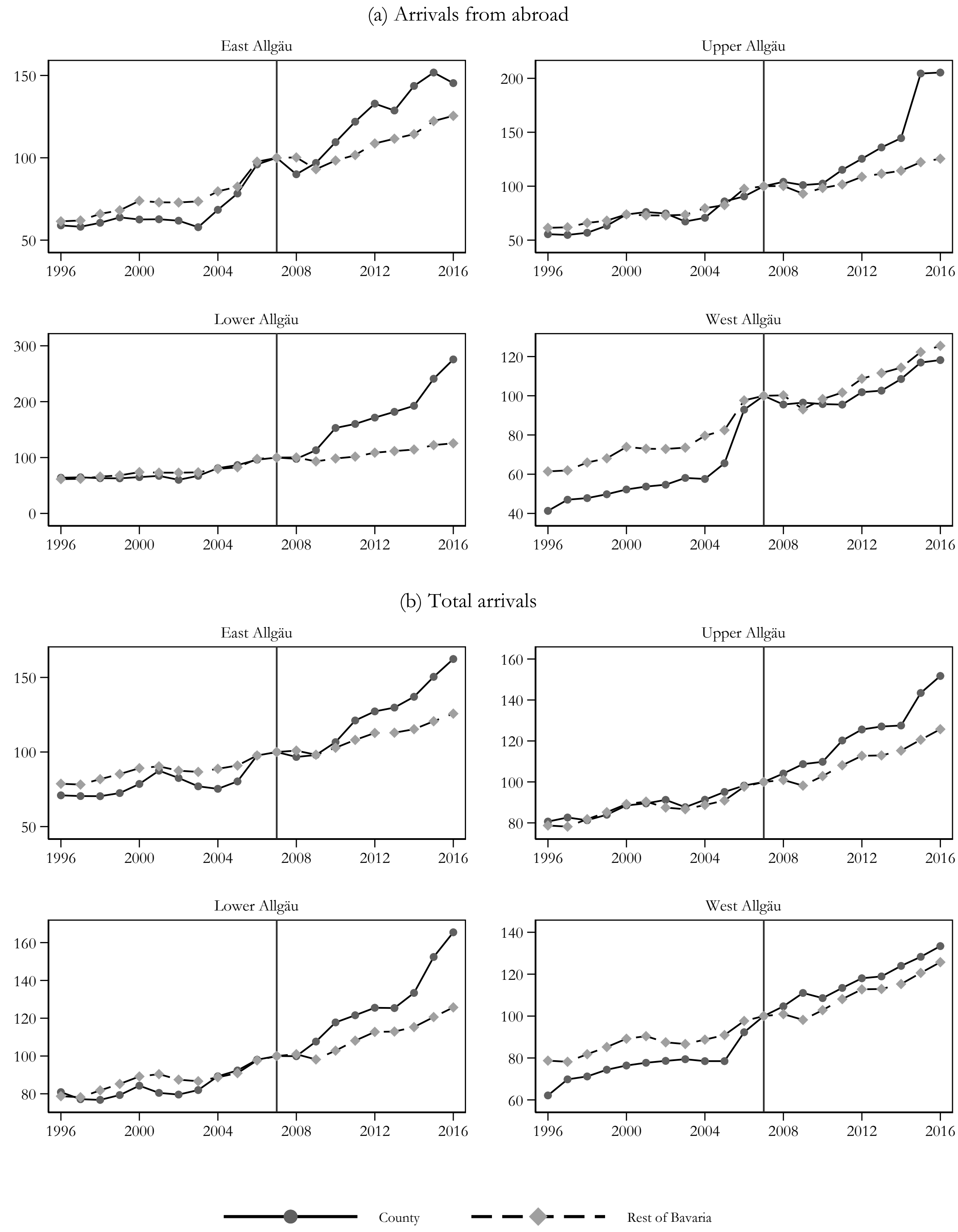

*Notes:* This map shows the treatment regions (italic, thick boundaries) and their counties (thin boundaries): Lower Allgäu (Memmingen and Unterallgäu), East Allgäu (Kaufbeuren and Ostallgäu), Upper Allgäu (Kempten (Allgäu) and Oberallgäu) and West Allgäu (Lindau (Bodensee)).

Figure A4.5 : 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 A4.6 : 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 4.1). 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.

## Tables

Table A4.1 : 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 A4.2 : Descriptive statistics

| | Arrivals from abroad | | Total arrivals | |
|---|---|---|---|---|
| | (1)<br>Before 2007 | (2)<br>After 2007 | (3)<br>Before 2007 | (4)<br>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 A4.3 in the Appendix. We use yearly data over the 1996-2016 period (without 2007).

Table A4.3 : 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 A4.4 : Difference-in-differences, total arrivals (WLS)

| | Total arrivals | | | |
|---|---|---|---|---|
| | (1)<br>East Allgäu | (2)<br>Upper Allgäu | (3)<br>Lower Allgäu | (4)<br>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 A4.2 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 A4.3 in the Appendix). We use yearly data over the 1996-2016 period (without 2007). Significance levels (standard errors robust to heteroskedasticity in parentheses): ***p $< 0.01$, ** $< 0.05$, * $< 0.10$.

Table A4.5 : Robustness (III) – Difference-in-differences, arrivals from abroad (OLS)

| | 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 4.1). In panel B only the counties that received a weight in the synthetic control approach form the control group (see Table A4.3 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 parentheses): ***p $<$ 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 $j$:

$$\hat{Y}_{it} = \sum_{i \neq j} w_j Y_j, \qquad \sum w_j = 1 \tag{A4.1}$$

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 $\hat{Y}_{it}$

$$\min RMSPE_i = \min \sqrt{\sum_{t=1}^{T_0} \frac{(Y_{it} - \hat{Y}_{it})^2}{T_0}} \tag{A4.2}$$

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 $t \geq T_0$):

$$Y_{it} - \hat{Y}_{it} \tag{A4.3}$$

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 (A4.2) 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:

$$Y_{it} = \alpha_i + \theta_t + \gamma(Allg\ddot{a}u_i \times Airport_t) + \epsilon_{it} \tag{A4.4}$$

where $Y_{it}$ describes our dependent variables arrivals in county $i$ and year $t$ (1996-2016). $Allg\ddot{a}u_i$ 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. $Allg\ddot{a}u_i \times Airport_t$ measures the interaction of the two dummies and $\gamma$ thus estimates our treatment effect. We include county and year fixed effects ($\alpha_i$ and $\theta_t$). The coefficient $\gamma$ 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 (A4.4) 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 A4.3 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 4.1) 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.

# 5 Elections and government efficiency: Evidence from German municipalities[1]

## Abstract

Politicians are expected to influence policy outcomes in a way to gain electoral advantage. There is, however, a pending question whether efficiency in the provision of public goods and services is affected by strategic behavior. I examine how electoral cycles influence local government efficiency by using OLS fixed effects, event study, and instrumental variable estimations in a large balanced panel of around 2,000 municipalities in the German state of Bavaria. Cost efficiency is estimated by employing a fixed effects semi-parametric stochastic frontier analysis. The results show that electoral cycles increase government efficiency in election and pre-election years by around 0.75 — 0.85 %. The effect is larger when executive and council electoral cycles coincide, and when incumbent mayors run for office again. My findings suggest an efficiency-enhancing effect of elections at given institutional conditions.

[1] I thank participants of the 2020 annual congress of the International Institute of Public Finance (IIPF), Klaus Gründler, Niklas Potrafke, Felix Rösel, and Roberto Zotti for helpful comments, Juliane Neumeier for proofreading, and Timo Wochner for valuable research assistance. This chapter is also published as *ifo Working Paper* No. 363, 2021.

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

## 5.1 Introduction

Election cycle theories suggest that incumbents seeking re-election manipulate economic policies before elections (Nordhaus, 1975; MacRae, 1977; Rogoff and Sibert, 1988; Rogoff, 1990). Empirical evidence is mixed, but overall indicates that electoral cycles influence budgetary and political decisions (e.g., De Haan and Klomp, 2013; Dubois, 2016; Philips, 2016, for an overview). Electoral cycles have been shown to influence fiscal variables such as expenditure, budget composition, deficits, taxation, and fees at individual governmental layers.[2] Incumbents also increased the provision of public goods and services before elections. Evidence shows, for example, how electoral motives influence labor market policies (e.g., Mechtel and Potrafke, 2013), public employment and subsidies (e.g., Coelho *et al.*, 2006; Dahlberg and Mörk, 2011; Tepe and Vanhuysse, 2009, 2013, 2014), or the quantity of decisions in public administration (e.g., Garmann, 2017a). While politicians are expected to allocate public resources in a manner to gain electoral advantage, a pending question is whether this allocation comes at the cost of wasteful public spending before elections.

I examine whether elections influence governments' efficiency in the provision of public goods and services. Government efficiency measures a ratio which puts the overall provision of public goods and services (*output*) in relation to their costs (*input*). Using panel data for around 2,000 municipalities in the German state of Bavaria for the period 2007-2017, my findings do not show that politicians increase public spending in a wasteful way before elections. In contrast, electoral cycles in the executive branch increase cost efficiency by around 0.75 — 0.85 % in election and pre-election years. The results challenge the literature on electoral cycles.

This paper makes several contributions. First, studies examining electoral cycles often focus on single outcome variables. Because of several municipal responsibilities, however, it is not obvious which outcome variable is affected by the strategic behavior of incumbents prior to elections. Cost efficiency scores, by contrast, provide a composite approach and relate proxies for several output variables of the local government to an input variable used by the government to produce these outputs. I use the total net expenditure of the municipalities as

[2] For the national government level (e.g., Schuknecht, 2000; Brender and Drazen, 2005; Shi and Svensson, 2006; Potrafke, 2010, 2020; Reischmann, 2016), the state and regional level (e.g., Kneebone and McKenzie, 2001; Akhmedov and Zhuravskaya, 2004; Sjahrir *et al.*, 2013; Kauder *et al.*, 2017, 2018), or the local government tier (e.g., Baleiras and da Silva Costa, 2004; Ashworth *et al.*, 2006; Drazen and Eslava, 2010; Aidt *et al.*, 2011; Englmaier and Stowasser, 2017; Foremny and Riedel, 2014).

input factor in the cost production function. Local governments which produce its multitude of tasks in the most economical way define the cost efficiency frontier. Deviations from the estimated best practice frontier represent cost inefficiency. The median value of cost inefficiency in my sample suggests that the local government in the median municipality can reduce expenditures by about 11 % to reach the efficiency frontier at a given output level. I use the inefficiency scores as the dependent variable and employ OLS fixed effects, event study and instrumental variable estimation approaches to examine how executive electoral cycles influence cost inefficiencies.

Second, several related studies calculate government efficiency — the dependent variable — by (*non-parametric*) deterministic approaches ignoring measurement errors, or by (*semi-parametric*) models without accounting for time-invariant heterogeneities across units. Ignoring time-invariant characteristics, which may affect the output but cannot be influenced by the government (e.g., geography), give rise to biased government efficiency estimates. I address the concerns by calculating cost inefficiency scores of municipalities based on a semi-parametric stochastic frontier analysis (SFA) including the "true fixed effects" specification by Greene (2005), which is innovative among empirical studies examining government efficiency. My estimation approach disentangles government inefficiencies from measurement errors, and from time-invariant factors to produce conditional and unbiased efficiency estimates.[3]

Third, I investigate electoral cycles in the executive branch (*mayoral elections at the local government level*). Empirical studies examining the effects of executive cycles are scarce (e.g., Rose, 2006; Garmann, 2017a,b; Hessami, 2018; Foremny *et al.*, 2018) because chief executives are oftentimes either not directly elected, or elections are held simultaneously in all units in the same year. Another reason occurs if executive (e.g., mayoral) and legislative (e.g., council) elections always coincide such that the effects on policy outcomes cannot be clearly attributed. Scholars have shown that the effect of electoral cycles may differ among both governmental branches and when cycles overlap (e.g., Foremny *et al.*, 2018). I disentangle executive electoral cycles from overlapping cycles in which mayoral and council elections coincide, while accounting for general annual effects. My results show that marginal effects of pre-election and election years on cost savings (at a given output level) are larger in overlapping than in individual executive election years. The results, moreover, suggest that effects are larger when the incumbent mayor runs for office again.

Finally, examining how electoral cycles influence overall government efficiency is new. I contribute to the understanding of the role of democratic institutions (in particular elections) for a cost-efficient use of public resources, and provide evidence whether re-election incentives give rise to (in)efficient policies.[4]

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[3] Asatryan and De Witte (2015) use another novel, non-parametric approach which is not deterministic and allows to calculate conditional efficiency including time-invariant factors.

[4] Related studies examine how electoral cycles affect corruption (e.g., Potrafke, 2019a), or misallocation of public funds (e.g., Finan and Mazzocco, 2020).

Identifying the effect of electoral cycles on government efficiency is challenging at the national or state level. Governments at higher tiers are often not comparable in size, institutions, rights or functions, and are affected by many confounding factors. By contrast, local governments within a single state operate under a more homogenous institutional framework than higher tiers of governments across states or countries. Studies using local data are thus less likely to suffer from unobserved heterogeneity. In addition, there are many more municipalities within one state, than (German) states or even countries. Exploiting variation at the local level thus increases both the sample size and the variation in electoral cycles which give rise to the power of the statistical tests.

I focus on the local level in the German state of Bavaria. The institutional setting of Bavarian municipalities provides some more advantages for examining the relationship of elections and efficiency. First, decentralization of responsibilities and fiscal autonomy are prerequisites for local politicians to influence the provision of public goods and services by own budgetary decisions.[5] Historically, the Bavarian municipalities have a high degree of autonomy in the provision of public goods and services and a high degree of fiscal autonomy. Bavarian municipalities are jointly governed by a council and a mayor (executive) who acts as head of the council and head of the municipal administration. The mayor is very powerful and can, for example, set the political agenda and can influence the duration of decision making processes and the implementation of outcomes. Decisions of the local government are time determinable and targetable and thus fulfill an important requirement for strategic behavior to promote re-election. Second, a high degree of transparency facilitates to better monitor activities of (local) politicians and for them to be punished and rewarded accordingly in direct elections (see Alt and Lassen, 2006a,b; Borge *et al.*, 2008; Geys *et al.*, 2010).[6] The on average small population size of the Bavarian municipalities enables citizens to get involved in local government affairs (e.g., Asatryan and De Witte, 2015). Moreover, fiscal transparency makes the politicians accountable for their decisions. As council meetings on municipal budget plans are public, budgetary decisions and outcomes can be attributed to individual local politicians.[7] Given the institutional environment in Bavaria, electoral cycles are expected to increase the efforts of incumbents seeking re-election.

[5] Scholars have shown that (fiscal) decentralization and autonomy increase government performance (Seabright, 1996; Feld and Voigt, 2003; Barankay and Lockwood, 2007; Kappeler and Välilä, 2008; Hindriks and Lockwood, 2009; Geys *et al.*, 2010).

[6] Empirical evidence suggests that transparency reduces expansionary public spending and budget deficits before elections (Akhmedov and Zhuravskaya, 2004; Alt and Lassen, 2006a,b; Montes *et al.*, 2019), and increases accountability and government efficiency at the cross-country and local government level (Montes *et al.*, 2019; Guillamón and Cuadrado-Ballesteros, 2020). Moreover, it is argued that communication and information increases citizen participation (Lassen, 2005; Ebdon and Franklin, 2006). In addition, related studies using samples of German municipalities show that mayors directly elected by voters are more effective in providing public goods than appointed mayors, and have more incentives to attract government grants in election years (Hessami, 2018; Gaebler and Roesel, 2019).

[7] In contrast, studies examining electoral cycles in the fiscal policies of German states may not be able to account for the state governments' discretionary influence (e.g., Galli and Rossi, 2002; Schneider, 2010).

## 5.2 Theoretical background and related literature

From a theoretical perspective, it is not *a priori* clear whether electoral cycles change governments' efficiency in providing public goods and services. Following the literature on electoral cycles, governments can influence fiscal aggregates and public policies around election years (see Section 5.1). On the one hand, expenditure changes may well coincide with analogous changes in the level of provided public goods and services. Electoral cycles may therefore increase both, public spending and public service provision without changing cost efficiency. On the other hand, electoral cycles are likely to give rise to a wasteful use of public resources if incumbent politicians seeking re-election inefficiently pursue expansionary policies. Electoral cycles, however, may also decrease inefficiencies. While voters may appreciate an extended provision of public goods and services, empirical evidence suggests that voters do not like to pay much for an extended provision of public goods and services — the latter supports the idea of a fiscally conservative voter (see Peltzman, 1992; Brender, 2003; Brender and Drazen, 2008; Drazen and Eslava, 2010; Feld and Matsusaka, 2003; Garmann, 2017b; Geys and Vermeir, 2008).[8] Therefore, "an efficient provision of public goods and services [...] is likely to win voters' hearts" (Kalb *et al.*, 2012, p. 201), or as Asatryan and De Witte (2015, p. 59) emphasize, "voters arguably have much clearer positions against inefficient governments than regarding the level of its expenditures or taxes".

The relationship between elections and government efficiency may well be studied in a (nonmarket) principal-agent-setting in which the population (voters) acts as principal and the incumbent politicians (bureaucrats) act as agents. For a given level of fiscal costs, the population demands as many public goods and services as possible. The conflict of interests between the principal and the agent arises because the incumbents are expected to benefit from less efforts and less productive activities (e.g., rent-seeking, wasteful spending and over-employment) which increase cost inefficiency (Niskanen, 1968; Migué and Bélanger, 1974). Democratic institutions (e.g., elections) which ensure political competition and political participation by citizens, and which allow the citizens to monitor and reward or punish incumbent politicians accordingly, are expected to enhance the incumbents' incentives and efforts to prevent inefficiency in the use of public resources (see Leibenstein, 1966; Niskanen, 1975; Ostrom and Ostrom, 1971).

Empirical evidence on the effect of democratic institutions – and in particular elections – on government efficiency is, however, limited.[9] Some studies at the local level show that democratic participation and voter involvement (e.g., Borge *et al.*, 2008; Geys *et al.*, 2010),

[8] Empirical evidence on the effect of public spending on electoral gains is ambiguous (see Brender, 2003; Brender and Drazen, 2008; De Haan and Klomp, 2013). Some studies find that incumbent politicians benefit from pursuing expansionary policies (e.g., Akhmedov and Zhuravskaya, 2004; Freier, 2015), while others show that in developed countries incumbents are more likely to be punished by voters for loose fiscal policies and deficit spending with strong democratic and fiscal institutions (e.g., Brender, 2003; Brender and Drazen, 2008).
[9] In a cross-country study, Adam *et al.* (2011) find that democratic governments are more efficient than autocratic governments.

political competition (e.g., Ashworth *et al.*, 2006; Geys *et al.*, 2010; Kalb, 2010), and direct democratic institutions (e.g., Asatryan and De Witte, 2015; Matsusaka, 2009) improve the efficiency of the provision of public goods and services by local governments.[10] By contrast, Finan and Mazzocco (2020) use data in Brazil and find evidence that electoral incentives give rise to misallocation of public funds. They conclude that welfare effects of electoral cycles depend on the institutional framework of (local) governments.

Some scholars use the local level in Germany to elaborate on the relationship between democratic institutions and government efficiency. Geys *et al.* (2010) and Kalb (2010) show how political competition and voter involvement increase efficiency of municipalities in the state of Baden-Württemberg. The efficiency-enhancing effect of democratic institutions, however, is positively affected by the degree of the fiscal autonomy of local governments. Similarly, Asatryan and De Witte (2015) show that citizens' initiatives as an element of direct democracy give rise to greater efficiency of local governments in a cross-section of Bavarian municipalities.

Related studies also use the institutional setting at the local level in Germany to provide empirical evidence for electoral cycles in political decisions and fiscal outcomes. Scholars suggest that local incumbent politicians use their influence in public firms to ensure that voter-friendly firm decisions are made before elections, for example decisions on electricity prices by public energy providers (Englmaier *et al.*, 2017) or the quantity of savings bank lending by public banks (Englmaier and Stowasser, 2017). Elaborating on large panels using West German municipalities, empirical evidence is shown for electoral cycles in local government fees (Krause, 2019), and on local tax growth rates around council election years (Foremny and Riedel, 2014; Furdas *et al.*, 2015). However, results for the effect of council election cycles on expenditures are ambiguous (Furdas *et al.*, 2015; Foremny *et al.*, 2018). Evidence for the executive branch rather suggests that expenditures increase in post-election years (Foremny *et al.*, 2018). Based on a sample of municipalities in the states of Baden-Württemberg and Bavaria, Foremny *et al.* (2018) show that the effect of electoral cycles is more pronounced when executive and council elections coincide and that it depends on the incumbent mayor's decision to run for office again.[11] Findings in a small sample of municipalities in the German state of Hesse show that incumbents rather decrease spending before elections when the electorate is fiscally conservative (Garmann, 2017b). By contrast, the number of building permits is increased by local public administrations in years in which executive elections are held (Garmann, 2017a).

[10] Several studies examine determinants of government efficiency at the country level (e.g., Afonso *et al.*, 2005), or the regional and local government tier (see Kalb *et al.*, 2012; Narbón-Perpiñá and De Witte, 2018a,b, for surveys of empirical studies on local government efficiency).

[11] Findings by Freier (2015) suggest that incumbents in municipalities of the German state of Bavaria over the period 1945-2010 gain an advantage in the next mayoral elections if they succeed in increasing public spending during the entire election term. He does, however, not disentangle the effects for electoral cycles and the timing of public spending.

## 5.3 Institutional background

Bavaria has a population size of around 13 million people living in 2,056 municipalities. This means that a municipality has an average of about 6,000 inhabitants, while half of all municipalities have a population of less than 2,800. The municipal level is the lowest layer of four governmental tiers in Germany.[12] Local governments in Bavaria act under a quite homogenous institutional framework, the same administrative and election laws, responsibilities and rights. All municipalities are influenced by the same federal and state government decisions, have access to the same capital market, and use quasi-identical labor and capital cost structures.[13]

### Local constitution

Local governments have a high degree of autonomy in providing public goods and services and are responsible for approximately 46 % of public spending in the German state of Bavaria.[14] The high degree of autonomy of municipalities was first established by the Bavarian royal decree of 1818 (Gemeindeedikt, 1818). After World War II, German and Bavarian constitutional laws reaffirmed the rights of municipalities to self-administration, including the direct election of the local government, personnel sovereignty and a high degree of financial autonomy.[15] Direct responsibilities of governments at the municipal level include, for example, child care provision, primary and secondary schools, investments in local infrastructure (e.g., buildings, municipal roads, public transport, and water supply), and tasks of public order (e.g., fire protection and municipal cleanliness).[16] Local governments, moreover, oversee local public firms, and have opportunities to promote culture (e.g. music schools and libraries), to provide infrastructure for recreation, or to invest in economic development. In addition, the local public administration fulfills some mandated tasks of higher governmental tiers such as several administrative activities (e.g., registry of population and building licenses), social welfare spending, or the local organization of elections. Local governments in Bavaria spend an average of around 3,600 Euros per capita, whereas about 1/3 of the budget were for child

[12] Germany has 16 federal states, about 300 counties and about 11,000 municipalities. In Bavaria, 25 of the 2,056 municipalities are consolidated and independent city-counties. That is, the independent city likewise fulfills responsibilities of municipalities and counties.

[13] Factor price differences are not a problem as labor and capital costs are largely the same in all Bavarian municipalities. Wages are quite homogenous because of collective labor agreements for the public sector and civil servants. The absence of differences in risk premia for German jurisdictions at the capital market is guaranteed by the federal government. Property prices may vary across municipalities. This, however, is captured by the fixed effects estimation approach.

[14] Gross expenditures, including budgets of the local governments and local public firms (Federal Statistical Office, 2020).

[15] Art. 28 GG (*Grundgesetz der Bundesrepublik Deutschland*); Art. 11 BV (*Bayerische Verfassung*). See also Art. 1 GO (*Bayerische Gemeindeordnung*).

[16] See Art. 83 BV and Art. 57 GO.

care provision and other social welfare spending.[17] However, local governments in Bavaria have an obligation to balance the annual budget, and to follow the "principles of economic and cost efficiency" (*minimum principle*).[18] Budget plans of the local governments are highly transparent and publicly available, and council meetings on budgetary decisions are public for citizens and media. To guarantee sustainable and transparent budgeting, local governments must publish a (*non-binding*) five-year plan on future investments and large expenditures each year.

Municipalities in Bavaria are governed in joint responsibility by a directly elected mayor (*executive branch*) and a directly elected council (*legislative branch*). The council is the legislative body of the government and makes budget and policy decisions by absolute majority. Council members also have an auditing function to control the implementation of decisions. The mayor, as executive, depends on council decisions and must implement the decisions accordingly.[19] The constitutional setting in Bavaria, however, historically grants the mayor a powerful position including far-reaching rights and duties following the South German Council Constitution (*Süddeutsche Ratsverfassung*).[20] The mayor automatically becomes head of the council and all council committees, and has active voting and some veto rights. As principal-agenda-setter, the mayor is free to put topics on the agenda of the council meeting, executes the decisions of the council, and is thus able to influence the timing of fiscal decisions and implementation of policies to a significant extent. The constitution, moreover, puts the mayor as head of the municipal administration, and mandates the mayor with all personnel and day-to-day administrative and minor fiscal decisions.

### Municipal elections

In Bavaria, both executive and council elections are held every six years, and usually on the same state-wide election day. The timing of local elections is regulated by state law and largely beyond control of the local government. Exceptions are made in some executive elections if a mayoral term of office ended prematurely. A mayor's term may end prematurely if a mayor dies, or resigns for personal (e.g., sickness or preferable outside options) or political reasons (e.g., lack of political support). In that case, the municipality starts a new independent six-year executive electoral cycle. The council and mayor are free to choose whether they want to

[17] Corrected expenditure calculations show which expenses were finally necessary to fulfill the tasks in 2018 (Bavarian Statistical Office, 2018). Budget composition in 2018: social welfare (32.2 %); construction, housing and traffic (10.7 %); education and schools (10.5 %); general administration (10.3 %); public facilities and economic development (8.0 %); health, sports and recreation (4.3 %); public order and security (3.7 %).

[18] The minimum principle of the Bavarian regulation for municipalities requires to fulfill the given municipal tasks with lowest possible resources (= cost efficiency) (Art. 61 GO). In Germany, local governments have several sources to finance their policies and investments, including autonomy in several charges, fees, and taxation (e.g., business and property taxes), see Art. 106 GG.

[19] Studying overlapping electoral cycles in Bavaria is promising as the institutional setting leaves room for collusion between both branches of the local government.

[20] Many other states in Germany have constitutions with a less powerful mayor position. Bavaria thus provides a more promising institutional framework to examine executive election cycles.

return to an overlapping cycle, but this is not mandatory. By contrast, council elections do not deviate from the state-wide electoral cycle. Council elections occurred twice in the sample period, in 2008 and 2014.

Mayors are elected directly by voters in majoritarian elections. A second round of voting is required if no mayoral candidate achieves absolute majority (over 50 % of votes) in the first election. The second round is held as classical run-off election between the two leading candidates. Electoral campaigns of elections at the municipal level are usually focused on the individual candidates rather than the party affiliation. The accountability and the behavior of incumbents may therefore be decisive for electoral success and thus may lead to strategic behavior of the incumbent before elections. Incentives to get reelected are expected to be larger for mayors than for council members. Council members are usually part-time politicians, while the position of the mayor can be either full-time or part-time. However, the institutional setting give rise to incentives for the mayor to collude with council members, because the extent of the mayor's strategic influence on political decisions depends on the intensity of collaboration. Members of both governmental branches have a high degree of political exposure to one another, which is makes collaboration more likely. If electoral terms of mayors and councils overlap, it is a reasonable assumption that both governmental branches agree on large projects and investments at the beginning of the joint term of six years. Expenditure plans are transparent in the mandatory five-year financial plan, and the plans are not expected to change significantly during an overlapping electoral term.

## 5.4 Empirical strategy

The empirical analysis is based on a balanced panel of 2,012 Bavarian municipalities in the period 2007-2017. I collected the data on elections, political and municipality characteristics, fiscal outcomes and output indicators of local governments from the Bavarian Statistical Office. Summary statistics are shown in Table 5.1. The empirical analysis is based on a two-stage empirical approach: First, I employ a stochastic frontier model to estimate inefficiencies of local public governments. Second, I examine whether electoral cycles influence inefficiency scores across municipalities and time. In the following, I discuss the variables, data and model specifications in detail.

### 5.4.1 Efficiency frontier model

Local government efficiency is estimated by a relative comparison of the government's performance against a frontier consisting of best practice observations across time and other municipalities within the sample. Deviations from this best practice frontier represent inefficiency. Estimating efficiency first requires a selection of output indicators for public goods and services provided by the local government, as well as input indicators of the government

Table 5.1 : Summary statistics

| | Obs. | Mean | SD | Median | Min | Max |
|---|---|---|---|---|---|---|
| *Dependent variable* | | | | | | |
| Cost inefficiency (%) | 21935 | 13.446 | 9.161 | 10.86 | 1.61 | 152.17 |
| *Input variable (log)* | | | | | | |
| Real net expenditure | 21935 | 15.369 | 1.065 | 15.25 | 11.78 | 22.49 |
| *Output variables (log)* | | | | | | |
| Population | 21935 | 8.078 | 0.899 | 7.97 | 6.34 | 14.20 |
| Pupil population, age 6-15 | 21935 | 5.686 | 0.878 | 5.59 | 3.18 | 11.56 |
| Old age population, age$>$65 | 21935 | 6.409 | 0.952 | 6.28 | 4.39 | 12.47 |
| Employed, place of work | 21935 | 6.366 | 1.419 | 6.22 | 2.30 | 13.65 |
| Kindergarten places | 21935 | 4.888 | 0.905 | 4.74 | 2.48 | 11.34 |
| Recreational area (hectare) | 21935 | 2.309 | 1.125 | 2.21 | -4.61 | 8.24 |
| *Baseline controls* | | | | | | |
| Population density | 21935 | 192.269 | 291.362 | 104.00 | 6.00 | 4,713 |
| Migration share (%) | 21935 | 5.002 | 3.744 | 3.92 | 0.00 | 40.36 |
| Unemployment share (%) | 21935 | 2.574 | 1.067 | 2.37 | 0.35 | 9.51 |
| Incumbent runs again | 21935 | 0.494 | 0.500 | 0.00 | 0.00 | 1.00 |
| Leftwing incumbent | 21935 | 0.129 | 0.335 | 0.00 | 0.00 | 1.00 |
| Leftwing council share | 21935 | 0.126 | 0.147 | 0.06 | 0.00 | 0.68 |
| *Executive elections* | | | | | | |
| Pre-election year | 21935 | 0.186 | 0.389 | 0.00 | 0.00 | 1.00 |
| Election year | 21935 | 0.182 | 0.386 | 0.00 | 0.00 | 1.00 |
| Post-election year | 21935 | 0.183 | 0.386 | 0.00 | 0.00 | 1.00 |
| *Election characteristics* | | | | | | |
| Joint pre-election year | 21935 | 0.166 | 0.372 | 0.00 | 0.00 | 1.00 |
| Joint election year | 21935 | 0.167 | 0.373 | 0.00 | 0.00 | 1.00 |
| Joint post-election year | 21935 | 0.166 | 0.372 | 0.00 | 0.00 | 1.00 |
| Pre-election year (incumbent) | 21935 | 0.123 | 0.328 | 0.00 | 0.00 | 1.00 |
| Election year (incumbent) | 21935 | 0.122 | 0.328 | 0.00 | 0.00 | 1.00 |
| Post-election year (incumbent) | 21935 | 0.123 | 0.328 | 0.00 | 0.00 | 1.00 |
| Joint pre-election year (incumbent) | 21935 | 0.114 | 0.318 | 0.00 | 0.00 | 1.00 |
| Joint election year (incumbent) | 21935 | 0.115 | 0.319 | 0.00 | 0.00 | 1.00 |
| Joint post-election year (incumbent) | 21935 | 0.114 | 0.318 | 0.00 | 0.00 | 1.00 |
| Regular election | 21935 | 0.172 | 0.377 | 0.00 | 0.00 | 1.00 |
| *Instruments for incumbency* | | | | | | |
| Incumbent is pensionable | 21935 | 0.283 | 0.451 | 0.00 | 0.00 | 1.00 |
| Incumbent, age$\geq$60 | 21935 | 0.280 | 0.449 | 0.00 | 0.00 | 1.00 |
| *Robustness test variables* | | | | | | |
| Population size | 21935 | 6,308 | 34,689 | 2,881 | 565 | 1.46m |
| Real public debt (log) | 19656 | 7.379 | 1.553 | 7.47 | 0.00 | 15.00 |
| Independent municipality | 21935 | 0.013 | 0.111 | 0.00 | 0.00 | 1.00 |
| Full-time mayor | 21934 | 0.567 | 0.495 | 1.00 | 0.00 | 1.00 |
| CSU council share | 21935 | 0.254 | 0.210 | 0.31 | 0.00 | 1.00 |

to produce the output. The input-output combinations determine efficient behavior of the decision-making units in my model, namely the local governments which use the input in the most productive way.

The best practice frontier is estimated by using an efficiency model. The efficiency model is based on a semi-parametric, multi-output stochastic frontier analysis (SFA), following the seminal work of Aigner *et al.* (1977) and Meeusen and van Den Broeck (1977). Because of the Bavarian minimum principle (see Section 5.3), I employ the cost efficiency approach and relate multiple public policy outputs of local governments to the overall costs. The cost frontier model characterizes the minimum expenditure required to produce a fixed bundle of multiple outputs (given the input prices used in the production). Cost efficiency at the municipality level is estimated by a classical Cobb-Douglas production specification:

$$lnC_{it} = \alpha_i + \sum_{r=1}^{s}(\delta_r \times lnY_{rit}) + \rho \times \tau + \omega_{it} \tag{5.1}$$

Here, $C_{it}$ indicates the input (costs) in municipality $i$ in year $t$, $Y_r$ indicates multiple outputs ($r$) of local governments, and $s$ is the number of included outputs in the model. $\delta_r$ and $\rho$ are the parameters to be estimated to determine the cost efficiency frontier. Moreover, the efficiency frontier model (5.1) accounts for the overall efficiency-enhancing time trend $\tau$ over the years 2007-2017 ($\tau$ = 1 ,..., 11), for example due to technological progress, and uses robust standard errors clustered at the municipality level. The composed error term $\omega_{it}$ consists of the symmetric and idiosyncratic error term ($\upsilon_{it}$) and a one-sided non-negative component ($u_{it} \geq 0$) representing inefficiency.

$$\omega_{it} = \upsilon_{it} + u_{it} \tag{5.2}$$

The cost inefficiency values $u_{it}$ are computed by using the estimator by Jondrow *et al.* (1982) and are assumed to follow a truncated normal distribution.[21] The latter assumption ensures that inefficiency values are larger than or equal to zero. The mean value for the point estimate $u_{it}$ indicates to what extent inputs can be reduced without reducing current output levels.

The stochastic parametric estimation approach allows to distinguish the measurement error from inefficiency. The parametric SFA is preferable to non-parametric approaches. Non-parametric approaches are often deterministic in nature, interpreting all deviations from the efficiency frontier as inefficiency measures, ignoring measurement errors and outliers, and are therefore likely to produce biased estimates.[22] Non-parametric approaches have been

[21] The estimator uses the mean value of the conditional distribution of $u_{it}$.

[22] Scholars often use Data Envelope Analysis (DEA) and Free Disposal Hull (FDH) as non-parametric estimation approaches. By contrast, semi-parametric models may produce biased estimates if the functional form of the parametric assumptions on the cost frontier is misspecified.

used in several previous studies to estimate local public-sector efficiency (e.g., De Borger *et al.*, 1994; De Borger and Kerstens, 1996; Balaguer-Coll *et al.*, 2007; Afonso and Fernandes, 2006). Other scholars use SFA approaches in cross-sectional efficiency models, but do not account for time-invariant heterogeneity across municipalities (e.g., Geys *et al.*, 2010; Geys, 2006; Kalb *et al.*, 2012; Kalb, 2010; Grossman *et al.*, 1999; Lampe *et al.*, 2015). Municipalities may differ in many characteristics, for example due to natural (e.g., geography), structural (e.g., economic environment), or socio-economic (e.g., composition of population) reasons. These factors may affect the performance of the government, even though the local government cannot influence these factors in either the short or long run. Ignoring time-invariant factors within the sample period may give rise to a misspecification bias.[23]

By contrast, I use a panel data model and employ the "true fixed effects" stochastic frontier specification by Greene (2005), which uses a maximum-likelihood dummy variable (MLDV) estimation and allows to account for unobserved time-invariant heterogeneity among municipalities ($\alpha_i$).[24] Notably, my panel on government efficiency is quite large compared to related studies. Most studies use cross-sectional data for single years or pooled data for a few years to calculate government efficiency and do not allow to examine within-variation of determinants over time.[25]

### Input and output variables

Estimating inefficiencies by employing equations (1) and (2) requires the selection of input and output indicators of local governments in Germany. My selection of indicators is related to previous studies examining public sector efficiency in German municipalities (e.g., Geys *et al.*, 2010; Kalb *et al.*, 2012; Kalb, 2010; Lampe *et al.*, 2015; Asatryan and De Witte, 2015). I employ municipality's *real government expenditure (as net of transfers)* as input $C_{it}$ in the efficiency model (5.1). Cost efficiency is measured as a relative concept which relates the total expenditure of the municipality to several output variables which capture key tasks under the direct control of local authorities in the German state of Bavaria (see Section 5.3). As proxies for

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[23] As this approach assumes the intercept $\alpha$ to be the same across all municipalities, time-invariant unobservable factors may affect the output. The effect may be captured by the inefficiency term and produce biased estimates, which result in an overestimation of the inefficiency term.

[24] Greene (2005) shows that the MLDV is computational feasible for large samples ($N >$ 1000), but an incidental parameters problem may occur for samples with large $N$ and small $T$ (Neyman and Scott, 1948; Lancaster, 2000). The MLDV approach, however, is appropriate for panel data models with $T \geq 10$ (Belotti and Ilardi, 2018; Belotti *et al.*, 2013). The signal-to-noise ratio in the stochastic frontier specification does not suggest identification problems because of the distributional assumptions of the error components (Appendix, Table A5.1).

[25] See Kalb *et al.* (2012) for an overview. Some scholars use SFA and pooled data for a few years. Grossman *et al.* (1999), for example, employ four years to calculate inefficiency scores for a sample of 49 US cities, whereas Geys *et al.* (2010) and Lampe *et al.* (2015) both use three years in a sample of around 1,100 or 400 German municipalities. Only a few exceptions use large panels: Kalb (2010), for example, use around 1,100 German municipalities from 1990 to 2004 and examine the effect of intergovernmental and vertical grants on cost efficiency scores. Dorn *et al.* (2021b) employ non-parametric DEA scores of county governments in a panel of 96 German counties for the 1995-2016 period to examine the effect of public accounting standards.

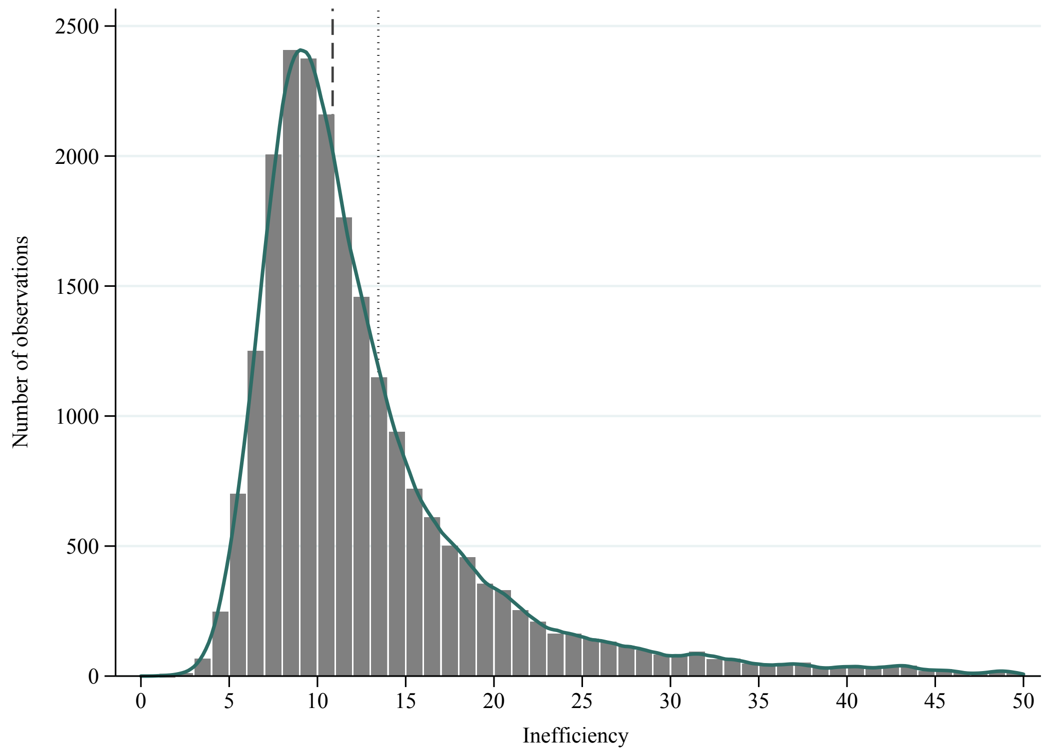
the provision of public goods and services provided by the local government, I cover as many output indicators as the data for the period 2007-2017 allow. I include five output variables in the vector term $Y_r$ ($s$ = 5) of model (5.1): (i) *population size* as an indicator for several administrative tasks which depend on the number of people living in a municipality; (ii) *pupil population aged 6-15 years*, and (iii) *kindergarten places*, both are indicators of the legal obligations of the local governments to provide child care, education (nursery/primary/secondary school), and public transport for children living in the municipality; (iii) *population older than 65* as an indicator for local public provision of services for senior citizens; the number of (iv) *employed people (paying social security contributions) at the workplace* captures the economic performance within a municipality and provides information on the role of the local government in providing infrastructure and services; and finally the share of (v) *recreational area* of the total land is used as an indicator for local public facilities for health, sport and leisure, as well as tourism. All input- and output indicators are used in their natural logarithm form.

A criticism of efficiency frontier models is that efficiency scores are imperfect measures of local government performance, which are mainly due to data limitations. On the one hand, the output indicators only proxy key tasks of local authorities, but they may omit further relevant dimensions of the local provision of public goods and services that are under discretionary control of the local government. On the other hand, local governments may increase efficiency by producing either more or better public goods and services (quantity, quality, or both). For a full assessment of the efficiency of local governments, the (often unobservable) quality of the provision of public services needs to be considered (see Balaguer-Coll *et al.*, 2007; Dorn *et al.*, 2021b; Fritzsche, 2019). However, quality indicators are often not available, or are only available for individual public services. As this study aims at examining the overall efficiency of local government in a composite approach, only quantitative proxies are included in the model to capture as many output indicators as possible.

### Inefficiency scores

The cost efficiency model of equations (5.1) and (5.2) yields inefficiency scores for all municipalities and years within the sample consisting of 21,935 observations (Table A5.1, column 1). The median value of cost inefficiency is 10.9 %, while the mean value relative to the efficiency frontier within the model is 13.4 % (see Table 5.1). That is, the efficiency of the provision of public goods and services in the Bavarian municipalities is on average approximately 13 % below the efficiency frontier. In other words, municipalities could reduce expenditures by about 13 % for a given output level. Figure 5.1 shows the distribution of all inefficiency scores of Bavarian municipalities between 2007 and 2017. The pattern is quite homogenous. About 40 % of the Bavarian municipalities could reduce costs by 5 to 10 %, whereas most observations show a cost inefficiency of 8-10 %.[26] Less than 2 % of the observations are close to the cost efficiency frontier with an inefficiency value of below 5 %. By contrast, only a few outliers in the sample have high inefficiency in the production of public goods and services; for example, around 4 % of all observations could reduce expenditures by more than 30 % at the given output level.

Figure 5.1 : Cost inefficiency distribution among municipalities, 2007 – 2017




*Notes:* Deviations from zero represent cost inefficiency in percent. Outliers above an inefficiency score of 50 are not displayed. The green solid line shows the density function; the first vertical (dashed) line is the median value, the second vertical (dotted) line is the mean value.

The baseline efficiency model estimates the cost inefficiency of local governments conditional on time trends and time-invariant factors (Appendix, Table A5.1, column 1). I regress inefficiency on several variables which may explain inefficiency differences between municipalities (see Section 5.4.2). Previous studies have also included the controls (as *background variables*) in an one-step approach within the SFA frontier model (e.g., Geys *et al.*, 2010; Kalb *et al.*, 2012; Kalb, 2010). In robustness tests, I also examine one-step approaches (see Section 5.6.1).

[26] The semi-conditional inefficiency scores of the model are similar to cross-sectional results of previous studies on German municipalities. Kalb (2010) show an average inefficiency of about 17-20 % in an unconditional SFA model, and an average of 11-13 % once several control variables are included. Asatryan and De Witte (2015) employ a full conditional non-parametric model and show an average inefficiency of about 8.7 %. My baseline model relies on time trends and time-invariant factors. Once including further controls in the efficiency model, the mean inefficiency is 9.0 % (see Section 5.6.1 and Table A5.1, column 3 in the Appendix).

### 5.4.2 Electoral cycle models

#### Baseline model

I assume that inefficiency is a function of exogenous variables such as municipality characteristics and political variables.[27] The inefficiency term ($u_{it}$) released from the stochastic frontier model (5.2) is used as the dependent variable. I examine how electoral cycles influence observable differences in inefficiency scores across local governments $i$ and year $t$ by using the following OLS panel fixed effects model:

$$u_{it} = \alpha_i + \boldsymbol{\beta} \times \boldsymbol{Elect}'_{it} + \boldsymbol{\Theta} \times \boldsymbol{Z}'_{it} + \gamma_t + \varepsilon_{it} \ , \tag{5.3}$$

where the vector $\boldsymbol{Elect}'_{it}$ includes dummy variables capturing (pre-, post-) executive election dates as

$$\boldsymbol{Elect}'_{it} = \begin{bmatrix} PreElection_{it} \\ Election_{it} \\ PostElection_{it} \end{bmatrix} \text{ and } \begin{cases} = 1 & \text{in pre-election years, 0 otherwise} \\ = 1 & \text{in election years, 0 otherwise} \\ = 1 & \text{in post-election years, 0 otherwise.} \end{cases} \tag{5.4}$$

As executive election years vary across municipalities and time, my approach is comparable to a difference-in-differences estimation in which municipalities with no election in a particular year serve as control group to identify the effect of electoral years in other municipalities. The summary statistics show that around 18 % of all observations in the sample are executive election years (Table 5.1). The parameter vector $\boldsymbol{\beta}$ captures the estimated effect of (pre-, post-) election years on local government inefficiency, respectively. By estimating OLS as fixed effects model, I exploit the within variation over time and eliminate all observable and unobservable municipality-specific time-invariant effects $\alpha_i$. The term $\gamma_t$ captures the year fixed effects of other confounding factors that simultaneously influence the municipalities in the German state of Bavaria (e.g. year specific shocks such as the financial crisis; federal and state policies; or simultaneous elections for the federal, state or county parliament, or the municipal council).[28] Standard errors are robust to heteroscedasticity and clustered at the municipality level. $\varepsilon_{it}$ captures the idiosyncratic error term and $\boldsymbol{\Theta}$ is the vector of parameters of the control variables.

[27] The exogeneous variables are not inputs or outputs in the cost production function of the local public governments (Section 5.4.1), but are expected to affect relative performance of municipalities. An exception arises for the municipality fixed effects as the efficiency model (equation 5.1) already captures fixed effects to calculate unbiased cost inefficiency estimates. The control variable population density is also directly related to the output variable population(log) of the efficiency model (equation 5.1). Section 5.5 provides results including and excluding municipality fixed effects and controls.

[28] During the period of observation, three federal elections (2009, 2013, 2017), two Bavarian state elections (2008, 2013), and two council elections (2008, 2014) were held.

The vector $Z'_{it}$ includes time-varying municipality characteristics (*population density*; *migration share*; *unemployment share*) and political variables (*incumbent(reelection)*; *council share(leftwing)*; *incumbent(leftwing)*) as baseline controls which could explain inefficiency differences among Bavarian municipalities (see Geys *et al.*, 2010; Kalb *et al.*, 2012; Kalb, 2010; Lampe *et al.*, 2015). The first set of controls includes time-varying municipality characteristics and the socio-economic composition of the population: population density captures the urban-rural divide. Higher population density is expected to make the provision of public goods and services more cost-effective, but also to increase property prices and agglomeration problems of the municipality. Similarly ambiguous results are expected for the share of unemployed. Higher unemployment goes along with higher administrative efforts and higher spending on unemployment-related benefits. However, unemployed voters are expected to demand less expensive (lower quality) public goods and services (*as the demand for higher quality public goods is expected to increase with income*). Administrative tasks and services are moreover expected to increase with a higher migration share. The migration share in the sample is on average 5 %, whereas the maximum share is 40 % (Table 5.1). The average unemployment rate in the population is about 2.5 % and the maximum is around 9.5 %. The second set of controls include political characteristics of the incumbent mayor and the municipal council. The controls include the share of seats of leftwing parties in the municipal council (*council share(leftwing)*) and a dummy *incumbent(leftwing)* indicating whether the incumbent mayor belongs to one of the parties on the left of the ideological scale in Germany.[29] Around 13 % of seats and incumbents are left (Table 5.1). Leftwing governments are expected to favor higher spending (see Potrafke, 2017), though it is not clear whether higher spending affects efficiency. The dummy variable *incumbent(reelection)* takes the value one if the incumbent mayor seeks re-election in the next executive election, otherwise it takes the value zero. It is set at zero or one for the whole electoral term assuming that the incumbent considers to run for office again.[30] In the sample, the incumbent seeks re-election in around 49 % of all observations (Table 5.1).

### Event study model

The event study model is based on a difference-in-differences approach and visualizes whether local governments perform differently during, before and after election years. The executive elections of the municipalities take place at different times. However, the design of an event study includes all electoral cycles in one parallel world, irrespective of the year in which the executive election takes place. Event studies therefore use all treated municipalities as coun-

[29] The share of leftwing parties in the council includes the seats of the Social Democrats (SPD), the Greens (Bündnis90/Die Grünen), and the Left Party (Die Linke).

[30] In robustness tests, further control variables are included in the model such as the natural logarithm of the municipality debt level, a dummy indicating whether the mayor is a full-time politician, or the council seat share of the CSU as the dominant party in Bavaria (see Section 5.6.1). For years without information on the next election, the dummy is set to zero. Robustness tests show the results for setting the dummy to one for unknown years.

terfactuals for each other and account for common trends assumptions across municipalities in non-election years. The event study examines whether years within a full election cycle deviate from the common trend.

I extend the baseline panel data model (5.3) by estimating dummy variables for all years before and after executive elections. My event window includes six years and illustrates individual year effects in a regular executive election cycle in the German state of Bavaria (see Section 5.3). The identifying assumption is that only election years deviate from the common trend of all municipalities. The event study regression takes the following form:

$$u_{it} = \alpha_i + \gamma_t + \sum_{T=t-2}^{T=t+3} \beta_T \times (\text{election year})_{it}^{T} + \boldsymbol{\Theta} \times \boldsymbol{Z'_{it}} + \varepsilon_{it} \quad , \tag{5.5}$$

where $u_{it}$ describes inefficiency in municipality $i$ in year $t$. Municipality and year fixed effects are captured by $\alpha_i$ and $\gamma_t$. $\boldsymbol{Z'_{it}}$ is the vector of control variables following equation (5.3), and $\varepsilon_{it}$ denotes the idiosyncratic error term. $\sum \beta_T$ refers to the coefficients of interest and estimates the effect of election cycles from $t-2$ years before an executive election to $t+3$ years afterwards. The year $t+3$ is the midterm of the executive election cycle and corresponds equally to the effect $t-3$ years before the next executive election. The year $t-2$ serves as the reference year in the event study. The sample (of the event window) allows to include several election cycles for each municipality in a parallel world. It is, however, restricted to cycles without cross-elections of executive elections within a municipality. A year is defined as a cross-election year if (pre-, post-) executive election years overlap within the same municipality. This may occur if an executive election is held prior to its regular cycle of six years, for example if the post-election year of the previous executive election is also the pre-election year of the succeeding election. Cross-election years within one municipality would cause biased estimates in the event study approach and are therefore either excluded from the sample (see Section 5.5.2), or included as an additional control variable in the regression model (see Appendix, Table A5.2).

### Interaction effects model

The effects of executive election cycles on efficiency may differ, depending on whether the years of executive and council elections overlap. On the one hand, the municipal council approves the municipal budget and investment plan and monitors the fiscal decisions of the local government. On the other hand, council members may have incentives to get re-elected in council election years. In addition, the effect of executive election cycles may depend on the incumbent's decision to rerun for election. Incumbent mayors may pursue different policies when they seek re-election. To examine differences that may occur when election cycles overlap and when the incumbent seeks re-election, I include interaction terms in the estimation model (5.3) following the approach of Foremny *et al.* (2018).

First, I estimate interaction terms for joint (pre-, post-) executive and council election years to test whether effects of overlapping electoral cycles on local government efficiency differ from individual executive election effects. The interaction term effect $\phi \times \boldsymbol{Joint}_{it}^{'}$ is included in equation (5.3). The coefficient $\phi$ estimates the difference between the effects of individual and joint elections on government efficiency. The dummy vector $\boldsymbol{Joint}_{it}^{'}$ takes the value of one if (pre-, post-) election years overlap, and zero otherwise. With a few exceptions, the executive elections in the state of Bavaria are held at the same time as council elections. In the sample period from 2007 to 2017, 342 executive elections were held uncoupled from the council election year. I use these exceptions as source of variation to examine differences between the effects of individual executive and overlapping elections. In 2008 and 2014 the council elections in all Bavarian municipalities were held simultaneously. The interaction model allows to examine the effects of joint elections, although year fixed effects eliminate the identification of the effects of individual council elections.

Second, the incumbent mayor sought re-election in 2/3 of all executive elections in the sample. To separate the effects of efforts for re-election from the general effects of executive cycles in election years, I include the term $\varphi \times \boldsymbol{Incumbent}_{it}^{'}$ in equation (5.3). The dummy $\boldsymbol{Incumbent}_{it}^{'}$ takes the value one in (pre-, post-) election years if the incumbent mayor seeks (sought) re-election, and has the value zero otherwise. $\varphi$ is the parameter which estimates the difference.[31] To examine whether the effect of the incumbent's decision to seek re-election on efficiency differs when council elections are held in the same year, I add triple interactions in the model. Triple interactions capture the effect of the incumbent's decision to seek re-election depending on whether elections are held individually or jointly. Similarly, triple interactions also capture the effect of overlapping elections on efficiency depending on the incumbent's decision to run for office again.

## 5.5 Results

### 5.5.1 Baseline results

Table 5.2 presents the baseline results when estimating the effect of executive election cycles on cost inefficiency following the OLS model in equation (5.3). Column (1) reports the unconditional relationship, and column (2) the coefficient estimates of the fixed effects model for the within variation in municipalities over time. The results in columns (3) - (4) show the coefficients conditional on time-variant municipality characteristics and political control variables. Additionally the full model in column (5) captures year fixed effects which affect all municipalities simultaneously.

[31] The baseline model already included a dummy for the incumbent's decision to seek re-election in the next election (equation 5.3). The interaction vector, however, explicitly separates the general effects of executive election years from effects of years in which the incumbent sought re-election.

The results show that inefficiency in the provision of local public goods and services decreases in pre-election and election years. In executive election years, local governments reduce expenditures by 0.85 - 0.95 % at the given output level. The election year effect is statistically significant at the 1 % - level in all specifications. In the year prior to the executive election, local governments reduce cost inefficiencies by 1.2 - 1.5 % at the 1 % - significance level. The results, however, refer to estimates before year fixed effects are included in the model. Once year fixed effects are captured, local government efficiency increases by 0.75 % at the significance level of 5 % in the pre-election year. Hence, local governments reduce costs by 0.75 % and 0.85 % in pre-election and election years without reducing the quantity of public goods and services according to the full baseline model results (column 5). The efficiency-enhancing effect of electoral cycles already seems to diminish in years following the elections. In the year after the executive election, the relationship is economically less pronounced and lacks statistical significance in the full model (column 5).

Table 5.2 also provides coefficient estimates for the baseline controls. The results show that local government efficiency increases with population density, suggesting positive agglomeration effects. In contrast, higher shares of unemployed and migrants in the population give rise to an inefficient use of municipal resources by local governments. Given a constant output in public goods and services (column 5), a one percentage point increase in the unemployment rate is associated with a 0.50 % increase in expenditures. The migration effect, however, drops in size and lacks statistical significance once year fixed effects are considered. This implies that a year specific shock such as the migration and refugee crises in 2015, has affected all local governments in Bavaria and gave rise to cost inefficiency. If the incumbent mayor seeks re-election, public resources seem to be used more efficiently. The rerun effect of the incumbent, however, lacks significance when controlling for year effects. Political ideology seems to play a minor role. Leftwing ideology of the incumbent and in the council are both positively correlated with cost inefficiency, but lack statistical significance in all specifications.

### 5.5.2 Event study results

The efficiency-enhancing effect of election cycles is confirmed by the results of the event study. Figure 5.2 shows the coefficient estimates of the event study model following equation (5.5); corresponding numerical difference-in-differences estimates are presented in Table A5.2 in the Appendix. Note that all estimates are conditional on baseline controls as well as municipality and year fixed effects. The event study shows how effects evolve over a full six-year window of an electoral cycle. The reference category is two years before an executive election ($t-2$). Each dot in the figure represents one point estimate, the vertical lines are 90 % confidence intervals. Local governments significantly reduce cost inefficiency in the pre-election year ($t-1$) and the election year compared to the reference ($t-2$). The point estimates suggest that local governments increase cost efficiency compared to the reference year by 0.80 - 0.90 % in both pre-election and election years. The estimated coefficients do not turn out to be statistically different from the refence year ($t-2$) in the years after the

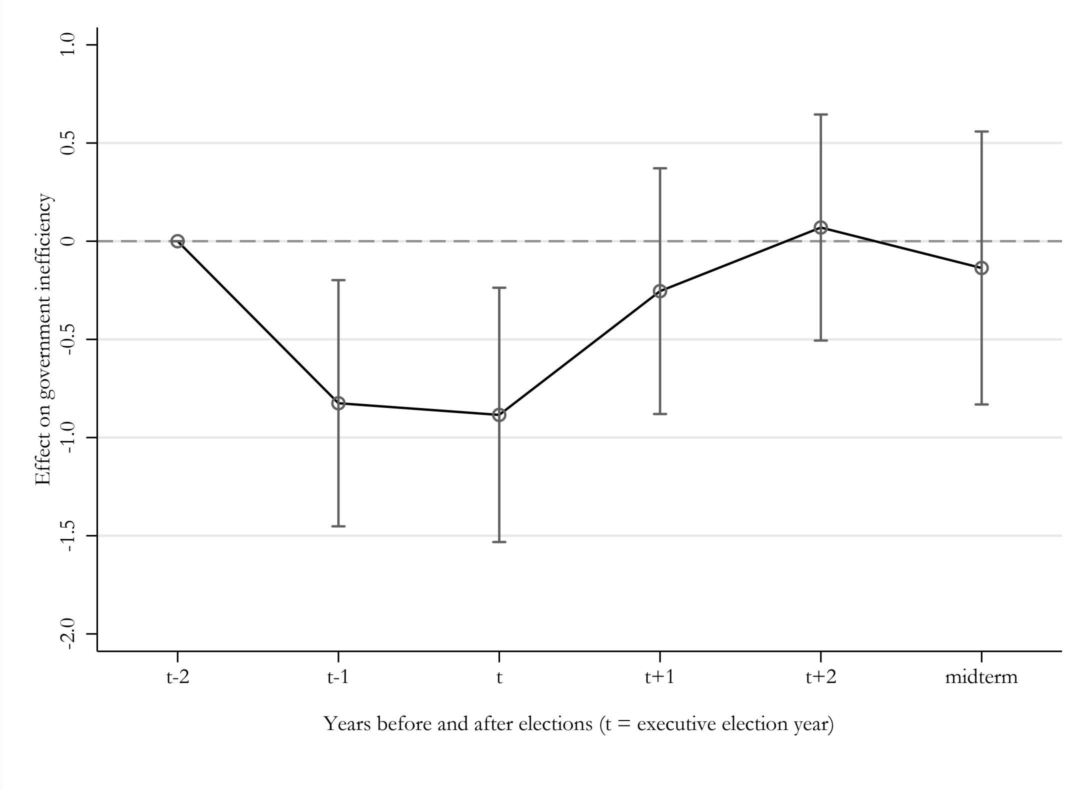
Table 5.2 : Baseline results – electoral cycles and cost inefficiency (OLS)

| | Dependent variable: Cost inefficiency | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *Executive elections* | | | | | |
| Pre-election year | -1.217*** | -1.247*** | -1.529*** | -1.486*** | -0.750** |
| | (0.165) | (0.166) | (0.172) | (0.173) | (0.355) |
| Election year | -0.877*** | -0.914*** | -0.936*** | -0.851*** | -0.848*** |
| | (0.169) | (0.171) | (0.170) | (0.173) | (0.308) |
| Post-election year | -0.063 | -0.092 | -0.324* | -0.372** | -0.173 |
| | (0.168) | (0.170) | (0.174) | (0.177) | (0.314) |
| *Controls* | | | | | |
| Population density | | | -0.012** | -0.012** | -0.015*** |
| | | | (0.005) | (0.005) | (0.005) |
| Migration share (%) | | | 0.201*** | 0.167*** | 0.023 |
| | | | (0.047) | (0.048) | (0.057) |
| Unemployment share (%) | | | 0.762*** | 0.832*** | 0.518** |
| | | | (0.144) | (0.144) | (0.210) |
| Incumbent runs again | | | | -0.413** | -0.012 |
| | | | | (0.191) | (0.207) |
| Leftwing incumbent | | | | 0.222 | 0.195 |
| | | | | (0.454) | (0.456) |
| Leftwing council share | | | | 1.813 | 1.995 |
| | | | | (2.318) | (2.315) |
| Constant | 13.844*** | 13.862*** | 13.208*** | 13.227*** | 17.190*** |
| | (0.104) | (0.073) | (1.061) | (1.099) | (1.160) |
| Year fixed effects | No | No | No | No | Yes |
| Municipality fixed effects | No | Yes | Yes | Yes | Yes |
| Municipalities (cluster) | 2012 | 2012 | 2012 | 2012 | 2012 |
| Observations | 21935 | 21935 | 21935 | 21935 | 21935 |

*Notes:* OLS FE model with standard errors clustered at the municipality level in parentheses. Significance levels: ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$. Cost inefficiency estimates released from a true fixed effects multi-output stochastic frontier model. Cost inefficiency, unconditional mean value: 13.45 % (see Section 5.4.1).

executive election — neither in the post-election year $(t+1)$, nor two years after the election $(t+2)$, nor the midterm year $(t+3, t-3)$. This finding suggests that the common trends assumption holds in non-election years.

Figure 5.2 : Event study results – electoral cycle and cost inefficiency




*Notes:* The dots represent the point estimates; the vertical lines indicate the 90 % confidence interval. The corresponding numerical numbers are shown in Appendix, Table A5.2, column (1).

### 5.5.3 Overlapping cycles and incumbency effects

#### Overlapping cycles

The baseline results show that the size and significance of some coefficients change when year fixed effects are considered. Year fixed effects include the effects of council elections which are held simultaneously in all Bavarian municipalities. It is not possible to test individual council election effects. The effect of executive elections, however, may differ depending on whether executive and council election years overlap. To test the effect of individual and joint elections, I augment the baseline model with interactions of a dummy indicating whether executive and council election years overlap (see Section 5.4.2 – Interaction effects model).[32] Figure 5.3A highlights the marginal effects of the interaction model, while distinguishing between pre-election, election, and post-election years.

The results show that the efficiency of local governments increases by around 1.5 % in both, joint pre-election and election years (Figure 5.3A). Marginal effects are statistically significant at the 1 % level and are larger in size than the general effect of executive elections estimated in

[32] Year fixed effects control for individual council effects.

the baseline results (Table 5.2). The effect of individual executive elections in non-overlapping electoral cycles also decreases inefficiency in pre-election and election years. However, the effect of individual executive electoral cycles in non-overlapping years lacks statistical significance and is smaller than the marginal effect of joint elections (Figure 5.3A). The difference between the effect of individual and joint elections is larger and even significantly different from zero in pre-election years. The findings suggest that cost savings are larger in years in which elections overlap, while the output of the local government remains constant. The results for the post-election years, however, are not statistically significant.

### Incumbency effects

Panel B in Figure 5.3 shows marginal effects of executive electoral cycles on local government inefficiency conditional on whether the incumbent seeks re-election. Cost inefficiency decreases in pre-election and election years irrespective on the incumbent's decision to re-run for election. Coefficients are statistical significant in the pre-election year only when the incumbent seeks re-election. In the election years, however, coefficients are significant independent from the incumbent's decision to re-run. The marginal effects are more pronounced if the incumbent mayor wants to be re-elected. Cost savings are 0.25 - 0.50 % lower in pre-election and election years if the incumbent mayor does not run for office again. The exact opposite holds for post-election years suggesting that the incumbent mayor behaves strategically during election years conditional on her decision to re-run for office. Differences are, however, not statistically different from zero.

### Conditional effects

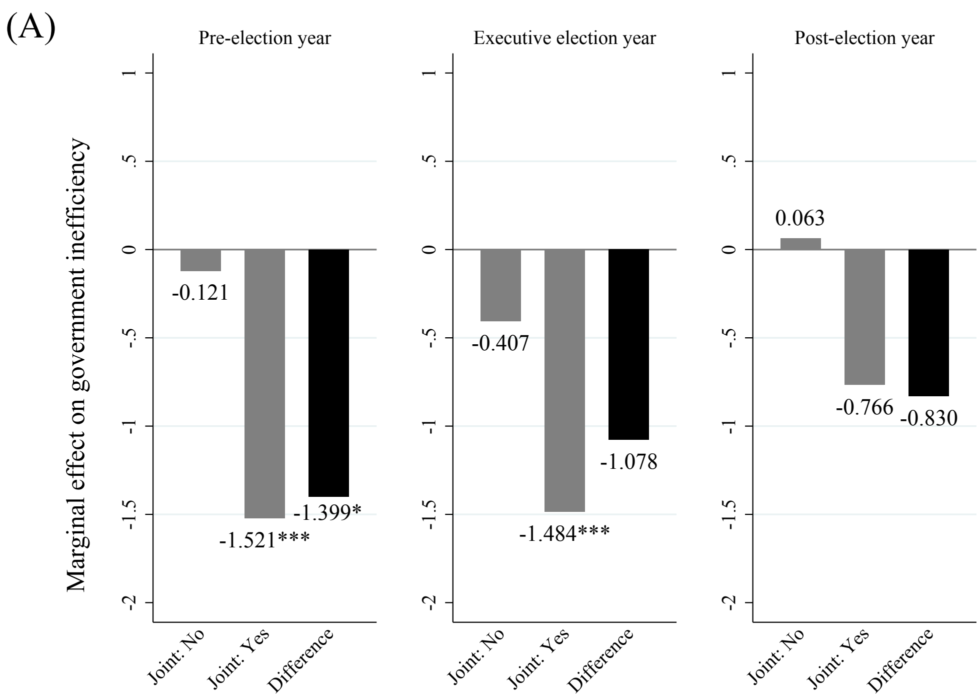
To explore underlying mechanisms in more detail, I examine triple interactions of the dummies for the executive election years, the incumbent's decision to run for re-election, and dummies whether executive and council elections overlap (see Section 5.4.2 – Interaction effects model).[33]

Figure 5.3C highlights the effects of joint elections conditional on the incumbent's decision to seek re-election. The results show that effect of joint elections are statistically significant in pre-election and election years, irrespective of the incumbent's decision to run for office again. However, the efficiency-enhancing effect of overlapping electoral cycles is more pronounced when the incumbent mayor runs for re-election. If the incumbent does not seek re-election, local governments reduce costs at a given output level by about 1.3 % in joint election years and by about 1.1 % in the year before the joint elections. The cost savings are 1.7 % (election year) and 1.9 % (pre-election year) if the incumbent decides to run for office again. In contrast, in post-election years cost-savings are less pronounced if the incumbent mayor sought re-election. This suggests that the incumbent mayor's decision to seek re-election leads to agreements on cost-saving policies between the mayor and the council before and during

[33] Appendix Table A5.3, column (3) presents all combined effects in detail.

Figure 5.3 : Marginal effects of electoral cycles on cost inefficiency – conditional on overlapping elections and incumbent's decision to seek re-election

(a) Executive and overlapping cycles (Joint election: No / Yes)




(b) Incumbent seeks re-election (Rerun: No / Yes)

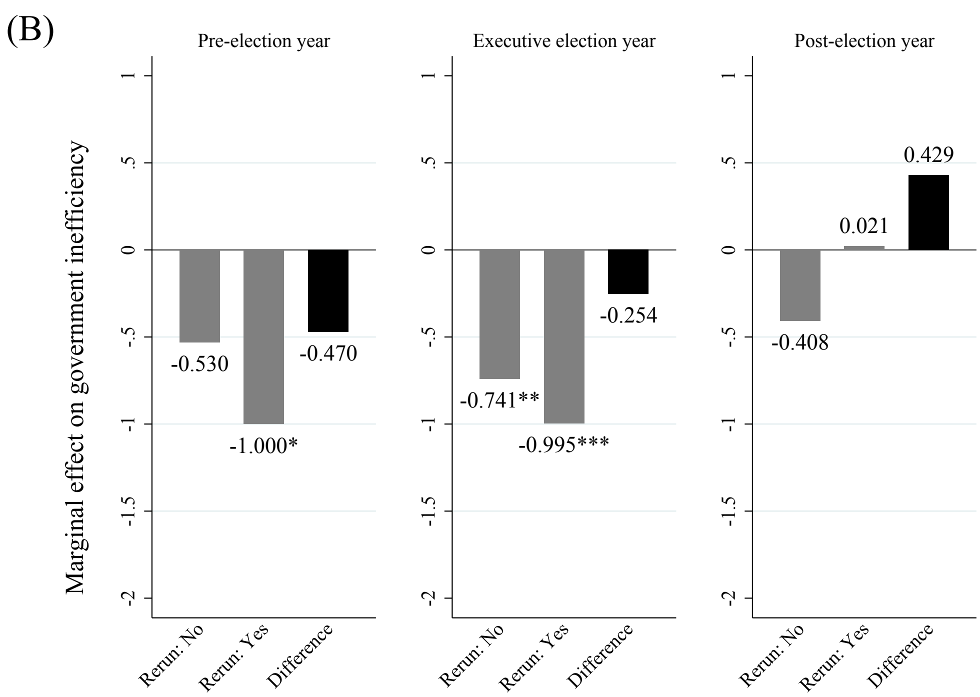
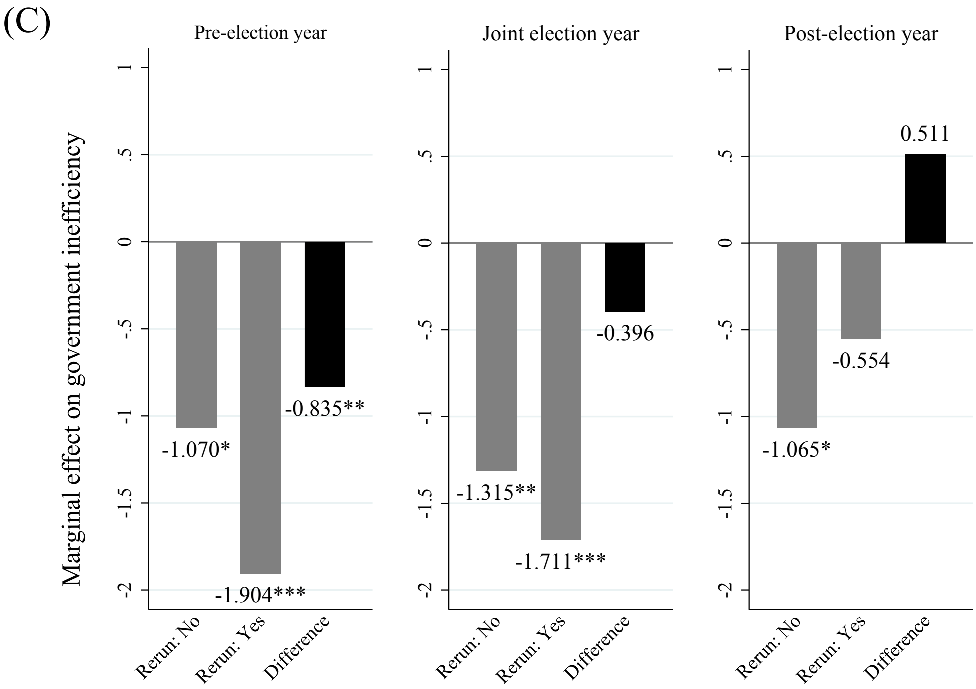
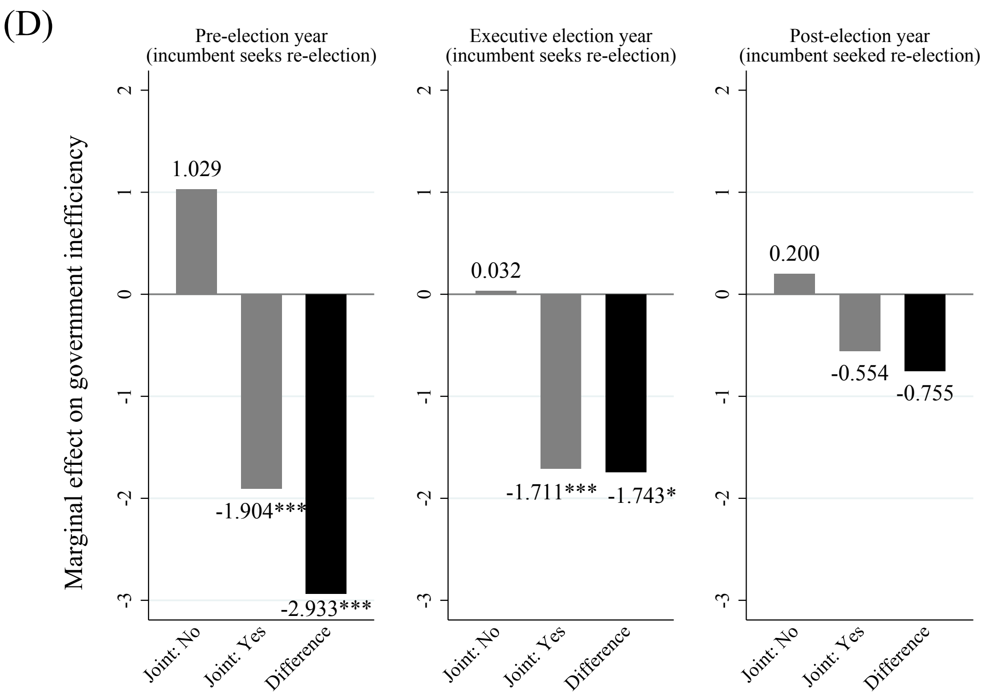


Figure 5.3 : continued

(c) Rerun in overlapping cycles (Rerun: No / Yes)




(d) Incumbent seeks re-election (Joint election: No / Yes)




*Notes:* Marginal effects of executive electoral cycles and interaction terms. Corresponding marginal effects are shown in the Appendix, Table A5.3. Panel (A) refers to column (1). Panel (B) refers to column (2). Results for panels (C) and (D) are shown in column (3).

joint election years, and that decisions on cost-intensive investments are postponed to the post-election period. In contrast, incumbents who do not run for office again may be more likely to press for some costly or even unpopular and luxurious investments to be finalized before the end of their term in office. Nonetheless, the difference in coefficients for effects of joint elections which are conditional on the incumbent's decision to re-run for election is only statistically significant different from zero in pre-election years (Figure 5.3C).

Panel D in Figure 5.3 presents the effects of electoral cycles when the incumbent re-runs for office, conditional on whether executive and council elections overlap. Again, the effect of joint elections on local government inefficiency is economically sizeable and significant at the 1 % level in pre-election and election years (as discussed in panel C). However, if the incumbent mayor runs for office in non-overlapping election years, inefficiency increases (Figure 5.3D). Cost inefficiency is about 2.9 % larger in pre-election years and about 1.7 % larger in election years than in joint election years. Differences between years of joint and individual executive elections are statistically different from zero and are even larger than the difference between joint elections and individual executive elections discussed in Figure 5.3A. The findings suggest that incumbent mayors only reduce cost inefficiencies in elections in which they can collude with the council.[34]

## 5.6 Extensions

### 5.6.1 Robustness checks

First, I include additional control variables in the full baseline model of equation (5.3). The public debt of a municipality may affect fiscal constraints and cost inefficiency of local governments. Scholars moreover show that the party alignment with the state government as well as political competition may influence local government expenditure and efficiency (e.g., Ashworth *et al.*, 2006; Geys *et al.*, 2010; Kalb, 2010). I use the seat share of the CSU in the municipal council as an indicator for the degree of local political competition and alignment with the state government.[35] Table A5.4 in the Appendix shows estimation results when including the natural log of real public debt (column 3) and the CSU seat share (column 4). Both higher public debt and a higher CSU cseat share significantly increase local government inefficiency. Inferences about the effects of the executive electoral cycles, however, do not change after including additional controls.

[34] The calculations for the incumbency effect in non-overlapping years are based on 162 sample observations. Therefore, the point estimate of the marginal effect should be interpreted with caution.
[35] The CSU was by far the most dominant party in Bavaria during the sample period and is in power of the state government since 1957.

Second, I exclude the 25 independent city-counties from the baseline sample of 2012 clusters (Appendix Table A5.4, column 2). Independent municipalities have more political responsibility in the state of Bavaria as they take on tasks of the local and county governments. Inferences about the effect of executive cycles on inefficiency do not change when independent cities are excluded.

Third, the size of municipalities in Bavaria varies. In my sample, the smallest municipality has 565 inhabitants, whereas Munich as the largest city has 1.46 million inhabitants (see Table 5.1). I split the sample at the median size of 2,881 inhabitants and examine whether effects differ in small and large municipalities (Appendix, Table A5.5). The pre-election effect is larger in small municipalities. When including year fixed effects, cost inefficiency in small municipalities decreases by 2.2 % (at the 5 % significance level) in the pre-election year, whereas the effect vanishes in the sample of larger municipalities. In the election year, however, local governments in small and larger municipalities reduce costs by about 0.7 %. Differences between small and large municipalities are also related to the type of mayor. Smaller municipalities are often governed by part-time mayors rather than full-time politicians.[36] Political competition and incentives for strategic behavior to get re-elected are more pronounced among full-time mayors since the remuneration and expected pension entitlements are higher. In another robustness test, I therefore distinguish between municipalities with full-time and part-time mayors. The results show that electoral cycles decrease inefficiency in both groups, whereas the efficiency-enhancing effects of pre-election and election years are larger in the sample of municipalities with part-time mayors (Appendix, Table A5.6).

Fourth, I employ a two-step estimation approach in my baseline models. Scholars discuss that two-step approaches may give rise to biased estimates (Simar and Wilson, 2007; Wang and Schmidt, 2002), for example, when election procedures influence municipal resources and consequently efficiency outcomes. Serial correlation among estimated efficiency scores may particularly arise in non-parametric estimation approaches without a coherent data generating process. Biased estimates are less likely in my efficiency frontier model (5.1) which is based on a semi-parametric form. In a robustness test, however, I implement simultaneous estimations to avoid potential biased estimates which arise from a two-step approach. I follow related studies and employ the maximum likelihood estimations in the SFA model without fixed effects using the model specifications by Battese and Coelli (1995) (see Kalb *et al.*, 2012; Geys *et al.*, 2010). Inferences do not change. Results are shown in 5.1 (columns 4 and 5).[37]

[36] About 57 % of the municipalities in the sample employ full-time mayors (see Table 5.1).
[37] I use the model extensions in Stata's sfpanel command as suggested by Belotti *et al.* (2013). In addition, I employ a translogarithmic frontier model (Christensen *et al.*, 1973) by using the model of Battese and Coelli (1995). I also test efficiency results by excluding individual output variables in the stochastic frontier model (e.g., recreational area). Inferences do not change. Results for the translogarithmic approach and efficiency estimates excluding individual output indicators are provided upon request.

### 5.6.2 Endogeneity tests and instrumental variable

An important condition for identifying effects of electoral cycles is the exogeneity of election dates. In Bavaria, election dates are set exogenously and are regulated by state law. Municipal executive elections are held every six years after a complete mayoral term. Municipalities and the incumbent mayor do not even have influence on the election date within an election year, or only very limited influence within a short time window in the election year.[38] Politicians therefore adjust their strategic behavior given the exogenous election dates.

Endogeneity, however, may be an issue if a mayoral term ends prematurely. This may be the case if (1) the incumbent mayor dies or resigns because of sickness (which is arguably exogenous), or if (2) the mayor resigns for other (personal or politically) motivated reasons (which could arguably be endogenous). In a robustness test, I exclude all (649) observations in the sample with incomplete - prematurely terminated - electoral cycles from the baseline model to avoid endogenous timing of the election dates. The results of the reduced sample are highly significant and are consistent with the baseline results before including year fixed effects (Appendix, Table A5.7). If year fixed effects are included, the coefficient estimate of the reduced sample is slightly smaller (than in the full sample), and lacks statistical significance in the pre-election year. The effect remains robust in size and significance in the election year across all specifications.

Another endogeneity concern may arise in the decision of the incumbent mayor to run for office again. A self-selection problem could particularly be relevant if the decision is directly linked to the fiscal and political situation in the municipality which also affects cost efficiency. To deal with potential endogeneity of the incumbency re-run variable (*incumbent runs again*), I employ an instrumental variable approach following Foremny *et al.* (2018). The institutional design in Bavaria allows to construct two instrumental variables (IV) for the decision of the incumbent mayor seeking re-election. The first instrument is a dummy that takes the value one if the incumbent mayor is entitled to a public pension (*Incumbent is pensionable*). In the German state of Bavaria, mayors are eligible if they have served as civil servant for at least 10 years.[39] The instrument is exogenous by institutional design because no direct link arguably exists between cost efficiency of local governments and the threshold for pension eligibility in the state law. The second instrument is a dummy variable that takes the value one if the incumbent mayor is above the threshold of 60 years in the year of the election (*Incumbent, age* $\geq 60$). The threshold is based on the retirement regulations for full-time civil servants in Bavaria, and the average retirement age in Germany.[40] While the retirement regulations of the state law are again exogenous by design, it cannot be ruled out that the incumbent's age has effects on political decisions. Foremny *et al.* (2018) provide sensitivity tests on the age

[38] Executive elections oftentimes coincide with the dates of the state-wide council elections. In these years, the dates of the executive elections are set.
[39] See Art.21 KWBG (*Bavarian law on local elections and civil servants*).
[40] Full-time mayors are not allowed to run for office again if they would be at the age of 67 when the new term begins (see Art 39 GLKrWG – *Gemeinde- und Landkreisgesetz*).

threshold and do not find that the age of Bavarian mayors and expenditure of municipalities are correlated. Nevertheless, I provide estimation approaches using one or the other or both instruments.

In the first stage regression, I regress the incumbency re-run dummy on the full set of baseline controls and one year lags of the instruments.[41] Both instruments are statistically significant at the 1 % - level and are highly relevant for explaining the incumbent's decision to seek re-election (Appendix, Table A5.8, first stage results).[42] Incumbents have fewer incentives to re-run for office if they are above the age of 60 and/or are entitled a civil servant pension. When the instrumented variable (*incumbent runs again*) is included in the (baseline) model as control, the results of the election year dummies do not change (Appendix, Table A5.8).

I use predicted values for the instrument of the first stage regression and construct interaction dummies of the predicted values variable with the (pre-, post-) election years, respectively (see Foremny *et al.*, 2018). I include the instrumented variable in the interaction model to account for endogeneity caused by the incumbent's decision to seek re-election (Section 5.4.2 – Interaction effects model). The results do not suggest that the coefficients of the pre-election and election years are statistically different from zero depending on the incumbent's decision to seek re-election (Appendix, Table A5.9). The results confirm the previous finding that inefficiency is higher in post-election years when the incumbent sought re-election.

## 5.7 Conclusion

Principal-agent-theories suggest that democratic institutions (e.g., elections) enhance politicians' incentives and efforts to constrain wasteful public spending and to increase efficiency. Electoral cycle theories, in contrast, predict that incumbent politicians seeking re-election are more likely to pursue expansionary policies to get reelected. There is, however, no empirical evidence whether this comes at the cost of wasteful public spending. This paper is the first approach examining whether electoral cycles influence government efficiency in the overall provision of public goods and services. I used a large panel of German municipalities and employed cost efficiency as an indicator to measure performance. Local government cost efficiencies are computed using a multi-output stochastic frontier approach which takes into account the heterogeneity beyond the control of local governments.

Interestingly, estimation results do not suggest that politicians increase wasteful spending before elections. In contrast, my results show that electoral cycles rather increase cost efficiency in the provision of public goods and services before and in election years. The

[41] Employing one-year lags of both instruments is useful for two reasons: First, the pre-election situation is likely decisive for the incumbent's decision to run for office again. Second, detailed information on the incumbents decision for the election year is missing in the data when the incumbent loses in the election or when she does not re-run for office. Both indicators are set equal to zero/one for the full term of office.

[42] Overidentification and weak instrument tests support the credibility of the IV approach.

findings are consistent with empirical studies suggesting that democratic institutions and political participation increase performance at the local government level (e.g., Feld and Kirchgässner, 2001; Borge *et al.*, 2008; Matsusaka, 2009; Geys *et al.*, 2010; Asatryan and De Witte, 2015; Hessami, 2018).

The positive effect of elections on efficiency in the executive branch is more pronounced when the incumbent mayor re-runs for office, suggesting that mayors increase their efforts and avoid (wasteful) spending in election and pre-election years to improve their chances for re-election. Another interesting finding is that the efficiency-enhancing effect of elections is pronounced in years in which mayoral and municipal council elections overlap, indicating that both governmental branches collude. Given the Bavarian institutional framework, it is likely that the council and the mayor both agree on new projects and investments at the beginning of their joint six-year electoral term — which might be reinforced by the obligation that both have to agree on transparent five-year financial plans. Wasteful spending at the end of the electoral term — and consequently before elections — is therefore less likely. My findings suggest that the widespread view that democratic politicians seek electoral gain at the cost of welfare needs some qualification.

I conclude that elections increase government efficiency given the institutional framework in the German state of Bavaria. The provision of public services and the fiscal autonomy are highly decentralized. A high degree of (fiscal) transparency moreover enables citizens to monitor local political and budget decisions. Direct mayoral elections allow voters to reward and punish efforts of incumbents accordingly. In addition, budget rules limit the possibilities for unsustainable budgeting, and citizens in Bavaria are considered to be fiscally conservative. In a referendum in 2013, for example, about 89 % of voters supported the introduction of a debt brake for the state government in the Bavarian constitution. Incumbent politicians seeking re-election in Bavaria are thus forced to place more emphasis on an economic use of public money than on (wasteful) expansionary local policies (see Garmann, 2017b).

Clearly, it would be interesting to investigate whether more cost-efficient politicians enjoy electoral advantage. The institutional context, however, might describe the preconditions for efficiency-enhancing effects of electoral cycles. An avenue for future research would be to examine how electoral cycles influence government efficiency within other institutional settings, and on different governmental layers.

# Appendix

## Tables

Table A5.1 : Stochastic frontier results

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **I. Stochastic frontier model** | Cobb-Douglas cost production function | | | | |
| *Output variables (log)* | | | | | |
| Population | 0.539*** | 0.583*** | 0.363*** | 0.689*** | 0.714*** |
| | (0.110) | (0.112) | (0.120) | (0.090) | (0.081) |
| Pupil population, age 6-15 | -0.027 | -0.041 | -0.052 | 0.073 | -0.020 |
| | (0.037) | (0.038) | (0.039) | (0.53) | (0.049) |
| Old age population, age>65 | 0.263*** | 0.267*** | 0.243*** | -0.132*** | -0.038 |
| | (0.067) | (0.069) | (0.071) | (0.049) | (0.044) |
| Employed, place at work | 0.131*** | 0.128*** | 0.129*** | 0.235*** | 0.233*** |
| | (0.022) | (0.023) | (0.023) | (0.012) | (0.011) |
| Kindergarten places | -0.013 | -0.018 | -0.024 | 0.150*** | 0.122 |
| | (0.019) | (0.020) | (0.020) | (0.023) | (0.021) |
| Recreational area | -0.011 | -0.006 | -0.009 | 0.011 | 0.014** |
| | (0.007) | (0.007) | (0.008) | (0.007) | (0.007) |
| *Time trend* | | | | | |
| Year | 0.020*** | 0.018*** | 0.020*** | 0.017*** | 0.011*** |
| | (0.001) | (0.002) | (0.002) | (0.002) | (0.002) |
| **II. Inefficiency model** | Dependent variable: Cost inefficiency | | | | |
| *Executive elections* | | | | | |
| Pre-election year | | Decrease*** | Decrease*** | Decrease*** | Decrease** |
| Election year | | Decrease*** | Decrease*** | Decrease*** | Decrease** |
| Post-election year | | Decrease** | Decrease*** | Decrease*** | Decrease |
| *Controls* | | | | | |
| Population density | | | Decrease** | | Increase* |
| Migration share | | | Increase*** | | Increase*** |
| Unemployment share | | | Increase*** | | Decrease*** |
| Incumbent runs again | | | Decrease | | Decrease |
| Leftwing incumbent | | | Increase | | Increase |
| Leftwing council share | | | Increase | | Decrease* |
| Year fixed effects | No | No | No | No | Yes |
| Municipality fixed effects | Yes | Yes | Yes | No | No |
| Municipalities (cluster) | 2012 | 2012 | 2012 | 2015 | 2015 |
| Observations | 21935 | 21935 | 21935 | 21938 | 21938 |
| *Inefficiency* | | | | | |
| Mean | 13.446 | 15.283 | 8.981 | 31.753 | 24.058 |
| Median | 10.865 | 13.713 | 8.352 | 26.788 | 18.014 |
| Model | TFE | TFE | TFE | BC95 | BC95 |
| $\lambda$ | 26.269*** | 1.066*** | 0.004 | 1.723*** | 2.810*** |
| $\sigma_\vartheta$ | 5.028*** | 0.213*** | 0.001 | 0.426*** | 0.735*** |
| $\sigma_u$ | 0.191*** | 0.200*** | 0.233*** | 0.247*** | 0.261*** |

*Notes:* Multi-output stochastic frontier models: TFE (true fixed effects) by Greene (2005), BC95 model by Battese and Coelli (1995). $\lambda$ provides the signal-to-noise ratio. Standard errors clustered at the municipality level in parentheses. Significance levels: ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Table A5.2 : Event study results

| | (1) Executive elections | (2) Executive elections | (3) Joint elections |
|---|---|---|---|
| t-2: Reference year | 0 | 0 | 0 |
| t-1: Pre election year | -0.803** | -0.801** | -1.382** |
| | (0.384) | (0.378) | (0.683) |
| t: Election year | -0.879** | -0.873** | -1.367** |
| | (0.393) | (0.391) | (0.545) |
| t+1: Post election year | -0.248 | -0.231 | -0.593 |
| | (0.382) | (0.381) | (0.584) |
| t+2: Two years later | 0.0617 | 0.0683 | 0.302 |
| | (0.350) | (0.348) | (0.518) |
| Midterm (t+3, t-3) | -0.144 | -0.135 | 0.114 |
| | (0.423) | (0.421) | (0.710) |
| *Excluded years from cycle* | | -0.870 | 0.380 |
| | | (1.177) | (0.783) |
| Constant | 17.29*** | 17.31*** | 16.95*** |
| | (1.236) | (1.232) | (1.339) |
| Controls for excl. cycle years | excl. | Yes | Yes |
| Baseline controls | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes |
| Municipality fixed effects | Yes | Yes | Yes |
| Municipalities(cluster) | 2012 | 2012 | 2012 |
| Observations | 21888 | 21935 | 21935 |

*Notes:* Estimations use the event study model (5.5). Standard errors clustered at the municipality level in parentheses. Column (1) refers to Figure 5.2. Cross-elections (several pre-, post-, election years within one year) are not included in the event study cycle years of column (1); columns (2) and (3) control for the excluded years in the estimation model. Column (3) shows event study results for overlapping executive and council electoral cycles (joint elections). Significance levels: ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Table A5.3 : Marginal effects - overlapping cycles and incumbent seeks re-election

| | Dependent variable: Cost inefficiency | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| *Overlapping elections (no=0 / yes=1)* | | | *(incumbent=0)* |
| (A) Pre-election year (joint=0) | -0.121 | | -0.931 |
| | (0.492) | | (0.586) |
| (B) Pre-election year (joint=1) | -1.521*** | | -1.070 |
| | (0.592) | | (0.605) |
| Difference (B-A) | **-1.399*** | | -0.139 |
| | **(0.830)**♯ | | (0.831) |
| (C) Executive election year (joint=0) | -0.407 | | -0.770 |
| | (0.497) | | (0.615) |
| (D) Executive election year (joint=1) | -1.484*** | | -1.315** |
| | (0.529) | | (0.575) |
| Difference (D-C) | -1.078 | | -0.544 |
| | (0.824) | | (0.899) |
| (E) Post-election year (joint=0) | 0.063 | | -0.027 |
| | (0.508) | | (0.580) |
| (F) Post-election year (joint=1) | -0.766 | | -1.065* |
| | (0.541) | | (0.593) |
| Difference (F-E) | -0.830 | | -1.038 |
| | (0.846) | | (0.898) |
| *Executive elections × incumbent runs again (no=0 / yes=1)* | | | *(joint=0)* |
| (G) Pre-election year (incumbent=0) | | -0.530 | -0.931 |
| | | (0.388) | (0.586) |
| (H) Pre-election year (incumbent=1) | | -1.000 | 1.029 |
| | | (0.416) | (0.831) |
| Difference (H-G) | | -0.470 | **1.959*** |
| | | (0.371) | **(1.010)**♯ |
| (I) Executive election year (incumbent=0) | | -0.741** | -0.770 |
| | | (0.367) | (0.615) |
| (J) Executive election year (incumbent=1) | | -0.995*** | 0.032 |
| | | (0.360) | (0.727) |
| Difference (J-I) | | -0.254 | 0.802 |
| | | (0.381) | (0.906) |
| (K) Post-election year (incumbent=0) | | -0.408 | -0.027 |
| | | (0.351) | (0.580) |

Continued on next page

| *Table continued* | (1) | (2) | (3) |
|---|---|---|---|
| (L) Post-election year (incumbent=1) | | 0.021 | 0.200 |
| | | (0.365) | (0.818) |
| Difference (L-K) | | 0.429 | 0.227 |
| | | (0.347) | (0.969) |
| *Overlapping elections (yes=1)* | | | |
| *X incumbent runs again (no=0 / yes=1)* | | | |
| (M) Joint election year (incumbent=0) | | | -1.070* |
| | | | (0.605) |
| (N) Joint election year (incumbent=1) | | | -1.904*** |
| | | | (0.605) |
| Difference (N-M) | | | **-0.835**** |
| | | | **(0.399)**♯♯ |
| (O) Joint election year (incumbent=0) | | | -1.315** |
| | | | (0.575) |
| (P) Joint election year (incumbent=1) | | | -1.711*** |
| | | | (0.560) |
| Difference (P-O) | | | -0.396 |
| | | | (0.408) |
| (Q) Joint post-election year (incumbent=0) | | | -1.065* |
| | | | (0.593) |
| (R) Joint post-election year (incumbent=1) | | | -0.554 |
| | | | (0.558) |
| Difference (R-Q) | | | 0.511 |
| | | | (0.365) |
| *Incumbent runs again (yes=1)* | | | |
| *× overlapping elections (no=0 / yes=1)* | | | |
| (S) Incumbent pre-election year (joint=0) | | | 1.029 |
| | | | (0.831) |
| (T) Incumbent pre-election year (joint=1) | | | -1.904*** |
| | | | (0.605) |
| Difference (T-S) | | | **-2.933***** |
| | | | **(1.126)**♯♯♯ |
| (U) Incumbent election year (joint=0) | | | 0.032 |
| | | | (0.727) |
| (V) Incumbent election year (joint=1) | | | -1.711*** |
| | | | (0.560) |
| Difference (V-U) | | | **-1.743*** |
| | | | **(1.009)**♯ |
| (W) Incumbent post-election year (joint=0) | | | 0.200 |
| | | | (0.818) |

Continued on next page

| *Table continued* | (1) | (2) | (3) |
|---|---|---|---|
| (X) Incumbent post-election year (joint=1) | | | -0.554 |
| | | | (0.558) |
| Difference (X-W) | | | -0.755 |
| | | | (1.083) |

Notes: Results correspond to Figure 5.3 and the interaction effects model described in section 5.4.2. Marginal effects of interaction terms computed conditional on whether elections coincide and whether the incumbent runs for reelection. Own calculations based on Stata command *lincom*. Robust clustered standard errors in parentheses. Coefficients are bold if the difference of marginal effects is statistically different from zero. Significance levels: * p $<$0:10, ** p $<$ 0:05, *** p $<$ 0:01.

Table A5.4 : Robustness (I) – Baseline including additional controls, and excluding independent city-counties

| | Dependent variable: Cost inefficiency | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Executive elections* | | | | |
| Pre-election year | -0.750** | -0.734** | -0.652* | -0.735** |
| | (0.355) | (0.364) | (0.354) | (0.354) |
| Election year | -0.848*** | -0.839*** | -0.551* | -0.856*** |
| | (0.308) | (0.318) | (0.290) | (0.307) |
| Post-election year | -0.173 | -0.122 | -0.021 | -0.186 |
| | (0.314) | (0.323) | (0.293) | (0.313) |
| *Additional controls* | | | | |
| Real public debt (log) | | | 0.815*** | |
| | | | (0.171) | |
| CSU council share | | | | 3.293* |
| | | | | (1.948) |
| Constant | 17.190*** | 17.114*** | 11.297*** | 16.247*** |
| | (1.160) | (1.310) | (1.622) | (1.265) |
| Independent city-counties included | Yes | No | Yes | Yes |
| Municipality controls | Yes | Yes | Yes | Yes |
| Political controls | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes |
| Municipality fixed effects | Yes | Yes | Yes | Yes |
| Municipalities (cluster) | 2012 | 1987 | 1948 | 2012 |
| Observations | 21935 | 21660 | 19656 | 21935 |

*Notes:* OLS FE model with standard errors clustered at the municipality level in parentheses. Significance levels: ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Table A5.5 : Robustness (II) – Heterogeneity by population size

| | Dependent variable: Cost inefficiency | | | |
|---|---|---|---|---|
| | Below median size (<2881) | | Above median size (≥2881) | |
| | (1) | (2) | (3) | (4) |
| *Executive elections* | | | | |
| Pre-election year | -2.398*** | -2.151** | -0.558*** | 0.093 |
| | (0.275) | (0.851) | (0.211) | (0.388) |
| Election year | -1.392*** | -0.718 | -0.307 | -0.680** |
| | (0.284) | (1.050) | (0.195) | (0.283) |
| Post-election year | -0.736** | -0.616 | -0.025 | 0.038 |
| | (0.294) | (0.969) | (0.195) | (0.310) |
| Constant | 11.201*** | 14.866*** | 12.840*** | 16.241*** |
| | (1.500) | (1.532) | (1.646) | (1.620) |
| Municipality controls | Yes | Yes | Yes | Yes |
| Political controls | Yes | Yes | Yes | Yes |
| Year fixed effects | No | Yes | No | Yes |
| Municipality fixed effects | Yes | Yes | Yes | Yes |
| Municipalities (cluster) | 1042 | 1042 | 1032 | 1032 |
| Observations | 10965 | 10965 | 10970 | 10970 |

*Notes:* OLS FE model with standard errors clustered at the municipality level in parentheses. Significance levels: ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Table A5.6 : Robustness (III) – Heterogeneity by mayor type

| | Dependent variable: Cost inefficiency | | | |
|---|---|---|---|---|
| | Full time mayor | | Part time mayor | |
| | (1) | (2) | (3) | (4) |
| *Executive elections* | | | | |
| Pre-election year | -0.791*** | -0.019 | -2.465*** | -3.120*** |
| | (0.201) | (0.363) | (0.292) | (1.201) |
| Election year | -0.456** | -0.459 | -1.387*** | -2.795** |
| | (0.191) | (0.299) | (0.311) | (1.395) |
| Post-election year | -0.019 | 0.117 | -0.856*** | -1.312 |
| | (0.199) | (0.294) | (0.313) | (1.501) |
| Constant | 13.091*** | 16.633*** | 12.658*** | 16.387*** |
| | (1.533) | (1.605) | (1.108) | (1.216) |
| Municipality controls | Yes | Yes | Yes | Yes |
| Political controls | Yes | Yes | Yes | Yes |
| Year fixed effects | No | Yes | No | Yes |
| Municipality fixed effects | Yes | Yes | Yes | Yes |
| Municipalities (cluster) | 1186 | 1186 | 965 | 965 |
| Observations | 12442 | 12442 | 9492 | 9492 |

*Notes:* OLS FE model with standard errors clustered at the municipality level in parentheses. Significance levels: ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Table A5.7 : Robustness (IV) – Regular elections

| | Dependent variable: Cost inefficiency | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *Executive elections* | | | | | |
| Pre-election year | -1.197*** | -1.230*** | -1.498*** | -1.447*** | -0.526 |
| | (0.170) | (0.170) | (0.177) | (0.179) | (0.395) |
| Election year | -0.909*** | -0.942*** | -0.958*** | -0.853*** | -0.730** |
| | (0.172) | (0.174) | (0.173) | (0.176) | (0.309) |
| Post-election year | -0.028 | -0.054 | -0.283 | -0.340* | -0.089 |
| | (0.175) | (0.176) | (0.181) | (0.184) | (0.347) |
| Constant | 13.846*** | 13.863*** | 13.219*** | 13.309*** | 17.128*** |
| | (0.104) | (0.072) | (1.083) | (1.119) | (1.172) |
| Municipality controls | No | No | Yes | Yes | Yes |
| Political controls | No | No | No | Yes | Yes |
| Year fixed effects | No | No | No | No | yes |
| Municipality fixed effects | No | Yes | Yes | Yes | Yes |
| Municipalities (cluster) | 2012 | 2012 | 2012 | 2012 | 2012 |
| Observations | 21286 | 21286 | 21286 | 21286 | 21286 |

*Notes:* OLS FE model with standard errors clustered at the municipality level in parentheses. Significance levels: ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Table A5.8 : Robustness (V) – Instrumented variable for rerun decision (2SLS)

| | Dependent variable: Cost inefficiency | | | | | | |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| **Second stage results** | | | | | | | |
| *Executive elections* | | | | | | | |
| Pre-election year | -0.478 | -0.475 | -0.481 | -0.478 | -0.474 | -0.480 | -0.457 |
| | (0.344) | (0.347) | (0.347) | (0.345) | (0.347) | (0.347) | (0.342) |
| Election year | -1.051*** | -1.049*** | -1.052*** | -1.051*** | -1.049*** | -1.052*** | -1.040*** |
| | (0.311) | (0.312) | (0.312) | (0.311) | (0.312) | (0.312) | (0.309) |
| Post-election year | -0.349 | -0.353 | -0.346 | -0.349 | -0.353 | -0.346 | -0.374 |
| | (0.315) | (0.323) | (0.316) | (0.315) | (0.323) | (0.316) | (0.311) |
| *Instrumented variable* | | | | | | | |
| Incumbent runs again | 0.216 | 0.192 | 0.236 | 0.217 | 0.193 | 0.236 | 0.068 |
| | (0.471) | (0.603) | (0.520) | (0.473) | (0.604) | (0.521) | (0.233) |
| Municipality controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Political controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Municipality fixed effects | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Municipalities (cluster) | 2012 | 2012 | 2012 | 2012 | 2012 | 2012 | 2012 |
| Observations | 19956 | 19956 | 19956 | 19956 | 19956 | 19956 | 19956 |
| **First stage results** | | | | | | | |
| *Excluded instruments* | | | | | | | |
| Incumbent is pensionable (lag) | -0.296*** | -0.423*** | | -0.296*** | -0.422*** | | |
| | (0.010) | (0.009) | | (0.010) | (0.009) | | |
| Incumbent, age≥60 (lag) | -0.318*** | | -0.422*** | -0.317*** | | -0.422*** | |
| | (0.009) | | (0.008) | (0.009) | | (0.008) | |
| Estimation approach | xtivreg2 | xtivreg2 | xtivreg2 | 2-step | 2-step | 2-step | OLS |
| Cragg-Donald Wald F statistic | 4260.76 | 4696.15 | 5491.52 | | | | |
| Kleibergen-Paap rk Wald F statistic | 1757.31 | 1990.99 | 2502.84 | | | | |
| Hansen J statistic (overidentification) | 0.005 | 0.000 | 0.000 | | | | |

*Notes:* 2SLS model estimations with standard errors clustered at the municipality level in parentheses. The control variable of incumbent's decision to seek re-election (*Incumbent runs again*) is instrumented. Columns (1)-(3) are based on Stata's *xtivreg2* command. Columns (4)-(6) show results using separate first stage and second stage regressions. Column (7) shows baseline OLS FE results for comparison. Significance levels: ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Table A5.9 : Robustness (VI) – Marginal effects conditional on incumbent's decision to seek re-election (2SLS)

| | Dependent variable: Cost inefficiency | | | | | |
|---|---|---|---|---|---|---|
| | (1a) | (1b) | (1c) | (2a) | (2b) | (2c) |
| *Executive elections x incumbent runs again (no=0 / yes=1)* | | | | | | |
| (A) Pre-election year (incumbent=0) | -0.585 | -0.721 | -0.654 | -0.326 | -0.489 | -0.303 |
| | (0.457) | (0.482) | (0.484) | (0.638) | (0.673) | (0.680) |
| (B) Pre-election year (incumbent=1) | -0.273 | -0.082 | -0.238 | -0.481 | -0.452 | -0.467 |
| | (0.512) | (0.529) | (0.552) | (0.364) | (0.362) | (0.370) |
| Difference (B-A) | 0.312 | 0.639 | 0.415 | -0.155 | 0.037 | -0.164 |
| | (0.682) | (0.735) | (0.769) | (0.658) | (0.694) | (0.728) |
| (C) Executive election year (incumbent=0) | -1.301*** | -1.691*** | -0.929 | -1.442** | -2.073*** | -0.771 |
| | (0.505) | (0.547) | (0.564) | (0.675) | (0.730) | (0.795) |
| (D) Executive election year (incumbent=1) | -0.875** | -0.610 | -1.118*** | -0.974*** | -0.847** | -1.099*** |
| | (0.379) | (0.388) | (0.413) | (0.342) | (0.345) | (0.360) |
| Difference (D-C) | 0.426 | 1.081 | -0.189 | 0.468 | 1.226 | -0.328 |
| | (0.614) | (0.682) | (0.737) | (0.743) | (0.809) | (0.911) |
| (E) Post-election year (incumbent=0) | -1.055** | -0.408 | -1.168** | -0.985 | -0.667 | -1.078 |
| | (0.532) | (0.351) | (0.584) | (0.668) | (0.698) | (0.756) |
| (F) Post-election year (incumbent=1) | -0.073 | -0.160 | 0.052 | -0.316 | -0.349 | -0.310 |
| | (0.373) | (0.385) | (0.403) | (0.337) | (0.341) | (0.354) |
| Difference (F-E) | 0.982 | 0.805 | **1.220** | 0.669 | 0.318 | 0.767 |
| | (0.623) | (0.670) | **(0.737)**$^\sharp$ | (0.711) | (0.753) | (0.846) |
| Municipality controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Political controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Municipality fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Municipalities (cluster) | 2012 | 2012 | 2012 | 2012 | 2012 | 2012 |
| Observations | 19940 | 19940 | 19940 | 19940 | 19940 | 19940 |
| *Excluded instruments* | | | | | | |
| Incumbent is pensionable (lag) | Yes | Yes | No | Yes | Yes | No |
| Incumbent, age$\geq$60 (lag) | Yes | No | Yes | Yes | No | Yes |
| *Assumption on unknown rerun decision* | no rerun | no rerun | no rerun | all rerun | all rerun | all rerun |

*Notes:* 2SLS model estimations with standard errors clustered at the municipality level in parentheses. The incumbent's decision to seek re-election (*Incumbent runs again*) is instrumented in the interaction effects model. All estimations are based on Stata's *xtivreg2* command. Assumptions when rerun decision in the next election is unknown: Columns (1a)-(1c) assume "no rerun" in all unknown years, while Columns (4)-(6) label all unknown years as "rerun" — assuming that the incumbent mayor runs again in the next election. Significance levels: ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

# 6 The effect of public sector accounting standards on budgets, efficiency, and accountability: Evidence from German counties[1]

## Abstract

**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 Stefanie Gäbler and Felix Rösel. It is based on our paper "Ineffective Fiscal Rules? The Effect of Public Sector Accounting Standards on Budgets, Efficiency, and Accountability" published in *Public Choice*, 2021, 186(3–4), 387–412.
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## 6.1 Introduction

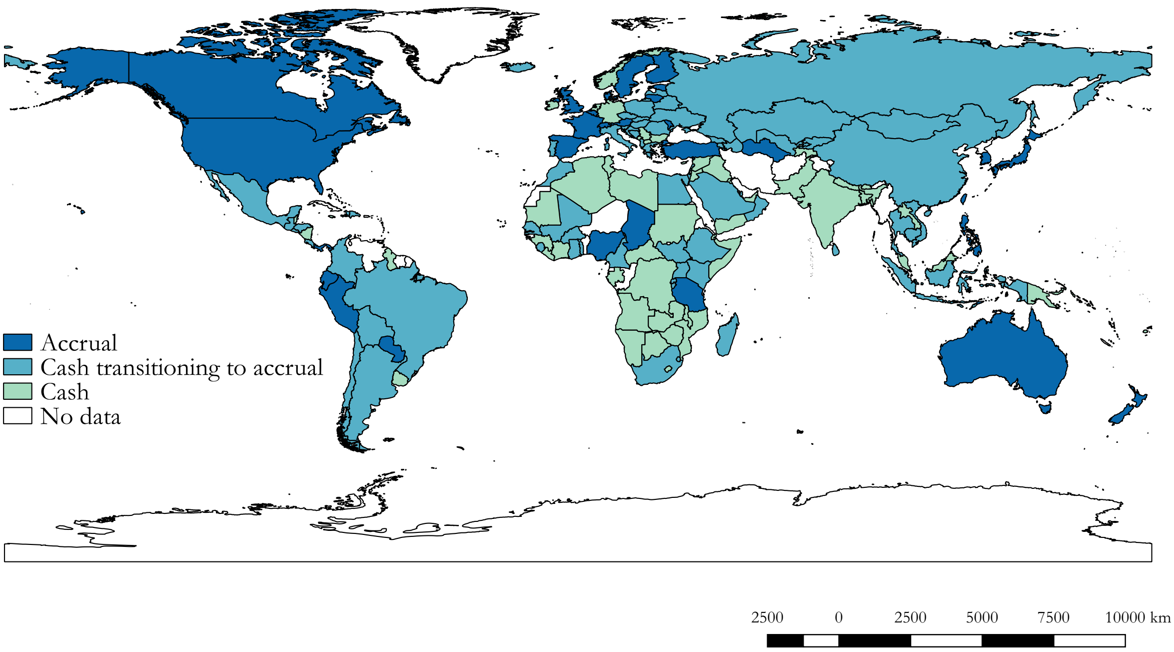
Two different accounting standards are used for reporting in the public sector: traditional cash-based accounting and business-like accrual-based accounting. Pure cash accounting statements do not report assets, liabilities, or depreciation. Business-like accrual accounting statements, by contrast, provide 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 6.1).

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 not find that switching accounting standards had affected financial balances. Her findings suggest that accrual accounting somewhat altered the composition of revenues. Raffer (2019)

[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 6.1 : 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.

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

[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.

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 (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 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

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

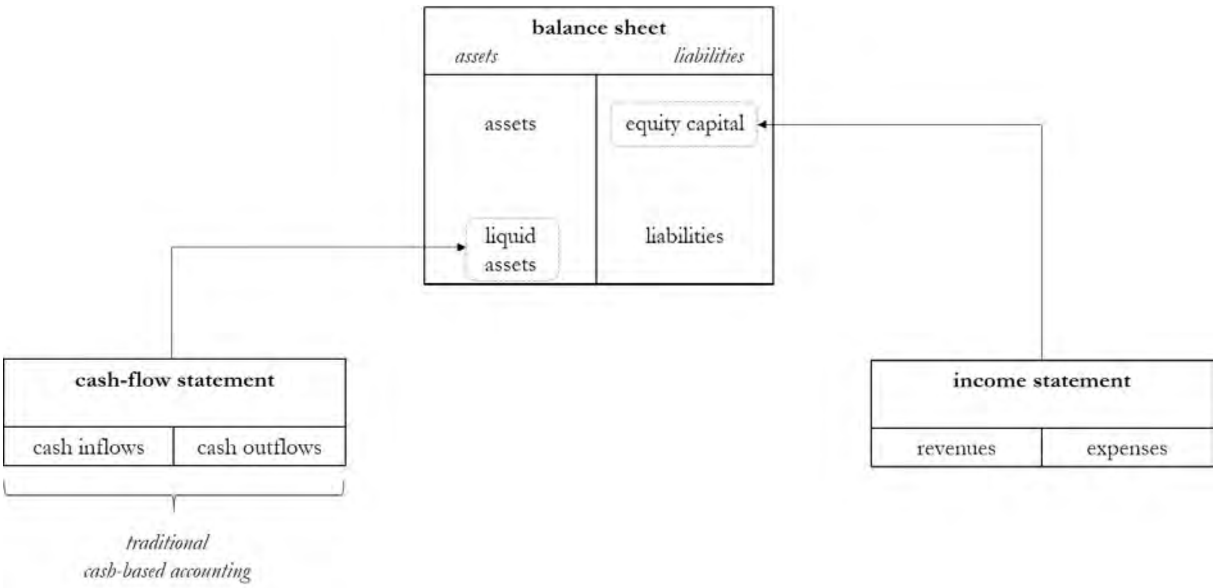
(EMU) (Dafflon and Rossi, 1999; Milesi-Ferretti and Moriyama, 2006), and after the introduction of the European Stability and Growth Pact (SGP) to sugarcoat the budget balance requirements (Von Hagen and Wolff, 2006; Buti *et al.*, 2007; Beetsma *et al.*, 2009; Alt *et al.*, 2014). Our study is one of 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.

## 6.2 Public sector accounting standards

### 6.2.1 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 6.2). 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 6.2, 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.

Figure 6.2 : Components of a simplified accrual accounting system




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

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. 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]

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 6.1.

[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 A6.1 in the Appendix gives a numerical example.

Table 6.1 : Key features and pros and cons of cash-based and accrual 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 6.2.1) and the pros and cons of public sector accrual accounting (Section 6.2.2).

### 6.2.2 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 increases 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 A6.1).

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

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.

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 appropiate 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.

## 6.3 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

[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`.

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 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

[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.

statements on an annual basis (see Figure 6.2). 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]

## 6.4 Methods

### 6.4.1 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.

[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`.

[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.

### 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 6.2 shows summary 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 (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

[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.

[16] County governments do not raise own taxes. County rates, however, do not occur in consolidated city counties.

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.

Table 6.2 : Summary 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.

[17] Data on debt of all local government enterprises is not available as panel dataset in the period of observation.

### 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 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 A6.2 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 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 6.2). 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).

[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.

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 6.2).

**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 6.2).

### 6.4.2 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 6.2).

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 6.3). 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:

$$y_{it} = \alpha_i + \delta_t + \beta(Accrual_{it}) + X'_{it}\gamma + \epsilon_{it} \tag{6.1}$$

where $y_{it}$ describes outcome $y$ in county $i$ in year $t$. $\alpha_i$ and $\delta_t$ are county and year fixed effects, $X'_{it}$ is a vector of control variables, and $\epsilon_{it}$ denotes the error term. The coefficient of interest is $\beta$ 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 6.3 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 6.3 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 Tables A6.3 and A6.4 in the Appendix).

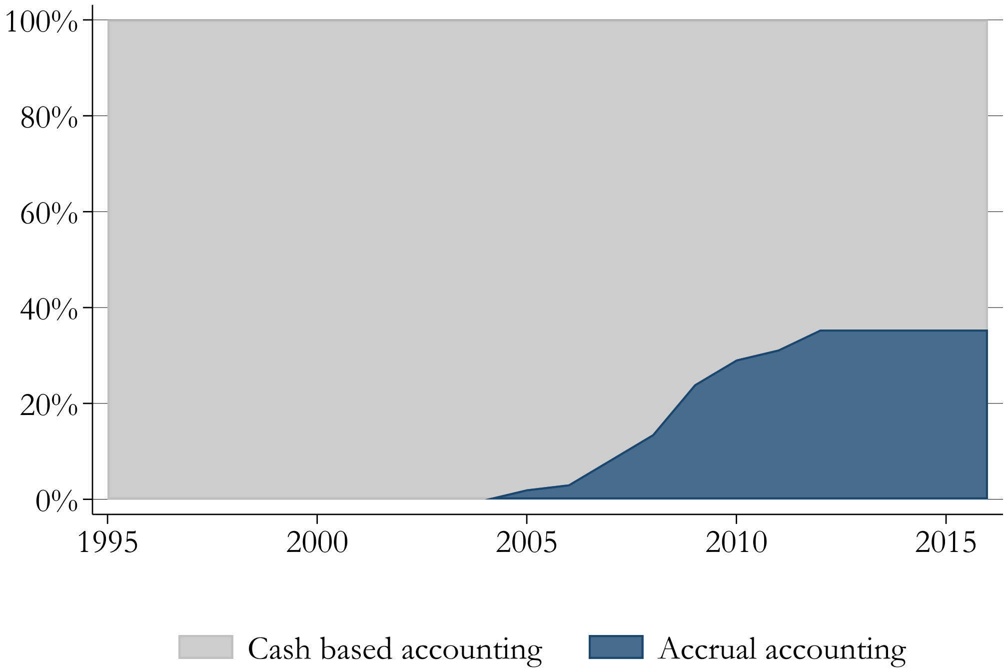
Pre-reform characteristics do not predict the selection into accounting standards. Table 6.3 shows that socioeconomic, political and fiscal outcomes in the pre-reform period are not correlated with switching to accrual accounting.[21] First, we estimate survival models with switching accounting standards as the failure event using Cox regressions (columns 1-3). Socioeconomic, political and fiscal outcomes do not significantly alter the hazard rate. Second,

[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.

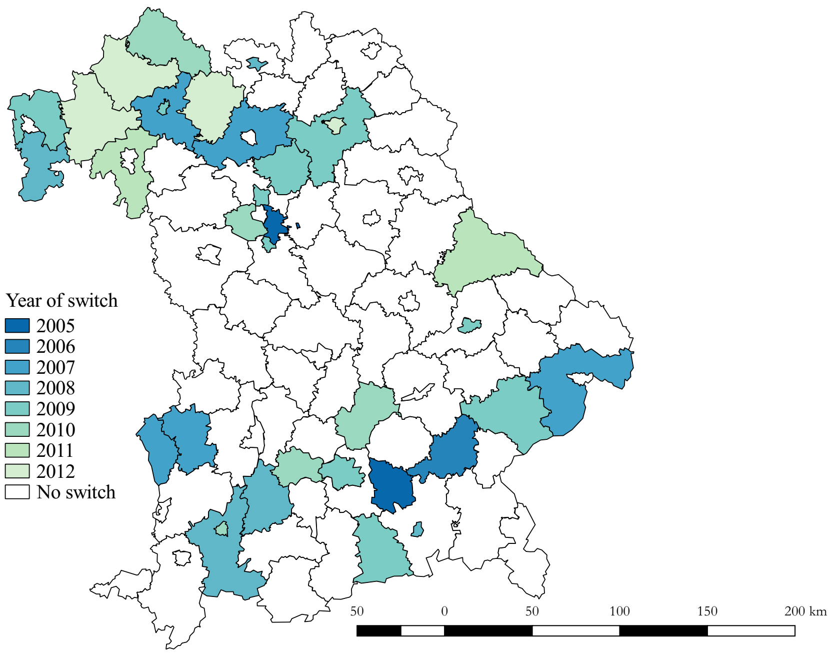
[21] Inferences hardly change when we include *Regierungsbezirk*-year fixed effects instead of year fixed effects (see, Table A6.3 for Cox and probit regressions with district-year fixed effects; Table A6.4 for the difference-in-differences results and Figures A6.1 and A6.2 for the event-study results 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.

Figure 6.3 : 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.

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 A6.5 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.

Table 6.3 : Previous development does not predict switching to accrual 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. Standard errors in parentheses. Significance levels: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$ (no significant values to report).

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:

$$y_{it} = \alpha_i + \gamma_t + \sum_{j=c}^{C} \beta_j (Accrual_{it}^{j}) + X_{it}' \gamma + \epsilon_{it} \tag{6.2}$$

where $y_{it}$ describes outcome $y$ in county $i$ in year $t$. $\alpha_i$ and $\delta_t$ are county and year fixed effects, $X_{it}'$ is a vector of control variables following equation (6.1), and $\epsilon_{it}$ 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).

## 6.5 Results

### 6.5.1 Baseline results

Table 6.4 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 A6.7 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.

[22] Table A6.6 in the Appendix shows the results for our control variables.

Table 6.4 : 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. Standard errors clustered at the county level in parentheses. Significance levels: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

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 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.

We also do not observe statistically significant effects of accrual accounting on non-budget outcomes. Table 6.5 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 6.5 : 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. Standard errors clustered at the county level in parentheses. Significance levels: ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.10. Technical efficiency multiplied by 100.

[23] Results do not change for time lags of voter turnout. See Table A6.8 in the Appendix.

### 6.5.2 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 6.4 and 6.5 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).

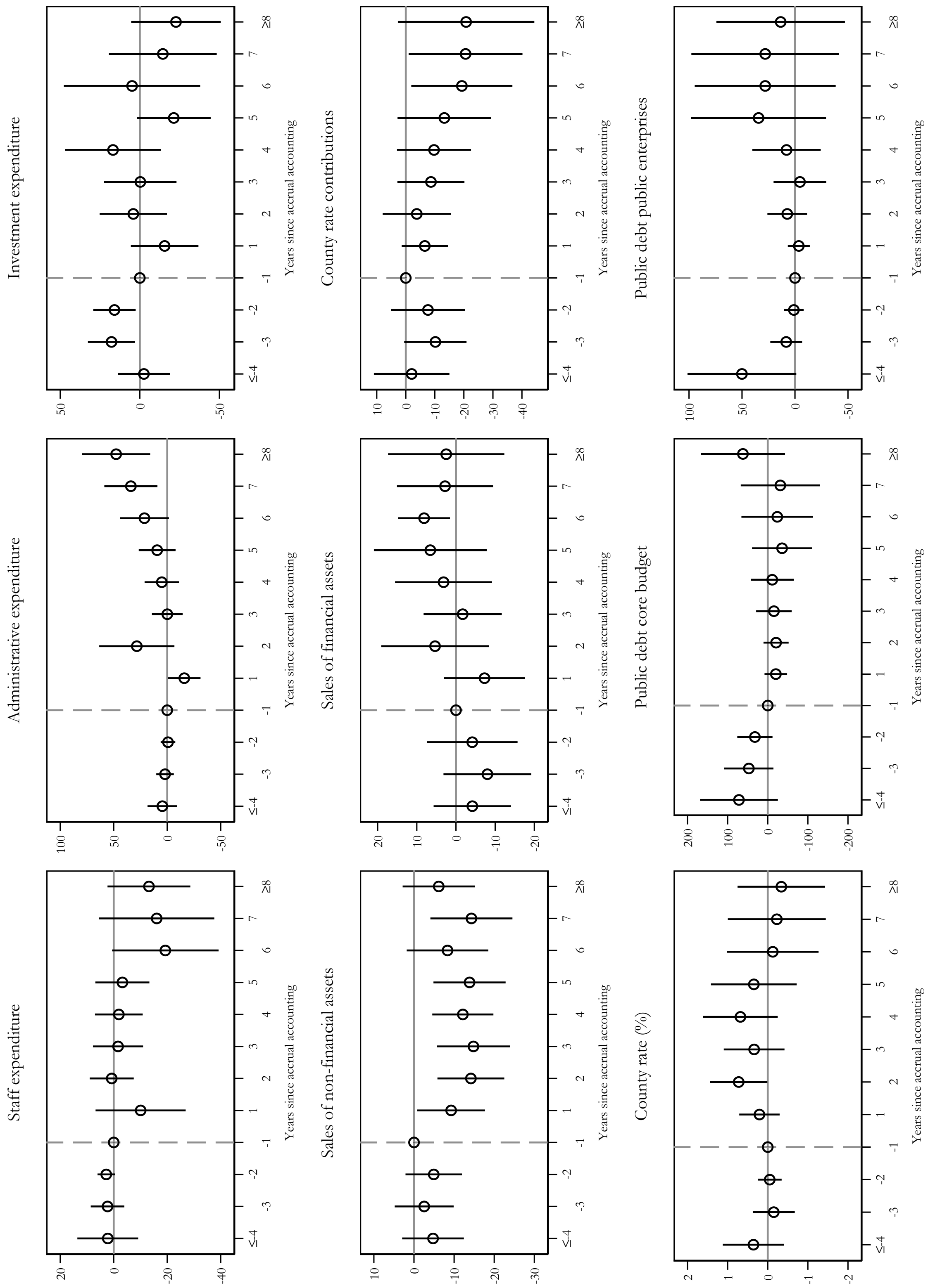
Again, we first turn to fiscal policies representing our main outcome variables of interest (Figure 6.4). Pre-reform trends look promising: counties switching to accrual accounting do not deviate from counties using cash-based accounting before changing accounting standards. Both changing and not-changing 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 6.4 and 6.5 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 6.5). Technical efficiency steadily increases after introducing accrual accounting, but effects are never statistically significant at the 10 % level.

[24] The Appendix provides full event study regression outputs in Tables A6.9 and A6.10.

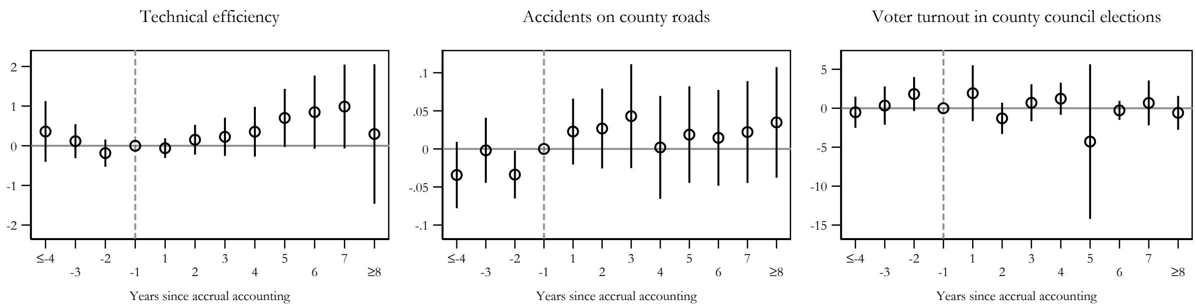
[25] Another minor exception is that road accidents are somewhat lower some two years before switching (10% significance level). See Table A6.10 in the Appendix.

Figure 6.4 : Event study results (I) – Fiscal outcomes




*Notes:* Circles represent point estimates from event study estimations, bars are 90% confidence intervals (equivalent to *p < 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 6.5 : Event study results (II) – Non-fiscal outcomes




*Notes:* Circles represent point estimates from event study estimations, bars are 90% confidence intervals (equivalent to *p $< 0.1$). -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.

However, Figure 6.4 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 6.4 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 not help to balance the income statement (see Figure 6.2). 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.

### 6.5.3 Robustness checks

Our main findings hold in several robustness and heterogeneity tests. Excluding control variables (Appendix, Table A6.11), or including further control variables such as unemployment rates and dummies for flood events in 2002 and 2013 (Table A6.12) barely change the results.[26] When we exclude consolidated city-counties from the sample (Table A6.13), however, our findings suggest less outsourcing to public enterprises under accrual accounting: in rural

[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.

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 A6.14). 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 (Appendix, Table A6.14). 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 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.

## 6.6 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 6.4.2). Even more, event study results corroborate that the common trends assumption in our outcome variables hold (see Section 6.5.2). 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

## 6.7 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.

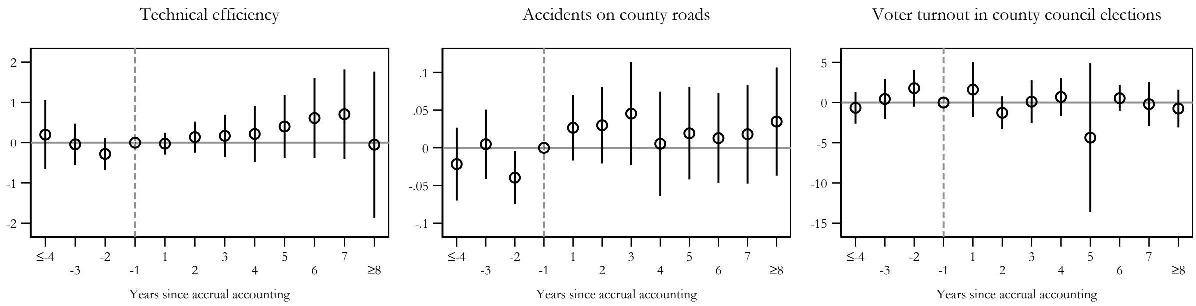
An important next research step includes examining whether inferences change in the very long run when governments are used to accrual-based accounting for several years. Results may depend on specific public management skills of decision makers and on the institutional context. Reforms at other levels 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.

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.

# Appendix

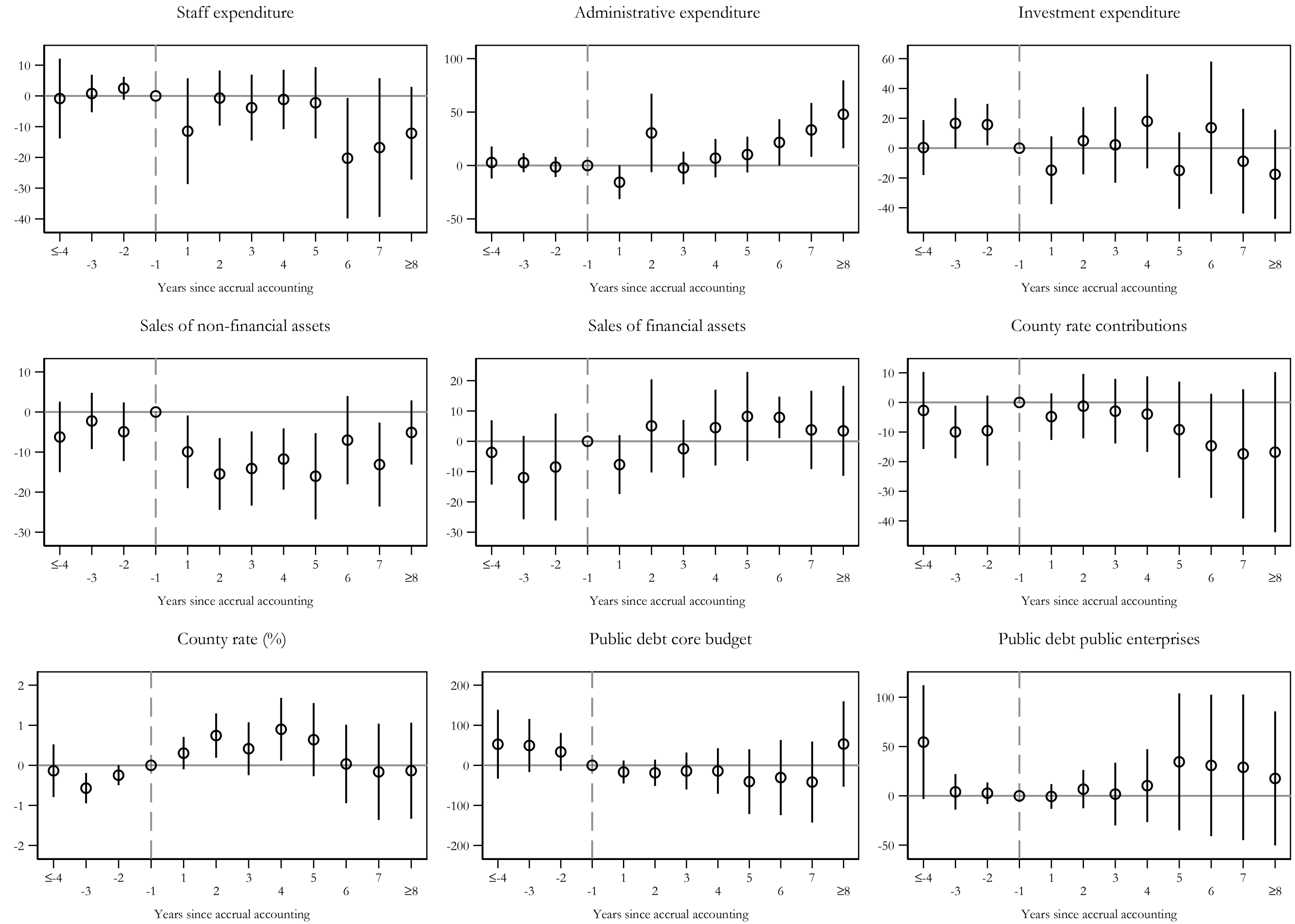
## Figures

Figure A6.2 : Event study results (IV) – Non-fiscal outcomes incl. 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 *p $< 0.1$). -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.

Figure A6.1 : Event study results (III) – Fiscal outcomes incl. 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 $*p < 0.1$). -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.

## Tables

Table A6.2 : Summary 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 summary 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 A6.1 : 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*<br>(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 A6.3 : 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 6.3 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. Standard errors in parentheses. Significance levels: ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.10.

Table A6.4 : Baseline 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 6.4 and 6.5 including district-year fixed effects. Standard errors clustered at the county level in parentheses. Significance levels: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$. Technical efficiency multiplied by 100.

Table A6.5 : 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: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$ (no significant values to report).

Table A6.6 : Baseline results – displaying all baseline controls

| | 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 dependency ratio | 2.98** | -0.38 | 3.07* | 0.64 | 0.40 | 3.10* | -0.06 | 6.45 | -1.37 | -0.22 | 0.01* | -0.04 |
| | (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 foreigners | 1.95 | -3.46 | -7.79 | 1.57 | -1.70 | 6.39 | 0.35 | 60.08** | -4.76 | -0.04 | 0.01 | -0.22 |
| | (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 per capita) | 0.75 | 0.84 | -1.11 | 0.28 | -0.55** | 5.75* | -0.09 | -16.20*** | -4.11 | -0.05 | 0.00** | -0.06 |
| | (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 government | 1.03 | -3.25 | -2.98 | 0.32 | -1.40 | -4.22 | -0.28 | -10.74 | -26.24 | -0.65 | 0.03 | -0.39 |
| | (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 6.4 and 6.5 but shows all control variables. Standard errors clustered at the county level in parentheses. Significance levels: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$. Technical efficiency multiplied by 100.

Table A6.7 : 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. Standard errors clustered at the county level in parentheses. Significance levels: ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.10 (no significant values to report).

Table A6.8 : 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 | 0.54 | 0.86 |
| | (0.80) | (0.62) | (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. Standard errors clustered at the county level in parentheses. Significance levels: ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.10 (no significant values to report).

Table A6.9 : 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: $\leq$ -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: $\geq$ 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 6.4. Standard errors clustered at the county level in parentheses. Significance levels: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$.

Table A6.10 : 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 6.5. Standard errors clustered at the county level in parentheses. Significance levels: ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.10. Technical efficiency multiplied by 100.

Table A6.11 : Robustness (I) – Baseline 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. Standard errors clustered at the county level in parentheses. Significance levels: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$. Technical efficiency multiplied by 100.

Table A6.12 : Robustness (II) – Baseline controlling for unemployment and floods of 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 6.4 and 6.5 controlling for the unemployment rate and a dummy, which is 1 in the year a country was affected by a flood and zero otherwise. Standard errors clustered at the county level in parentheses. Significance levels: ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.10. Technical efficiency multiplied by 100.

Table A6.13 : Robustness (III) – Baseline excluding independent 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)*. Standard errors clustered at the county level in parentheses. Significance levels: ***p $<$ 0.01, **p $<$ 0.05, *p $<$ 0.10. Technical efficiency multiplied by 100.

Table A6.14 : Robustness (IV) – 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). Standard errors clustered at the county level in parentheses. Significance levels: ***$p < 0.01$, **$p < 0.05$, *$p < 0.10$. Technical efficiency multiplied by 100.

# 7 Conclusion

This dissertation contributes to the research in public economics and political economy. My findings provide new empirical evidence on the role of economic inequality, trade and infrastructure policies, and political and fiscal institutions. The self-contained chapters include detailed information on their contribution and policy implications. In this concluding chapter, I review and highlight main results and implications of the individual chapters.

**Chapter 2** contributes to the discussion on consequences of economic inequality. Many pundits believe that the rise in economic inequality is linked to the increase in political polarization that many advanced economies have experienced in the last decade. My co-authors and I provide evidence from German counties that regional economic inequality and economic deprivation are indeed important drivers of political polarization. We find a sizeable effect of the prevalence of relative economic deprivation on vote shares of radical parties at the county level. Our findings indicate that prevalence of economic deprivation at the county level may trigger perceptions on economic threat and the fear of social decline. Populists often use a strategy to appeal voters by emphasizing the perceived state of crisis and economic decline (Moffitt, 2016).

The threat of economic inequality for the stability of social cohesion and the democratic political order raise the question on its underlying determinants. **Chapter 3** contributes to the debate on the causes of economic inequality. My co-authors and I investigate the external validity of the widespread view that trade openness influences income inequality. We use a large sample of countries and do not come to a general conclusion. The effect of trade on inequality seems to be context-specific and may rather depend on the income level and institutional framework of countries. In line with the classical Stolper-Samuelson theorem (Stolper and Samuelson, 1941), trade tends to disproportionately benefit the very poor in developing economies. In advanced economies, however, we do not find clear effects apart from some outliers. By contrast, our results show a strong effect of trade on income inequality in transition countries from Eastern Europe and China. Welfare states and institutions were less developed in these countries than in many advanced economies in the rest of the world — especially during the rapid opening phase of the transition countries. In most advanced economies (particularly in Western Europe), stable and established democratic institutions, widely accessible opportunities for public education, and large redistribution programs may have moderated effects of trade on income inequality. We conclude that institutions matter for distributional outcomes of trade policies. Future research should examine how institutions influence the effects of trade on economic inequality.

**Chapters 5 and 6** investigate the role of political and fiscal institutions for public-sector performance at the local government level in the German state of Bavaria. In **Chapter 5**, I examine whether elections influence governments' overall efficiency in the provision of public goods and services. The chapter contributes to the question whether reelection incentives give rise to (in)efficient policies, and contributes to the understanding of the role of institutions for a cost-efficient use of public resources. While the political budget cycle literature suggests that incumbents influence budgetary and political decisions in a way to get reelected, my results indicate that this does not come at the cost of wasteful spending before elections. By contrast, local governments' cost efficiency is rather increasing before and in election years. My findings support the idea that local democratic institutions enhance politicians' efforts to increase government performance.

The institutional framework at the local government level in the German state of Bavaria includes, among others, direct elections, decentralization of responsibilities and fiscal autonomy, balanced budget rules, and high fiscal transparency. These institutions might well describe some preconditions for the efficiency-enhancing effect of elections. Findings of **Chapter 6**, however, show that not all improvements in transparency and fiscal rules directly map into greater public-sector performance and improvements in sustainable budgeting. My co-authors and I evaluate whether switching public sector accounting standards from cash-based to accrual-based reporting is justified for all governments. We conclude that cost-benefit-ratios may not always pay off. Clearly, scope for benefits from improvements in fiscal transparency might be greater in countries where the institutional framework is less developed and corruption may be more prevalent than in high-income countries with well-established institutions. Future research should evaluate how electoral cycles, accounting standards, and further institutions influence government performance at different institutional frameworks, governmental layers, and levels of development.

Sustainable public finances and fiscal capacities to provide public goods and to fulfill public services are also related to economic development. Public stakeholders often argue that investments and subsidies in large infrastructure projects pay off in terms of long-term economic growth. Infrastructure projects are therefore popular regional policies aiming at reducing economic inequality between regions and thus to promote social and economic cohesion. In **Chapter 4**, my co-authors and I evaluate how new transportation infrastructure promotes regional economic development in a rural region in the German state of Bavaria. More precisely, we examine the effect of new airport infrastructure in the touristic region Allgäu by exploiting the conversion of a former military airbase into a commercial regional airport. Our findings show that the airport increases tourist inflows and endorses economic development in the Allgäu region.

Whether new infrastructure projects – and conversions of military bases – are welfare improving needs to be evaluated individually. Size and direction of regional economic and fiscal effects, for example, may well differ among airports depending on airport competition, geography, the catchment area of population, and touristic attractions within the region. Large infrastructure projects also come along with external costs such as reduced property values, as well as adverse health and environmental impacts. Public infrastructure decisions should be based on a sound evaluation of cost-benefit trade-offs and considerations on a sustainable economic, social and environmental development. Future research should identify favorable contextual factors for welfare-improving infrastructure projects.

This dissertation provides new empirical evidence that economic inequality might be a threat for social cohesion and the democratic order. My findings also show how institutions and infrastructure policies can improve welfare and political stability. The results of the individual chapters, however, indicate that marginal effects may well depend on contextual factors. The design of economic policies and institutions should be based on a sound understanding of causal relationships. An avenue for future research would therefore be to further examine the external validity on how inequality, institutions, and economic policies are related, and to identify which contextual factors — e.g., the development level, industry composition, or institutional framework — are decisive for welfare improving political decisions and institutions.

# Addendum

I am co-author of three other research articles that I have written during my doctoral studies. These articles are not part of my dissertation, but they are also related to the empirics on public economics and political economy, and well contribute to my research agenda on how institutions, political forces, and economic policies affect public budgeting, distributional outcomes, and overall welfare. Below I provide an extended abstract of each article.

## Globalization, government ideology, and top income shares: Evidence from OECD countries

This article is joint work with Christoph Schinke (Dorn and Schinke, 2018).[1] We investigate how government ideology and globalization are associated with top income shares in 17 OECD countries over the period 1970 to 2014. We use top income shares of the World Inequality Database (WID). To disentangle effects within the top decile, we distinguish between two groups: the top 1 % (P100-P99) as the rich, and the next 9 % (P99-P90) as the upper-middle class. Globalization is measured by the KOF index of globalization. We employ static first-difference OLS estimations in a panel model with fixed effects, and dynamic panel data models.

Static and dynamic panel model results show that top income shares increased more under rightwing governments than under leftwing governments. Our interaction coefficients indicate that the correlation of government ideology and top income shares was stronger when globalization proceeded more rapidly. Globalization was positively correlated with income shares of the upper-middle class (P99-P90), but negatively with income shares of the rich (top 1 %) in the overall sample. Our results also indicate that the relationship differs between Anglo-Saxon countries and other OECD countries. Globalization was more pro-rich in Anglo-Saxon countries than in other OECD countries. Government ideology does not turn out to have a statistically significant relationship with top income shares in Anglo-Saxon countries after the 1980s, whereas ideology-induced differences in the distributional outcomes continued in other OECD countries.

Our findings provide empirical evidence how globalization, economic policies of different democratic forces, and distributional outcomes are related. The results also indicate some heterogeneity within the group of advanced economies.

[1] The corresponding paper "Globalisation, government ideology, and top income shares: Evidence from OECD countries" is published in *The World Economy*, 2018 (Dorn and Schinke, 2018). We thank Niklas Potrafke, seminar participants at the ifo Institute, the participants of the 2014 International Institute of Public Finance (IIPF) conference in Lugano, and an anonymous referee for helpful comments. Alexander Schwemmer and Claudius Willem provided helpful research assistance.

## Political institutions and health expenditure: New empirical evidence

This research project is joint work with Johannes Blum and Axel Heuer (Blum *et al.*, 2021).[2] We examine how political institutions influence health expenditure by using a panel of 151 developing and developed countries for the years 2000 to 2015.

We employ health expenditure data from the World Health Organization and use four democracy measures. We estimate the relationship by using OLS cross-section regression models, panel fixed effects models and event study models. Similar to the approach of Acemoglu *et al.* (2019), we address endogeneity concerns by using an instrumental variable (IV) approach which exploits geographical patterns and the diffusion of democratic regimes across countries.

We find that health expenditure of governments are indeed higher in democracies than in autocracies. Our cross-country analysis shows that democracies have 20-30 % higher government health expenditure relative to GDP than their autocratic counterparts. Panel fixed effects and event study models also suggest a positive within-country effect of democratization on government health expenditure within a short period after regime transition. Our IV results confirm the positive effect of democracy on government health expenditure. By contrast, private health expenditure do not turn out to be significantly affected by political institutions.

Our findings suggest that democratic institutions give rise to government health expenditure. We conclude that democracies care more for their citizens and strive to decrease inequalities in the access to health care.

[2] The corresponding paper "Political institutions and health expenditure" is published in *International Tax and Public Finance*, 2021, 28, 323-363. We thank Klaus Gründler, Niklas Potrafke, Gèrard Roland, two anonymous referees, the participants of the 2019 meeting of the European Public Choice Society (EPCS) in Jerusalem, the 2019 conference of the International Institute of Public Finance (IIPF) in Glasgow, and the 2020 workshop on *The Political Economy of Democracy and Dictatorship* (PEDD) in Münster for valuable comments.

## Health protection and the economy: Evidence from Covid-19 containment policies

This research project is joint work of an interdisciplinary group of researchers during the corona crises (Dorn *et al.*, 2020b).[3]

Several countries use shutdown strategies to contain the spread of the Covid-19 epidemic at the expense of massive economic costs. While this suggests a conflict between health protection and economic objectives, we examine whether the economically optimal exit strategy can be reconciled with the containment of the epidemic.

We use a novel and unique combination of epidemiological and economic simulation models and employ scenario calculations based on empirical evidence from Germany. We model the death toll and economic activity as a function of the infection reproduction number $R_{\mathrm{t}}$, using an empirical relationship between $R_{\mathrm{t}}$ and economic activity at the industry level as well as the time until the economy fully recovers. In the model, different shutdown strategies are associated with different $R_{\mathrm{t}}$ values; more relaxed (restrictive) restrictions yield larger (smaller) $R_{\mathrm{t}}$ values, implying a longer (shorter) period until the containment of the epidemic is completed. A longer period is associated with larger death tolls, but due to more relaxed restrictions also with higher economic activity in the short run. However, larger $R_{\mathrm{t}}$ values imply that the time is extended until the level of new infections allows a full opening of the economy. That way, it is *a priori* not clear which strategy is economically optimal in the long run.

Our model-based scenario calculations suggest that a prudent opening is economically optimal. The optimal reproduction number $R_{\mathrm{t}}$ is around 0.75. We cannot identify a conflict between the economy and health protection in relation to a stronger relaxation — the costs would be higher in both dimensions. Accelerated opening leads to substantially more Covid-19 deaths and increased economic costs.

Our findings reject the view that there is a conflict between economic objectives and health protection. Instead, it is in the common interest of public health and the economy to relax non-pharmaceutical intervention policies in a manner that keeps the epidemic under control.

[3] The corresponding research paper "The Common Interests of Health Protection and the Economy: Evidence from Scenario Calculations of COVID-19 Containment Policies" is available as preprint at *medRxiv* (2020.08.14.20175224; doi: https://doi.org/10.1101/2020.08.14.20175224). The article has been submitted and is *Under Review* for publication. I am *first author* in this research project. The team includes the other *first co-authors* Sahamoddin Khailaie and Marc Stöckli, the *corresponding co-authors* Clemens Fuest and Michael Meyer-Hermann, and the further contributing *co-authors* Sebastian Binder, Berit Lange, Stefan Lautenbacher, Andreas Peichl, Patrizio Vanella, and Timo Wollmershäuser. This research paper is based on a German policy paper we prepared during the first shutdown in spring 2020 (Dorn *et al.*, 2020c).

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# Curriculum Vitae – Florian Dorn

## Education

Florian Dorn submitted his dissertation on "*Inequality, Infrastructure, and Institutions – Empirical Studies in Public Economics and Political Economy*" in September 2020. After his successful disputation of the dissertation, he graduated as Doctor oeconomie publicae (Dr. oec. publ.) from the Ludwig-Maximilians-University of Munich (LMU) in February 2021.

Before, he completed the Ph.D. program in Economics at the Munich Graduate School in Economics (MGSE) at LMU Munich between 2016 and 2020. During his doctoral studies, he visited the Summer School programs in *Advanced Econometrics* and *Quantitative Methods for Public Policy Evaluation* at the Barcelona Graduate School in Economics (BGSE) in 2017. He also obtained a specialization in *Regional and Urban Economics* at the Bavarian Graduate Program in Economics in 2018. Between 2016 and 2019, Florian Dorn presented his research at various scientific peer-reviewed conferences and workshops around the world, including Amsterdam, Brussels, Budapest, Freiburg, Jerusalem, Marburg, Munich, New York, Paris and Tokyo.

In 2016, Florian Dorn received a Master's degree in Economics (M.Sc.) from the LMU Munich. He specialized in Applied Econometrics and Public Sector Economics. During his graduate studies, he visited the University of California in Berkeley (USA) and studied in the graduate programs of the Department of Economics and Goldman School of Public Policy. During his undergraduate studies at the Universities of Mannheim and Munich (LMU), he completed courses and seminars in business administration, economics, sociology and political sciences. He earned a Bachelor of Arts (B.A.) in Sociology (2009) and a Bachelor of Science (B.Sc.) in Economics (2012).

## Employment and affiliations

Florian Dorn joined the ifo Institute – Leibniz-Institute for Economic Research in 2016. Until 2020, he worked as Junior Economist and Doctoral Student at the ifo Center for Public Finance and Political Economy. Since 2020, Florian Dorn has been working as an Economist, Post-doctoral Researcher and Personal Assistant to the President of the ifo Institute for ifo's Executive Board.

Florian Dorn is also a Lecturer in Economics and Public Finance at the Ludwig-Maximilians-University of Munich (LMU) and Guest Lecturer at the International School of Management (ISM) in Munich. In 2019, he was a Visiting Research Fellow at the London School of Economics and Political Science (LSE). As CESifo Affiliate, he is also a member of the global CESifo Research Network since 2021.

Prior to joining the ifo Institute, he worked in economic research and strategy and policy consulting in the areas of *Economy and Labor* and *Innovation and Technology* at Prognos AG. He was also an intern at the Economic Policy Department of the Bavarian Ministry of Economic Affairs, Infrastructure, Energy and Technology, as well as intern in the German parliament (Deutscher Bundestag).