Advances in Spatial Econometrics and the Political Economy of local housing supply

Essays in Regional and Political Economics

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Zusammenarbeit mit Koautoren und Vorveröffentlichungen

Die fünf Kapitel dieser Dissertation basieren auf Arbeiten, die bereits an anderer Stelle – entweder als Working Paper oder in wissenschaftlichen Fachzeitschriften – publiziert wurden. Die folgende Aufzählung gibt Aufschluss über diese Vorveröffentlichungen. Ebenso wird aus ihr deutlich, welche (und wie viele) Ko-Autoren an welchem Projekt beteiligt waren.

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Kapitel 5:
Arnold, F., Freier, F., Martin, T. (2017). Not in my backyard? – Evaluating the impact of citizen initiatives on housing supply

Kapitel 6:
Martin, T. (2017). You shall not build! (until tomorrow) – Electoral cycles and housing policies in Germany *MPRA Paper*, No. 15-081.

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Thesis by

Thorsten Martin

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When I assumed my Ph.D. position in October 2012, I knew that I would have a long way ahead of me. Now I am reaching the final steps of my thesis and I have to admit that I am partly happy but also partly sad since I grew with my research and always enjoyed working in the academic world. I look back to a time where I met interesting people, was constantly challenged and thus learned a lot. During my PhD I also enjoyed the academic freedom to pursue the agenda I consider worthwhile and interesting, an experience I wouldn't like to have missed. The final year of my research was nevertheless the hardest, where there were many times I was convinced I would never finish this part of my life. But, in hindsight, I cherish every single moment of my research activities. Of course, this thesis would have never been possible without a lot of people who supported me and contributed to my work.

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Contents

Chapter 1

General Introduction

1.1 Opening Remarks

One of the most famous principles in Geography states:

> "Everything is related to everything else, but near things are more related than distant things." (Tobler, 1970)

This principle, also known as Tobler's First Law of Geography, was always an important guideline to my research on German municipalities and their regional interrelations. German municipalities are an interesting subject of study since they are the most local administrative unit in Germany. Where recent Big Data trends suggest we "think big" and accumulate as much data as possible, my thesis focuses on "think local", i.e. explaining regional dependencies between a fixed set of units as well as explaining occurrences within units.

So how can we explain if and how municipalities react to each other with their policy parameters? How does this behavior relate to higher-tier administrative units? The regional occurrences on which I focus in this thesis are interactions between municipalities with respect to accumulation of municipal debt in chapter 2, local business tax agendas in chapter 3 and the regional spillovers of entrepreneurial activity in chapter 4. Furthermore, I investigate how tools of local political participation (petitions / elections) influence municipal housing policies in chapter 5 and 6.

It is important for designing a consistent economic policy to understand if and why municipalities react towards each other with respect to municipal debt and the local business tax rate. A common explanatory pattern for tax mimicking is usually competition for mobile capital which results in the policy implication of stricter oversight over municipal tax setting behavior (Wildasin, 1988). An alternative explanation considered in this thesis is social learning in the presence of uncertain policy outcomes (Glick, 2014), which implies a laissez faire approach as policy implication. Therefore it is important to understand whether tax mimicking is necessarily associated with competition for mobile capital and whether it could facilitate through other variables, since the policy implications are diametrical. Another question which is important for a coherent economic policy is whether entrepreneurial activity spills into neighboring regions and how long these spillovers last. This is important, since entrepreneurial activity is strongly associated with a higher GDP output (Audretsch and Keilbach, 2004). Finally, understanding which political tools influence local housing policies is important since the regulation of land use is one of the main tasks of municipalities and might also affect local housing prices.

In terms of research content, my thesis can be divided into two parts. Part one involves chapters 2 to 4 and examines local interactions and spillover effects between small regional governments using spatial econometric methods. The second part focuses in chapters 5 and 6 on patterns within municipalities and inspects which institutions of citizen participation, elections and local petitions, influence local housing policies.

In part one, I make use of spatial econometrics as a tool to model spatial dependencies between administrative units in Germany. This method allows me to specifically incorporate the dependent variable of the neighbors, the spatial lag, as an explanatory variable. Of course, when including the neighbors regressands, the causal question 'Do I influence my neighbors or do they influence me?', as well as the potential bias of common spatial shocks on the dependent and independent variable, naturally arises. Spatial econometric methods, estimated via Quasi Maximum Likelihood (QML), tackle this problem by including the Jacobian matrix of the regression equation into the Log-Likelihood function to take spatial simultaneity into account. Nevertheless, spatial econometric methods attract a lot of critique, which was pointedly described by Gibbons and Overman (2012). Basically they argue for a lack of natural experiments in spatial econometric applications as well as an arbitrary choice of neighboring specifications and questionable instruments in case of an instrumental variable regression.

In my research, I address these concerns in multiple ways. Chapter 2 tackles the problem of arbitrary neighbor definitions by putting a lot of emphasis on the question 'Which neighbors actually do matter' and especially how to identify them.

Many different neighboring schemes are used to verify the robustness of the research results. Chapter 3 applies a natural experiment to overcome the critique by Gibbons and Overman (2012) and confirm the issue of questionable instruments in the traditional spatial econometric instrumental variable (IV) literature. Chapter 4 extends the choice of optimal neighbor schemes and applies a systematic grid search over different neighbor specifications to single out the potential best neighboring scheme.

The research questions I explore add to the literature on local governmental interaction for mobile capital by adding municipal debt as another policy parameter (chapter 2) since most prior research does not consider municipal debt as an active mechanism in a setting of competition for mobile capital. Chapter 3 shows that local tax interaction between municipalities is not necessarily related to tax competition, as it is commonly done in the empirical literature. The literature on the spillovers of entrepreneurial activity established that entrepreneurship is a local phenomenon where nevertheless spillover does occur. Former research only addresses the time or the spatial dimension of entrepreneurial spillovers. Chapter 4 contributes to this strand of research and estimates jointly the spatial and time dimension of spatial spillovers in the manufacturing and hightech sector in Germany.

In chapter 5 and 6, the focus shifts to local housing policies within German municipalities. Here, I focus on the political economy side of housing approvals. My research contributes to the literature by asking whether tools of political participation are used to influence local housing policies. This question is important since the recent reurbanization process in Germany lead to an increase in housing costs. Hence it is important to determine on the political side which factors influence housing supply. Therefore, I will investigate in the last two chapters if and how local petitions (chapter 5) and/or local elections (chapter 6) influence local approving policies for housing construction projects. In the literature, political effects on housing policies are usually shown via local referendums. Chapter 6 is novel in this sense, since it is the first study that examines a direct link between local elections and housing policies.

In the following, I will give a brief introduction to each chapter and highlight how the chapters relate to each other.

1.2 Outline of chapters

Chapter 2: Race to the debt trap? – Spatial Econometric evidence from Germany

This chapter examines the spatial dependence of municipal debt in Germany. It is titled *Race to the debt trap? – Spatial Econometric evidence from Germany* and co-authored by Rainald Borck, Frank M. Fossen and Ronny Freier. Inspiration for this research is taken from the empirical (Allers and Elhorst, 2005, 2011; Brueckner, 2003; Büttner, 2001) and theoretical (Wildasin, 1988; Salmon, 1987) literature on interactions between local governments. In explaining spatial dependencies between local governments, the theoretical literature commonly assumes that municipalities try to either attract mobile capital or their local politicians try to increase reelection probabilities by mimicking the choices of their neighbors, which is labeled as yardstick competition. Most of the empirical literature usually examines interaction via local tax rates or spending and mainly uses spatial econometric methods. This paper adds municipal debt as another dimension to be considered in the literature on interaction between local governments, since the understanding of competition is otherwise incomplete if only tax interaction is analyzed in isolation (Allers and Elhorst, 2011). Theoretically, this chapter uses a variant of the tax competition model by Jensen and Toma (1991) that shows that the debt level of the municipalities are, under certain assumptions, interrelated.
The research is based on data for all municipalities between 1999 and 2006 in the two largest German states, Bavaria and North Rhine-Westphalia (NRW). Both federal states offer an interesting comparison, since both are of different urban and economic structure. The debt interdependencies between municipalities are empirically approached with a so called Spatial Durbin Panel model. This model does not only allow incorporation of the neighboring debt level as an explanatory variable, it also allows inclusion of neighboring covariates to control for other kinds of spatial shocks that might influence the local debt level as well. Furthermore, the panel structure allows the researcher to track the debt development over time. The results suggest that spatial interaction between municipalities within a state is present in Bavaria and NRW. Specifically, an average increase in neighboring debt of 100 Euro per capita results in an increase between 16 and 33 euro per capita in the respective municipality. Even when expanding the covariates, the results remain robust. Especially, it is interesting to see that debt categories beyond the discretion of politicians (i.e. debt of public companies) show no interaction which

can be regarded as a kind of placebo test. Furthermore, the results show how the interaction effect facilitates through different fiscal channels in both federal states. In NRW, the interaction is mainly driven via short term debt and the financing in Bavaria is driven via the regular core budget debt. When taking both different channels into account, the interaction effects are of the same magnitude. These effects remain unchanged when using a Spatial Durbin Dynamic Panel framework. This chapter contributes to the literature in two ways. First, it adds an additional parameter to the existing empirical literature in tax competition and interaction between local governments. In the past most research considered either tax or spending interaction in isolation. This research shows the interdependence of these parameters, which should not be neglected by future research. Furthermore, this research takes the advice of Gibbons and Overman (2012) seriously and examines carefully the effect of interest by applying thorough robustness checks on a variety of spatial weighting matrices. Since the results show plausible interactions only for debt categories where local politicians do have discretion, the initial theory of interaction via debt is facilitated.

Chapter 3: Let's stay in touch – Evidence on the role of social learning in local tax interactions

Chapter 3 examines the mimicking of local business tax rates between local governments during a reform in North-Rhine Westphalia in 2003 and whether this can be explained by tax competition or by social learning of politicians in an environment with uncertainty about policy outcomes. It is titled *Let's stay in touch – Evidence on the role of social learning in local tax interactions* and is joint work with Sebastian Blesse. It is related to chapter 2, because this research also focuses on interactions between local governments and the underlying reasons behind this. Chapter 3, however, analyzes a different variable of interest, the local business tax rate. Furthermore, this chapter uses a natural experiment to identify spatial interactions of local tax rates.

In 2003 there was a reform of the municipal financial equalization system in the German state of NRW. This reform increased the hypothetical business tax rate which is an implicit threshold for the minimum level of the local business tax rate. If the municipal business tax rate is below the hypothetical tax rate, the municipality will likely receive less funding from the municipal fiscal equalization scheme. Since the hypothetical tax rate was increased during the reform, municipalities had a strong incentive to raise their tax rate at least to the hypothetical tax rate if they were below this threshold. Since tax rates vary considerably between municipalities, the tax rate difference to this threshold provides an arguably exogenous variation to identify whether municipal tax choices are oriented towards neighboring tax changes. This setup can be used to verify whether municipalities react with their tax rate choice towards geographical neighbors or to municipalities that share the same institutions like counties. As an empirical model, an IV spatial lag regression is used. The instrument for the endogenous change of the average neighbor tax rate is the pre-reform tax rate difference to the new hypothetical tax rate.

By using a rich data set on tax rates and its determinants from 1993 until 2006, this research is able to identify potential long run effects of tax interaction. Furthermore, political data, tax revenue data and geographical data about historical counties, shared administration and common media are available to provide a rich series of placebo tests and allow verification of the social learning hypothesis against traditional explanations for local tax interactions.

The main results show a positive tax interaction of municipalities within the same county, which also translates to regions with shared administrative institutions as well as the same media regions. The effect seems to be short lived and a series of placebo tests shows that the interaction occurred in this specific timeframe. Interestingly, interaction effects with geographically contiguous neighbors were not statistically significant and of lower magnitude. When testing the same specifications with traditional spatial IV estimates, all interactions are of strong and high significance. These findings are consistent with the recent quasi-experimental literature on tax competition which shows that traditional spatial econometric instruments might have validity issues (Lyytikäinen, 2012; Baskaran, 2014b).

The research is concluded by explicitly testing for other explanatory approaches from the traditional tax competition literature. These are competition for mobile capital (Wilson, 1999) and yardstick competition (Salmon, 1987; Besley and Case, 1995), i.e. mayors adopt neighboring policy choices when they feel politically vulnerable. Since the results do not show any tax base effects nor any sensitivity towards close elections, yardstick competition and tax competition are ruled out as alternative explanations. These findings, combined with the only significant interaction effects with municipalities that share common institutions, are interpreted as suggestive for social learning. This is concluded since the reform provided an environment of substantial uncertainty about the effects of the municipal tax rate on perceived received funding from the municipal fiscal equalization scheme, which resembles the idea behind the model of Glick (2014). This uncertainty arises since the side effects of the new hypothetical tax with regard to the municipal transfer scheme are hard to predict for a single municipality due to the complexity and multiple factors influencing the municipal fiscal equalization scheme.

Chapter 4: Entrepreneurial Spillovers over space in time

Chapter 4 examines whether high entrepreneurial activity, i.e. the foundation of new businesses, stimulates the establishment of further companies in the same area. It is titled *Entrepreneurial Spillovers over space in time* and is co-authored by Frank Fossen. It is related to chapter 2 and 3, since it also relies on spatial econometric methods. Nevertheless the research focus changes from local governmental interaction to startup activity and its spillover effect within and across regions.

This chapter examines entrepreneurial spillover effects in the German hightech and manufacturing industry while measuring jointly for the spatial and time dimension of the spillover process. It is well known that entrepreneurial activity is a highly localized phenomenon but face-to-face contacts might stimulate new business foundations. This is novel since prior studies on entrepreneurial spillovers usually investigated either the space or the time spillover separately. The empirical method of choice is a Dynamic Spatial Durbin Panel Model.

The estimation strategy includes the time and the spatial component simultaneously. We also address the critique of a choice of arbitrary neighboring weighting matrices (Gibbons and Overman, 2012) by applying a systematic grid search over different possible geographical neighbor specifications. Furthermore, a variant of the dynamic spatial estimator by Lee and Yu (2010b,c) is proposed that incorporates the restriction of the spatial time lag as outlined by Parent and LeSage (2012). Lastly, the benchmark model is used to conduct an Impulse-Response analysis by increasing foundations of new business in the city of Frankfurt/Main by 10%. Afterwards, the estimated model is used to compare the development of entrepreneurial activity with and without this shock over space and time.

This research uses a data base that consists of a balanced panel of 402 German Nuts3 regions from 1996 until 2011. Furthermore, the NUTS3 regions are aggregated to labor market regions that minimize commuting between the regions but maximize commuting within the regions for robustness checks. The main results show that spatial and time spillovers do exist, although the spatial dimension is

of larger magnitude than the time spillover irrespective of different model specifications. Interestingly, the spillovers for the manufacturing sector are of lower magnitude than for the hightech sector which hints towards knowledge spillovers in the hightech industry.
When conducting the Impulse-Response analysis, the entrepreneurial shock of 10% is established in the city of Frankfurt/Main. The simulation shows that 41% of the immediate response due to the shock occurs within 100km from the origin. Furthermore, most of the response happens in the first two years after the shock and the degree of the spillovers in the hightech sector is of significantly higher magnitude, longer lasting and outreaches the spillover of the manufacturing sector. These results indicate that entrepreneurship is a highly local phenomenon and that the spatial component is still stronger than the time component.
This chapter contributes to the literature by investigating entrepreneurial spatial and time spillovers jointly, in contrast to their previous separate analysis. The results confirm the local nature of entrepreneurial capital. Furthermore, this chapter proposes a systematic grid search process for the spatial econometric literature to overcome the critique of arbitrarily choosing a neighbor definition. This is an important first step to take the critique of Gibbons and Overman (2012) into account. Furthermore, the restriction outlined by Parent and LeSage (2012) is taken into account in a maximum likelihood framework. Lastly, the simulation of the spillovers in an Impulse-Response analysis of entrepreneurial activity over space and time is novel within the entrepreneurship literature as well.
So far, all previous chapters of this thesis have examined spatial dependencies between regional units. The two forthcoming chapters shift the scope of the research by examining events inside municipalities. The focus on these chapters is whether tools of local political participation, elections and petitions, do influence housing policies in the respective municipality. Since the research scope shifts from occurrences between regional units to occurrences within regional units, the methodology will switch from Spatial Econometric estimators to non-spatial methods, such as a Difference-in-Difference (DiD) estimator.

Chapter 5: Not in my backyard? – Evaluating the impact of citizen initiatives on housing supply

Chapter 5 is the first study in this thesis that examines political determinants of housing policies in German municipalities. Specifically, this chapter studies whether local petitions have any consequences on housing approval in a municipality in the years preceding the petition. This chapter focuses on the federal state of Bavaria, where petitions have been allowed since 1995. Since then, Bavaria has been the German federal state where local petitions are most frequently applied. The empirical analysis uses an OLS regression with controls and fixed effects, a DiD estimator and a propensity score (PS) matching where municipalities were matched based on similar pre-petition trends in approved housing supply. The treatment group consists of all municipalities which experienced exactly one petition between 1995 and 2011. As control group serve municipalities that never experienced any petition during the given time period. To facilitate comparison, all of these municipalities had a random placebo initiative assigned to them. The assignment algorithm ensures that these placebo initiatives follow the same distribution over the years for all municipalities with exactly one initiative. Municipalities with more than one petition were excluded since these are mostly larger, which would aggravate a comparison.

This research is based on a dataset of all Bavarian municipalities from 1983 until 2011. We use the approved housing square meters as dependent variable. Since petitions were allowed from 1995 on, most of the analysis takes place between 1995 and 2010. The year 2010 is chosen as the upper threshold, since the national census occurred in 2011.

Neither of the aforementioned models showed any effect, nevertheless the estimated coefficients are of small magnitude with rather low standard errors, which indicates that local petitions do not have any influence on urban development policies in Germany. This is an interesting result, since the homevoter theorem (Fischel, 2001) predicts that homeowners would use local petitions to curb urban development to maintain the value of their house.

Chapter 6: You shall not build! (until tomorrow) – Electoral cycles and housing policies in Germany

So far, Chapter 5 has shown that local housing construction approvals are not affected by local petitions. Chapter 6 turns to a different political mechanism by examining the effect of local municipal elections on housing approvals.

That politicians adjust policies during election times to increase reelection probabilities is a well researched fact from the literature on Political Business Cycles (PBC) (Nordhaus, 1975). Nevertheless, this literature is mainly concerned with budgeting policies during election times. This chapter adds a novel parameter to the PBC literature, the housing approvals. The question arises which voter might have an incentive to influence housing policies? The literature identifies different political behavior of homevoters (Fischel, 2001), i.e citizens who own their residence, and leasevoters (Ahlfeldt and Maennig, 2015), citizens who rent their apartment. The theory suggests that homevoters do more strongly intervene in policies that affect their direct environment since they aim to maintain the value of their house. Empirically the presence of homevoters is mostly tested during local petitions but the existence of such an effect during local elections still remains to be analyzed.

This chapter investigates this question by using a sample of 4,983 small to medium sized West German municipalities from 2002 until 2010. The dependent variable is the approved residential square meters for construction per 1,000 inhabitants in the given year. This chapter examines whether housing approvals decrease during election times by introducing an election year dummy and the analysis that follows is done by using an OLS estimator with municipality and year fixed effects. Since the timing of the local elections differs across the German *Länder*, this fixed effects regression resembles a Difference-in-Difference approach. Furthermore, the within year timing of the elections also differs across states, which will be taken into account with a weighted election coefficient. This setup shows whether there is a willing adjustment of housing approvals during election times. In the next step, this chapter will explore whether the homevoter theory is able to explain this pattern.

The main results show a decrease of 11.4 percent in the approved housing area during election years evaluated at the mean, which is a reasonable sized effect given that it is unlikely that all municipalities adjust during election times. This result remains statistically and economically similar throughout a wide array of robustness checks. Furthermore, the baseline results also show a catch up effect in housing approvals two years after the election. The results also show that the adjustment is correlated with the share of homeowners and the approval of single/double family houses. This is interpreted as suggestive evidence for the homevoter.

This chapter contributes to the literature on Political Business Cycles by adding a novel and yet unexplored dimension. Furthermore, this delay in housing approval might be one the contributing factors that explain the rent hike in German agglomerations. A delay in housing approval in municipalities around agglomerations might slow down the potential outflows of the agglomeration and hence exacerbate the lack of housing supply. Furthermore, as it analyses the homevoter effect using local elections instead of petitions, this is the first research to show that the homevoter does not only act within local petitions but the presence is also taken into account during local elections.

Chapter 5 and 6 determine jointly the second part of the thesis which investigates the political economy of housing supply. Where the literature so far is able to measure the impact of homevoters via local petitions, the German case seems to be a bit different since the effect becomes apparent during local elections. One explanation might be that local petitions are still a rather novel tool in the German political context and homevoters already adapted to putting their weight before municipal councils before and during election times.

1.3 Tabular overview

This section provides a compact overview of all chapters of the dissertation. Starting with the title and the number of co-authors, the table also gives a summary of the main question and the used data. The table also informs where the chapters were published prior to the dissertation, this may be either in a journal or in a discussion paper series.

Table 1.1: Overview

	Chapter 2	Chapter 3	Chapter 4	Chapter 5	Chapter 6
Title	Race to the debt Trap? – Spatial Econometric Evidence from Germany	Let's stay in touch – Evidence on the Role of Social Learning in Local Tax Interactions	Entrepreneurial spillovers over space and time	Not in my backyard? – Evaluating the impact of citizen initiatives on housing supply	You shall not build! (until tomorrow) – Electoral cycles and housing policies in Germany
Co-Authors	Rainald Borck, Frank Fossen, Ronny Freier	Sebastian Blesse	Frank Fossen	Felix Arnold, Ronny Freier	
Research Question	Is there a spatial spillover of municipality debt?	What causes tax mimicking between municipalities?	How far do entrepreneurial spillovers reach?	Does the introduction of local referendums reduce housing approvals?	Do municipal housing policies change during election times?
Data source	Bavarian and North-Rhine Westphalian Statistical office	North-Rhine Westphalian Statistical office	Regional Statistical Data Catalog of the Federal Statistical Office and the statistical offices ot the Länder and Mannheim Enterprise Panel	*[Mehr Demokratie]* petition database and Bavarian Statistical office	Regional Statistical Data Catalog of the Federal Statistical Office and the statistical offices ot the Länder and *Statistik Lokal Cd'S* and Census 2011 data for homeowner shares
Time Period	1998 - 2008	1992 - 2008	1996 - 2011	1995 - 2011	2002 - 2010
Data Structure	Balanced Panel of German Municipalities	Balanced Panel of German Municipalities	Balanced Panel of German NUTS3 and Labor Market Regions.	Balanced Panel of German Municipalities	Balanced Panel of German Municipalities
Identification	Quasi-Maximum Likelihood Spatial Durbin Model	IV Spatial Lag Model	Dynamic Quasi-Maximum Likelihood Spatial Durbin Model	OLS with fixed effects, DiD and PS matching	DiD Estimator
Main results	German municipalities react towards neighboring municipalities when they increase their debt level.	Municipalities react with their tax changes only to municipalities with common social ties, no signs of tax competitions.	Entrepreneurial spillovers do exist and are highly localized.	Local petitions in Bavaria do not have influence on local housing policies.	Housing approvals decrease during election times, which is correlated with the share of homeowners.
Discussion Paper	Fossen et al. (2014)	Blesse and Martin (2015)	Fossen and Martin (2016)		Martin (2017)
Journal Article	Borck et al. (2015)		Submitted to a Journal		Submitted to a Journal

Chapter 2

Race to the debt trap? – Spatial econometric evidence on debt in German municipalities[†]

2.1 Introduction

Large and growing public debt in many countries, such as the US, Japan and Eurozone members, is alarming policymakers and citizens. Similarly, ever-increasing sovereign debt at sub-national levels like US states or German federal states as well as at the municipality level is gaining attention. The on-going debt crisis has also spurred an academic debate focusing on the macro-economic and political economic mechanisms that drive sovereign debt. The insolvency of Detroit in 2013 put municipality debt into the spotlight.

This paper investigates spatial interdependencies between debt levels of local jurisdictions. While the existing spatial econometric literature focuses on tax and spending competition (see section 2.2), surprisingly, very little is known about spatial interaction via public debt. Does debt spread from one local jurisdiction to the other?

Our main argument for debt interaction between jurisdictions builds on (Jensen

[†]This chapter is based on joint work with Ronny Freier. A similar version has been published in Regional Science and Urban Economics, see Borck et al. (2015).

and Toma, 1991). Suppose that two jurisdictions compete for mobile capital. They can finance spending on public goods by taxing capital or issuing debt. If one jurisdiction increases its debt level, both jurisdictions increase their tax rates in the future. This in turn makes debt finance for the other jurisdiction today more attractive under certain assumptions. This model is developed in detail in section 2.3. Some other mechanisms may also lead to interactions in debt levels, see section 2.2.

Earlier research concludes that spatial tax and spending competition should not be analyzed in isolation (see Allers and Elhorst, 2011, and the detailed discussion in section 2.2). Similar to their argument, we now emphasize that debt is an additional dimension that needs to be considered in order to understand spatial competition if jurisdictions have the discretion to shift the costs of expenditures into the future. As argued by Allers and Elhorst (2011), the understanding of competition is necessarily incomplete if not all decision parameters are considered. The political economics literature on debt recognizes multiple mechanisms that amend the above argument. First, the decision on taxes, expenditures and debts are typically in the hands of politicians. Inherent in the democratic process, the political decision makers consider a shorter time horizon than the local constituency does, thus, favoring taxes tomorrow, i.e. debt, over taxes today. Second, a large literature on political business cycles illustrates that governments favor high spending and low taxes particularly in times of elections, which all else equal must lead to higher debt (see Nordhaus, 1975; Blais and Nadeau, 1992; Veiga and Veiga, 2007; Foremny et al., 2014; Foremny and Riedel, 2014).[1] Third, a literature on the strategic use of debt illustrates that political considerations lead to debt issuing instead of current taxation. Partisan politicians may incur debt instead of levying taxes not only to gain an advantage in tax competition but also to limit the opportunities of later governments (see Alesina and Tabellini, 1990; Fiva and

[1]Note that the above arguments raise the question why politicians get away with ever-increasing debt. Voters must ultimately realize that the debt needs to be repaid. Existing literature, however, illustrates that voters are indeed myopic. For German municipalities, Freier (2011) shows that mayors face higher chances of reelection if they increase expenditures above average levels. However, voters fail to punish local mayors as public debt is also increased above the average. Heinemann and Hennighausen (2012) report that voters' debt preferences are partly influenced by factors such as (lack of) trust and ideology.

Natvik, 2011).[2]

Finally, also the particular institutional setting in Germany incentivizes municipalities to favor debt over current taxation. While municipalities have constitutionally guaranteed rights to manage their own affairs, they face little to no actual risk of insolvency. Municipalities (and investors) ultimately expect complete bail out by state governments in case of fiscal distress, such that German municipalities might see all the more benefits to strengthen their position in the tax and spending competition by going into debt.[3]

In our empirical analysis, we investigate spatial interactions in debt levels between German municipalities. We focus on municipalities in the two largest states, Bavaria and North Rhine-Westphalia (NRW), mainly during the 1999-2006 time period. Studying the German case is of particular interest as the German municipalities are allowed to incur debts. The fact that all municipalities (within a state) operate within a common institutional framework facilitates the identification of spatial interaction effects isolated from confounding factors, which are often of concern in cross-country studies.

In our main specification, we use a spatial Durbin model in a panel framework (Elhorst, 2012). Using this model, we present estimates for spatial interactions in debt and also show the effects when we use taxes and spending as outcome variables. To assess the robustness of the results, among other tests, we alternate the spatial weighting matrix, we report results for specification tests and we show that 'competition' on debt does not work via subcategories of debt where municipalities have little discretion.

We explore various standard as well as three non-standard spatial weighting matrices. The first non-standard matrix considers the grouping of municipalities into counties; the second one implements a theoretical suggestion by Janeba and Osterloh (2013) where urban centers not only compete with their neighbors, but also with all other urban centers; and the third assigns only municipalities of a similar size as neighbors. Finally, we also consider dynamic spatial lag models.

[2]Another model of strategic debt is developed by Persson and Svensson (1989). Their mechanism builds on the idea that right and left governments typically differ in the amount of desired spending. Low reelection chances increase the incentives of a conservative government to borrow in order to restrict left governments in the amount of taxation (and spending) in later periods. Empirically, Pettersson-Lidbom (2001) confirms that the strategic rationale for debt is indeed of importance in Swedish municipalities.

[3]Thus, German municipalities operate subject to soft budget constraints, which may lead to vertical and horizontal strategic interactions with regard to public debt (see Baskaran, 2012). Limitations to and consequences of municipality debt institutionalized especially in North Rhine-Westphalia are discussed in section 2.4.

Our results show significant positive interaction effects between neighboring municipalities. We find interaction coefficients for debt on the order of 0.16-0.33, meaning that an increase in the debt level of the neighboring municipalities by 100 Euro (per capita) increases debt in a municipality by 16 Euro in NRW and by 33 Euro in Bavaria (per capita). The results are significant in both states and are robust to various specifications of the spatial model. Our estimates of spatial debt interaction lie in between estimates for tax and spending interaction (estimated from the same German data). The results indeed indicate that local government debt interaction must be regarded as an important dimension of local spatial interaction in addition to tax and spending competition between municipalities.

The remainder of the paper is structured as follows: In section 2.2, we discuss the relevant literature on tax and spending competition. In section 2.3, we develop a theoretical model, based on Jensen and Toma (1991), which shows how jurisdictions' debt levels interact. Section 2.4 delineates the institutional setting in German municipalities and introduces our data. The empirical model and estimation strategy is then described in section 2.5 before we present and discuss our findings in section 2.6. The analysis is concluded in section 2.7.

2.2 Literature on fiscal competition

There is a very large theoretical and empirical literature on fiscal competition. We briefly review here those contributions which inform our paper's approach. The literature has emphasized three theoretical mechanism to explain why jurisdictions' fiscal policies interact: competition for mobile tax bases, yardstick competition, or spillovers.[4]

The tax competition literature has examined competition among jurisdictions for mobile tax bases. In the classic tax competition model of Wildasin (1988), jurisdictions use taxes to attract mobile capital. There is strategic interaction because one jurisdiction's choice of tax rate influences the tax base of its neighbors through the induced relocation of capital. Theoretically, it is open whether tax rates are strategic substitutes or complements, i.e. whether jurisdictions react to increases in their neighbors' tax rates by increasing or reducing their own tax rates. A now large empirical literature has examined the strategic interactions of

[4] See Revelli (2005) for a good overview of these mechanisms and their theoretical and empirical implementation.

taxes on mobile capital, see Brueckner (2003) and Allers and Elhorst (2005) for overviews.

The tax competition argument has also been extended to choices of spending, see e.g. Keen and Marchand (1997) and Borck (2005) for theoretical and Case et al. (1993), Solé-Ollé (2006), and Borck et al. (2007) for empirical papers.

The second strand of literature argues that strategic interaction may be due to yardstick competition (e.g. Salmon, 1987; Besley and Case, 1995). The idea here is that voters have imperfect information on politicians' types ('good' or 'bad') but can observe their own and neighboring jurisdictions' fiscal choices. Strategic interaction results because neighboring jurisdictions' fiscal policies have an informational spillover. For instance, if the costs of providing public goods are unobserved by voters but correlated among jurisdictions, observing high spending levels in neighboring jurisdictions would induce voters to think that costs in their own jurisdiction are also high. This then affects politicians' optimal fiscal policies and leads to correlated policies between jurisdictions. Applications of yardstick competition include Besley and Case (1995) and Bordignon et al. (2003).[5]

Third, budget spillovers provide another simple mechanism for strategic interaction. If, say, jurisdiction j increases spending on a public good whose benefits spill over into jurisdiction i, then jurisdiction i will naturally react to changes in spending by its neighbors. If the spillover is positive, we would expect jurisdiction i to reduce its own spending. The spillover rationale for strategic interaction lies at the heart of Case et al. (1993).

What does all this imply for the interaction of debt levels? In the next section, we use a simple theoretical model based on Jensen and Toma (1991) which shows that in a dynamic fiscal competition framework with debt financing, jurisdictions react to the debt levels of their neighbors. The reason is that in this model, if one jurisdiction increases its debt, both jurisdictions increase their tax rates in the future. This in turn makes debt finance for the other jurisdiction today more attractive.[6]

In the yardstick competition framework, it is likewise easy to argue that jurisdictions strategically interact with their debt levels. Suppose that jurisdictions j increases its debt level. This would induce voters in jurisdiction i to revise their estimate of their home politicians' type (good or bad, see above), which in

[5] Yardstick competition may also apply if, irrespective of political economy considerations, lenders evaluate jurisdictions' fiscal performance using other jurisdictions' performance as a benchmark (Landon and Smith, 2000).

[6] Note that these results obtain under certain assumptions which are detailed below; instead of being strategic complements debt levels could also be strategic substitutes.

turn would influence the optimal fiscal policies, i.e. taxes and debt levels set by politicians in jurisdiction i. It is likely that increases in neighbors' debts lead a jurisdiction to increase its own debt, although this need not be the case (see Revelli, 2005).

With spillovers, if jurisdiction j increases its debt level to finance some public good whose benefit spills over into jurisdiction i, jurisdiction i would also adjust its debt level. If the spillover is positive (say, higher education), jurisdiction i would decrease its own debt.[7]

It is now interesting to consider the effects of soft budget constraints. If jurisdictions believe that a higher level government authority would bail them out in case of an imminent insolvency, all towns have an even stronger incentive to engage in competition for mobile tax bases (as they do not face the consequences in the future when this debt must be retired).[8] Note that, a different mechanism that prescribes horizontal interactions between local jurisdictions is competition for bailouts (Baskaran, 2012). If there is a budget constraint at the higher level that limits the scope for bailouts, jurisdiction i might react by reducing its debt level when jurisdiction j increases its debt because fewer funds are now available and a bailout would seem less likely.[9]

How can one discriminate between different potential mechanisms underlying strategic interaction in debt levels? One approach would be to use theory-driven arguments about the sign of the slopes of reaction functions. For instance, if public goods spillovers are positive on average, the spillover argument would imply negatively sloped reaction functions. Likewise, competition for bailouts would also imply negative slopes. Hence, if we find that debt reaction functions have positive slopes, this would mainly leave yardstick competition or competition for mobile tax bases as mechanisms. We will use some auxiliary evidence below to shed more light on these potential mechanisms.

[7] If the spillover were negative, then jurisdiction i would increase its debt level. An example might be public spending which decreases pollution in j but increases pollution in i.

[8] This is especially true if towns expect a bailout without further consequences. Note that Baskaran (2014a), however, shows that municipalities that receive particular funds for debt services from the state (partial bailout) do in fact face significant austerity measures. This works against the argument that bailout expectations are driving the competition in debt.

[9] Similarly, if the creditworthiness of a jurisdiction depends on the creditworthiness of other jurisdictions, debt levels will be related (Landon and Smith, 2000).

2.3 Theoretical model

We use a variant of Jensen and Toma's (1991) model to derive jurisdictions' reaction functions which show how one jurisdiction's debt level influences that of its neighbors.[10] We take the simplest case of 2 periods and 2 identical jurisdictions (generalizing to more than 2 jurisdictions or periods or asymmetric jurisdictions is straightforward). Each jurisdiction has a representative resident who owns $K/2$ units of capital and inelastically supplies one unit of labor (or another fixed factor such as land). There is one private good, denoted x, produced by using capital and labor, and one public good, denoted g. In period $t = 1, 2$, production in jurisdiction $i = 1, 2$ is given by the production function $f(k_{it})$ with $f' > 0 > f''$. Residents' instantaneous utility function is $U_{it} = x_{it} + u(g_{it})$ with $u' > 0 > u''$. We assume that lifetime utility is simply the sum of the periodic utilities (we abstract from discounting for simplicity).

Jurisdictions have access to a unit source tax on capital at rate τ_{it}. Capital is mobile between jurisdictions. There is perfect competition within jurisdictions so capital is paid its net marginal product. Letting ρ_t be the net rate of return on capital, mobility implies that in each period

$$f'(k_{it}) - \tau_{it} = \rho_t \tag{2.1}$$

$$k_{1t} + k_{2t} = K. \tag{2.2}$$

These 2 equations implicitly define the capital levels $k_{it}(\tau_{1t}, \tau_{2t})$ and the net return $\rho_t(\tau_{1t}, \tau_{2t})$, where a jurisdiction's capital decreases in its own and rises in the other's tax rate, and the net return falls with both tax rates.

Period 2 spending is financed by the unit tax on capital, while period 1 spending is entirely financed by debt, which has to be repaid in period 2.[11] Let d_i be jurisdiction i's debt level and r the interest rate on bonds. The periodic government budget constraints are given by

$$g_{i1} = d_i \tag{2.3}$$

$$g_{i2} = \tau_{i2} k_{i2} - (1 + r) d_i. \tag{2.4}$$

[10] See Schultz and Sjöström (2001) for a similar model.

[11] In the model of Jensen and Toma 1991, period 1 spending can be financed by debt or taxes. Our simplification leaves the derivation of debt reaction functions intact. In Appendix 2.10, we derive a more general model, where debt levels also depend on the neighbours' tax rates. We assess the empirical relevance in a robustness check in section 2.6.4.

Residents' total income in each period consists of labor income (fixed factor remuneration) and capital income:

$$y_{it} = f(k_{it}) - (\rho_t + \tau_{it})k_{it} + \rho_t K/2. \tag{2.5}$$

Individuals save by buying government bonds. The residents' periodic budget constraints are

$$x_{i1} = y_{i1} - d_i \tag{2.6}$$

$$x_{i2} = y_{i2} + (1+r)d_i, \tag{2.7}$$

where d_i denotes the level of saving (i.e. bond holdings).

Since utility is linear in x_{it}, the intertemporal elasticity of substitution is infinite. Therefore, in equilibrium the interest rate must be zero. We can then write lifetime utility as

$$v_i = y_{i1} + y_{i2} + u(d_i) + u(\tau_{i2}k_{i2} - d_i). \tag{2.8}$$

The game is solved by backward induction. In period 2, for given debt levels, jurisdictions set tax rates τ_{i2} to maximize their utility

$$v_{i2} = y_{i2} + u(\tau_{i2}k_{i2} - d_i). \tag{2.9}$$

The first order conditions are

$$-k_{i2} + u'_{i2}\left(k_{i2} + \tau_{i2}\frac{dk_{i2}}{d\tau_{i2}}\right) = 0, \tag{2.10}$$

where $u'_{it} \equiv u'(g_{it})$. Since the capital levels are functions of both regions' tax rates, these equations implicitly define the reaction functions $\tau_{12}(\tau_{22}, d_1), \tau_{22}(\tau_{12}, d_2)$, i.e. the optimal tax rates as functions of the other region's tax rate.[12] These are shown in Fig. 2.1. The figure shows the reaction functions $\tau_{i2}(\tau_{j2}, 0)$ in the absence of debt and the ensuing equilibrium which is given by the intersection of the reaction functions. As usual in the literature, the slopes of the reaction functions generally have ambiguous signs, see e.g. Brueckner and Saavedra (2001b). When jurisdiction 1 increases its debt level, its reaction function shifts out to $\tau_{12}(\tau_{22}, d_1)$ while that of jurisdiction 1 is unaffected (see Jensen and Toma, 1991). The intuition is that increasing debt in period 1 increases the amount of repayment necessary

[12] Here and below, we use the fact that the terms-of-trade effects, i.e. effects of the tax rate and debt levels on the net return of capital are zero in a symmetric equilibrium.

in period 2, which reduces the level of public good supplied for unchanged tax rate. Since marginal utility is decreasing in g, the optimal response to a rise in debt is to increase the period 2 tax rate. In the case of positively sloped reaction functions, as a result, both jurisdictions' tax rates increase.[13]

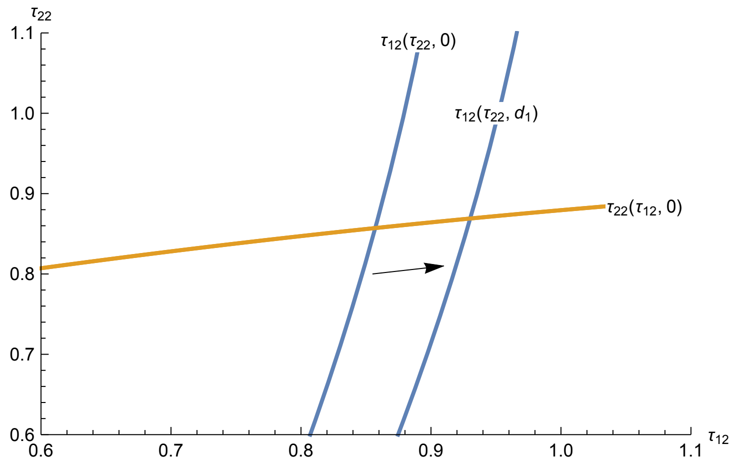


Figure 2.1: Jurisdictions' tax reaction functions in period 2

In period 1, jurisdictions set their debt levels d_i, taking the other jurisdiction's debt as given. The first order conditions are given by

$$u'_{i1} - u'_{i2} + u'_{i2}\frac{dk_{i2}}{d\tau_{j2}}\frac{d\tau_{j2}}{dd_i} \quad = \quad 0. \tag{2.11}$$

Equation (2.11) shows jurisdictions' strategic incentive to issue debt. The first two terms on the left of the equations show the standard tax smoothing incentive a la Barro (1979). The third term shows that the incentive to issue debt is affected by the reaction of period 2 taxes. Since $\frac{dk_{i2}}{d\tau_{j2}} > 0$, the incentive for jurisdiction i to issue debt increases if jurisdiction $j \neq i$ increases its tax rate in response to the higher debt level of i. This would be the case if the tax reaction functions are positively sloped, as in the example. In this case, jurisdictions would be driven to issue excessive debt, compared to the case without strategic interaction.

The two equations in (2.11) define the two jurisdictions' debt reaction functions $d_i(d_j)$ which show how the optimal period 1 debt levels interact. The slopes of the reaction functions are found by differentiating (2.11) and have, again, ambiguous signs. Fig. 2.2 displays the debt reaction functions of the two jurisdictions in a numerical example. For the example, we assume production functions $f(k_{it}) = k_{it} - \beta k_{it}^2/2$ with $\beta = 1.5$, and $u(g_{it}) = \log(g_{it})$.

[13] Here and below, we additionally assume that the equilibrium is stable.

In this example, it turns out that reaction functions are positively sloped. Intuitively, if jurisdiction 2 increases its debt level, given the tax reaction functions in our example, both jurisdictions' period 2 tax rates will increase. This increases jurisdiction 1's period 2 spending and hence decreases the marginal utility from public good supply in period 2. The optimal response is then to increase the debt level in order to realign the marginal utility of public goods across the two periods.[14]

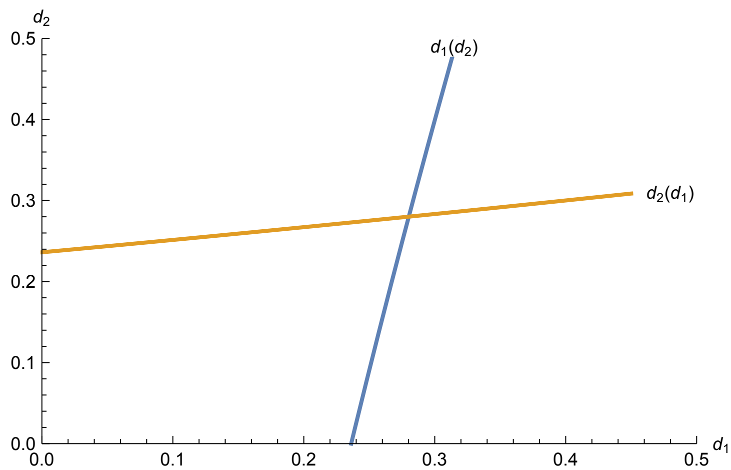


Figure 2.2: Jurisdictions' debt reaction functions

In section 2.6, we will test for strategic interaction, i.e. we will test whether jurisdictions' debt reaction functions have nonzero slopes.

2.4 Institutional setting, data and descriptives

This section discusses the institutional setting in which German municipalities operate. Moreover, we introduce the data that we use in the empirical analysis and feature descriptive statistics.

[14]Differentiating (2.11) shows that the derivative of the jurisdiction i's first order condition with respect to d_j is somewhat complicated and depends inter alia on the second derivatives of k_{i2} and τ_{j2}.

2.4.1 Institutional setting

The municipality level is the lowest and most disaggregated level of the public institutions in Germany.[15] The main areas of local public good provision involve general administration, public order, infrastructure, cultural institutions and public transport. Together with the other government tiers, municipalities also administrate expenditures for child care, schooling and social security. Furthermore, the local level often supervises and administrates basic services such as water and energy supply or waste disposal. To finance those services, municipalities receive income from three own local taxes (two types of property taxes and a tax on local businesses) along with allocated tax revenue from local income taxes and the VAT as well as state-allocated grants. Overall, municipalities have considerable (constitutionally guaranteed) discretion in their budgeting. All decisions on the finances of a municipality are in the hands of an elected mayor and the elected local town council.[16]

Importantly for our analysis, municipalities in Germany have the right to incur debt. Municipal debt makes up about six percent of the overall public debt in Germany throughout our observation period. The prime lenders to German municipalities are German savings and loan associations (*Sparkassen*), German private banks as well as state run public banks (Freier and Grass, 2013). Municipalities in Germany are generally free to incur debt for investment or investment support measures (core debt).[17] There is direct surveillance of municipal borrowing in core debt by upper level jurisdictions, which are part of the administrative structure of the municipality's federal state and ultimately report to the federal

[15]Besides the federal level, there are 16 federal states, about 450 counties and a total of about 12,500 municipalities. Following the Nomenclature of Territorial Units for Statistics (NUTS) developed by the European Union, all German LAU-1 and LAU-2 regions are labeled as municipalities. In addition, we include the independent cities which are NUTS3 regions, but have the same administrative tasks as municipalities.

[16]In NRW and Bavaria, voters elect mayors through majoritarian elections and councils through proportional elections. In both states, the local elections for mayor and council are held on a state-wide election date every 5 years in NRW and every 6 years in Bavaria. In our period, elections were held in 1999 and 2004 in NRW and in 2002 in Bavaria. Note that, the responsibilities of the mayors include the operative management of the administration as well as preparation of all decisions that are to be made in the council. Also, the mayor has active voting rights in the council and often heads the different spending committees of the council. Ultimately, the legislative body that makes the final decisions on all municipal affairs is the town council.

[17]While the regulation to use debt financing only for investment purposes may limit the options of municipalities to some degree, it is not a very hard constraint. A municipality that wants to fund new investment with 60% debt and 40% equity can decide to simply fund the new investment with 100% debt and use the remaining funds for operative spending if the financial situation warrants it.

state's interior ministry.[18] Further, municipalities can also incur short-term debt (*Kassenverstärkungskredite*), which does not require approval during our observation period. Although this type of short-term debt is intended to bridge short-term liquidity shortages only, it is de facto also used for long-term financing of all kinds of municipal activities (Gröpl et al., 2010). Municipalities that, left alone, would suffer from insolvency, loose active steering rights in their municipal finances, resulting in state regulators taking over local decision making. The actual procedure is organized in different steps ranging from more oversight when the financial situation becomes critical to complete take-over when insolvency would be reached. Regulations differ between the sixteen federal states of Germany. Specifically, in 1991, the state of NRW implemented a concept in its municipal code that mandates municipalities in financial distress to present a budget consolidation plan (*Haushaltssicherungskonzept*) to the regulating authority. The plan is approved if it shows how a balanced budget can be reached within three years.[19]

The current institutional setting in Germany implicitly guarantees that the federal states stand in for public debt incurred by their municipalities. Investors are guaranteed full compensation with states bailing out local authorities. As a result, we do not see interest rate spreads as a function of the economic conditions of a municipality (see Ade, 2011). Given this institutional design, incentives to incur debt to gain an advantage in tax and spending competition are increased.

The two states in our analysis are NRW and Bavaria. There are multiple reasons why we focus on those states in particular. First, they represent two of the biggest states both in terms of population as well as size.[20] Together, about 37% of Germany's population resides in one of these two states. Data on municipal finances can be obtained for both states and can also be linked to relevant background in-

[18]Our institutional research showed that Bavaria and NRW are generally using similar institutions that govern the rules of financial oversight. While there are no decisive legal differences, it is still the case that regulation is perceived as more lenient in NRW, see Gröpl et al. (2010).

[19]In 2011 this time frame was extended to ten years. If the budget consolidation plan is not approved, the municipality is in the state of an emergency budget (Art. 82 Municipality Code NRW). In this case, only expenditures are permitted that are mandated by law or that continue necessary tasks and cannot be postponed. Conversion of debt is permitted, but credit necessary to continue investments already begun must be approved by the regulating authority. Tax rates of local taxes must stay at the last year's level. If the municipality keeps on failing to present an approvable consolidation plan, the regulating authority can limit the municipality's administrative autonomy and order saving measures or even impose them. In 2011, after our period of analysis, the state of NRW started a large active program to limit municipal debt (*NRW Stärkungspakt*).

[20]In 2013, NRW had a population of 17.85 million inhabitants on an area of about 34,000 km^2 and is the largest German state in terms of population. Bavaria had a population of 12.67 million people (2nd largest state) and a size of about 70,500 km^2 making it the largest state in terms of area.

formation. Generally, the specific tasks and responsibilities of the municipal level in Germany differ by state, which makes state comparisons complicated. The two states in our sample, however, each put a relatively large share of the overall responsibilities in a state at the discretion of their municipalities.[21]

While our two states are similar in importance and the institution setup, there are also interesting differences. Municipalities in NRW are generally large, relatively urban and industrial. Moreover, the level of debt is comparatively high. In Bavaria, there are many small to medium-sized towns, which are often rather rural and agricultural. Moreover, the overall level of debt is much lower in Bavaria than in NRW.[22]

2.4.2 Data and descriptive statistics

We use data from a complete panel of municipalities in the German federal states of North Rhine-Westphalia and Bavaria. In the main estimations, we use data from 1999 until 2006. In various specifications in the robustness section, we additionally employ the year 1998 to provide lagged variables. We also run estimations based on extended time periods reaching back to 1987 maximum.[23] In Bavaria, we observe data for all 2,056 municipalities, and in NRW, for all 396 municipalities, in each cross-section. For both states and all years, we combine the official statistics at the municipal level on fiscal variables (debt, taxes and expenditures), population data (number of inhabitants, structure of the population, information on unemployment) and regional data on local GDP (at the county level). The data are provided by the Research Data Centers of the Federal and State Statistical Offices.

[21] In 2005, 50.7 percent of all state and municipal expenditures in NRW were under the control of the municipalities. The share in Bavaria was almost as high, at 47.1 %, as well. While few other states have similarly high numbers (e.g. Hesse and Baden-Württemberg), other states have a considerably lower level of local activity (East German states reach on average 44.2 %, Saarland has the lowest numbers with only 36.5 %.), see Böttcher et al. (2010), p. 107. States such as Rhineland-Palatinate and Lower Saxony would be even more complicated to compare, because local activity there involves an additional tier of government (*Amt*) between municipalities and counties. Moreover, states in the East can often not be compared over time as the municipal structures have seen important changes in administrative reforms.

[22] In fact, in 2009 NRW was one of the three (out of thirteen) territorial states with the largest per capita municipal debt levels in Germany, along with Rhineland-Palatinate and the Saarland, and Bavaria was one of the five states with the smallest, see Freier and Grass (2013).

[23] There are two reasons why we do not include years after 2006. First, we lack important data, especially short-term debt, unemployment per capita, and per capita welfare spending. Second, Christofzik and Kessing (2014) report that the introduction of accrual accounting in NRW allowed municipal governments to temporarily escape fiscal oversight. Almost 90% of the municipalities implemented accrual accounting after 2006.

Descriptive statistics for all variables used in the analysis (pooled for all years and by state) are found in Table 2.6 in Appendix A. Notably, NRW has much larger municipalities in terms of population, higher average per capita debt and higher unemployment rates than Bavaria. Per capita net spending (the sum of all expenditures net of obligations due to the fiscal equalization system) is somewhat higher in NRW than in Bavaria in terms of mean and median. Looking at particular spending categories, municipalities in NRW exhibit larger current operating and personnel expenditures and much higher levels of welfare spending.[24] The age structures of the population in both federal states, as indicated by the shares of persons below 15 and above 65 years of age, are similar. Net migration is defined as population inflow minus outflow, normalized by the number of inhabitants, and is a bit larger on average in Bavaria. We further collected the real growth rate in GDP at the county level (total value added, in domestic prices, deflated by the federal state consumer price index).[25]

As our main outcome variable in the empirical model, we use the per capita debt of a municipality. The data allow for a distinction between accumulated per capita debt of the core budget (*Kernhaushalt*), short term debt (*Kassenverstärkungskredite*) and debt of a municipality's public companies. In our basic models we use the sum of the core budget and the short term debt, because this is the total debt under direct control of the municipality. In further estimations, we also analyze all three types of debt separately.

In addition to public debt, we examine per capita net spending and rates of the local independent taxes as dependent variables. With regard to taxes, we explore the property tax rates A and B[26] as well as the local business tax rate. All aforementioned taxes have in common that municipalities choose multipliers (labeled tax rates in Table 2.6) that are applied to a uniform basic tax.[27] Since 2004 the tax rate multiplier for the local business tax must range between 200

[24] Bavaria includes some strong spending outliers. Most outliers with high values of per capita net spending are attributed to the municipality of Unterföhring. This municipality, which is located just outside Munich, is indeed special in that it is an exceptionally attractive business location. Among the many firms residing in that municipality are public and private media companies as well as big insurance companies such as Allianz and Swiss Re.

[25] Unfortunately, data on local GDP is not available on the level of each individual municipality. Instead, we use county level growth rates for all municipalities from a county. Note that for large urban municipalities (*kreisfreie Städte*) the municipal and the county level coincide, thus, they have individual GDP growth rates.

[26] Property tax A is used for agricultural and property tax B for all other real estate. The tax bases shown in Table 2.6 indicate that the property tax B is relatively more important.

[27] The effective local business tax rate in 2009 is calculated as 0.035 * multiplier, for example; see Bach and Fossen (2008) for details.

and 800.[28]

The spatial distribution of per capita debt is depicted in Figure 2.3 for NRW and Figure 2.4 for Bavaria in Appendix B. We compare maps for 1999 and 2006 for each state. The maps highlight some patterns of clustering, which shift slightly over time. Moreover, we plot annual means of per capita public debt and net spending as well as the tax rate multipliers for the local business tax and the property tax B in Figures 2.5 for NRW and 2.6 for Bavaria in Appendix B. We observe that the per capita debt in NRW is steadily increasing whereas the increase is only moderate in Bavaria. The per capita net spendings are comparably more cyclical; however, they also show an increasing trend. In comparison the patterns for the local business tax and the property tax B are rather stable.[29]

2.5 Empirical model and strategy

The goal of this paper is to estimate the extent of spatial dependency of debt between German municipalities. To this end, we need to incorporate debt in a spatial panel framework. As a starting point, the simplest specification of this approach incorporates the neighboring debt into a regression framework (Spatial Lag Model - SLM):

$$y_{it} = \lambda \sum_{j=1}^{N} w_{ij} y_{jt} + x_{it}\beta + \mu_i + \zeta_t + \epsilon_{it}, \qquad i, j = 1, ..., N, \tag{2.12}$$

where y_{it} denotes per capita debt of municipality i at time t. According to our argumentation, municipalities will be affected by the debt of a predefined set of neighbors. This is described by the term $\sum w_{ij} y_{jt}$, where w_{ij} is the i,jth element of a nonnegative $N \times N$ weighting matrix, W, which assigns neighboring municipalities. By assumption, a municipality cannot be a neighbor with itself and therefore the main diagonal of W equals zero. The response to neighboring municipalities is captured in the estimation parameter λ. The term x_{it} is a $1 \times K$

[28] No municipalities in NRW or Bavaria were directly affected by this restriction, as indicated by the minimum and maximum values of the local business tax multiplier in the period 1999-2006.

[29] The jumps in the tax rates in NRW in 2003 are likely to be related to an increase in the standardized tax multipliers (*fiktive Hebesätze*) in that year, which are set by the state government and are used in the local fiscal equalization scheme (see Baskaran, 2014b, for details). As the adjustment of these standardized tax multipliers is the same for all municipalities in NRW, this is accounted for in our estimations by the time fixed effects.

vector of socio-demographic variables from municipality i at time t (in further specifications, we will additionally include structural characteristics) and β is a related $K \times 1$ vector of estimation parameters. Furthermore, ϵ_{it} represents a normal, independent and identically distributed error term.[30] μ_i and ζ_t denote municipality and time fixed effects (FE), respectively.

The inclusion of spatially lagged independent variables leads to the Spatial Durbin Model (SDM):

$$y_{it} = \lambda \sum_{j=1}^{N} w_{ij} y_{jt} + x_{it}\beta + \sum_{j=i}^{N} w_{ij} x_{jt} \theta + \mu_i + \zeta_t + \epsilon_{it}, \tag{2.13}$$

where $w_{ij}x_{jt}$ represents the characteristics of neighboring municipalities and θ denotes the corresponding $K \times 1$ vector of their respective parameters.

In order to find the most adequate spatial specification, we conduct various LM and LR tests along the lines of Elhorst (2012). The test results guide us to choose the SDM over the SLM.[31]

The panel structure of the data allow us to exclude time and municipality fixed effects by a double de-meaning procedure. Thus, we identify our models from changes in the per capita debt variable within a municipality over time. As the current local government takes the level of debt from last year as given, we interpret changes in debt as the actual decision parameters for municipal politicians.

The estimations of eq. (2.13) are conducted in Matlab by using routines provided by Elhorst (2012).[32] These routines estimate the model via (Quasi) Maximum Likelihood and allow us to apply the bias correction that has been proposed by

[30] One might argue that the assumption of normal distributed error terms for Maximum Likelihood is strong and that estimates in practice may therefore be inconsistent. However, Lee (2004) shows that without the assumption of the normal distribution of the residuals, the resulting Quasi-Maximum Likelihood estimator is asymptotically consistent. These results apply here because our sample is sufficiently large.

[31] The test statistics can be reviewed in Table 2.7 in Appendix 2.8. We also tested the Spatial Durbin Model against the Spatial Error Model (SEM) in which the spatial dependency is modeled through the residuals. Our test results speak in favor of the SDM here, too. Also note that the SDM is a generalization of the SEM and it therefore produces correct standard errors of the coefficient estimates even when the true model has spatial autocorrelation (see Elhorst, 2010a, p. 14).

[32] http://www.regroningen.nl/elhorst/software.shtml.

Lee and Yu (2010a).[33]

To find the spatial weighting matrix W that fits the data best, we subsequently estimate the baseline SDM in (2.13) using various matrices suggested by the literature: binary contiguity matrices of first and second order, row normalized matrices where all municipalities within a certain radius around the municipality centroid are assigned as neighbors, and Inverse Distance Matrices with different cut-off radii with row and eigenvalue normalization. Following Elhorst (2010a), we compare the log likelihood values of the models using the different weighting matrices. For both federal states, the models employing Inverse Distance Matrices with row normalization perform best among the aforementioned matrices. In NRW, the Inverse Distance Matrix with a cut-off after 15km results in the highest log likelihood value, and for Bavaria, the best cut-off radius is 20km (see Table 2.9 in Appendix A).[34] Therefore, in our main specifications we will use these weighting matrices to incorporate geographical interaction. Row normalizing an Inverse Distance Matrix implies that the distance loses its cardinal interpretation. While the exact distance is relevant in the context of transportation costs, for example, in our context of fiscal interaction between municipalities it is the relative distance to neighboring municipalities in comparison to other neighbors that is decisive, a feature well captured by row normalization.[35] In the robustness section, we will further highlight that our estimates are robust with respect to alternative weighting matrices. We will also present results for non-standard geographical weighting matrices.

As the dependent variable, we use total municipality public debt, i.e. the sum of the core budget and short term debt. To identify spatial interaction in debt, we have to separate the interaction effect from potential common regional economic shocks. Therefore, it is important to control for economic and demographic developments which may have occurred in neighboring municipalities at the same

[33] Lee and Yu (2010a) show that due to the incidental parameter problem, the estimation of a model that includes both a spatial lag and spatial residuals may be inconsistent. They derive a bias correction that allows for consistent estimation. Elhorst (2012) adopts their approach and translates their bias correction to the SAR, SER and SDM models. Due to our relatively large sample size of 396 and 2,056 observations in the cross-section, we expect the bias correction to mainly affect the standard errors but not the parameter estimates (see equation (8) in Elhorst, 2012).

[34] We could not use a cut-off after 10km, since some large municipalities in NRW would have no neighbors in this case. As a cut-off after 14km leads to a smaller log likelihood, the cut-off after 15km seems to be at least a local maximum.

[35] Consider an example where municipality A has two neighbors at distances of 2km and 4km and municipality B has three neighbors at distances of 2, 4 and 6 km. By normalizing the row sums to one, the neighbor at 2km distance has a stronger influence on municipality A than on B, which is what one would expect given the influence of the third neighbor on B.

time. The choice of independent variables in our benchmark specification follows the tax and spending interaction literature. We include the population size and its square plus the population structure, i.e. the shares of persons below 15 years and above 65 years of age, as indicators of the work force available in the municipality and the dependency rate. In addition, the number of unemployed persons per 100 inhabitants is included, capturing the impact of economic shocks on municipalities. In the SDM all these explanatory variables enter in levels as well as spatial lags.

In an extended model, we employ additional control variables. First, we include per capita expenditures on personnel and current operating expenditures. Municipalities have limited control over these expenditure categories, at least in the short run. Including these controls in the regression helps to identify the amount of spatial debt interaction due to deliberate decisions of local governments. Similar considerations lead to the inclusion of spending on social needs in the model. This control might be important because a reform of unemployment insurance and social assistance in Germany in 2004 (*Hartz IV reform*) shifted the costs of social assistance to the local level, which affected municipalities differently depending on the number of inhabitants eligible for benefits. We consider social spending exogenous at the municipal level because municipalities have to follow regulations set at the federal level and have very little discretion over these kinds of expenditures. Moreover, we add measures of the municipality's revenues as controls, i.e. the first time lag of the tax bases (*Grundbetrag*)[36] both from the local business tax and the two local property taxes. We also control for county GDP growth because the ability to issue debt might be influenced by regional business cycle effects (on top of country-wide year effects that we capture with time fixed effects). Finally, we include per capita net migration because this exerts a mechanical effect on our per capita debt measures.

When interpreting spatial econometric results as causal effects, some qualifications are necessary. Gibbons and Overman (2012) argue that spatial econometric methods are a useful tool, but often lack a credible identification strategy in applied research. With regard to the traditional spatial econometric IV methods, they question whether the instruments fulfill the exogeneity assumption. We agree that exogeneity cannot be taken for granted. When we run a static Spatial Lag IV regression[37], the over-identification J-Test for Bavaria is not passed, and the

[36]The *Grundbetrag* is calculated by dividing the tax revenue by the tax rate. It is used to determine transfers in the inter-municipal fiscal equalization scheme.

[37]Results are available from the authors upon request.

IV estimator is not our preferred method in this paper.
With regard to the spatial ML methods, the critique focuses on the assumption that the specification of W reflects exactly the real world ties between the units. Given that W is perfectly known, it is possible to correct for the endogeneity of λ via the Jacobian term in an ML setting. Of course, this assumption is very strong and we do not pretend to know the specification of the real W. To cope with this problem, we offer a wide range of reasonable specifications of W for our setting and compare the results. We find that the coefficient for the interaction stays significant within a certain range in all specifications. In addition, we allow for heterogeneity over municipalities and years (spatial and time FE) and control for economic shocks via independent variables. Thus, we offer a well-controlled spatial interaction that takes feedback effects between municipalities into account.

2.6 Results

In this section, we first present our main results for the spatial interaction in public debt. Then, we compare the findings to estimation results for taxation and expenditure interaction in the same data. In the third subsection, we consider the spatial interaction in different types of debt. In the final three subsections, we provide a number of robustness checks, including the use of various standard and non-standard weighting matrices, as well as dynamic spatial lag models.

2.6.1 Main results

Table 2.1 shows the main results. Column (1) presents the basic model as outlined in equation (2.13) for NRW and column (3) the same model for Bavaria. Throughout this section, we will refer to these results as our benchmark. We observe significant interaction effects λ of 0.163 for NRW and 0.327 for Bavaria. That is, if the neighbors increase their debt by 100 Euro per capita, a municipality will increase its own debt by 16.3 Euro per capita in NRW or 32.7 Euro in Bavaria. The estimated model in both federal states explains about 90 percent of the variation ($R^2 \approx 0.9$), which is largely due to the municipality and time fixed effects, as indicated by the large difference between R^2 and $corr\left(Y,\hat{Y}\right)^2$. Another important result is that most of the spatially lagged independent variables

are significant as well. This justifies the use of the Spatial Durbin Model.[38]
Our estimates for λ are robust to the inclusion of additional control variables. We include operating and personnel expenditures, welfare spending, the local tax bases, as well as county-level GDP growth and net migration flow (see columns (2) and (4) of Table 2.1 for NRW and Bavaria, respectively). LR-tests (reported in the bottom of Table 2.1) indicate that these additional controls and their spatial lags are jointly significant.
As outlined in section 2.4.1, the results for NRW and Bavaria allow for an interesting comparison. Despite the relatively large differences in municipal size, the economic structure and particularly the debt level, the results for the estimated interaction effects are positive and highly significant in both federal states. The fact that the independent results point in a similar direction for both states highlights that the evidence is not confined to only one particular sample. However, the fact that the effects are smaller in NRW may highlight that the degree of interaction is potentially limited when municipalities face constraints. In particular, the high debt level in many municipalities in NRW may restrict them from incurring additional debt in response to their neighbors' policies.

2.6.2 Comparison between spending, taxes and debt

This section compares the spatial interaction effects of debt, spending, and the three different tax rate multipliers. In order to keep the results comparable, we use the same independent variables from the benchmark model and only change the dependent variable. This comparison is performed in Table 2.2. Column (1) repeats the results from the benchmark specification with per capita debt as the dependent variable. Table 2.2 now allows for the comparison of the spatial debt interaction effect with the interaction effects of net spending amounts per capita (column (2)), the local business tax rate multiplier (column (3)) and the two property tax rate multipliers (columns (4) and (5)).
The results show a relatively large interaction effect among the local business tax rates of 0.290 (0.472) for NRW (Bavaria). Thus, a municipality in NRW will increase its multiplier by 0.290 basis points if its neighboring municipalities

[38] In addition, we show that the application of the SDM largely removes the spatial autocorrelation among the residuals. To test this, we extract the SDM residuals and calculate Moran's I statistic for each of the eight cross sections in the two federal states. The spatial autocorrelation among the residuals of the SDM is found to be close to zero: The Moran's I statistics range from -0.07 to 0.04 in NRW and from -0.018 to 0.008 in Bavaria, and they are statistically different from zero at the 10% level in only four out of the 16 estimations. Full results are available from the authors on request.

Table 2.1: Spatial interactions of municipality debts 1999 - 2006

	North Rhine Westphalia		Bavaria	
	(1) Basic Model	(2) Full Model	(3) Basic Model	(4) Full Model
λ	0.163***	0.157***	0.327***	0.329***
	(0.028)	(0.028)	(0.022)	(0.022)
Population_t	-0.080***	-0.079***	0.028**	0.037***
	(0.009)	(0.009)	(0.012)	(0.014)
$\text{Population}_t^2/100,000$	0.030***	0.032***	-0.007	-0.009*
	(0.007)	(0.007)	(0.005)	(0.005)
Share young people_t	46.653***	41.544**	9.011***	10.261***
	(16.567)	(16.821)	(3.357)	(3.331)
Share old people_t	-24.860**	-26.501**	25.114***	23.191***
	(10.587)	(10.743)	(2.662)	(2.657)
Unemployed per inh._t	29.085*	30.752*	7.348	6.261
	(15.712)	(15.696)	(6.229)	(6.207)
PC personnel expend._t		0.603***		0.416***
		(0.202)		(0.053)
PC operating expend._t		-0.052		0.546***
		(0.099)		(0.046)
PC welfare spending_t		0.249**		-0.016
		(0.113)		(0.054)
Local business tax base_{t-1}		-0.006***		-0.010***
		(0.002)		(0.002)
Property tax base A_{t-1}		-0.164		0.370
		(-0.516)		(1.482)
Property tax base B_{t-1}		-0.003		-0.030
		(0.022)		(0.073)
County GDP growth_t		-5.774*		0.393
		(2.969)		(1.077)
Net mig. flow per 1,000 inh._t		1.021		-0.222
		(0.772)		(0.177)
W × Population_t	-0.038**	-0.024	-0.340***	-0.326***
	(0.017)	(0.018)	(0.038)	(0.047)
W × Population_t^2	-0.037*	-0.046**	0.130***	0.118***
	(0.021)	(0.021)	(0.020)	(0.023)
W × Share young_t	55.309*	36.245	7.140	6.916
	(30.205)	(31.725)	(11.902)	(12.017)
W × Share old_t	60.016***	52.0648***	1.818	3.788
	(17.229)	(17.659)	(6.281)	(6.387)
W × Unemployed per inh._t	15.795	20.941	25.138*	29.923**
	(24.862)	(25.027)	(13.779)	(13.791)
W × PC personnel expend._t		0.065		-0.340*
		(0.484)		(0.229)
W × PC operating expend._t		0.404		-0.133
		(0.251)		(0.204)
W × PC welfare spending_t		0.150		-0.378
		(0.244)		(0.238)
W × Business tax base_{t-1}		0.0004		0.007
		(0.005)		(0.012)
W × Property tax base A_{t-1}		-0.248		-3.756
		(0.520)		(5.333)
W × Property tax base B_{t-1}		0.027		0.100
		(0.034)		(0.416)
W × County GDP growth_t		4.959		-0.281
		(3.982)		(1.557)
W × Net mig. flow per 1,000 inh._t		1.686		1.827**
		(1.853)		(0.791)
No. of municipalites	396	396	2,056	2,056
No. of observations	3,168	3,168	16,448	16,448
R^2	0.9227	0.9236	0.8915	0.8935
$\text{Corr}^2\left(Y,\hat{Y}\right)$	0.1501	0.1606	0.0279	0.0455
Log-likelihood	-21,330.159	-21,311.004	-111,030.840	-110,878.870
LR-Test Full vs. Basic		38.31***		303.91***

Notes: The dependent variable is the sum of debt from the core budget and short term debt of a municipality, λ denotes the spatial interaction effect. All models are estimated with maximum likelihood and are bias corrected. Year and municipality fixed effects are taken into account by double demeaning. W is a row normalized Inverse Distance Matrix with a cut-off after 15(20)km for NRW (Bavaria). Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

increase theirs by one basis point. Similarly for the non-agricultural property tax B, we find an effect of 0.389 (0.343) for NRW (Bavaria). In comparison, the spatial interaction between net spending is rather low with an effect of 0.098 (0.156) for NRW (Bavaria): A municipality in NRW will increase its expenditure by 9.8 Euro if its neighbors increase spending by 100 Euro. All these coefficients are significant at the one percent level. Note that, the spatial interaction effects do not depend on the choice of units, so the effects for tax rates can be directly compared to the effects for spending and debt amounts. Accordingly, a standardization of the dependent variables (to mean 0 and standard deviation of 1) does not change the results.

The comparison of spatial interaction in the three fiscal choice variables is interesting for at least two reasons. First, we can show spatial interdependence between German municipalities not only in debt, but also in taxes and spending. To the best of our knowledge, our contribution is the first to highlight the interaction in different fiscal variables using a unified estimation technique, the same set of municipalities and the same control variables. Second, the estimated interaction effect for debt lies in between the effect for spending and for tax rates in both states. While the interaction in taxation is for the most part larger in magnitude than debt, the spending interaction is somewhat smaller. Given those magnitudes, our findings indicate that the interaction in debt should not be ignored when considering the policy interactions between jurisdictions.

Table 2.2: Comparison of debt, spending and tax interaction

Basic model with X_{it} and WX_{it} from 1999 - 2006					
Interaction	(1) PC debt	(2) PC net spend	(3) Business tax	(4) Prop tax A	(5) Prop tax B
λ NRW	0.163***	0.098***	0.290***	0.127***	0.389***
Standard error	(0.028)	(0.029)	(0.026)	(0.029)	(0.024)
λ Bavaria	0.327***	0.156***	0.472***	0.360***	0.343***
Standard error	(0.022)	(0.025)	(0.019)	(0.021)	(0.021)

Notes: All models are estimated using the maximum likelihood method and are bias corrected. Independent variables in all specifications are: population, population^2, share of young people, share of old people and unemployed per 100 inhabitants. The averaged neighboring equivalents are included as well as independent variables. Year and municipality fixed effects are taken into account by double demeaning. W is a row normalized Inverse Distance Matrix with a cut-off point after 15 (20) km for NRW (Bavaria). Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

2.6.3 Spatial interactions in different types of debt

So far, we show that spatial correlation via debt indeed exists among German municipalities. In this section, we will analyze different types of debt to explore in more detail where the interaction takes place. First, we will separate the municipality debt used in the main analysis into the core budget and short term debt and re-estimate our benchmark model using these debt components. Particularly in NRW, it is likely that municipalities interact with their debt mainly through short term debt, as those debts are less strictly regulated and remain available even when the core budget is in distress. Then, we will repeat our analysis for debt issued by local public companies and the sum of all three debt categories.

Descriptive results from dividing the per capita debt into the regular core budget and short term debt are depicted in Figure 2.7 in Appendix B. The graphs demonstrate that municipalities in NRW have increasingly gone into short term debt in our observation period, whereas Bavarian municipalities use this form of debt far less extensively (note the different scales). This is also confirmed when looking at the share of municipalities using short term debt as shown in Figure 2.8 in Appendix B. The share of municipalities using short term debt in Bavaria is rather stable around ten percent. In contrast, the share in NRW increases monotonically from about twenty percent in 1998 to 50 percent in 2006. The reason is that the core debt levels in NRW were already very high and increasingly came under Federal State supervision during this period, whereas short term debt was still available (Gröpl et al. (2010)).

Debt of local public companies is substantial in NRW (see Table 2.6), but not so in Bavaria. This category is interesting for a comparison, because local governments only have limited control over this type of debt. Since local public companies underlie regulation, often have private companies as additional stakeholders, and sometimes are joint ventures of a number of municipalities, a single municipal government is often not in a position to exercise full control over their debt policy.

The results for the subcategories of debt are shown in Table 2.3. The results using the core budget, the short term debt and the debt of public companies as dependent variables are presented in columns (1), (2), and (3). Estimations using the sum of all three types of debt are reported in column (4). In NRW we observe that the interaction among the core budget debt is not significant, while the interaction among short term debt is larger than when the sum is considered. This indicates that municipalities in NRW interact via their short term debt. This can be explained with the institutional setting and debt situation in NRW. As debt levels are generally high, and the core budget (here in particular investment spending) is under state regulation, the limits in this type of debt seem to be exhausted. Instead, municipalities seem to turn to short term debt, in which no institutional boundaries exist (in our observation period) to limit further debt issuing. The interaction close to zero for public company debt (although statistically significant at the 10 % level) shows that municipalities have little scope for using this kind of debt to finance interaction.[39]

Looking at Bavaria, the interaction effect does not change much when comparing full per capita debt to debt in the core budget. The interaction among short term debt is smaller; as mentioned before, only about 10 % of the observations in Bavaria exhibit any positive short term debt. As in NRW, we detect a small negative spatial interaction between public company debt, which here is insignificant. Generally, we conclude that spatial interaction in Bavaria is mainly through core budget debt, which is reasonable since debt levels in Bavarian municipalities are low, and one would not expect constraints in adapting the core debt.

Table 2.3: Separate debt classes 1999 - 2006

Basic model with X_{it} and WX_{it} from 1999 - 2006 and different kinds of debt				
Interaction	(1) PC core budget	(2) PC short term	(3) PC public company	(4) $\sum$ (1) - (3)
λ NRW	0.021	0.346***	-0.058*	0.148***
Standard error	(0.030)	(0.025)	(0.031)	(0.028)
λ Bavaria	0.329***	0.181***	-0.032	0.333***
Standard error	(0.022)	(0.024)	(0.028)	(0.021)

Notes: All models are estimated using the maximum likelihood method and are bias corrected. The same control variables as in Table 2.2 are included. Year and municipality fixed effects are taken into account by double demeaning. W is a row normalized Inverse Distance Matrix with a cut-off after 15(20)km for NRW (Bavaria). Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

In both states, the estimated magnitude of debt interaction remains similar to the benchmark estimations when we consider the sum of all three types of debt. Thus, the estimates are robust to the inclusion of public company debt. We prefer the benchmark model without public company debt because these are typically not under full control of municipal governments, as mentioned before.

As noted, the municipalities in NRW do not have as much financial leeway as their Bavarian counterparts. For a shortened panel from 2002 until 2006, we obtained

[39] The fact that we find zero effects in the core budget and close to zero for the debt in public companies highlights that our main finding of positive debt spillovers in the benchmark model reflects economically meaningful interaction and not some pure mechanical effect that would show up in any dependent variables.

data on whether an individual municipality in NRW was required to present a budget consolidation plan to the regulating state authorities because of the inability to balance the budget (see section 2.4.1). 28% of the observations in NRW fall under this financial supervision status at least once in this period. To investigate spatial interactions, we interpret the presence of a budget consolidation plan as an indicator of having reached a ceiling for debt and estimate a linear model using this indicator as a binary dependent variable. We include the explanatory variables from our benchmark model, as well as the per capita debt in the core budget and in the short term.

Corresponding to our results above, we find spatial correlation ($\lambda = 0.063$, $SE = 0.037$) in the probability of reaching the debt ceiling (in this sense) which is significant at the ten percent level. This coefficient implies that the likelihood of running into a situation that requires a municipality to present a budget consolidation plan to the state authorities increases by 6 percentage points if its neighbors are in this situation. The fairly large effect could indicate that municipalities that interact via debt might reach a debt ceiling over time. The spatially clustered financial supervision highlights that we might indeed be observing a race to the debt trap in NRW.

2.6.4 Robustness and heterogeneity

In this section, we first conduct several robustness checks for the main specification with public debts, and then explore heterogeneity by municipality types to learn more about possible mechanisms. Table 2.4 provides the results of the robustness checks. Row (1) presents the benchmark results for comparison. In line (2) we extend the panel by additionally including 1998.[40] In rows (3) and (4), we extend the period of analysis to earlier years. Here, we can only analyze the core debt without short-term debt, because information on the latter is not available in the earlier years. As independent variables, we use all variables from the benchmark model, except the unemployed per capita. For the longest time period, we can only consider Bavaria, as we lack data for NRW. In row (5), we use the first time lags instead of contemporaneous independent variables, and in row (6), we add a trend instead of year fixed effects. These variations do not change the estimated spatial interaction effect of debt significantly, except for the larger estimate for Bavaria for the longest time period. The estimated coefficients in row (7) do

[40] We do not include 1998 in our benchmark specification because we use lagged independent variables in various robustness checks of the model.

not change much either when we omit the bias correction described in section 2.5. However, it is worth noting that without the bias correction, we would overestimate the significance of the interaction effect in Bavaria (corresponding t-value of 19.4 instead of 14.9).

Table 2.4: Spatial interactions of municipality debts: Robustness checks

	North Rhine Westphalia			Bavaria		
Specification	λ	Standard error	N	λ	Standard error	N
(1) 1999 - 2006	0.163***	(0.028)	3,168	0.327***	(0.022)	16,448
(2) 1998 - 2006	0.159***	(0.027)	3,564	0.339***	(0.020)	18,504
(3) 1995 - 2006	0.087***	(0.024)	4,752	0.327***	(0.018)	24,672
(4) 1987 - 2006	-	-	-	0.456***	(0.012)	41,120
(5) X_{t-1} instead of X_t	0.163***	(0.028)	3,168	0.330***	(0.022)	16,448
(6) Trend instead of time FE	0.200***	(0.028)	3,168	0.347***	(0.017)	16,448
(7) No bias correction	0.156***	(0.028)	3,168	0.330***	(0.017)	16,448
(8) No outer boundary	0.102***	(0.034)	2,344	0.368***	(0.022)	14,208
(9) No fixed effects	0.277***	(0.027)	3,168	0.664***	(0.025)	16,448
(10) Change in Y	0.128***	(0.029)	3,168	0.125***	(0.026)	16,448
(11) Including neighboring tax rates	0.158***	(0.028)	3,168	0.325***	(0.022)	16,448
(12) Including vertical interaction	0.158***	(0.028)	3,168	0.326***	(0.022)	16,448
(13) Placebo weight	0.013	(0.041)	3,168	0.010	(0.045)	16,448

Notes: The dependent variable is the sum of debt from the core budget and short term debt of a municipality. All models are estimated using the maximum likelihood method. Independent variables in all specifications are: population, population^2, share of young people, share of old people and unemployed per 100 inhabitants. The averaged neighboring equivalents are included as well as independent variables. W is a row normalized Inverse Distance Matrix with a cut-off after 15(20)km for NRW (Bavaria). (1) estimates the basic model from 1999 - 2006. (2),(3) and (4) estimate the basic model with the given time ranges, where (3) and (4) only use the debt in the core budget. (5) uses the first lag instead of the contemporaneous independent variables. (6) replaces the time fixed effects by a linear time trend. (7) applies the basic specification, however no bias correction is conducted. (8) uses the specification from (1), but frontier municipalities are excluded. (9) drops both municipality and time fixed effects. (10) replaces the dependent variable and its spatial lag with their first differences. (11) includes the first time lag of the averaged neighboring rates of the business tax and property taxes A and B as independent variables. (12) includes the per capita debt of the county administration as well as the sum of the county population. (13) estimates the basic model with a Placebo row normalized 15 (20) km radii matrix for NRW (Bavaria) with randomly assigned neighbors. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

In addition, one might wonder whether the interaction along the boundaries of the federal states to other federal states in Germany or to other countries might differ. There may be little or no interaction across the borders to other countries; Cassette et al. (2012) show that no tax competition exists between French and German municipalities along the Rhine Valley. On the other hand, Geys and Osterloh (2013) find that municipalities near a subnational or international border perceive stronger competitive pressure from across the border in the struggle to attract firms. In our framework, structural differences between municipalities at a border and other municipalities should be captured by the municipality fixed effects. To investigate whether border municipalities influence our results, in specification (8), we remove municipalities that share a border with another federal state or another country, and find similar spatial interaction effects as in the benchmark model.[41]

In the estimations reported so far, we account for municipality and time fixed effects. As these unobserved effects are likely to be correlated with the dependent

[41] The number of cross section units decreased from 396 to 293 in NRW and from 2,056 to 1,776 in Bavaria.

and independent variables, an estimation not accounting for these fixed effects is likely to be biased. In row (9), we nevertheless report such estimates for comparison. We observe an increase in the point estimates of the interaction coefficient both for Bavaria and NRW, which indicates that these estimates of the spatial interaction effect are biased upwards. The analysis of spatial interactions based on cross-sectional data, where time and unit fixed effects cannot be controlled for, must thus be regarded with caution.[42]

While we allow for more variation in the pooled estimation in row (9), we drastically limit the variation in the robustness test presented in row (10). Here, we use the change in total debt as an outcome variable instead of the level. As we still include municipality fixed effects, we identify the model with deviations from the municipality-specific trend. The results are reassuring as for both NRW and Bavaria the estimates are still positive and significant (even though the point estimate drops significantly in the case of Bavaria).

In the simple model we develop in section 2.3, tax rates of neighboring municipalities do not enter the reaction function of debt. However, in a more general model, neighboring tax rates may influence the choice of the debt level, as detailed in Appendix 2.10. Therefore, in the robustness check in row (11), we add the first time lag of the averaged neighboring rates of the local business tax and the property taxes A and B as independent variables. We use the lagged values to mitigate potential endogeneity of the neighboring tax rates. The point estimates of the spatial debt interaction remain very similar to the baseline estimates.

Vertical interaction between different tiers of government may also be important, as reported by Revelli (2003). To check if this influences our estimates of horizontal interaction, in row (12) we follow Revelli (2003) and add variables on the level of counties, which is the tier above the municipal level. Specifically, we add the per capita debt of the county administration as well as the sum of the county population. Again, the estimated horizontal spatial interaction almost does not change.

As a placebo test, row (13) shows the results of using a random spatial weighting matrix instead of the matrix capturing the actual spatial distribution of the municipalities. This idea is similar to tests conducted by Case et al. (1993) and Geys (2006), who construct a placebo weighting matrix based on an alphabetical order.

[42] The spatial interaction effects in this model without fixed effects are identified not only by changes in debt over time, but also by debt level differences between municipalities. The upward bias in this estimation may indicate that municipalities with unobserved time-invariant preferences for debt are spatially clustered.

For our experiment, we generate a row normalized radii weighting matrix with a cut-off after 15 (20) km for NRW (Bavaria) as in the benchmark model, but with randomly assigned neighbors. To generate the number of random neighbors for each municipality, we take the mean and standard deviation of the number of neighboring municipalities from the real geographical distribution and draw from a normal distribution with corresponding first and second moments. Reassuringly, the results using the placebo random weighting matrix do not show any significant interaction effects.

After this series of robustness checks, we explore heterogeneity in the extent of spatial interaction by type of municipality. Methodologically, we follow Elhorst and Freret (2009) and interact the spatial lag of the dependent variable with a dummy variable indicating one of two regimes that a municipality may belong to. The results appear in Table 2.8 in Appendix 2.8. First, we classify the municipalities into relatively rich and poor regimes based on whether the sum of the tax bases (see footnote 36) of the local business tax and the two local property taxes is above or below the median in their federal state. We obtain a larger point estimate for the spatial interaction for relatively poor municipalities, but the difference between the two regimes is not statistically significant (see columns 3 and 4).

Second, we intend to assess whether yardstick competition may be responsible for spatial debt interaction, as a possible mechanism that may work alternatively to or in addition to the competition for mobile tax bases that we describe in our theoretical model. In this case, one would expect municipalities where the largest political party has a large majority in parliament to be less inclined to participate in yardstick competition, because here the majority party's re-election is comparably secure. If the largest party has a majority just above 50% or below, re-election is more uncertain, and the municipality should engage more in yardstick competition. We classify municipalities where the largest party has more than 55% of the votes into the large majority regime and other municipalities into the small majority regime. This groups less than a third of the municipalities in NRW and about half of the municipalities in Bavaria into the large majority regime.

In both federal states, we obtain larger point estimates for the spatial interaction of the municipalities with a small majority, which may indicate that yardstick competition plays a role indeed (columns 1 and 2). However, the difference is significant in Bavaria only. Future research should investigate more deeply the extent to which the different possible mechanisms contribute to the overall spatial debt interaction that we document. One way to do this could be the consideration of other political variables such as party ideology.

2.6.5 Different spatial weighting matrices

We also assess the robustness of our results with respect to the definition of neighboring municipalities by estimating the benchmark model with different specifications of the spatial weighting matrix. In section 2.5, we note that we tested the standard matrices used in the literature to choose the matrix that best fits the data for our benchmark model, as indicated by the highest log likelihood value. We considered binary contiguity matrices, matrices assigning all municipalities within a certain radius as neighbors, and Inverse Distance Matrices with row or eigenvalue normalization. We identified Inverse Distance Matrices with row normalization and a cut-off radius of 15 and 20 km as the best weighting matrices for NRW and Bavaria, respectively. Table 2.9 in Appendix A not only reports the log likelihood values, but also the estimated coefficients of spatial debt interaction when the various weighting matrices are used. They are always positive and highly significant. The point estimate tends to become larger when Inverse Distance Matrices with eigenvalue normalization or larger cut-off radii are used, so the lower estimates of our preferred benchmark model estimates are conservative. In addition, we construct non-standard geographical weighting matrices, which reflect the particular institutional setting of municipalities in Germany, and use them to re-estimate our benchmark model. The first matrix, which we refer to as county matrix, codes all municipalities within a particular county as neighbors. 2,031 of the 2,056 municipalities in Bavaria belong to 71 counties, the remaining 25 municipalities are independent cities; in NRW, 373 of the 396 municipalities belong to 30 counties, and 23 are independent cities (here we include Aachen, which is partly independent). We assign all independent cities (which are independent from counties) within each federal state as neighbors to each other, as their similarity makes them likely to interact more with one another.[43] The idea behind this weighting matrix is that mayors from municipalities within a county have regular institutional exchange in party meetings or county events. It is likely that these meetings intensify information spillovers and may directly or indirectly affect the

[43]For reasons of comparability with the other weighting matrices, we include independent cities as well. However, due to the political nature of this matrix, we believe that most interaction stems from the adjustment of municipalities within one county.

decisions of politicians to incur debt.

As the second non-standard spatial weights, we constructed weighting matrices inspired by the theoretical work of Janeba and Osterloh (2013). They argue that urban centers compete with other centers as well as with their surrounding municipalities while smaller municipalities only compete locally. We implement this approach by assigning all large municipalities with a population above a certain threshold (30k, 50k) in 1998 as neighbors. In addition, they are neighbors as well with their surrounding municipalities within a radius of 20km from their centroid. All other municipalities exclusively regard municipalities within a radius of 20km as their neighbors. Finally, we also tested a specification, where we assign as potential competitors only towns of similar size. For this, we grouped the municipalities into three classes for their degree of urbanization (urban, partly urban, rural) and redefined the weighing matrices to focus on those neighbors only.[44]

The results for these novel matrices also indicate positive and significant spatial debt interaction in both states. The point estimates obtained from using the matrices suggested by Janeba and Osterloh (2013) are similar to the benchmark estimates using the Inverse Distance Matrices with row normalization and 15 (20) km cut-off radii (the standard errors widely overlap). The county matrix generates a larger point estimate for NRW and a smaller point estimate for Bavaria. Interestingly, the log likelihood value becomes even larger when the innovative county matrix is used in NRW than when the best traditional matrix is used, so the county matrix seems to reflect the spatial debt interaction in NRW particularly well.[45] The specification using the degree of urbanization also leads to comparable estimates, however, here the log likelihood is not as large.

2.6.6 Dynamic spatial panel models

One might argue that per capita debt implies a strong path dependency. To assess the relevance of dynamics for our estimated spatial debt interaction of interest, we also apply a dynamic panel Quasi Maximum Likelihood (QML) estimator. Specifically, we use the SDM from (2.13) and include the first time lag as well as the first time lag of the spatial lag of the dependent variable. Yu et al. (2008)

[44]In practice, we assign a neighbor of the same urbanizational degree within 30km. When no such neighbor exists, we increase the radius to 45km.

[45]We nevertheless decide to use the Inverse Distance Matrix in our benchmark model to facilitate comparisons with the literature. It also leads to the smaller, more conservative point estimate.

derive this dynamic estimator with spatial fixed effects. Lee and Yu (2010b) extend this routine to additionally include time fixed effects. The resulting model can be written as follows:

$$y_{it} = \tau y_{it-1} + \lambda \sum_{j=1}^{N} w_{ij} y_{jt} + \eta \sum_{j=1}^{N} w_{ij} y_{jt-1} + x_{it}\beta + \sum_{j=i}^{N} w_{ij} x_{jt}\theta + \mu_i + \zeta_t + \epsilon_{it} \tag{2.14}$$

In (2.14), τ denotes the coefficient of the first time lag and η the coefficient of the first time lag of the spatial autoregressive coefficient. This specification does not only allow us to control for possible path dependency of debt, furthermore it allows to determine whether the spatial process occurs simultaneously or with a time lag.[46]

To account for the occurring bias due to the dynamic part of the model, Lee and Yu (2010b) use asymptotic theory to derive a bias correction, which is incorporated in their estimator.[47]

The model is stable if $\tau + \lambda + \eta < 1$. If the model is unstable, Lee and Yu (2010c) propose an additional method labeled Spatial First Differences (SFD), where each variable is taken in deviation of its spatially lagged value, which eliminates the time fixed effects. Due to the SFD transformation, we lose one observation per cross section. We successfully check the correct transformation by the test suggested in Elhorst et al. (2013). The condition for stability is $\tau + \omega_{max-1}(\lambda + \eta) < 1$ now, where ω_{max-1} denotes the second largest eigenvalue of W which is unequal to one.

We estimate (2.14) using the estimator suggested in Lee and Yu (2010b) and the SFD model using the estimator proposed in Yu et al. (2008). Due to the fact that T is rather small in our panels, we additionally consider a dynamic model in a System GMM framework as proposed by Arellano and Bover (1995) and Blundell and Bond (1998) and implemented in Stata by Roodman (2009). This estimator also allows us to include the spatial lag and the time lag and can be used for a comparison with the aforementioned QML estimators. Here, a system of equations in levels and first differences is estimated, where differences serve as instruments for

[46]The estimation routines for the dynamic QML estimator including the dynamic bias correction can be found at http://www.regroningen.nl/elhorst/software.shtml.

[47]For a detailed description of the bias correction of the dynamic part, see equation (17) in Yu et al. (2008) for only spatial FE and equation (17) in Lee and Yu (2010b) for spatial and time FE.

the levels and levels serve as instruments for the differences for the endogenous time lag. Kelejian and Robinson (1993) recommend using spatially lagged control variables as instruments for $\sum_{j=1}^{n} w_{ij} y_{jt}$. For the endogenous spatial lag, we use the spatially lagged values of the unemployed per capita[48]. The neighboring demographic variables are excluded due to potential endogeneity concerns. In Bavaria, we use lags for the other X variables as well since the null-hypothesis of the Hansen J test is rejected otherwise.

The results from the dynamic specifications appear in Table 2.5. Columns (1) and (4) apply equation (2.14) and column (2) and (5) the SFD model for NRW and Bavaria, respectively. We obtain estimates for the coefficients of the time lag of per capita debt τ that are significantly larger than zero in both states; the point estimate is even larger than one in NRW, indicating exploding debt in the observation period. Importantly, the spatial interaction effect λ remains positive and highly significant in the dynamic estimations. Moreover, we find that the spatial interaction effect occurs simultaneously, because the first lag of the spatial interaction effect is not significantly different from zero. This indicates that a time lag of spatial interaction can safely be removed from the model. The results indicate instability of the debt process, especially in NRW, even when estimating the SFD model.

In columns (3) and (6), we employ the dynamic SAR model within a system GMM framework. We report the p-values for the Arellano Bond test statistic for AR(2) correlation and the Hansen over-identification test statistic. Both tests are passed, which indicates that the estimated GMM is feasible. Again we observe a positive and significant influence of the first time lag of debt and positive and significant spatial debt interaction, as in the QML estimations, which highlights that the results are not very sensitive to the choice of estimator.

Since the dynamic estimates may suffer from the relatively small number of time periods in our samples, we abstain from overinterpreting the dynamic effects and take from the estimation of the dynamic models that the spatial interaction effects of debt we are interested in are robust and seem to occur simultaneously, which increases confidence in our benchmark model.

[48] In this setting we do not include the term $\eta \sum_{j=1}^{n} w_{ij} y_{jt-1}$, because separate identification of λ and η by GMM is weak due to the similarity of these terms and their instruments. The omission should not bias the estimation because the QML results indicate that η is insignificant.

Table 2.5: Dynamic specifications

	North Rhine Westphalia			Bavaria		
	(1) BC-QML	(2) Spat-FD	(3) GMM	(4) BC-QML	(5) Spat-FD	(6) GMM
τ	1.138***	1.138***	1.071***	0.907***	0.907***	0.946***
	(0.013)	(0.013)	(0.016)	(0.006)	(0.006)	(0.018)
λ	0.157***	0.155***	0.083***	0.166***	0.157***	0.102***
	(0.028)	(0.028)	(0.030)	(0.025)	(0.031)	(0.027)
η	0.052	0.049	-	-0.029	-0.026	-
	(0.047)	(0.047)		(0.034)	(0.039)	
$\tau + \lambda + \eta$	1.348***	1.342***	-	1.044***	1.038***	-
Standard error $\tau + \lambda + \eta$	(0.033)	(0.033)	-	(0.023)	(0.023)	-
$\tau + \omega_{max-1} (\lambda + \eta)$	-	1.341	-	-	1.038	-
Log-likelihood	-19,038	-18,792	-	-99,983	-99,692	-
Hansen J test	-	-	0.137	-	-	0.321
Arellano Bond AR(2) test	-	-	0.559	-	-	0.302
Number of lags for Y_{t-1}/X			3-8 / -			3-8 / 2-8
Number of Instruments total			36			205
N	3,168	3,160	3,168	16,448	16,440	16,448

Notes: Independent variables in all specifications are: population, population^2, share of young people, share of old people and unemployed per 100 inhabitants. The averaged neighboring equivalents are included as well as independent variables. W is a row normalized Inverse Distance matrix with a cut-off point after 20 km for Bavaria and a row normalized 25 km radii Matrix for NRW. (1) and (4) estimate the BC-QML with spatial and time FE from Lee and Yu (2010b). (2) and (5) estimate the BC-QML using Spatial First Differences as proposed in Lee and Yu (2010c) and apply the estimator with spatial FE by Yu et al. (2008). (3) and (6) estimate a system GMM as outlined in Arellano and Bover (1995); Blundell and Bond (1998) with average neighboring unemployed per capita as excluded instrument. Robust standard errors in parentheses for (3) and (6). Standard errors in parentheses for (1), (2), (4) and (5): * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

2.7 Conclusion

In a theoretical model, we show that jurisdictions who compete for mobile capital to finance public spending strategically react to the debt levels of their neighbors. We provide evidence for spatial interdependence between municipality debt in Germany. Our spatial econometric estimates are based on municipality panel data from the two federal states of NRW and Bavaria and take into account municipality and time fixed effects. The results suggest that a municipality increases its per capita debt by 16 Euro (in NRW) or 33 Euro (in Bavaria) if its neighboring municipalities increase their debt by 100 Euro per capita on average. The results are robust across numerous econometric specifications of spatial panel models, including various standard and three novel, non-standard definitions of spatial weighting matrices, as well as dynamic spatial lag models. Interestingly, the estimated degree of spatial debt interaction lies in between larger estimates for the spatial interaction of taxes and smaller estimates for spatial spending interactions. Thus, our findings indicate that debt should not be ignored when considering policy interactions between jurisdictions.

The new evidence that debt tends to spread across jurisdictions supports the view that stronger mechanisms are needed to curb trends of rising public debt.

Possible approaches include strong regulation, e.g. implemented at a higher level of government (cf. Epple and Spatt, 1986; Krogstrup and Wyplosz, 2010), or a market solution by maintaining a credible threat of insolvency which would punish excessive public debt at the level of local jurisdictions by higher interest rates. Further research is necessary to assess the impact of such actions on spatial debt interaction and their effectiveness in limiting the growth of local public debt.

2.8 Appendix - Tables

Table 2.6: Descriptive statistics 1999 - 2006

	Median	Mean	Std. Dev.	Min	Max
	Panel 1: North Rhine Westphalia, n=3,168				
Sum of per capita core budget and short term debt	857	997	729	0	6,069
Per capita core budget debt	780	860	578	0	3,927
Per capita short term debt	0	137	334	0	4,465
Per capita public company debt	214	434	517	0	3,964
Population	21,602	45,574	86,893	4,249	989,766
Share young people (<15 yrs)	17.03	17.17	1.92	11.98	25.67
Share old people (>65 yrs)	23.01	22.44	3.60	10.55	34.91
Unemployed per 100 inh.	3.64	3.88	1.14	1.73	10.78
Net migration per 1,000 inhabitants	1.92	2.52	7.07	-101.93	59.24
Growth of gross domestic product (county level)	0.48	0.41	2.34	-9.94	10.82
Per capita personnel expend.	317	344	106	158	806
Per capita operating expend.	286	305	102	83	1,084
Per capita welfare spending	158	232	175	23	1,327
Per capita net spending	1,189	1,272	426	495	4,560
Tax base bus. tax in 1,000 Eur	1,472	4,167	12,698	-464	226,683
Tax base prop. tax A in 1,000 Eur	35	42	32	-635	222
Tax base prop. tax B in 1,000 Eur	583	1374	3,035	58	38,434
Tax rate local business tax	403.0	404.8	27.4	300.0	490.0
Tax rate property tax A	205.0	214.0	38.2	110.0	400.0
Tax rate property tax B	375.0	365.5	49.6	200.0	530.0
	Panel 2: Bavaria, n = 16,448				
Sum of per capita core budget and short term debt	599	714	626	0	14,139
Per capita core budget debt	592	700	613	0	14,139
Per capita short term debt	0	13	70	0	2,221
Per capita public company debt	0	0.7	13	0	510
Population	2,754	6,014	31,031	193	1,294,608
Share young people (<15 yrs)	17.58	17.53	2.29	6.60	25.33
Share old people (>65 yrs)	20.79	20.53	4.61	6.74	46.19
Unemployed per 100 inh.	2.59	2.75	1.03	0	8.801
Net migration per 1,000 inhabitants	2.38	2.77	11.91	-121.44	119.46
Growth of gross domestic product (county level)	1.35	1.14	3.34	-15.38	19.21
Per capita personnel expend.	237	254	101	34	3,145
Per capita operating expend.	185	208	108	22	1,831
Per capita welfare spending	70	88	71	0	1,154
Per capita net spending	1,055	1,178	668	149	21,514
Tax base bus. tax in 1,000 Eur	94	613	5,629	-1,554	299,856
Tax base prop. tax A in 1,000 Eur	9	11	9	-14	86
Tax base prop. tax B in 1,000 Eur	58	177	1,249	-45	53,638
Tax rate local business tax	320	323	24	240	490
Tax rate property tax A	320.0	325.9	56.7	140.0	800.0
Tax rate property tax B	310.0	321.8	49.4	150.0	800.0

Source: Own calculations based on official statistics provided by the Federal Statistical Office.

Table 2.7: Specification tests for spatial panels 1999 - 2006

	NRW		Bavaria	
(Robust) LM test Spatial Lag vs OLS	(78.63***)	83.28***	(41.10***)	547.99***
(Robust) LM test Spatial Error vs OLS	(41.18***)	45.83***	(9.52***)	516.42***
LR test SDM vs Spatial Lag	70.78***		131.08***	
LR test SDM vs Spatial Error	99.97***		131.70***	
Spatial Hausman Test Fixed vs Random Effect	385.51***		166.47***	

Notes: All models are estimated with population, population^2, share of young / old people and unemployed per inhabitant as independent variables. Year and municipality fixed effects are taken into account by a double demeaning. W is a row normalized Inverse Distance Matrix with a cut-off point after 15 (20) km for NRW (Bavaria). Test statistics are significant at: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

Table 2.8: Yardstick competition and income regimes

	(1) Seat shares > 55%	(2) Seat shares < 55%	(3) rich	(4) poor
NRW				
N	946	2,222	1,584	1,584
λ_i	0.144***	0.198***	0.124***	0.201***
	(0.037)	(0.052)	(0.060)	(0.061)
$\lambda_1 - \lambda_2$	-0.053		-0.077	
t-stat	-0.799		-1.24	
LogLik	-21,329.436		-21,328.94	
Bavaria				
N	8,412	8,036	8,224	8,224
λ_i	0.226***	0.467***	0.331***	0.341***
	(0.035)	(0.033)	(0.034)	(0.033)
$\lambda_1 - \lambda_2$	-0.242***		-0.010	
t-stat	-4.644		-0.191	
LogLik	-111,019.18		-111,029.93	

Notes: Independent variables in all specifications are: population, population^2, share of young people, share of old people and unemployed per 100 inhabitants. The averaged neighboring equivalents are included as well as independent variables. λ_i denotes the respective interaction effect for the regime defined by a dummy as outlined in Elhorst and Freret (2009). W is a row normalized Inverse Distance Matrix with a cut-off after 15(20)km for NRW (Bavaria). (1) and (2) distinguish the regimes by the seat share of the party with the largest number of seats in a municipality council. (3) and (4) group municipalities into a rich or poor regime, depending on whether the sum of the tax bases of the three local taxes in 1998 is above or below the median. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

Table 2.9: Spatial weights model comparison

	NRW		Bavaria	
Weight specification	λ	Log-likelihood	λ	Log-likelihood
Binary Contiguity First Order	0.137*** (0.027)	-21,349.782	0.156*** (0.012)	-111,111.21
Binary Contiguity Second Order	0.287*** (0.041)	-21,343.501	0.233*** (0.019)	-111,061.28
Neighbors within 15km	0.191*** (0.029)	-21,333.954	0.242*** (0.020)	-111,082.03
Neighbors within 20km	0.229*** (0.037)	-21,343.292	0.288*** (0.025)	-111,059.83
Neighbors within 25km	0.324*** (0.042)	-21,336.966	0.249*** (0.032)	-111,074.97
Neighbors within 30km	0.403*** (0.046)	-21,334.917	0.245*** (0.038)	-111,083.67
Inverse Distance 14km (row norm)	0.127*** (0.027)	-21,335.176	0.262*** (0.017)	-111,055.93
Inverse Distance 15km (row norm)	0.163*** (0.028)	-21,330.158	0.277*** (0.018)	-111,051.57
Inverse Distance 20km (row norm)	0.209*** (0.036)	-21,333.959	0.327*** (0.022)	-111,030.84
Inverse Distance 25km (row norm)	0.253*** (0.041)	-21,334.262	0.294*** (0.027)	-111,050.65
Inverse Distance 30km (row norm)	0.277*** (0.047)	-21,337.922	0.282*** (0.031)	-111,062.40
Inverse Distance 15km (eigen norm)	0.237*** (0.041)	-21,345.859	0.476*** (0.029)	-111,058.17
Inverse Distance 20km (eigen norm)	0.272*** (0.047)	-21,352.520	0.514*** (0.030)	-111032.63
Inverse Distance 25km (eigen norm)	0.337*** (0.051)	-21,348.647	0.480*** (0.034)	-111,034.36
Inverse Distance 30km (eigen norm)	0.417*** (0.055)	-21,343.107	0.482*** (0.036)	-111,033.74
County	0.293*** (0.030)	-21,303.260	0.236*** (0.022)	-111,127.29
Radius 20km, pop>30,000	0.204*** (0.037)	-21,334.851	0.316*** (0.025)	-111,070.03
Radius 20km, pop>50,000	0.199*** (0.037)	-21,337.969	0.316*** (0.025)	-111,079.41
Same Urbanization	0.191*** (0.034)	-21,333.559	0.228*** (0.023)	-111,143.54

Notes: The dependent variable is the sum of debt from the core budget and short term debt of a municipality, λ denotes the spatial interaction effect. All models are estimated using the bias corrected maximum likelihood method. Independent variables in all specifications are: population, population^2, share of young people, share of old people and unemployed per 100 inhabitants. The averaged neighboring equivalents are included as well as independent variables. The specification of W is described in each row. Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

2.9 Appendix - Figures

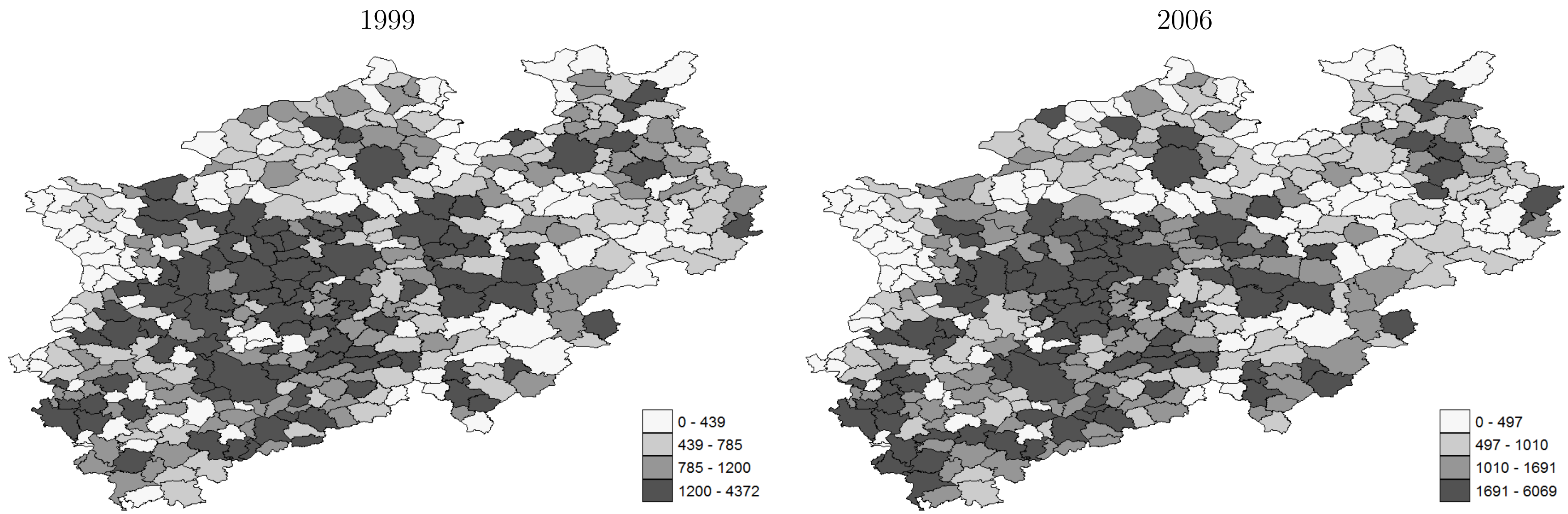


Figure 2.3: Quartile map of per capita debt in NRW in 1999 and 2006

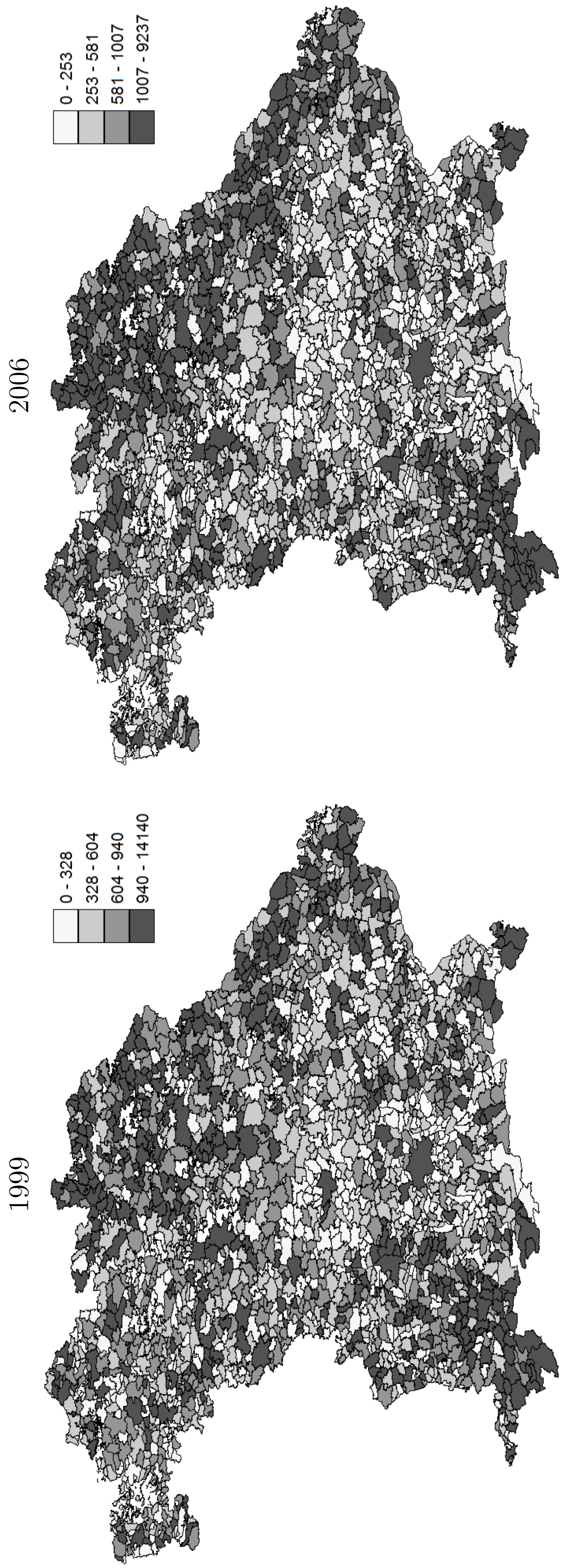


Figure 2.4: Quartile map of per capita debt in Bavaria in 1999 and 2006

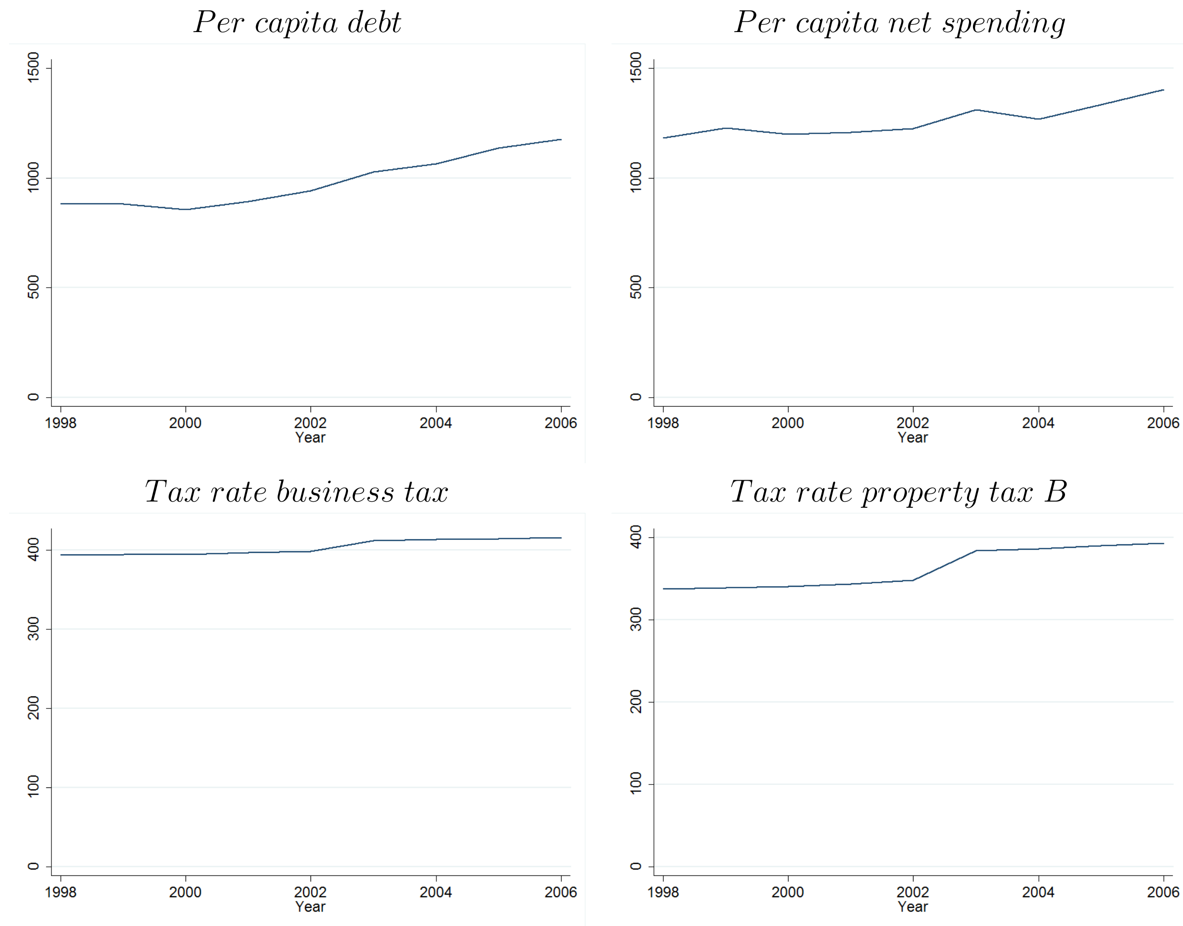


Figure 2.5: Policy parameters in means by year for NRW

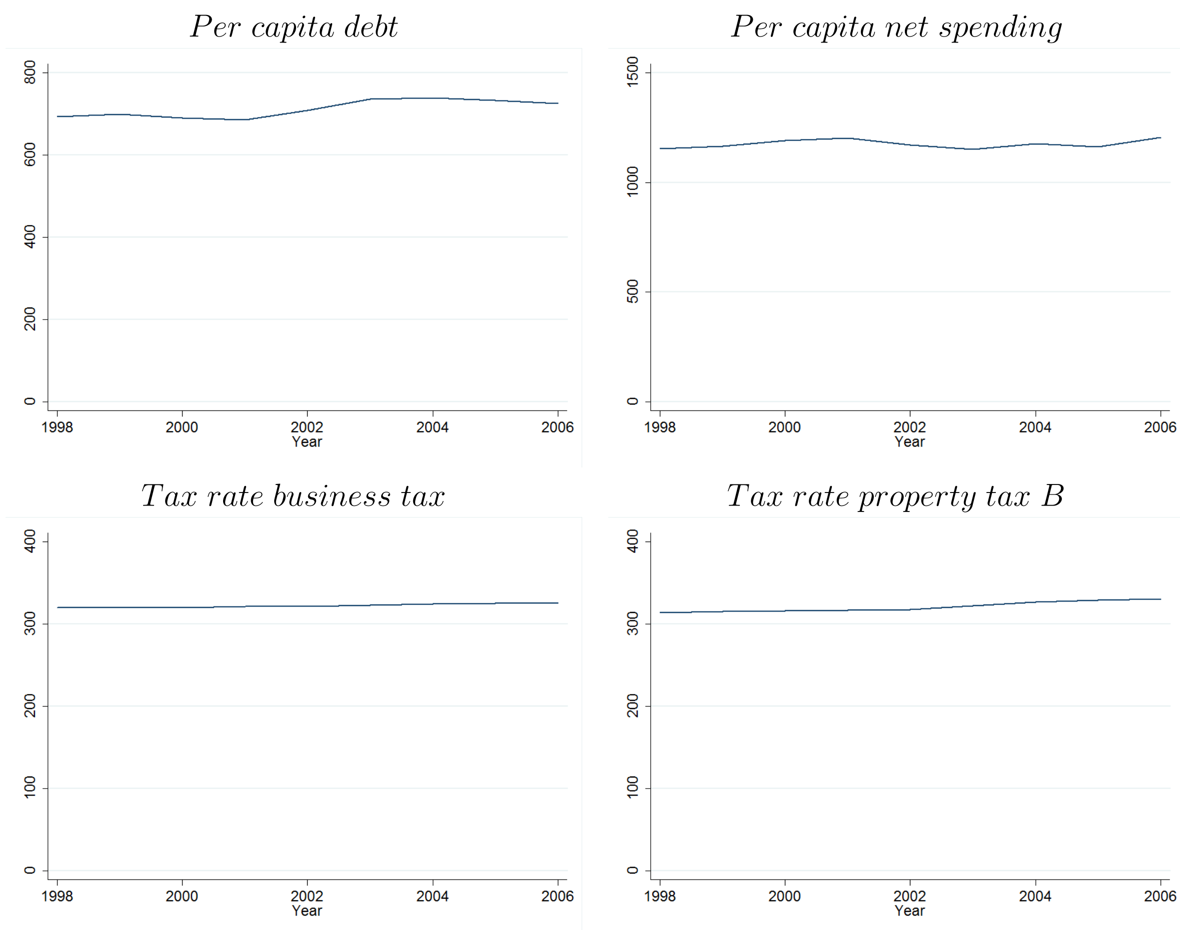


Figure 2.6: Policy parameters in means by year for Bavaria

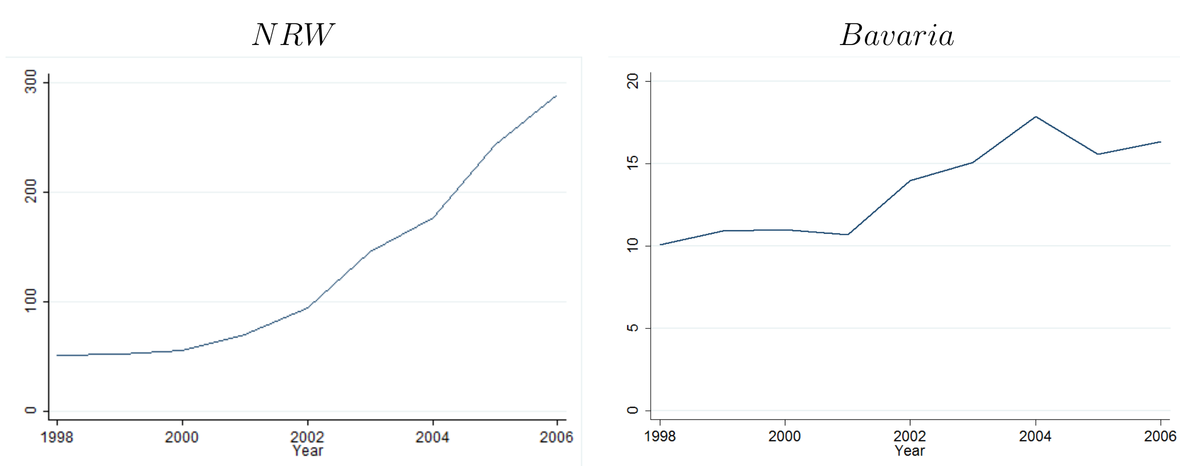


Figure 2.7: Mean per capita short term debt 1998 - 2006

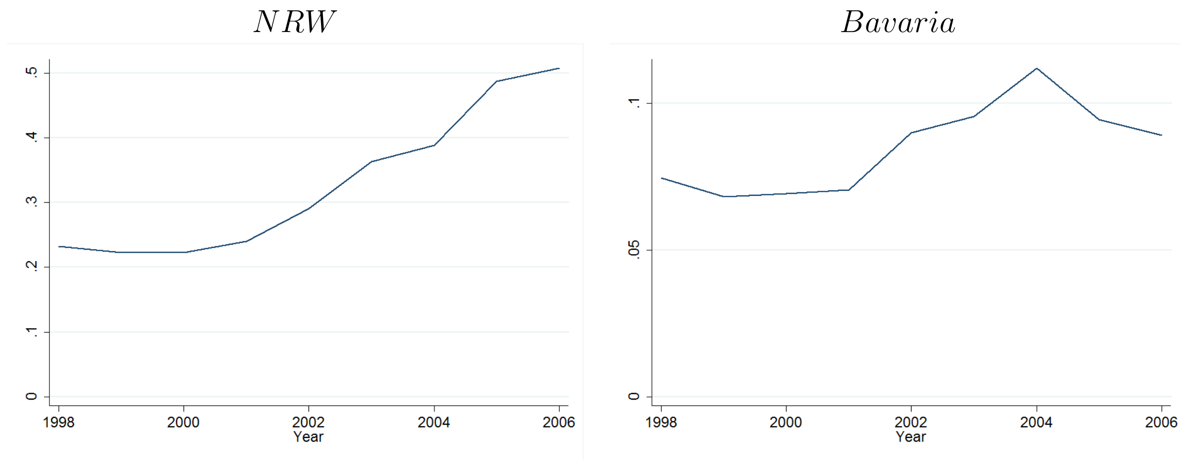


Figure 2.8: Share of municipalities using short term debt 1998 - 2006

2.10 Appendix - Theoretical Model

In this Appendix, we briefly describe the more general theoretical model where regions use the capital tax in both periods. Instead of (2.3) and (2.4), the periodic budget constraints are now

$$g_{i1} = \tau_{i1} k_{i1} + d_i \tag{2.15}$$

$$g_{i2} = \tau_{i2} k_{i2} - (1+r) d_i. \tag{2.16}$$

In period 1, regions now choose the debt level *and* tax rate. The first order conditions are given by

$$-k_{i1} + u'_{i1}\left(k_{i1} + \tau_{i1}\frac{dk_{i1}}{d\tau_{i1}}\right) = 0 \tag{2.17}$$

$$u'_{i1} - u'_{i2} + u'_{i2}\frac{dk_{i2}}{d\tau_{j2}}\frac{d\tau_{j2}}{dd_i} = 0. \tag{2.18}$$

Together, (2.17) and (2.18) define the two jurisdictions' reaction functions $\tau_{i1}(\tau_{j1}, d_j)$, $d_i(\tau_{j1}, d_j)$ which show how the optimal period 1 taxes and debt levels interact. In contrast to the simplified model in the main text, in this more general model, debt levels depend on neighbors' debt levels *and* tax rates. In a robustness check reported in section 2.6.4, we find that the estimated magnitude of spatial debt interaction is not sensitive to the inclusion of the tax rates of neighboring municipalities in the empirical model, however.

Chapter 3

Let's stay in Touch - Evidence on the Role of Social Learning in Local Tax Interactions[‡]

3.1 Introduction

Who interacts with whom in local taxation? Local governments have several motives to set tax rates with respect to neighboring jurisdictions. In particular, local tax choices of neighboring jurisdictions might be a subject of competition for mobile tax bases, a benchmark or even a learning device for local politicians. However, it is not clear to whom local politicians refer in their tax policies, i.e. who are their effective neighbors for local tax mimicking. In particular, we investigate which channels particularly matter local tax interactions, for e.g. we ask whether institutional proximity intensifies interactions compared to pure geographical distance. Understanding the exact nature of local tax interactions is important for the implications of the spatial distribution of income in the long run. Moreover, a benevolent social planner should harmonize taxes if local tax interactions are motivated by a harmful competition over mobile resources in a race to the bottom style and rather follow a laissez-faire policy if taxes rate strategies are communicated between local governments and no tax base effects are present. Traditional empirical studies on tax rate interactions typically define neighbors as

[‡]This chapter is based on joint work with Sebastian Blesse. A similar version has been published as a ZEW Discussion Paper, see Blesse and Martin (2015).

a (weighted) average of neighboring jurisdictions[49]. However, local governments in most federations are strongly interrelated with respect to the institutions they share both horizontally (e.g. joint administration bodies like courts or tax offices) and vertically, for example in overlapping jurisdictions like counties and municipalities. Therefore, local politicians might have different social or professional ties to other local decision makers. Based on this, tax interactions might be stronger in settings where local politicians or bureaucrats interact more intensely with each other rather than only with geographically close jurisdictions. Our contribution is to provide evidence on the importance of several coordination channels for tax interactions. In particular, we intend to show that local institutions and media via an inherent information transfer might be more important than pure geographical criteria for the significance of local tax interactions.

We use detailed geocoded data from local networks of institutions and media coverage to construct neighbor matrices consisting of municipalities sharing the same institutions as well as geographically close municipalities to identify local tax interactions. Weighting matrices usually assign the average values of the neighboring tax rates, which are in our setting institutional or geographical neighbors. Our setting is particularly interesting as potential vertical externalities from higher tier institutions which are shared with other local governments are unlikely as we focus on institutions with no own tax autonomy rights. Therefore, institutions function as a coordination device of political actions and not as a competitor for local tax bases. Using this we are able to separate the coordination effect of institutions from their potentially depriving effects on common tax bases. Moreover, NRW is interesting for institutional coordination effects as it is described as the most professionalized state in Germany regarding local political decision making (Arnold et al., 2015).

To identify local tax mimicking, we use a policy reform of local fiscal equalization in the German state of NRW as a quasi-experimental setting that created an incentive for municipalities to increase their tax rates. We construct an instrumental variable that predicts reform incentives to increase tax rates for neighboring municipalities by constructing the so called predicted imposed increase in tax rates (Lyytikäinen, 2012). Using this policy reform in the commonly applied spatial lag IV estimation, we yield three main findings for business tax rates in NRW.

Using counties as our baseline weighting matrix, first, we find, positive significant

[49] Geographical distance is either measured by the adjacency of neighbors, the N nearest number of neighbors or the inverse distance between two jurisdictions. See Revelli (2005) for a review.

tax rate interactions. Moreover, shared administration services and common access to local media yield similar results. However, interactions with geographical neighbors are not significant in our setting which contrasts most of the traditional literature. Unlike geographical neighbors, political and social proximity might be asymmetric in distance and adjacency. Even though geographical neighbors are more likely to be in the same institution, political and social proximity weights might be asymmetric in distance and adjacency to their geographical counterparts, for e.g. with neighboring municipalities in different counties or across intermunicipal cooperations. Therefore, we find that some institutions indeed elevate tax rate interactions when there are in fact no interactions over geographical distance. Second, there are positive and significant tax interactions during the reform. However, significant effects phase out already two years after the reform. Therefore, tax interactions are not a general phenomenon but only reform-induced in the present study.[50]

Third, we find suggestive evidence for local tax interactions via social learning processes through institutions and common local media. Several reasons provide evidence for a learning process. For instance, tax interactions are short-lived, which provides supportive evidence for a one-time learning process rather than continuous tax interactions. In addition, interaction is strongest when common institutions are considered as a neighbor framework. In fact, institutions where local politicians and bureaucrats can be thought to be the most interactive with each other, namely within the same county and the same administrations. Therefore, counties, joint administrations as well as local media are effective coordination mechanisms for local tax policies during the reform. However, other channels like inter-municipal cooperation in individual projects or regional marketing and tourism as well as interest group coverage do not intensify tax interactions.

We also argue that other forms of tax mimicking are unlikely in our setting.[51] We rule out tax rate interactions via tax competition due to the absence of tax base effects of neighboring tax rate changes during the reform, interactions through institutions being stronger than geographical criteria and the short term adjustment during the reform. Additionally, we rule out yardstick competition because it implies that municipalities with majorities have less intensely interactions than those without. We do not find evidence for this. Moreover, a subtle change in local

[50] Changes in tax rates are often only triggered by reforms of local fiscal equalization (Baskaran, 2014b), changes in minimum tax rates (Lyytikäinen, 2012), integration of new regions into a federation (Baskaran et al., 2015) or election dates (Foremny and Riedel, 2014).

[51] See Section 3.2 for a detailed overview of theoretical motives on local tax interactions.

fiscal equalization might also not be visible or important to voters even though the impact on local tax rates is strong. Although we find media to be an important transmission channel for local tax interactions (Revelli, 2008), there seems to be no voter effect of local media but rather a coordination of local decision makers via media over issues like local tax policies. Furthermore, benefit spillovers are unlikely in the present context as there is no negative interaction effect.

Our findings are consistent with recent quasi-experimental evidence that local tax rate interactions are not a general phenomenon (Baskaran, 2014b; Lyytikäinen, 2012; Isen, 2014; Eugster and Parchet, 2011).[52] In particular, we show that there are significant tax rate interactions but that they are only short-lived and not relevant in the common adjacent neighbor definitions but rather arise within certain channels.

We contribute to the literature on who competes with whom in tax policy. We explicitly distinguish institutional and geographical distance using detailed information on different local institutions, for example administration and cooperation, media, and standard geographical criteria. Hence, we are among the first to show that institutions also matter in local tax interactions. Revelli (2003) and Agrawal (2015a), however, show that vertical interactions matter in local taxation for British and US local governments, respectively. We find evidence for the relevance of counties for interactions of local fiscal policy in a multi-tier federation (Borck et al., 2015; Agrawal, 2015a; Büttner, 2001; Büttner and von Schwerin, 2016).

Moreover, we add to a small literature which defines factors that determine tax interactions like cultural borders (Eugster and Parchet, 2011), integration of economic areas (Holzmann and Schwerin, 2015; Baskaran, 2014b) or regions (Baskaran et al., 2015), metropolitan areas versus periphery (Brueckner and Saavedra, 2001a; Kauder, 2014; Charlot and Paty, 2010; Koh et al., 2013), and borders for both states (Geys and Osterloh, 2013; Cassette et al., 2012; Baskaran, 2014b;

[52] Agrawal (2015a) finds horizontal and vertical interactions via local sales tax rates for states in the US by taking state border discontinuities into account. Agrawal (2015b) exploits state border discontinuities to show interaction among local sales tax rates at state borders. Eugster and Parchet (2011) find small scale tax competition effects of the local income tax in Switzerland along cultural borders. Parchet (2012) finds that personal income tax rates are strategic substitutes in Switzerland. Holzmann and Schwerin (2015) find tax rate interactions in a highly integrated economic area. (Traditional) studies not using quasi-experimental methods to identify tax rate interactions also find strong tax rate interactions; for a survey see Allers and Elhorst (2005).

Agrawal, 2015b) or nations (Cassette et al., 2012), respectively.[53]
The paper proceeds as follows. In Section 3.2, we describe several motives for local tax interactions. Section 3.3 outlines the institutional framework, most notably the system of local fiscal equalization of NRW and its reform in 2003. Section 3.4 and 3.5 describe the empirical approach and results, respectively. Section 3.6 concludes.

3.2 Theoretical motives for local tax interactions

Local governments can have several motives to interact in tax rates. An important one is certainly discussed in the tax competition literature, where local jurisdictions try to attract a mobile capital tax base by setting lower tax rates (Wilson, 1999; Zodrow and Mieszkowski, 1986). Tax rates become interdependent as the tax reduction of one jurisdiction lets others experience a fiscal externality in form of an outflow of capital. In a symmetric setting, tax competition results in a harmful race to the bottom. However, asymmetries in the assumptions of the model can make the sign of tax interdependence ambiguous. Secondly, jurisdictions might experience informational spillovers in tax choices when comparing themselves to neighboring units as yardsticks (Besley and Case, 1995). Here, voters can use tax rates of other municipalities ceteris paribus as a benchmark to determine how successful the respective incumbent is and accordingly, either punish or reward him at the polls. Thus, in order to get re-elected local politicians might mimic tax choices of neighboring places. Therefore, one would expect positive tax interactions in the presence of yardstick competition. However, a prominent reason for negative tax interactions are benefit spillovers (Case et al., 1993). The idea is that local public goods provision in a given municipality entails positive externalities in other units due to the non-excludable character of quasi-public club goods. Hence, spending needs of the neighboring jurisdictions are lower and allow for lower tax rates.
Beside these traditional theories of local tax interactions, the literature has recently discussed other transmission channels of tax rate mimicking. Potentially relevant to our setting are especially interactions through social learning or (partial) tax coordination.
For instance, Glick (2014) sets up a model where social learning can overcome

[53] Moreover, Reiter (2015) provides a survey on the question of who competes with whom in international tax competition. In a centre-periphery framework, Janeba and Osterloh (2013) show that metropolitan and rural jurisdictions compete sequentially over mobile tax bases.

situations with substantial uncertainty about policy outcomes such as policy reforms. Thus, learning from others' tax choices with limited information of policy makers on the consequences of one's own tax rate decisions can be efficient. Accordingly, tax mimicking should constitute a positive sign if social learning or knowledge diffusion is present. Moreover, Becker and Davies (2013) show that tax mimicking via social learning is lower if adjustment costs are present. For example intensive communication of local governments might lower adjustment costs between policy makers and can elevate tax interactions.

There are also incentives to coordinate local tax choices as competing over a mobile tax base might lead to an inefficient underprovision of public goods. Whereas most of the literature discusses difficulties of jurisdictions to coordinate their tax choices (Keen and Konrad, 2012), we believe that multi-tier federations like Germany indeed offer scenarios in which coordination might be effective. For instance, institutions (like counties) and joint administration (bureaucracy) shared by multiple municipalities might provide a platform for knowledge diffusion with respect to tax strategies or even actively provide guidelines for setting tax rates. Given coordination, one would expect perfect harmonization of tax rates within that specific area of coordination. In the presence of asymmetries of regions, however, one would not expect identical tax rates as some municipalities might be harmed by coordination (Kanbur and Keen, 1993). However, strong institutions like counties might potentially initiate coordination if their members are sufficiently similar.

Partial tax coordination takes place in similar regions if tax competition takes place repeatedly (Cardarelli et al., 2002; Cotenaro and Vidal, 2006). With partial coordination, groups of similar jurisdictions compete over resources with other regions and yield harmonized tax rates just as with with social learning. For example, jurisdictions belonging to the same county or particularly similar or close neighborhoods within the same county might entail sufficient homogeneity of municipalities for partial tax coordination to take place. Note that partial coordination implies a fiscal externality (between similar regions) unlike tax harmonization or social learning.

3.3 Institutions

3.3.1 Local governments and public bodies in Germany

In the present paper we want to show the relevance of learning across different institutions for local tax interactions in the German federal state of NRW. German local governments display a high degree of fragmentation and heterogeneity. Generally one can distinguish jurisdictions and non-jurisdictional bodies, each with substantial differences in terms of autonomy and accountability.[54]

Jurisdictions are constitutionally recognized units with own territories and directly elected representatives. There are many jurisdictions in Germany, such as states (2011: 16), counties (295), and municipalities (11,442). NRW is the most populous state in Germany with about 17.6 Million residents and 396 municipalities, including 30 counties and 23 district-free cities. Local governments are usually part of several jurisdictions in a multi-tier federation like Germany. Municipalities, for example, belong to a certain county and state. Below state-level all local governments are subject to the constitutional right of self-governance (Article 26 II Grundgesetz). Like in other countries, municipalities offer several local public goods such as general administration, infrastructure, waste disposal, and culture activities. However, municipalities also share duties with other jurisdictions, for example with counties or other municipalities, or even with private firms. Municipalities have substantial spending and also some revenue autonomy (see Section 3.2). Counties, however, cooperate with municipalities in service provision. They do not have tax autonomy and rely largely on contributions and grants.

Non-jurisdictional bodies are not legitimated by elections and also do not possess revenue autonomy, i.e. they do not have taxation rights. NRW for instance, has semi-autonomous bodies like regional districts (*Regierungsbezirke*), which are administrative districts of the state government, and various general or single purpose inter-municipal cooperations. Cooperations in administration or local economy issues are initiated locally and represent horizontal cooperation. They usually serve to exploit economies of scale or increase bargaining power in political issues. Regional districts, however, follow a classical top-down model of bureaucracy and are a typical example of local institutions which do not pass legislation but implement arm's length decisions from the state.

In this study, we exploit information on institutions which could either elevate horizontal or vertical tax interactions at the municipal level. Vertical tax interactions refer to the coordination of tax policies by (or passively in the area of) a higher tier of government, for example within a county or regional district. Jurisdictions might also interact horizontally with other units from the same tier, for example between municipalities. Local governments might also be influenced by non-governmental interest groups. Economic, political or cultural associations

[54]For a detailed overview of the German local government system, see Zimmermann (1999).

might lobby for certain policies at the local level.

3.3.2 Local business taxation

Municipalities can set the tax rate for the business and property tax autonomously. In fact, the business tax (*Gewerbesteuer*) is the most important source of local revenues under own discretion in Germany. Municipalities in NRW earn on average 18-24% of their overall revenues from business taxes. Note that the municipalities can only levy a business tax multiplier τ_i, but as it is applied to the respective tax base B_i with a percental surcharge which is fixed throughout the federation (S), the tax multiplier actually represents the effective tax rate[55]:

$$T_i = \tau_i \cdot S \cdot B_i \tag{3.1}$$

where T_i is the business tax revenue of a given municipality i. The surcharge S (*Steuermesszahl*) is fixed and equals 3.5% since a corporate tax reform in 2008 (Büttner et al., 2014). [56] Business taxes are levied by a municipality on all firms located in that municipality. The respective tax base is the firms' net profits (*Gewerbeertrag*), although there are some exceptions.[57].

Municipalities can also tax housing and land property within their borders. The tax base is the assessed value determined by the respective local tax office. There are separate property taxes for agricultural (*Grundsteuer A*) and both residential and commercial usage (*Grundsteuer B)*. Overall, property taxation is less important to German municipalities than income from business tax. Note that we focus in the following on the business tax.

However, municipalities also receive income from taxes which are shared vertically across governmental tiers (*Gemeinschaftssteuern*). Note that shared income taxes and VAT income accounted for about 19.34% and 1.84% of municipal revenues in NRW, respectively. Although municipalities receive certain shares of tax revenues from related economic activities within their borders, they do not possess tax autonomy on these taxes.

[55] Therefore, we use the terms tax rate and tax rate multiplier interchangeably.

[56] Before, the surcharge depended on the business type of the firm with incorporated and most non-incorporated firms facing a 5% surcharge rate. Non-incorporated companies like private business partnerships faced a maximum rate of 5% when taxable income for business tax exceeded 48,000 euro and a minimum rate of 1% when earnings were below 12,000 euro.

[57] For instance, local business tax payments can be deducted from either personal income or corporate income tax, for non-incorporated and incorporated companies, respectively (Büttner et al., 2014)

3.3.3 Local fiscal equalization and its reform in 2003

The present paper exploits a reform in local fiscal equalization in NRW in 2003 to study its effect on local tax interactions and related transmission channels. Indeed, the single most important source of local revenues are transfers from the state government, provided within a local fiscal equalization scheme. They account for about 50% of overall municipal revenues in NRW. Whereas grants can be either discretionary[58] or rule-based (*Schlüsselzuweisungen*), the latter are within the focus of this paper as they are economically more important and are also subject to our natural experiment.

Rule-based transfers from the federal state target on giving municipalities sufficient funds to provide local public goods in an sufficient quantity and quality. Fiscal equalization mainly intends to balance out differences in municipal fiscal need and fiscal capacity according to some formula apportionment. First, fiscal need is some benchmark level of (obligatory) spending per inhabitant to meet citizen's needs. Second, fiscal capacity, however, is a measure of the municipal ability to finance these spending needs.[59]

Specifically, the rule-based transfers per capita $g_{i,t}$ from the federal state are distributed to the $i = 1, ..., N$ in order to reduce the difference between the *fiscal need* $n_{i,t}$ and the *fiscal capacity* $c_{i,t}$ in a given year t. This fiscal equalization reads as follows

$$g_{i,t} = \begin{cases} 0.9(n_{i,t} - c_{i,t}) & \text{if } n_{i,t} > c_{i,t} \\ 0 & \text{else,} \end{cases} \tag{3.2}$$

with 0.9 being the equalization rate in our sample period. Therefore, municipalities with lower fiscal capacity than fiscal need will receive 90% of that fiscal gap from state equalization transfers. With fiscal capacity at least as high as the respective need, the municipality does not receive rule-based transfers at all (i.e. it is fiscally abundant).

Fiscal need $n_{i,t}$ is a standardized amount of spending fixed by the state government (*Grundbetrag*) in the previous fiscal year to avoid manipulations of the assumed costs of service provision. It is also determined by municipality-specific characteristics, mostly by population size. Whereas all localities should receive

[58] Discretionary or purpose-related grants (*Zweckzuweisungen*) are occasional transfers from the state-government to municipalities and are paid in form of matching grants for which municipalities have to apply. Frequently granted transfers are for example infrastructure projects.

[59] For details on local fiscal equalization in German federal states, see Lenk and Rudolph (2004).

similar revenues per capita, more populous regions have disproportionately higher fiscal needs to compensate for assumed higher costs of public goods provision with increasing population size (Brecht, 1932).[60]

The reform of fiscal equalization in 2003 leaves fiscal need unaffected but changes a parameter referring to the fiscal capacity. Fiscal capacity is defined as the sum of tax revenues from all tax bases, i.e. from autonomously set taxes and vertically shared taxes with no autonomy, such as VAT or federal income tax. Specifically, it is given by:

$$c_{i,t} = \frac{T_{i,m,t-1}}{\tau_{i,m,t-1}} \cdot \overline{\tau_{t,m}} + T_{shared,t-1}, \tag{3.3}$$

where $c_{i,t}$ is the fiscal capacity in the current year, $T_{i,m,t-1}$ the tax revenues for m which denotes the three local taxes (business tax, property tax A and B) whose tax multipliers municipalities are free to set as well as vertically shared tax revenues $T_{shared,t-1}$ from the previous year[61]. Each tax base m is divided by actual tax rates $\tau_{i,m,t-1}$ from the previous year, respectively. Moreover, the latter term is multiplied with the so called hypothetical tax rate $\overline{\tau_{t,m}}$ (*fiktiver Hebesatz*), which is set by the state government and is constant across municipalities.

The transfer system aims at providing sufficient equality in relative fiscal power but also does not want municipalities to rely strategically on transfer payments. Municipalities should also engage in tax competition and therefore, the focus of fiscal capacity is shifted from actual to potential revenues. In particular, revenues are normalized by dividing actual revenues with tax multipliers and are then multiplied with a statewide reference rate, i.e. the hypothetical tax rate. This procedure makes transfer payments independent of actual tax revenues (and hence, actual tax multipliers) but instead relying on effective tax capacity. This procedure prevents the state to perceive low-tax municipalities as fiscally weak via mechanically lower tax revenues and vice versa for high-tax jurisdictions. Therefore, the hypothetical tax rate should have no direct effect on transfer payments and therefore, on actual tax rates. However, actual tax rates respond strongly and increase as a response to an increase in the hypothetical rate in 2003 as will be shown below.

The state government of NRW increased the hypothetical multiplier for the busi-

[60]However, also other factors like municipal centrality or the number of school children matter (GFAG NRW, several years).

[61]More precisely, the tax multipliers and revenues applied in the formula apportionment use figures from July two years ago and June 30th of last year. For instance, fiscal capacity in 2003 is calculated from tax revenues between July 1st 2001 and June 30th, 2002 (GFAG NRW, 2003).

ness tax from 380 to 403%. The same applied to the reform of hypothetical property tax rates. NRW changes hypothetical tax rates occasionally and usually in large steps. Changes occur when the hypothetical multiplier does not align with the (weighted) population average of recent realized tax rates (Ministry of Interior NRW, 2010;Lenk and Rudolph (2004)). Therefore, one might argue that some cities with large tax bases might be important drivers of the level of the hypothetical tax rate. For small municipalities which cannot individually influence the weighted average of tax rates, the policy change is arguably exogenous. Nonetheless, we also show later that our findings are robust to different population classes and several placebo reforms in pre-reform years. Moreover, changes in hypothetical tax rates by the state government are irregular and come only with little prior notice. Therefore, the timing of these reforms is hardly predictable, especially from the viewpoint of an individual municipality.[62] The reform should also increase municipalities' tax effort and incentivizes them to rely less on fiscal grants.

Hypothetical tax rates are essentially a normalization of tax bases and there should be no direct negative effects on transfers in equ. (3.2) by an increase in the hypothetical multiplier. Thus, no significant effects on actual tax rates can be expected. A change in the hypothetical multiplier changes the assessed fiscal capacity of all municipalities. Total transfer payments, however, would only change if the total amount of allocated transfers would change too or the relative fiscal gap changed. First, the state government of NRW did not change the amount of grants from 2002 to 2003. Specifically, grants were constant with 4.576 billion euro and 4.581 billion euro, respectively (NRW GFAG, 2002, 2003). Therefore, fiscal need had to decrease proportionately per capita as the fiscal capacity increased but total transfers were constant. Second, the fiscal gap could actually change via an increase in the hypothetical multiplier but these effects on relative distribution on transfers were minor.[63] Thus, the increase in the hypothetical multiplier should not have substantial effects on rule-based transfer payments and therefore on actual tax rates.

However, the hypothetical tax rate is a strong reference rate for actual business tax multipliers. Figure 3.1 shows the development of average tax rates and hypothetical tax rates over time. Note that the increase in the hypothetical tax

[62] We provide evidence on the absence of potential anticipation effects in the robustness checks.

[63] An increase in the hypothetical multiplier raises the fiscal capacity for municipalities with larger tax bases disproportionately. Hence, richer municipalities might transfer more resources to poorer municipalities (Baskaran, 2014b, 2015). However, the respective amount should be fairly small and thus, not have an effect on actual tax rates.

rate in 2003 led to an accompanying increase in the average business tax rate in the following years. On average, the business tax rates are always significantly above the hypothetical tax rates and indeed most municipalities choose a tax rate which is at least as high as the hypothetical multiplier. Moreover, the number of municipalities with tax rate changes per year increased in 2003 substantially up to 250 out of 396.

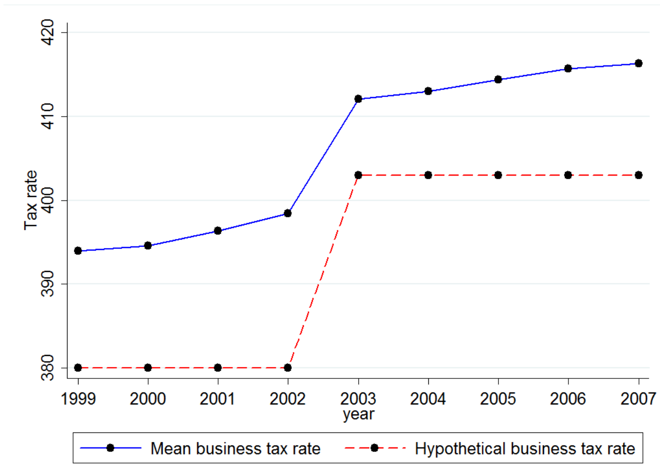


Figure 3.1: Reform and Impact 2003.

Several reasons may account for this. First, the hypothetical tax rate punishes efforts to attract a mobile tax base because a higher multiplier implies that larger tax bases reduce transfers received in equ. (3.2). Thus, own tax efforts are diminished by higher hypothetical tax rates and thus imply higher tax rates. Fiscal equalization schemes are shown to have such effects on local tax rates both theoretically and empirically (Kelders and Koethenbuerger, 2010; Egger et al., 2010; Büttner, 2006). Holzmann and Schwerin (2015) argue that tax rates are not set too low for another reason. Often, federal states in Germany make it a condition for municipalities applying for a discretionary grant to make a sufficient tax effort themselves. Therefore, if tax rates are too low municipalities might have less of a chance to receive task-related grants from the state government. Hence, municipalities should not set their tax rates too low below the hypothetical tax rate which is essentially a tax rate of which the state government thinks that municipalities can tax appropriately.

Second, political economy considerations might play a role. When increasing the hypothetical tax rate the state government might lower the political costs of own tax increases by local policy-makers. They can use the veil of the state-wide tax increase to hide increases in their own tax rates. Parts of the tax increase can be attributed to the state government to avoid the loss of voter support. Moreover, Baskaran (2014b) argues that hypothetical tax rates are means for firms lobbying for lower tax rates in municipalities if actual tax rates exceed this reference rate. Hypothetical tax rates are also important because of the common misbeliefs of local politicians and the local media that even a tax rate that is negligibly lower than the hypothetical rate leads ceteris paribus to direct losses in rule-based transfer payments (DIHK, 2009). However, fiscal capacity and therefore transfer payments are ceteris paribus independent from actual tax rates. Therefore, politicians of municipalities below the reference rate might increase the respective actual tax rate due to an increase in the hypothetical tax rate, even though no direct negative effects on grant allocation are to be expected without it. This establishes a ratchet-effect, where the actual tax rate should be at least as high as the hypothetical rate.

3.4 Methodology

3.4.1 Spatial lag models

This paper estimates municipal tax reactions to a change of neighboring municipalities' tax rates. A common procedure to address this question is the spatial lag model, which explicitly incorporates neighbor outcomes into the regression. The standard model is estimated as follows:

$$\tau_{i,t} = \rho \sum_{j \neq i} w_{i,j} \tau_{j,t} + \beta x_{i,t} + \mu_i + \epsilon_{i,t}, \tag{3.4}$$

where $\tau_{i,t}$ is the tax rate from municipality i in year t and $\tau_{j,t}$ represents the averaged tax rates of all neighbor municipalities j of municipality i in period t. Moreover, significant estimates of ρ are interpreted as strategic tax rate interactions with neighbors. We also include $x_{i,t}$ as control variables, μ_i as municipality fixed effects and the error term $\epsilon_{i,t}$.

The spatial weighting matrix $w_{i,j}$ assigns the averaged tax rates of a pre-defined set of neighbors. Averaging via row normalization ensures the stability of the estimator. Note that the true weighting matrix is unknown. Traditionally, weighting matrices are either based on common borders, distances or population weights. Generally, adjacent units or close municipalities are assumed to have stronger interactions.

However, policy interactions might not only be triggered by geographical closeness but also by political or social ties. To separate distinct channels of tax interactions, we also introduce several weighting matrices for institutional or local media networks in addition to standard geographical weights. For instance, county membership (*County*) of municipalities is an important institutional network. Moreover, we use rich institutional data from Blotevogel et al. (2009) and Terfrüchte (2015) to construct our institutional weights. We assign neighbors based on joint access to local media (*Media*), administration (*Administrative*), municipal cooperation projects (*Cooperation*), cooperation in regional marketing and tourism (*Regional marketing*) and interest groups for social and economic issues (*Social and economic*). The definitions from Blotevogel et al. (2009) follow a heuristic approach and aim to resemble given borders. The same institutional weighting matrices are also proposed by Terfrüchte (2015), using the regional correlation of local institutions to assign the resp borders.[64]

In the baseline regressions we consider all municipalities in the same county as neighbors because of a superior model fit compared to other weighting matrices (for related Akaike and Schwartz criteria, see Table 3.2) and due to the expected interactions within counties[65].

Taking first differences of equ. (3.4) removes the municipality fixed effect μ_i and gives:

$$\Delta\tau_{i,t} = \rho\Delta\sum_{j\neq i} w_{i,j}\tau_{j,t} + \beta\Delta x_{i,t-1} + \Delta\epsilon_{i,t}. \tag{3.5}$$

Specifically, equ. (3.5) measures whether the change in the weighted average of neighboring business tax rates affects the change in the business tax rates of a given municipality.

However, the neighboring tax changes $\Delta\sum_{j\neq i} w_{i,j}\tau_{j,t}$ might be biased due to several sources of endogeneity. First, there is the issue of reverse causality, i.e. whether a municipality's tax rates influence neighbors tax rates or vice versa. Second, unobserved shocks during the reform might influence tax rates of a certain municipality and its neighbors jointly. For instance, this might be an exogenous reform that increases tax rates (like in our setting) or spatially correlated macro shocks

[64] For detailed descriptions of non-geographic weighting matrices, see Appendix 3.9 and Table 3.12. The mapping of the institutions by Blotevogel et al. (2009) is shown in Figure 3.5 of Appendix 3.8 and the mapping of the institutions using a functional approach based on Terfrüchte (2015) is show in Figure 3.4 of Appendix 3.8.

[65] We discuss the role of various institutional and geography-based weighting matrices for local tax interactions in Section 3.5.4.

affecting both the tax base and tax rates.

There are several ways to deal with this endogeneity problem. Traditional spatial lag IV regressions instrument the weighted average of neighboring tax rates with socioeconomic or political characteristics of the neighboring municipalities. However, it is unlikely that this solves potential endogeneity concerns (Gibbons and Overman, 2012).[66] First, municipal tax rates might not be well predicted by neighboring control variables because the weighting matrix might misspecify the exact influence of neighbor characteristics on neighboring tax rates. Second, observable neighboring control variables might have a direct significant effect on the tax rates as well. Moreover, there might be omitted variables that influence both the neighboring characteristics and the error term $\epsilon_{i,t}$.

However, recent quasi-experimental literature used exogenous variation from policy changes to identify causal tax rate reactions at the local level. Accordingly, the next subsection will propose an instrumental variable strategy based on a policy change in NRW to take the endogeneity problem in the common spatial lag framework into account.[67]

3.4.2 Identification using an exogenous policy change

We use the 2003 reform in NRW to identify reactions towards neighboring tax changes using the empirical method of Lyytikäinen (2012). This paper exploits exogenous variation from a country-wide statutory property tax increase in Finland as a natural experiment to identify tax mimicking behavior at local level. Unlike the Finish setting, we do not have a strictly binding minimum tax rate for business tax in NRW but municipalities have nevertheless strong incentives to increase tax rates as a response to the increase in the hypothetical tax rate. Hence, we believe that our instrument is relevant.

To capture the incentive to increase tax rates as a response to increased hypothetical tax rate we calculate the predicted imposed tax increase as we cannot observe the counterfactual of tax rate choices without the increase in the hypothetical rate. The update of the hypothetical tax rate is an arguably exogenous event and hence we can use the neighboring imposed increase to predict neighboring tax changes. In other words, we instrument tax rate choices of a municipality's

[66] Another method in traditional spatial econometrics is quasi-maximum likelihood (QML) estimation (LeSage and Pace, 2009). The underlying assumption with QML is that the true spatial interaction is known, which is a strong assumption (Gibbons and Overman, 2012).

[67] We report results of traditional Spatial IV estimations in the robustness checks in Section 3.3.

neighbors with their incentives to increase tax rates. Therefore, we propose the calculation of neighbors' predicted imposed increases from the reform in the first stage as follows:

$$\sum_{j \neq i} w_{i,j} \tau_{j,2003} = \sum_{j \neq i} w_{i,j} D(\overline{\tau_{2003,m}} > \tau_{j,2000,m})(\overline{\tau_{2003,m}} - \tau_{j,2000,m}) \tag{3.6}$$

The term $(\overline{\tau_{2003,m}} - \tau_{j,2000,m})$ calculates the difference of the actual tax rate in 2000 to the new hypothetical tax rate in 2003.[68] First, this term gives substantial information about the intended magnitude and probability of the tax increase as a response to the reform. We use the year 2000 as a base year since the respective tax rate should be a strong predictor for the tax rate in 2003, given the persistence of tax rates. Second, the choice for the year 2000 ensures exogeneity of the instrument of the newly updated hypothetical tax rate in 2003 because tax rates in 2000 should be uncorrelated with the error term in equ. (3.5).

Furthermore, $D(\overline{\tau_{2003,m}} > \tau_{j,2000,m})$ is a binary variable taking the value of 1 if the tax rate in 2000 is below the new hypothetical tax rate in 2003. This ensures, that our instrument only captures the local average treatment effect of municipalities which have a positive pre-reform distance of their realized tax rates to the later standard multiplier. This is because of the widely observed incentive of municipalities to perceive the hypothetical tax rate as a minimum value for their own tax rate choices. Moreover, except for one municipality all tax rate changes are positive. The previous discussion on municipal incentives to use the hypothetical tax rate as a benchmark for own tax rate choices and therefore, incentives to increase tax rates as a response to the reform in 2003, indicates how strongly our instrument predicts tax rate increases after the reform. Moreover, Figure 3.2 shows some preliminary evidence on the correlation of the predicted imposed increase with actual tax increases due to the reform. In fact, there is a positive correlation between these variables and thus, our instrument strongly predicts tax increases from 2002 to 2003.

Note that tax rates of municipalities are spatially correlated and hence, so are the predicted imposed increases. Not taking this into account would cause endogeneity since spatial autocorrelation would feed into the error term through our instrumental variable. By including the predicted imposed increase for a given

[68] Moreover, it is unlikely that in 2000 politicians already strategically reacted to the reform of 2003. Note, that the reform was only decided in late 2002. Rule-based grants are calculated on the basis of the 1st of June 2001 until the 1st of July 2002. Therefore the year 2000 is the first year not affected by the reform.

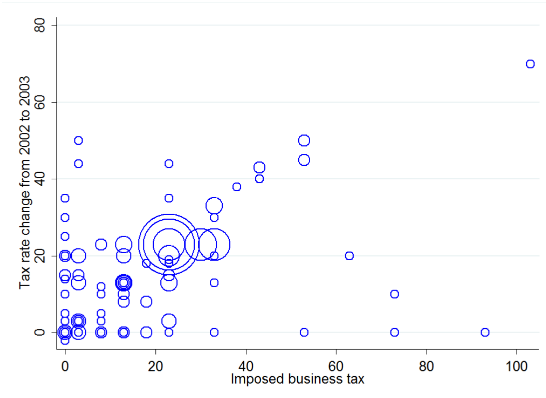


Figure 3.2: Correlation of reform incentives and actual tax increases. *Notes*: The magnitude of municipal tax rate changes are depicted with dots. Note that the size of dots represents the frequency of certain tax rate changes in the sample.

municpality i as well we control for the direct reform effect on this municipality. Hence, we can avoid endogeneity through the overall incentive of the reform to increase tax rates and the related issue of spatial autocorrelation from neighbors tax rates to own tax increases. Conditional on the own imposed increase in a given municipality, we can measure the causal effect of tax rate interactions of a given municipality to its neighbors in response to the tax reform.

3.5 Results

3.5.1 Data and descriptive statistics

We use a balanced panel for all 396 municipalities of NRW from 1993-2008. In the baseline IV regressions we only exploit data from 2000 onwards as all identifying variation for our instrument is from this period. In Section 3.3 we also perform placebo tests using information from previous years starting from 1993. Data about local tax rates, population, population structure (young and old), employment situation as well as received overall grants[69] and short- and long-term debt are obtained from the Statistical Office of NRW. The respective hypothetical tax rates are collected from laws on local fiscal equalization from the Ministry of Interior NRW (GFAG NRW, several years).

Descriptive statistics for the main observation period between 2002 and 2003 are shown in Table 3.6 in Appendix 3.7. The upper and lower panel show summary statistics for the variables in levels of 2003 and in first-differences from 2002 to

[69]This variable contains the overall sum of transfers (discretionary plus rule-based grants).

2003, respectively. Business tax rates in NRW have a large variation from 310 up to 490. Therefore, municipalities are both below and above the hypothetical tax rate in 2003. However, more than 60% of all municipalities change their tax rates. The change is 13.6 percentage points on average although it ranges from -2 to 70 percentage points. Grants also vary greatly across municipalities with a range from 0 to more than 600 euro per capita. NRW is also a highly urbanized state with comparably high municipal debt levels.

Moreover, the left panel of Figure 3.3 shows the spatial distribution of business tax rates in the year 2000. The right panel displays the distribution of related tax rate changes during the reform. Whereas it is clear that business tax rates are generally spatially correlated in NRW (left panel), the right panel shows that also the changes in tax rates are clustered in space. To see whether the spatial autocorrelated tax rate changes are the outcome of strategic interactions or only spurious correlations, we employ our spatial lag IV estimations using the policy change in 2003 as a source of exogenous variation.

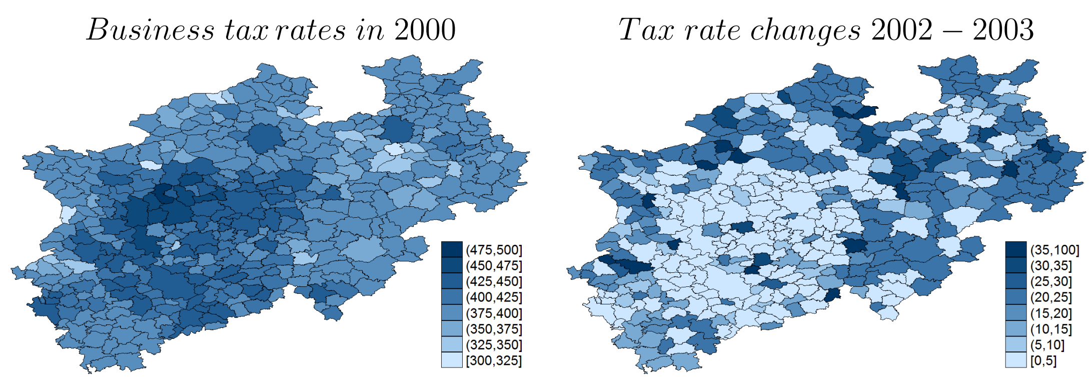


Figure 3.3: Spatial distribution of tax rates

3.5.2 Baseline results

The main results using municipalities in the same county as neighbors are displayed in Table 3.1. Model I gives the second stage results as depicted in equ. (3.5) and Model II shows the first stage results using the instrument as outlined in equ. (3.6).

Our policy change-based instrument proves to be a relevant instrument in the first stage in Model II. The Kleibergen-Paap F-test of about 190 in the first stage indicates the strong predictive power of our instrumental variable. Moreover, the neighboring imposed increase indicates the relevance of our instrument in

Table 3.1: Main Results

	Dependent Variable: $\Delta\tau_{i,2003-2002}$	
	2SLS	OLS
	(I)	(II)
	Second Stage	First stage
ρ	0.314**	
	(0.157)	
Non-zero own imposed increase (1/0)	6.403***	1.706**
	(2.085)	(0.690)
Own imposed increase	0.176**	0.000
	(0.089)	(0.020)
$\Delta Population_{i,2002-2001}$	-0.001	0.000
	(0.001)	(0.000)
$\Delta Share\ young_{i,2002-2001}$	-377.162	-333.537***
	(366.944)	(128.285)
$\Delta Share\ old_{i,2002-2001}$	424.271	-161.037
	(333.013)	(114.365)
$\Delta Employed\ per\ capita_{i,2002-2001}$	55.290	7.811
	(62.953)	(19.177)
$\Delta Short\ term\ debt\ per\ capita_{i,2002-2001}$	-0.011**	-0.004**
	(0.005)	(0.002)
$\Delta Core\ debt\ per\ capita_{i,2002-2001}$	-0.002	0.001
	(0.002)	(0.001)
W × own imposed increase		0.456***
		(0.033)
Constant	-0.320	4.842***
	(1.750)	(0.668)
Kleibergen-Paap F	190.255	
Adjusted R^2	-	0.726
N	396	396

Notes: W assigns all municipalities in the same county as neighbors with equal weights. Neighboring tax rates are instrumented with their predicted imposed increase based on the year 2000. Robust standard errors are in parentheses. Stars indicate significance levels at 1% (*), 5%(**) and 10%(***).

statistical and economic means.[70] These findings show that municipalities indeed respond strongly to the incentive caused by the increase in the hypothetical tax rate as outlined in Section 3.3.3.

The second stage shows that the spatial interaction effect is positive and significant. Therefore, municipalities seem to have reacted strategically to neighboring municipalities in their own county regarding their tax rate choices after the reform. An increase in one SD of the neighbors tax increase results in a substantial increase in the own tax rate by 20% $(0.314 \times 7.760)/12.822$). Moreover, the influence of the predicted imposed tax increase is 48.028 % of the SD.[71] Both effects allow for an interesting comparison because we can determine the degree of a direct response to the reform and to the response of neighboring decisions. Since the effect of 'own imposed increase' is larger than the neighboring interaction effect, we infer that politicians foremost respond to the policy change. This is an interesting result, since tax mimicking is mainly driven by the reform but is done only residually after adjusting ones' own tax rate to the policy change.

We also re-estimate our baseline model for various weighting matrices of both ge-

[70] An increase in one standard deviation (SD) of average neighboring tax imposed increase results in an increase in 75 % of the SD of the neighboring average increase in the tax rates.

[71] Using the SD of the dummy 'Non-zero own imposed increase (1/0)' (46.64 %) and the SD of own imposed increase (17.65), the effect on SD of the own tax increase is 48.03%. $((6.403 \times 0.4664) + ((17.647 \times 0.176)/12.687)$.

ographical and non-geographical nature. We present the most interesting results in Table 3.2 and a detailed overview of the results in Table 3.7 in Appendix 3.7. Interestingly, some institutional weighting matrices are of significance, whereas the geographical weighting matrices are not significant.[72] This might indicate that institutional proximity is more important for tax interactions in our setting than mere distance. Counties, administrations and functional media neighbor regions yield significant estimates and appear as important networks for local tax interactions. However, functional administrative regions do not show significant results. We attribute this to the large number of islands in this weighting matrix. Also aggregated media regions are not significant but we believe that the functional and more disaggregated media regions capture local variation in media access better. Functional cooperation in individual projects yields slightly significant tax interactions but, nevertheless, does not turn out to be robust against sensitivity checks. Cooperation in regional tourism and interest groups for political and societal issues are not significant either. Table 3.8 in Appendix 3.7 shows the robustness of selected institutional networks for local tax interactions. The disaggregated measure of common access to local media yields robust results. Joint administrations also appear widely robust but fail one placebo test in the pre-reform period.
We proceed with extensive robustness tests for the county matrix as our baseline weighting scheme and discuss the implications of different institutional matrices in Section 3.5.4.

Table 3.2: Institutional versus geographical weights

Model	Weighting type	ρ	SE	Kleibergen-Paap	AIC	BIC	N
(I)	Counties	0.314**	(0.157)	190.255	2960.422	3000.236	396
(II)	Administrative	0.271**	(0.137)	1607.584	2968.263	3008.077	396
(III)	Media functional	0.355***	(0.120)	1527.115	2966.355	3006.169	396
(IV)	Cooperation functional	0.229*	(0.124)	133.447	2969.105	3008.919	396

Notes: All presented matrices are row normalized. Control variables are the long differences of the population, the share young (<15yrs.) and old (>65yrs.), employed per capita, core budget debt per capita and short term debt per capita. Neighboring tax rates are instrumented with neighboring predicted imposed increase based on the year 2000. Furthermore, we also include the own imposed increase based on the year 2000 as well as a dummy whether own imposed increase is unequal to zero. Robust standard errors are in parentheses. Stars indicate significance levels at 10% (*), 5%(**) and 10%(***).

[72] Note that the confidence intervals of the institutional and geographical weighting matrices partly overlap. Nevertheless, the coefficients of the significant institutional matrices are have substantially larger interaction effects and model fit in terms of information criteria. For example, comparing the best best performing institutional and geographical matrix, the county and binary contiguity matrix, the interaction effect is 42% higher in the county matrix.

3.5.3 Robustness tests

For the reform to be arguably exogenous, own tax decisions during the reform should not be influenced by neighbors' tax decisions prior to the reform. We test this prediction with several placebo tests in Table 3.3. In Model I-III we regress the predicted tax changes of neighboring municipalities from 2002 to 2003 on tax changes in a municipality of years preceding the reform. In Model IV, we assigned a municipality to an arbitrary county. We do this by assigning a given municipality to all other municipalities in a county with the next higher county identifier number. This ensures that each county is only assigned once. Alternatively, we use historical county borders from 1960 as an additional placebo test in Model V to show the exogeneity of county borders.[73] County borders in West German states changed in an extensive wave of county and municipal merger reforms in the 1960s and 1970s. Hence, most county borders of today do not overlap with historical borders. If tax interactions between municipalities were biased by spatial autocorrelation of tax rates, tax rates should also be correlated across historical borders. If in fact today's institutions mattered for current tax decisions, interactions should be insignificant.

Table 3.3: Robustness checks

	Model	ρ	SE	Kleibergen-Paap F	N
(I)	Tax changes 1994 - 1993	-0.081	(0.112)	194.389	396
(II)	Tax changes 1995 - 1994	-0.018	(0.118)	204.934	396
(III)	Tax changes 1996 - 1995	-0.054	(0.135)	198.969	396
(IV)	W = Arbitrary county	-0.045	(0.083)	493.748	396
(V)	W = Counties 1960	0.173	(0.142)	127.085	396
(VI)	W = Contiguous neighbors from other counties	0.103	(0.173)	100.084	313

Notes: *W* assigns all municipalities in the same county as neighbors with equal weights in Models I-III. Model IV assigns municipalities form the county with the next higher county identifier as neighbors. Model V assigns municipalities based on the same county prior to the county mergers in 1960. Neighboring tax rates are instrumented in all models with their predicted imposed increase based on the year 2000. Control variables are the same as in Table 3.1. Model I uses the first difference of the control variables from 1992 until 1993. Model II uses the first difference of the control variables from 1993 until 1994. Model III uses the first difference of the control variables 1994 until 1995. Model IV and V use the first difference of the control variables from 2001 until 2002. Robust standard errors are in parentheses. Stars indicate significance levels at 10% (*), 5%(**) and 10%(***).

Note that throughout all specifications and years our instrument proves to be a strong predictor of neighboring tax rate changes. Regarding the robustness tests of Model I-III, the interaction effect of current neighboring tax changes on past tax changes is insignificant. This shows that our instrument significantly predicts

[73]The number of municipalities and counties decreased from 2365 to 396 and and from 57 to 30, respectively. There were more district-free cities, which results in 38 units without neighbors in our sample. For simplicity, we assume them to be each others' neighbors. However, dropping these observations does not change the results.

the actual reform but is not correlated via the error terms with earlier decisions. We also run placebo regressions where we gradually drop municipalities with more than 150,000 and 100,000 inhabitants. Results do not change qualitatively and show that large municipalities do not drive the effects of the reform. In addition, an insignificant interaction effect in Model IV indicates that the municipal decision to increase tax rates solely depends on its own county. In Model V we do not find significant effects which shows that not geographical proximity but current institutions of the county matter for local tax interactions. Counties seem to effectively coordinate contemporary tax policies at the local level.

We conduct further robustness checks in Table 3.9 in Appendix 3.7. Here, we omit control variables, cluster on the county level, include different regional dummies to account for regional heterogeneity and added accumulated contributions per county (*Kreisumlage*) as a control. Since our coefficient of interest does not differ much when excluding control variables in Model II, unobservable variation should not affect our variable of interest and we have suggestive evidence that our instrument is in fact exogenous (Altonji et al., 2005).[74] When clustering on the county level in Model III, the interaction effect remains significant. The addition of regional variables in row IV and V and county grants in row VI also do not change our findings qualitatively. Note that Model IV and Model V show that our results are not only accrue to a metropolitan area (Holzmann and Schwerin, 2015) or specific to a certain regional district, respectively. Moreover, we can show that contributions to the county are insignificant in Model VI (not reported) and thus, we can rule out the presence of vertical externalities Revelli (2003).[75]

We also compare our results to traditional estimates of tax interactions. Traditional IV estimates use neighbors characteristics as instruments for neighbors tax rates. Results are displayed in Table 3.10 in Appendix 3.7. F-statistics are much lower compared to our policy-change based instrument but are still nonweak by conventional standards. Both traditional spatial lag models and QML estimations yield tax interactions of higher significance and magnitude than our baseline model. In line with recent quasi-experimental literature on local tax interactions,

[74] Note that we keep the dummy's 'Non-zero own imposed tax increase' and 'Own increase' in our set of control variables as we only introduce exogeneity with our IV conditional on own incentives to increase the tax rates and avoid issues of spatial autocorrelation (Lyytikäinen, 2012). When omitting these factors tax interactions become highly overestimated.

[75] As a further exercise, we interacted tax interactions in different regressions with municipalities under fiscal supervision and municipalities that do not receive rule grants (i.e. are abundant). Tax interactions always remains significant but abundant and fiscally supervised municipalities, respectively do not react to their neighbors during the reform. The results are available from the authors upon request.

our findings cast some doubt on the validity of traditional instruments (Gibbons and Overman, 2012).

3.5.4 Discussion

Social learning and institutions

In Section 3.2 we argued that social learning is a likely motive for tax mimicking during the reform in question. Here, we present suggestive evidence to support this notion. Social learning is needed as individual municipalities or local politicians in particular are unlikely to be successful in predicting the outcomes, for example future grant allocation, as a result of the reform. This holds particularly for a reform of the complex grant system of local fiscal equalization in NRW. Instead, local politicians and bureaucrats need to communicate tax strategies to resolve the inherent uncertainty of the reform.

Moreover, social learning during the reform should be a one-step learning process rather than a continuous process as new information has to be communicated only once. In fact, we find supportive evidence for this in Table 3.4, where tax interactions become insignificant two years after the reform and effects disappear gradually. The sharp drop of the effect from 2007-2008 might reflect a federal reform of the business tax.[76]

The structure of tax interaction intensity provides another argument for social learning. Tax policies should be communicated on platforms where politicians or bureaucrats are likely to meet such as the county parliament or joint administration offices. Also local media might be an effective means of knowledge spillovers for tax policies. Other institutional cooperations such as inter-municipal cooperation for individual projects, cooperation in tourism or regional marketing or interaction with local interest groups are, however, less reasonable channels of information for local tax policies. Accordingly, we only find significant interactions within similar institutions and media as shown in Table 3.7 in Appendix 3.7, but not for neighbors measured by geographical distance only. This leads us to the conclusion, that we indeed observe social learning through the reform as reform outcomes are hard to predict for individual municipalities and coordination via counties, media and within bureaucracies is a feasible coping mechanism.

The absence of effects with geographical distance matrices shows that tax interac-

[76] Büttner et al. (2014) show that this reform induced municipalities with many non-incorporated firms to increase their tax rates as a response of the new business tax deductability to the income tax to be paid by those firms.

tions can be triggered by shared institutions or media rather than by geographical distance only. Moreover, while counties, common administrations and media are valuable communication platforms and intensify tax interactions during the reform, voluntary project cooperation of municipalities is only slightly significant.[77] Table 3.7 in Appendix shows that also local interest groups and cooperation in regional marketing and tourism do not yield significant interactions. Table 3.8 in Appendix shows the robustness of selected weighting schemes. Administration and media weights prove to be widely robust and show similar effects as the county weights. Single project cooperation, however, yields only significance at the 10% level and depends largely on the inclusion of control variables.

Table 3.4: Long run results

	Dependent Variable: $\Delta\tau_{i,t-2002}$					
	(I) t = 2003	(II) t = 2004	(III) t = 2005	(IV) t = 2006	(V) t = 2007	(VI) t = 2008
ρ	0.314**	0.345***	0.204	0.195	0.165	0.047
	(0.157)	(0.134)	(0.135)	(0.131)	(0.161)	(0.169)
Kleibergen-Paap F	190.255	258.486	242.926	186.253	178.527	169.204
Control Variables	Δ2002 − 2001	Δ2003 − 2001	Δ2004 − 2001	Δ2005 − 2001	Δ2006 − 2001	Δ2007 − 2001
N	396	396	396	396	396	396

Notes: W assigns all municipalities in the same county as neighbors with equal weights. All control variables are the same like in Table 3.1. Neighboring tax rates are instrumented with their predicted imposed increase based on the year 2000. Robust standard errors are in parentheses. Stars indicate significance levels at 10% (*), 5%(**) and 10%(***).

Evidence on competing explanations for tax interactions

We provide suggestive evidence in this section that social interactions and not other competing motives are the reason for the observed tax mimicking instead.
Table 3.5 tests for the presence of yardstick competition as well as tax competition. Both Model I and II interact local tax interactions with a measure of absolute majorities in the local council which represents a standard test of the yardstick competition hypothesis Allers and Elhorst (2005); Elhorst and Freret (2009). Majorities in the local council should decrease tax interactions if neighbor tax policies were effective yardsticks. Since the interaction effects with the majority term are insignificant, we are able to rule out yardstick competition as an explanation.
Moreover, Model III and IV test for tax base effects during the reform, i.e. the new dependent variable is the tax base of a given municipality. If competition for mobile tax bases was in place, one would observe significant effects in the respective models. Tax base effects also do not seem to be present and therefore,

[77] We regard the interaction effects of the inter-municipal cooperation scheme as a lower bound, since not all cooperation's could be included in our cooperation variable (Terfrüchte, 2015).

tax competition is not an issue in the present context. Also Baskaran (2015) does not find significant tax revenue or base effects of the reform in question. The timing of treatment effects is another reason against tax competition. We only find short-term effects whereas tax competition for mobile resources can be expected to trigger a continuous tax game. We also rule out benefit spillovers as we do not observe any negative tax interactions.

Another explanation for local tax interactions is (partial) coordination. While we do observe a strong role of counties in local policy making, active coordination through counties is unlikely as this implies a perfect harmonization of tax rates within a given county. However, business tax rates are still somewhat heterogeneous although they were synchronized substantially after the reform. There is no anecdotal evidence either that county executives dictate new tax rates for member communes. After all, counties also do not have legal tax autonomy. Therefore, counties and other institutions can be rather understood as a platform for local politicians or bureaucrats to communicate individual tax strategies. The effect of media does not seem to be voter driven as there are no differences between municipalities with or without narrow majorities. Therefore, media could also work as a mere communication platform for politicians and bureaucrats themselves to pick up information which are not spread in other ways such as county parliaments, joint bureaucracies or other forms of inter-municipal cooperation. Therefore, not active but rather passive coordination in form of knowledge diffusion seems to be in place.

Partial coordination is also unlikely to be present as it implies a repeated game structure and one would therefore expect to observe continuous tax interactions. However, tax interactions phase out quickly after the reform and have no effects on the tax base.

Table 3.5: Determination of interaction channels

Dependent Var:	$\Delta_{i,2003-2002}$ tax rate		$\Delta_{i,2003-2002}$ Taxbase per capita	
	subset: Majority > 50% (I)	subset: Majority > 55% (II)	W = County (III)	W = Binary contiguity (IV)
ρ	0.307*	0.346**	-0.179	-0.145
	(0.176)	(0.167)	(0.319)	(0.268)
$\rho\times$ subset	0.010	-0.065		
	(0.080)	(0.076)		
Kleibergen-Paap F	93.856	93.025	189.555	141.540
N	396	396	396	396

Notes: W assigns all municipalities in the same county as neighbors with equal weights. Neighboring tax rates as well as the interactions are instrumented with their predicted imposed increase based on the year 2000. All control variables are the same like in Table 3.1. Majority always represents an interaction term with the neighboring tax rate, when a party in the council has more than x percent of the seats in the municipal council. Robust standard errors are in parentheses. Stars indicate significance levels at 10% (*), 5%(**) and 10%(***).

3.6 Conclusion

This paper exploits a quasi-experimental setting in local fiscal equalization in the German state of NRW, to show the existence of local tax interactions for various weighting matrices. Using instrumental variable techniques and detailed information on local networks, we show that the reform in question triggered positive tax interactions immediately after the reform only. Our results are robust to various specification tests, including several placebo tests and random institution allocation.

Municipalities of the same county, those with the same bureaucrats or local media interact most intensely with each other. Other platforms like project-wise inter-municipal cooperation or interest group coverage do not intensify tax interactions. Hence, counties, joint administration and media are effective coordination mechanisms for local tax policies during the reform in question. These are typically also networks where local politicians and bureaucrats exchange information on salient political issues. We also show that institutional rather than geographical proximity matters for tax interactions.

These results are in line with the idea that municipalities engage in social learning in reform times. As reforms entail substantial political uncertainty, institutions like counties or joint bureaucracies may offer a communication platform for local politicians and bureaucrats to cope with uncertainty regarding future tax policy choices. Local media offer similar coordination devices. Baskaran et al. (2015) finds similar evidence on social learning in local tax choices between East German border municipalities with their West German counterparts immediately after German reunification. Tax or yardstick competition and benefit spillovers, however, do not seem to be drivers of the results.

Our results can be extended to other multi-tier federal contexts where federal and central state legislation (vertically) influences local parameters of fiscal policy, for example other German states (Büttner and von Schwerin, 2016), the US (Agrawal, 2015a), England (Revelli, 2003) or France (Breuillé et al., 2011). Büttner and von Schwerin (2016) show the importance of federal or state-wide institutional tax rates (for e.g. the hypothetical tax rate) which represent reference rates for most German states. NRW might be, however, a special case as municipal debt, tax rate levels and the share of aggregate local to state expenditures are comparably high in the German context (Arnold et al., 2015). Also its high average municipal size compared to other German states may imply that NRW municipalities are on average more professional in local policy making.

Future research might ask whether social learning via institutions is an efficient mechanism to coordinate local responses during a fiscal macro shock. Also knowledge diffusion through institutions or media for other local policies should be examined. Other policies of high relevance that might need central coordination are for example the provision of kindergarten places or the efficient allocation of refugees at the local level.

3.7 Appendix - Tables

Table 3.6: Descriptive statistics

	Mean	Std. Dev.	Min	Max
	2003			
Business tax rate	412.058	23.150	310	490
Change business tax rate (1/0)	.646	.479	0	1
Own imposed increase	16.475	17.648	0	103
Own imposed increase (1/0)	0.682	0.466	0	1
Population	45,655	86,863	4261	965,954
Share young	0.171	.018	.127	.238
Share old	0.174	.020	.115	.259
Employed per capita	0.252	.091	.052	.560
Rule-grants per capita	197.867	117.177	0	607.474
Total grants per capita	195.342	102.503	3.317	563.457
Short term debt per capita	147.289	315.416	0	2775.493
Core debt per capita	881.916	583.296	0	3739.515
	$\Delta 2002-2003$			
Δ Business tax rate	13.609	12.687	-2	70
Δ Population	8.412	637.410	-2685	8865
Δ Share young	-0.003	0.002	-0.009	0.0008
Δ Share old	0.005	0.002	-0.002	0.011
Δ Employed per capita	-0.006	0.008	-0.057	0.027
Δ Rule-grants per capita	-14.531	56.392	-188.359	198.959
Δ Total grants per capita	-56.265	56.019	-227.347	194.742
Δ Short term debt per capita	52.672	114.59	-181.78	638.62
Δ Core debt per capita	34.732	133.14	-1090.79	862.62

Source: Own calculations based on official statistics provided by the Federal Statistical Office.

Table 3.7: Comparison of action space based weighting matrices

Weighting type	ρ	Kleibergen-Paap	AIC	BIC	Rank AIC & BIC
	Institutional weighting matrices				
Counties	0.314** (0.157)	190.255	2960.422	3000.236	(1)
Administration	0.271** (0.137)	1607.584	2968.263	3008.077	(14)
Media	0.119 (0.141)	2555.645	2973.952	3013.767	(21)
Social and economic	0.188 (0.154)	249.789	2966.875	3006.689	(8)
Regional marketing	0.227 (0.142)	1069.799	2970.831	3010.645	(19)
Cooperation	0.225 (0.140)	2247.738	2972.044	3011.858	(20)
Media functional	0.355*** (0.120)	1527.115	2966.355	3006.169	(5)
Cooperation functional	0.229* (0.124)	133.447	2969.105	3008.919	(15)
Social functional	0.218 (0.147)	189.078	2961.399	3001.213	(2)
Administration functional	0.114 (0.160)	235.091	2970.377	3010.191	(18)
	Geographical weighting matrices				
Binary Contiguity	0.221 (0.145)	140.628	2963.362	3003.176	(3)
Contiguity Second Order	0.209 (0.176)	404.886	2966.4	3006.214	(6)
5 nearest neighbors	0.119 (0.152)	151.059	2967.266	3007.08	(10)
10 nearest neighbors	0.221 (0.166)	240.567	2968.184	3007.998	(13)
15 nearest neighbors	0.188 (0.163)	413.748	2966.703	3006.517	(7)
20 nearest neighbors	0.191 (0.162)	556.770	2969.169	3008.984	(16)
25 nearest neighbors	0.195 (0.170)	872.759	2969.666	3009.48	(17)
Inverse Distance 15 km	0.201 (0.152)	158.857	2967.598	3007.412	(12)
Inverse Distance 20 km	0.257 (0.165)	227.420	2967.028	3006.843	(9)
Inverse Distance 25 km	0.250 (0.168)	353.036	2965.198	3005.012	(4)
Inverse Distance 30 km	0.245 (0.167)	617.861	2967.386	3007.2	(11)

Notes: All presented matrices are row normalized. Control variables are the same as in Table 1. Neighboring tax rates are instrumented with their predicted imposed increase based on the year 2000. All control variables are the same like in Table 3.1. Robust standard errors are in parentheses. Stars indicate significance levels at 10% (*), 5%(**) and 10%(***).

Table 3.8: Robustness of relevant institutional weighting matrices

Model	W = Administration ρ	W = Administration Kleibergen-Paap	W = Media functional ρ	W = Media functional Kleibergen-Paap	W = Cooperation functional ρ	W = Cooperation functional Kleibergen-Paap
Baseline	0.271**	1607.584	0.355***	1527.115	0.229*	133.447
	(0.137)		(0.120)		(0.124)	
No covariates	0.241*	1562.300	0.327***	1651.484	0.190	167.551
	(0.138)		(0.120)		(0.124)	
Placebo tests						
Tax changes 1994 - 1993	0.044	1608.995	0.031	1642.932	-0.088	159.853
	(0.098)		(0.099)		(0.081)	
Tax changes 1995 - 1994	-0.200**	1611.568	-0.085	1707.909	-0.045	161.812
	(0.094)		(0.088)		(0.088)	
Tax changes 1996 - 1995	-0.130	1631.992	-0.161	1767.639	-0.041	162.615
	(0.136)				(0.110)	
Long run results						
Tax changes 2004 - 2002	0.251*	1634.552	0.351***	1397.958	0.249**	124.360
	(0.131)		(0.115)		(0.119)	
Tax changes 2005 - 2002	0.141	1186.243	0.221*	983.390	0.163	222.830
	(0.137)		(0.119)		(0.103)	
Tax changes 2006 - 2002	0.174	819.592	0.240**	688.715	0.160	213.374
	(0.139)		(0.122)		(0.107)	
Tax changes 2007 - 2002	0.133	857.318	0.191	645.068	0.148	174.992
	(0.148)		(0.136)		(0.116)	
Tax changes 2008 - 2002	0.057	723.986	0.117	477.690	0.058	172.563
	(0.164)		(0.148)		(0.121)	

Notes: All presented matrices are row normalized. Control variables are the same as in Table 1. Neighboring tax rates are instrumented with their predicted imposed increase based on the year 2000. All control variables are the same like in Table 3.1. Robust standard errors are in parentheses. Stars indicate significance levels at 10% (*), 5%(**) and 10%(***).

Table 3.9: Robustness checks

	Model	ρ	SE	Kleibergen-Paap F	N
(I)	Baseline	0.314**	(0.157)	190.255	396
(II)	No covariates	0.297*	(0.156)	201.232	396
(III)	Clustering county	0.314**	(0.137)	26.322	396
(IV)	Ruhr region FE	0.287*	(0.167)	177.150	396
(V)	Nuts2 FE	0.287*	(0.161)	208.064	396
(VI)	Share county	0.310*	(0.160)	188.643	396

Notes: W assigns all municipalities in the same county as neighbors with equal weights. Neighboring tax rates are instrumented with their predicted imposed increase based on the year 2000. All control variables are the same like in Table 3.1. Model II does not include control variables except the own imposed increase and the respective dummy. Model III clusters standard errors at the county level. Model IV includes a dummy that indicates the affiliation of the municipality to the Ruhr region. Model V includes dummy's that indicate the respective NUTS2 region. Model VI include the first-difference of the shared costs of the county from 2001 to 2002 (*Kreisumlage*) in the set of control variables. Robust standard errors are in parentheses. Stars indicate significance levels at 10% (*), 5%(**) and 10%(***).

Table 3.10: Traditional Spatial Econometric estimates

	Traditional Spatial IV (I) $\Delta\tau_{i,2003} - \tau_{i,2002}$	Quasi Maximum Likelihood (II) $\Delta\tau_{i,2003} - \tau_{i,2002}$
ρ	0.874***	0.540***
	(0.102)	(0.056)
Kleibergen-Paap	71.962	
Hansen J (p-val)	0.6575	
N	396	396

Notes: W assigns all municipalities in the same county as neighbors with equal weights. Model I uses the neighboring changes of all control variables as instrumental variables for neighboring tax changes. Model II performs a quasi-maximum likelihood estimation on equ. (3.5) above. All control variables are the same like in Table 3.1. Robust standard errors are in parentheses. Stars indicate significance levels at 10% (*), 5%(**) and 10%(***).

Table 3.11: Political weighting matrices

	Model	ρ	SE	Kleibergen-Paap F	N
(I)	County	0.277*	(0.153)	192.209	390
(II)	Radius 30km	0.117	(0.156)	756.885	394
(III)	Radius 50km	0.184	(0.168)	1874.137	396
(IV)	Radius 100km	0.163	(0.189)	1155.919	396

Notes: W assigns all municipalities with a relative majority of the same party (SDP,CDU) in the regional criterion as neighbors with equal weights. Neighboring tax rates are instrumented with their predicted imposed increase based on the year 2000. All control variables are the same like in Table 3.1. Model I includes all municpalities in the same county with same political relative majority as neighbors. Model II includes all municpalities in a Radius of 30km with same political relative majority as neighbors. Model III includes all municpalities in a Radius of 50km with same political relative majority as neighbors. Model IV includes all municpalities in a Radius of 100km with same political relative majority as neighbors. If N<396, municipalities where removed because they did not have neighbors with the given criterion. Robust standard errors are in parentheses. Stars indicate significance levels at 10% (*), 5%(**) and 10%(***).

3.8 Appendix - Figures

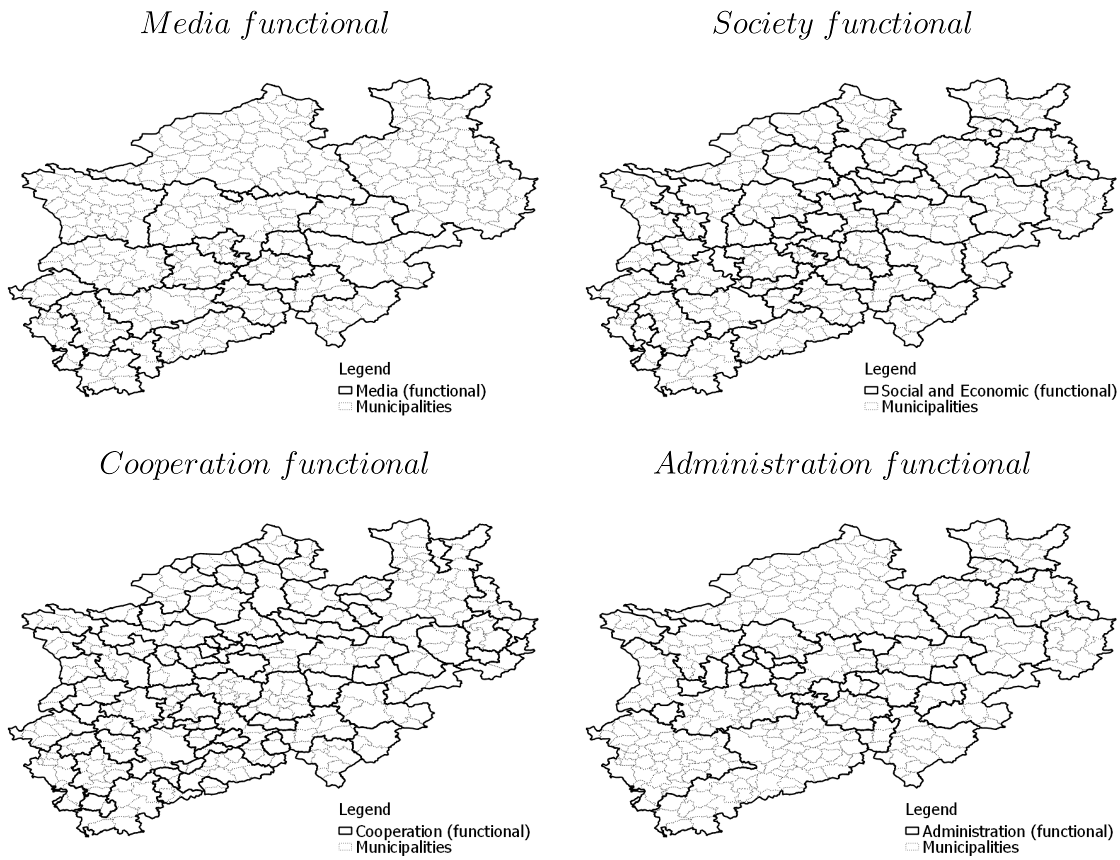


Figure 3.4: Institutional functional weighting matrices. Notes: Own coding and mapping based on Terfrüchte (2015)

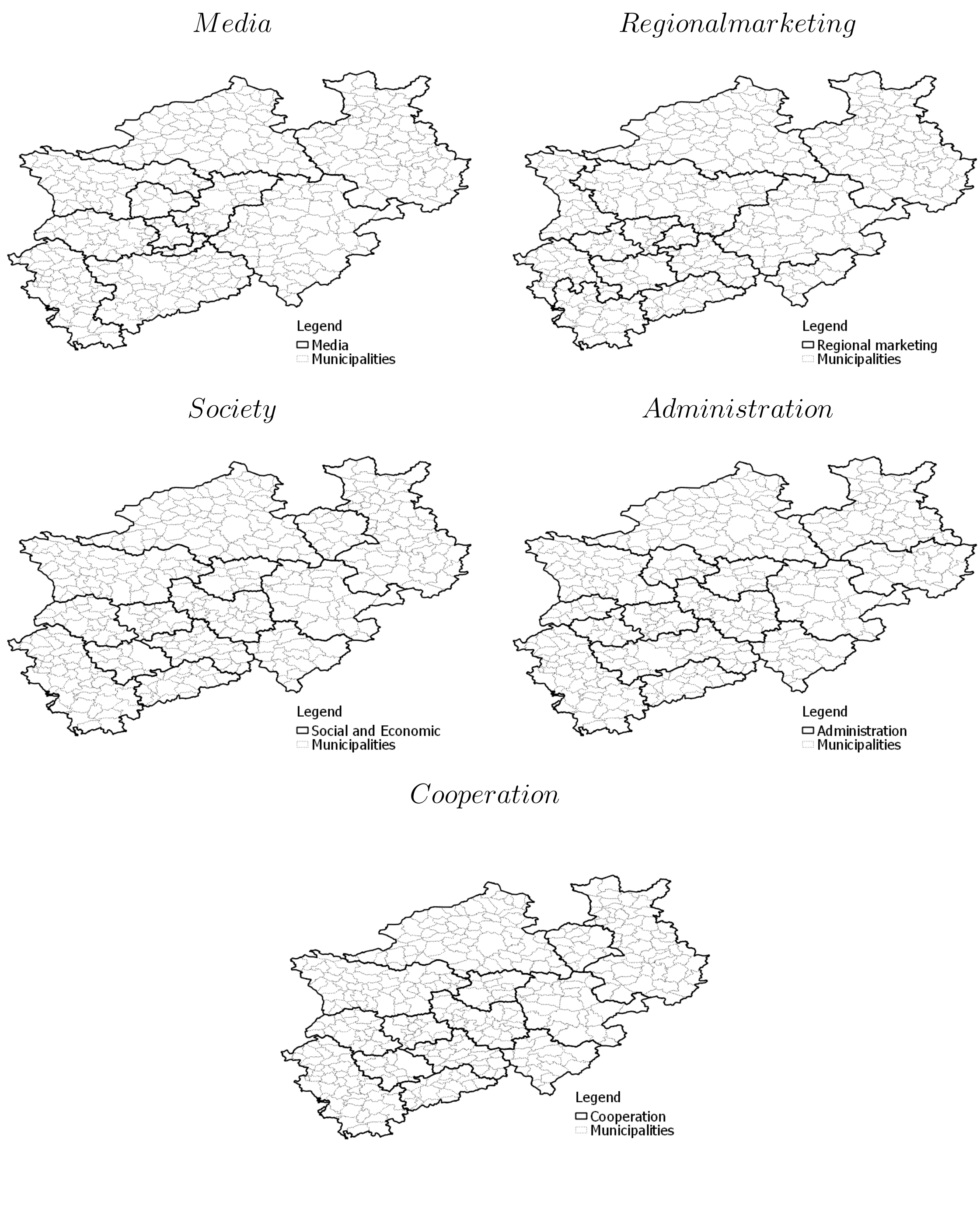


Figure 3.5: Illustration of the institutional weighting matrices. Notes: Own coding and mapping based on Blotevogel et al. (2009).

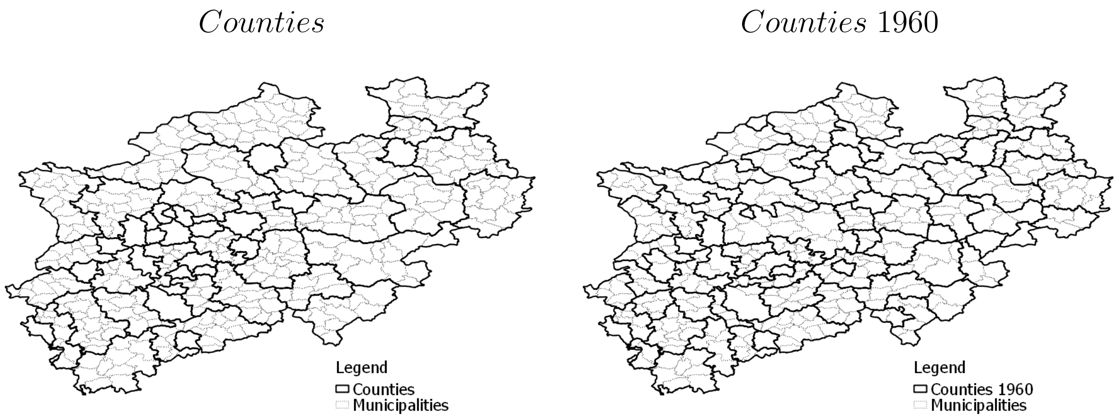


Figure 3.6: County weighting matrices

3.9 Appendix - Data description

3.9.1 Description of the institutional weighting schemes

The county weights reflects the existing counties. All other weighting schemes are obtained from Blotevogel et al. (2009) and Terfrüchte (2015). Blotevogel et al. (2009) use a more heuristic approach that observes whether different institutions within the same category share the same border. For each region, the authors plotted the radii of each institution on a map and aggregated municipalities to the regions, when they shared borders along these different institutions. Terfrüchte (2015) uses a functional approach where regional correlations between the institutions are used to construct regional action spaces. Whereas the approach by Blotevogel et al. (2009) is more oriented on existing borders, the approach by Terfrüchte (2015) is more functional. The first approach maps the borders of existing institutions and aggregates these by common overlaps, whereas the second approach measures the related regional correlation of institutions and constructs regions from these correlations. These action spaces are the basis for our weighting matrices in Table 3.7. In certain institutional setups, some municipalities end up as islands, i.e. do not have any neighbors. For simplicity, we assign these municipalities a zero for neighboring tax changes.

Table 3.12: Definitions of non-geographical weighting matrices - institutional proximity and media

Weighting scheme	Aggregated institutions	Units (excl. islands)	Mean links	Islands
Administrative (functional)	Labor court districts, bureau of standards districts, Chamber of agriculture. land court districts, Bau- und Liegenschaftsbetrieb NRW, NUTS2 regions, regional forestry commission office, public road construction, social court districts, local rail transports, regional planning institutions and administrative court districts	15 (24)	35.22 (31.67)	0 (12)
Cooperation (functional)	Voluntary local cooperation projects between municipalities	9 (75)	58.19 (8.24)	1 (5)
Media (functional)	Local newspapers, local radios and local television	11 (22)	49.72 (33.34)	0 (0)
Social and economic (functional)	Industrial chamber of commerce and chamber of crafts districts, regional associations of political parties, districts of employers' associations and unions and districts of environmental associations	14 (47)	37.55	0 (5)
Regional marketing	Local tourist associations and regional marketing initiatives	14	43.01	0

Notes: *Source*: All institutional characteristics were obtained by Blotevogel et al. (2009). Institutional setups with the suffix "functional" are obtained from Terfrüchte (2015).

Chapter 4

Entrepreneurial spillovers over space and time[§]

4.1 Introduction

Entrepreneurship is a local and dynamic phenomenon. Regarding locality, entrepreneurship tends to prosper in certain regions, the Silicon Valley/Bay Area in California or the Rhine-Main-Neckar region in southwest Germany being famous examples. Researchers recognize the regional embeddedness of entrepreneurship, and policymakers are interested in developing locally tailored policies to stimulate entrepreneurship in regions (see Fritsch and Storey, 2014, for a review of the literature). Despite the high awareness of the importance of the regional context for entrepreneurship, very little is known about spatial spillovers of entrepreneurship, or more specifically, start-up activity, into neighboring regions. Klotz (2004) and Audretsch and Keilbach (2007) provide initial evidence for the significance of such spillovers, but based on cross-sectional data without consideration of the time dimension. Regarding the time dynamics, high levels of new business formation seem to be very persistent within certain regions over time (e.g., Fritsch and Mueller, 2007; Andersson and Koster, 2011). Fritsch and Wyrwich (2014, 2017) report that regional differences in the levels of self-employment and new business formation in Germany persisted from 1925 to 2005 despite major disruptions such as World War II and forty years of a socialist regime in East Germany.

The ability of regions to generate a high start-up rate of new businesses is some-

[§] This chapter is based on joint work with Frank M. Fossen. A prior version has been published as a DIW Discussion Paper, see Fossen and Martin (2016).

times termed entrepreneurship capital (e.g., Audretsch et al., 2008). Besides regional economic opportunities, the industry structure and human capital relevant for entrepreneurship, entrepreneurship capital includes formal and informal local institutions favorable toward entrepreneurship.[78] The strong time persistency of entrepreneurship capital suggests that regional entrepreneurship culture, which changes very slowly, is an important part of it. Entrepreneurship culture describes intangible components of entrepreneurship capital, such as regional cultural norms and values that shape attitudes toward entrepreneurship (e.g., Fritsch and Wyrwich, 2017), personality traits (Sutter, 2008; Caliendo et al., 2014; Stuetzer et al., 2016) and creativity (Lee et al., 2004) prevalent in the region. Entrepreneurship culture is considered the most enduring component of entrepreneurship capital, because it may persist even if formal institutions and business opportunities are disrupted (Fritsch and Wyrwich, 2014).

Entrepreneurship capital manifests itself in the observed regional start-up rate. Thus, if we econometrically find spatial interaction in start-up rates, this indicates that entrepreneurship capital spills over to neighboring regions and suggests positive external effects. A time persistency in start-up rates confirms that entrepreneurship capital can build up and remain productive in a region for a long time, which is at the center of the concept of capital.

A better understanding of the time and spatial dynamics of entrepreneurship capital is important because the literature shows that entrepreneurship capital matters for regional growth. Audretsch and Keilbach (2004) and Pijnenburg and Kholodilin (2014) assess the returns to entrepreneurship capital by estimating production functions based on regional data for Germany. They operationalize entrepreneurship capital as start-up rates, as we do in this paper. Audretsch and Keilbach (2004) report that entrepreneurship capital increases regions' output in terms of GDP. Pijnenburg and Kholodilin (2014) focus on knowledge intensive industries and estimate a production function in a spatial econometric model, taking into account the effect of neighboring regions on local output. Their results suggest that not only more entrepreneurial regions, but also regions with more entrepreneurial neighbors perform better, although the spatial spillover effects are statistically insignificant in most of their specifications. Acs et al. (2009b) estimate spatial panel models of regional personal income growth and report that new high-tech venture creation has a positive influence. Carree and Thurik (2003) conclude from their literature review and own analysis that entrepreneurship has

[78] For factors explaining regional entrepreneurship see, e.g., Glaeser et al. (2010), Ghani et al. (2014), Glaeser et al. (2015), and Minniti (2005) on the role of the social environment.

a positive impact on growth.

One way how start-up activity may spill over inter-temporally and inter-regionally is via spillovers of knowledge created in start-up companies.[79] Arguably, geographical distance matters for knowledge spillovers because knowledge tends to be regionally bounded (Anselin et al., 1997; Boschma, 2005; Asheim and Gertler, 2006; Fritsch and Aamoucke, 2013, 2017). Even in a globalized world where physical distances partially lose importance due to electronic communication and virtual work spaces, local proximity and face-to-face interactions still seem to be crucial for spillovers of tacit knowledge and entrepreneurship (cf., Leamer and Storper, 2001; Kaufmann et al., 2003; Scott, 2006). However, little is known about the critical distances for entrepreneurial interaction in space and time. Our research question is thus the following: How much do entrepreneurial spillovers diminish in strength over geographical distance and over time?

In this paper, we are the first to investigate the spatial and time dynamics of entrepreneurship jointly in a consistent spatial econometric framework. We analyze how start-up activity spreads over to neighboring regions and its persistency over time. We use the Mannheim Enterprise Panel for the years 1996-2011 to measure firm births and consider complete sets of German regions of different sizes: all 402 German counties (NUTS 3 level), 258 labor market regions, which are defined as commuting zones, and 96 larger spatial planning regions. Rosenthal and Strange (2003) emphasize the importance of considering the geographic scope of spillovers for firm births. We econometrically eliminate unobserved time and regional unit fixed effects, which would otherwise be likely to bias the estimations. This allows us to identify spatial interactions of changes in regional start-up activity as well as path dependency in start-up rates. We avoid an arbitrary choice of the spatial weighting matrix and instead apply a systematic grid search to find the parameterized matrix that best reflects regional interactions of start-up activity (cf. Gibbons and Overman, 2012; Elhorst and Vega, 2015).

We consider firm formation in the high-tech industry (in both the manufacturing and services sectors) and in the manufacturing sector (including both low-tech and high-tech businesses). The high-tech industry is of particular importance for innovation and economic growth (cf. Audretsch and Keilbach, 2004; Audretsch et al., 2006; Shane, 2009). In addition, the manufacturing sector in Germany is

[79] Thereby our analysis is also related to the knowledge spillover theory of entrepreneurship (Audretsch et al., 2006; Acs et al., 2009a). In this theory, knowledge created endogenously results in intra-temporal knowledge spillovers, which allow entrepreneurs to identify and exploit opportunities not appropriated by incumbent firms. This paper extends the perspective by not only considering spillovers over time, but also over space.

of high interest because Germany is well-known for its small and medium-sized specialized manufacturing businesses that are often world leaders in their global niche markets, sometimes called "hidden champions" (Simon, 2009).

Based on our estimated models, we simulate impulse response functions showing that 41% of the immediate response to a temporary and local impulse in the high-tech start-up rate takes place within a distance of 100km (62 miles) from the local origin of the shock. This is about the distance between Frankfurt/M. and Walldorf in the Rhine-Main-Neckar region and more than the distance between San Francisco and San Jose (80km) in the Bay Area. Most of the response happens within a period of about two years after the time of the shock. Intertemporal and spatial spillovers are stronger in the high-tech than in the general manufacturing industry. This results is highly suggestive of the importance of knowledge spillovers.

The remainder of this paper is structured as follows. Section 4.2 discusses the dynamic spatial econometric model we estimate. In Section 4.3, we describe our panel data of German regions. The econometric results and impulse response simulations are presented in Section 4.4, and Section 4.5 concludes the analysis.

4.2 Dynamic spatial panel model

In order to investigate intertemporal and spatial spillovers of regional start-up activity, we estimate dynamic Spatial Durbin Models:[80]

$$y_{i,t} = \tau y_{i,t-1} + \rho \sum_{j=1}^{N} w_{i,j} y_{j,t} + \eta \sum_{j=1}^{N} w_{i,j} y_{j,t-1} + \mathbf{x}_{i,t}\beta + \sum_{j=1}^{N} w_{i,j}\mathbf{x}_{j,t}\theta + \mu_i + \delta_t + \epsilon_{i,t} \quad (4.1)$$

The endogenous variable $y_{i,t}$ is the log start-up rate in region i and year t, or more precisely, the log of the number of newly founded businesses per 10,000 inhabitants (see Section 4.3). We include the time lag $y_{i,t-1}$ with the intertemporal autoregressive coefficient τ, which captures path dependency of start-up activity within the geographical region. Potential spillovers from other regions are modeled by including spatial lags $\sum_{j=1}^{N} w_{i,j} y_{j,t}$, where N is the total number of regions in Germany and $w_{i,j}$ is the spatial weight. It represents element (i, j) of a nonnegative $N \times N$ spatial weighting matrix W, which defines the neighboring regions.

[80] Because of its generality and flexibility, the Spatial Durbin Model produces unbiased coefficient estimates for a broad range of data-generation processes Elhorst (2010a).

The weight $w_{i,j}$ may be zero when the distance between regions i and j is large. We discuss the precise definition of W further below. The spatial autoregressive coefficient ρ to be estimated indicates the strength of spatial spillovers of regional start-up activity. Hence, τ represents spillovers of start-up activity from within the geographical region over time, whereas ρ represents spillovers from neighboring regions. The $1 \times K$ vector $\mathbf{x}_{i,t}$ includes the control variables and the $K \times 1$ vectors β and θ the coefficients that reflect the influences of these variables on the same region and on the neighboring regions of the focal region. The model includes unobserved region fixed effects μ_i and year fixed effects δ_t, and $\epsilon_{i,t}$ is the remaining error term.

In our baseline specification, we do not include a spatial lag of the time lag of the dependent variable, i.e., we use the restriction $\eta = 0$, as often done in the literature (e.g., Elhorst, 2004, 2010b; Devereux et al., 2007). Parent and LeSage (2012) show that under certain assumptions, nonzero spatial and time autocorrelation coefficients ρ and τ imply $\eta = -\rho\tau$. Therefore, as a specification check we also estimate a model including the spatial lag of the time lag and imposing $\eta = -\rho\tau$. As control variables in $\mathbf{x}$, in our main estimations spanning 1996-2011, we use regional real gross value added per employee, the population size of the region, and variables describing the age structure of the population and the education structure of the employees. In additional estimations using the period of 2001-2011, when more variables are available, we also include the industry structure, unemployment rate, and the average wage per employee in the region.[81] All variables enter the model in logarithmic form, so the coefficients reflect elasticities; for comparison we also provide results for level equations. Because we account for region fixed effects, we control any time-invariant factors such as geographical conditions, climate, natural resources, and long-term infrastructure. Since the region fixed effect also captures the area size of a region, by including the population size we also account for the population density. By additionally including the spatial lags of all control variables in the model, we are able to separate influences of neighboring regions on a focal region's start-up rate through the neighbors' characteristics such as their population size from direct spillovers of neighboring start-up activity.

We estimate the model using the Quasi Maximum Likelihood (QML) estimator for dynamic spatial panels developed by Lee and Yu (2010b,c). Simply including region and time dummy variables would lead to the incidental parameter prob-

[81] See Table 4.1 in Section 4.3 for a more detailed description of the variables.

lem and inconsistent estimates. Therefore, the estimator we use eliminates the region and time fixed effects by a double data transformation procedure and corrects the bias that would otherwise occur.[82,83] For our purposes, we adapt an implementation in Matlab provided by Elhorst (2012).[84]

Regions are assigned as neighbors ex-ante via the definition of a spatial weighting matrix W. Regions that are at a closer geographical distance to a given region receive a higher weight, indicating that the regions are neighbors. As Gibbons and Overman (2012) point out, the consistency of the QML estimator rests on the assumption that the true connectivity matrix W is known. They criticize that most applied papers using QML only examine a single matrix or an arbitrary set of pre-defined matrices. To take this remark into account, we follow the idea of Elhorst and Vega (2015) and apply a systematic grid-search procedure to find the weighting matrix that best describes the data.

The literature outlined in the introduction predicts that geographically closer regions will have a stronger influence on a region in terms of start-up activity than more distant regions, but theory does not give guidance on how quickly the influence diminishes when distance increases. This is an empirical question that we aim to answer in this paper. We use an inverse distance matrix, where all regions are assigned as neighbors, but closer neighbors receive a larger weight. We parameterize the spatial weighting matrix to allow for a flexible distance decay and search for the most appropriate matrix. Let $d_{i,j}$ denote the geographical distance between the centroids of two regions i and j (in km). Similarly to Elhorst and Vega (2015), we use a power inverse distance matrix and compute the spatial weights $w_{i,j}$ using the formula $w_{i,j} = \frac{1}{d_{i,j}^{\gamma}}$, where the exponent γ is a positive distance decay parameter to be determined. When $\gamma = 1$, we obtain the standard form of an inverse distance matrix. When γ increases, the influence of neighboring regions decreases more quickly with their distance, as illustrated in Figure 4.10 in Appendix 4.7. We normalize the spatial weighting matrix with its largest eigenvalue to maintain the economic interpretation of the distance.

We employ a systematic search for the distance decay parameter γ that best describes the data. We re-estimate model (4.1) fifty times with different values

[82] Borck et al. (2015) use a similar model and estimator to analyze spatial interaction and time dynamics in local government debt.

[83] The QML estimator is consistent even without the assumption of a normally distributed error term if the number of neighbors with an influence does not become too large (Lee, 2004).

[84] In the specifications including the spatial lag of the time lag, we adjust the log-likelihood function used by Lee and Yu (2010b,c) to incorporate the restriction $\eta = -\rho\tau$. Parent and LeSage (2012) use a Monte Carlo Chain Method instead.

of γ, starting with $\gamma = 0.1$ and then gradually increasing γ in steps of 0.1 until it reaches 5.[85] This allows us to compare the estimated coefficients and the log-likelihood values over a wide range from highly localized to very distant spatial interactions. Based on the highest log-likelihood, we select the best feasible γ (Elhorst, 2010b).[86] As a second search dimension we also systematically explore the introduction of different cutoff distances beyond which neighbors are assumed to have no influence at all (zero weights). By finding an optimal spatial weighting between close and distant neighbors, this systematic approach delivers a good approximation of the underlying inter-regional connectivity of start-up activity. Furthermore, this procedure allows us to inspect the sensitivity of the estimated coefficients with respect to different weighting matrices.

4.3 Spatial panel data

For our analysis we use panel data of three sets of German regions that differ in the sizes of the geographical units. Each set covers the complete area of Germany for the period of 1996-2011. The set with the smallest regional units, which we use in our main estimations, includes all 402 German counties. These are administrative units that cover a city or several municipalities and correspond to the Geocode standard NUTS 3 of the European Union.[87] The second set is comprised of all 258 German labor market regions. These combine several counties and are defined in a way that maximizes commuting within and minimizes commuting between the regions (cf. Kosfeld and Werner, 2012), so they may also be called commuting zones. The third set covers Germany's 96 spatial planning regions. These again combine several counties and are generally used for statistical reporting in Germany. Labor market regions and spatial planning regions do not have administrative functions.

For the three sets of regions, we obtain annual characteristics such as the regional

[85] We experiment with values of γ up to 10, but the estimation results almost do not change anymore when γ exceeds 5.

[86] We do not use the same routine as Elhorst and Vega (2015) to find γ because unlike the simpler SLX model they use, we include the spatial lag of the dependent variable, which leads to the "perfect solution problem" (Elhorst and Vega, 2015, pp. 11–12).

[87] We take into account county reforms, in particular in Saxony-Anhalt in 2007, Saxony in 2008, and Mecklenburg-West Pomerania in 2011. To construct a consistent spatial panel, we use the final 2011 map of counties in all observation years. To do so, we sum up the variables of counties that are amalgamated in the observation years before amalgamation, and we split up variables according to the population size in the observation years before counties separate (which happened very rarely).

gross value added and demographics (population size, age structure, and employees' education structure) from the Regional Statistical Data Catalogue for our 16-years period.[88] In supplementary estimations, we use the shorter period of 2001-2011, which allows us to include the industry structure, the unemployment rate and the average wage in the region as additional control variables.

We obtain annual regional start-up rates for our regions and time period from the Mannheim Enterprise Panel (MUP by its German initials) provided by the Centre for European Economic Research (ZEW) in Mannheim (Bersch et al., 2014). The MUP is constructed from data provided by Creditreform, Germany's largest credit rating agency. Therefore, it covers companies with sufficient economic activity to be noticed and registered by Creditreform and mostly excludes micro and sideline businesses (Bersch et al., 2014) or letterbox companies. The start-up rates we measure using the MUP therefore reflect substantial entrepreneurship better than administrative business registration rates (Fritsch et al., 2002). The overall start-up rate based on the MUP is smaller than the official firm registration rate for the reasons mentioned above, but the time trends are similar (Fossen and König, 2015). Our start-up data is very complete and accurate because we focus on high-tech and manufacturing businesses that usually have non-negligible initial credit requirements and therefore quickly enter the database of Creditreform (cf. Audretsch and Keilbach (2004), who use similar data).[89]

Using the MUP, we measure the start-up rate as the number of newly founded businesses in a given region and year per 10,000 inhabitants at working age (18-65 years of age). Similarly, Audretsch and Keilbach (2004, 2007) and Pijnenburg and Kholodilin (2014) also use the number of new firms relative to the region's population as their measure of entrepreneurship capital. We consider start-ups in the high-tech and manufacturing industries. The high-tech industry is comprised of R&D-intensive manufacturing and technology-oriented services (including the software industry). The high-tech and manufacturing industries partially overlap because R&D-intensive manufacturing belongs to both sectors. The manufacturing industry also includes low-tech manufacturing businesses.

Table 4.1 provides descriptive statistics of our county data, and Table 4.3 in Appendix 4.6 shows mean characteristics of the labor market regions and spatial planning regions. During our period of analysis, the annual start-up rate in the

[88] The Regional Statistical Data Catalogue is provided by the Federal Statistical Office and the statistical offices of the Federal States of Germany.

[89] We abstain from analyzing the start-up rate over all industries because businesses in the low-tech services industry such as retail stores or catering firms often enter the MUP with a time lag, which would result in a less precise analysis.

high-tech industry was 2.9 per 10,000 working-age inhabitants in the average county and year. The respective rate in the manufacturing sector was 2.2. The average population is 204,000 in counties, 319,000 in labor market regions and 856,000 in spatial planning regions. About 7% of the employees have a university degree, ranging from 1.95% to 27.14% in the most extreme counties.

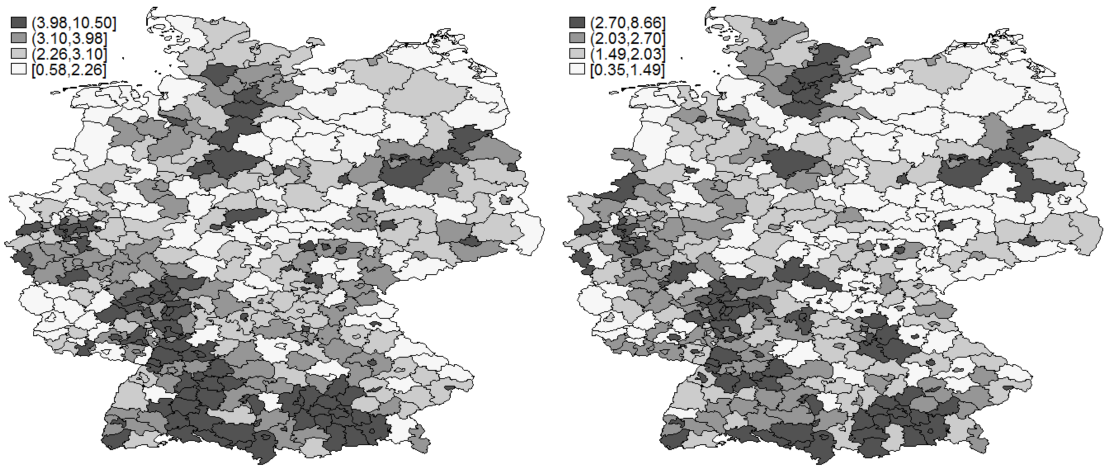
Table 4.1: Descriptive statistics for Germany's counties

	Mean	Std dev.	Min.	Max.
Period 1996-2011:				
Annual start-ups in high-tech per 10,000 inhabitants at working age	2.91	1.34	0.22	13.43
Annual start-ups in manufacturing per 10,000 inh. at working age	2.22	0.85	0.00	7.53
Population in 10,000	20.44	22.81	3.38	350.19
Share population at working age in total population (in %)	63.23	2.12	57.35	70.81
Share employees without apprenticeship (in %)	17.00	5.03	3.31	35.33
Share employees with apprenticeship (in %)	64.03	5.44	26.21	80.55
Share employees with university degree (in %)	7.20	3.61	1.95	27.14
Gross value added in real thousand Euro per employee	63.66	15.26	23.31	140.50
Period 2001-2011:				
Share workers in the manufacturing sector (in %)	28.78	8.93	6.17	59.98
Share workers in the services sector (in %)	68.71	9.67	35.48	93.59
Share unemployed in the working age population (in %)	7.21	3.56	1.22	20.32
Average wage per employee in 1,000 real Euro	32.21	5.14	10.31	49.07

Notes: The descriptive statistics are based on all 402 counties in Germany (NUTS 3 level) and not weighted. Thus, we have 6432 annual observations in the period 1996-2011 and 4422 in 2001-2011. Working age refers to ages 18-65. Real Euro are in prices of 2010. Concerning the education structure of the employees, the omitted base category is the share of employees without information on education. Concerning the industry structure, the omitted base category is the agricultural and mining sector.
Source: Own calculations based on regional data from the Federal Statistical Office and the Mannheim Enterprise Panel, 1996-2011.

Figures 4.1 and 4.2 show the start-up rates in the high-tech industry and the manufacturing industry, respectively, in the first and last years covered by our data, 1996 (left) and 2011 (right). Spatial clustering is clearly visible, perhaps even more so in the high-tech industry than in the manufacturing industry. In the high-tech sector (Figure 4.1), the largest cluster is in the southwest in the greater region Rhine-Main-Neckar spreading parts of the Federal States of Baden-Wuerttemberg, Hesse and Rhineland-Palatinate, and other clusters are found around the cities Munich, Hamburg-Hannover and Berlin and in the region Rhine-Rhur-Wupper within North-Rhine Westfalia. Interestingly, these clusters largely persist over the time span of 15 years, although some clusters become weaker and others emerge, notably around Nuremberg in Bavaria. In the manufacturing sector (Figure 4.2), spatial clustering as well as time persistency in start-up rates can also be observed. The clusters are different from the high-tech clusters to a large extent, with an important cluster spreading parts of Thuringia and northern Bavaria. Between 1996 and 2011, this cluster spreads further east into Saxony, suggesting a spatial spillover.[90]

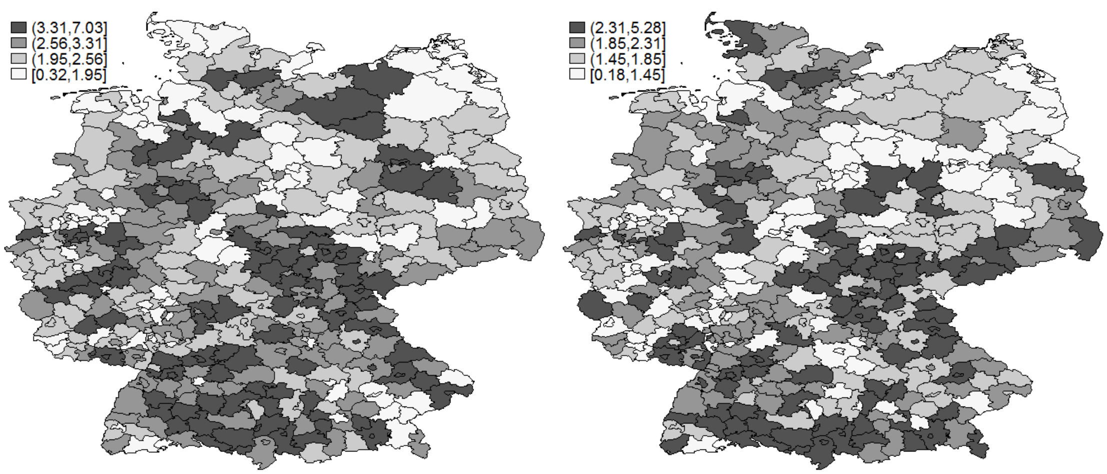
Figure 4.1: Start-up rates in the *high-tech* industry, German counties 1996 and 2011



Notes: Start-ups in the high-tech industry per 10,000 inhabitants in 1996 (left panel) and 2011 (right panel). The regions are counties (NUTS 3 level). Note the different scales: Start-up rates were generally higher in 1996 than in 2011.

Source: Own illustration based on the Mannheim Enterprise Panel, 1996 and 2011.

Figure 4.2: Start-up rates in the *manufacturing* industry, German counties 1996 and 2011



Notes: Start-ups in the manufacturing industry per 10,000 inhabitants in 1996 (left panel) and 2011 (right panel). The regions are counties (NUTS 3 level).

Source: Own illustration based on the Mannheim Enterprise Panel, 1996 and 2011.

In sum, the figures strongly suggests the importance of both, spatial clustering

[90]Figure 4.7 in Appendix 4.7 depicts the development of spatial autocorrelation in start-up rates in German counties from 1996-2011 as measured by Moran's I, using a binary contiguity matrix as a starting point (Elhorst, 2010a). Spatial autocorrelation is larger in the high-tech industry than in the manufacturing industry, although the difference vanishes at the end of the observation period.

and time persistence. The limitation of the extant literature is that neither cross-sectional spatial econometrics (Klotz, 2004; Audretsch and Keilbach, 2007) nor dynamic panel econometrics without consideration of spatial spillovers (Andersson and Koster, 2011) can fully capture the dynamics of firm formation. Therefore, this paper provides the first analysis of start-up activity jointly taking into account the spatial and the time dimension in a consistent dynamic spatial panel estimation.[91]

Which size of regional units is most appropriate to analyze the spatial distribution of start-up rates? Figures 4.8 and 4.9 in Appendix 4.7 show start-up rates in the high-tech and manufacturing industries in 2011, based on labor market regions and spatial planning regions, respectively. The comparison with the maps based on counties (Figures 4.1 and 4.2) suggests that the larger regions hide important heterogeneity, for example, between cities and surrounding counties, which sometimes have very different start-up rates. Therefore, we base our main analysis on county data and provide estimations for the larger regions for comparison.

4.4 Econometric results

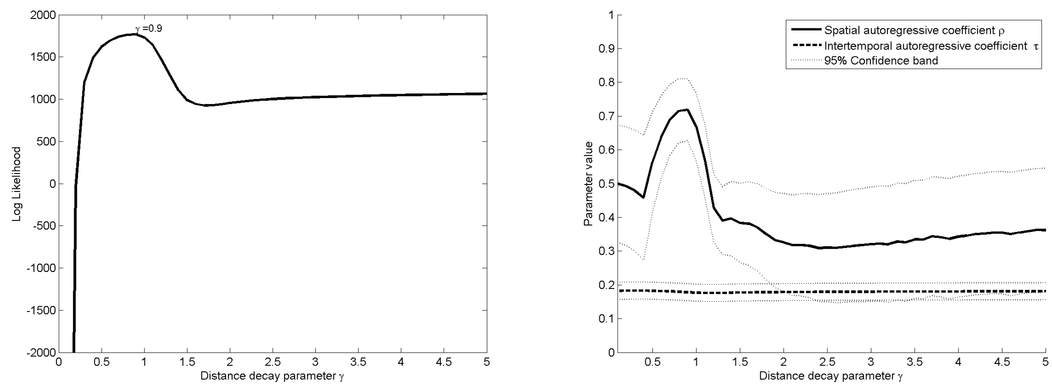
4.4.1 Model estimation and robustness

We start with exploring the impact of distance on spillovers of local start-up activity. To do so, we estimate model (4.1) on the county level with different distance decay parameters γ. Figure 4.3 displays the results for the high-tech industry and Figure 4.4 for the manufacturing industry. The left panels show the log-likelihood values and the right panels the estimates of the intertemporal autoregressive coefficient τ and the spatial autoregressive coefficient ρ. For both industries, a similar choice of γ leads to the highest log-likelihood values. A distance decay parameter of $\gamma = 0.9$ best describes spatial interaction of regional start-up activity in the high-tech industry, and the respective parameter for the manufacturing industry is $\gamma = 1.0$, which corresponds to the standard inverse distance matrix. For both industries, we also observe that the estimate of τ is almost completely insensitive to the choice of γ. Thus, even in cases where the spatial matrix is misspecified, a dynamic spatial econometric model is able to identify the intertemporal autoregressive coefficient in this context. In both industries, the spatial autoregressive

[91] Fritsch and Mueller (2007) estimate time dynamics using panel data of German spatial planning regions and also include the mean value of the residuals in the adjacent regions as a control variable, but without further discussion of the spatial dimension.

parameter ρ is always positive and significantly different from zero regardless of the choice of γ, which robustly indicates that spatial spillovers of start-up activity exist. However, the point estimate of ρ is sensitive to the choice of γ. This confirms that it is important to search systematically for the distance decay parameter γ that best describes the data.[92]

Figure 4.3: Influence of γ on the log-likelihood and autoregressive coefficients for *high-tech*



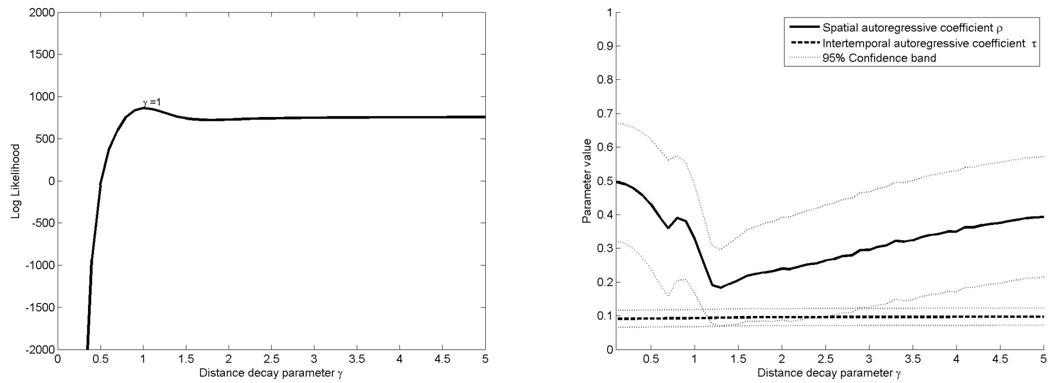
Notes: The figures show how the log-likelihood value (left panel) and the intertemporal and spatial autoregressive coefficients (right panel) change in the model of high-tech start-ups when increasing the distance decay parameter γ in the weighting matrix. We re-estimate model (4.1) fifty times with different values of γ, starting with $\gamma = 0.1$ and then gradually increasing γ in steps of 0.1 until it reaches 5. For the high-tech industry, the highest log-likelihood is reached when $\gamma = 0.9$.

Sources: Own estimations based on county level data from the Mannheim Enterprise Panel and the Regional Statistical Data Catalogue for 1996-2011.

We continue with the best distance decay parameters $\gamma = 0.9$ in the high-tech and $\gamma = 1.0$ in the manufacturing industry. The estimated intertemporal and spatial autoregressive coefficients τ and ρ appear in Table 4.2, where Panel I shows the results for high-tech and Panel II for manufacturing start-ups. Column (1) provides the baseline estimates using the preferred model. The estimates of τ and ρ are positive and significant at the 1%-level in both industries. Spatial spillovers are particularly large. When the start-up rate in the high-tech industry in neighboring municipalities increases by 1 percent (spatially weighted average), the rate increases by 0.72 percent in the focal county. In manufacturing, the spatial spillover effect is significantly smaller, but still 0.33 percent. The intertemporal

[92] When using labor market regions (spatial planning regions) instead of counties, the highest log-likelihood is reached when $\gamma = 1.0$ ($\gamma = 0.6$, respectively) in the high-tech industry. In manufacturing, $\gamma = 1.2$ ($\gamma = 1.0$) maximizes the log-likelihood. Again, the estimate of τ is insensitive to γ whereas ρ is more sensitive, exhibiting generally similar patterns as the ones we observe based on county data. The corresponding figures are available from the authors on request.

Figure 4.4: Influence of γ on the log-likelihood and autoregressive coefficients for *manufacturing*



Notes: The figures show how the log-likelihood value (left panel) and the intertemporal and spatial autoregressive coefficients (right panel) change in the model of manufacturing start-ups when increasing the distance decay parameter γ in the weighting matrix. We re-estimate model (4.1) fifty times with different values of γ, starting with $\gamma = 0.1$ and then gradually increasing γ in steps of 0.1 until it reaches 5. For the manufacturing industry, the highest log-likelihood is reached when $\gamma = 1.0$.

Sources: Own estimations based on county level data from the Mannheim Enterprise Panel and the Regional Statistical Data Catalogue for 1996-2011.

spillovers are 0.17 in high-tech and 0.09 in manufacturing, so again they are significantly larger in high-tech. In both industries, $\tau + \rho < 1$, so the processes are stable (Lee and Yu, 2010c).

In model (2), we additionally include a spatial lag of the time lag with the coefficient restriction $\eta = -\tau\rho$ following Parent and LeSage (2012) as discussed above. The estimated coefficients τ and ρ almost do not change. The coefficient of the space-time interaction term η is -0.127 (standard error obtained using the Delta Method: 0.0123) in the high-tech industry and -0.030 (std. err.: 0.0087) in the manufacturing industry. To assess the sensitivity of our results further, we use this model including η to re-examine how τ and ρ change when we vary the distance decay parameter γ. The patterns look very similar to those obtained from the baseline model, which shows that the results are very robust to the inclusion of η (see Figure 4.11 in Appendix 4.7). The finding that the estimates of η are close to zero explains why dropping the space-time interaction term has no effect on the other estimates in our application, and we thus continue with the simpler model.[93]

In model (3), we extend the set of control variables and their spatial lags at the

[93] We also estimate the model including the spatial lag of the time lag with unrestricted parameter η. Again the coefficients of intertemporal and spatial autocorrelation τ and ρ do not change significantly.

Table 4.2: Intertemporal and spatial spillovers of regional start-up rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel I: High-tech industry								
Intertemp. autoreg. coeff. τ	0.178*** (0.013)	0.176*** (0.013)	0.109*** (0.017)	0.112*** (0.017)	0.198*** (0.016)	0.341*** (0.025)	0.178*** (0.013)	0.252*** (0.012)
Spatial autoreg. coeff. ρ	0.719*** (0.047)	0.719*** (0.047)	0.426*** (0.106)	0.485*** (0.098)	0.572*** (0.071)	0.335*** (0.105)	0.714*** (0.047)	0.847*** (0.026)
Log-likelihood	1766	1867	1098	1038	1369	889	1784	-4431
Cutoff distance	None	None	None	None	None	None	460km	None
Panel II: Manufacturing industry								
Intertemp. autoreg. coeff. τ	0.093*** (0.013)	0.092*** (0.013)	0.057*** (0.016)	0.059*** (0.016)	0.128*** (0.016)	0.267*** (0.026)	0.091*** (0.013)	0.130*** (0.013)
Spatial autoreg. coeff. ρ	0.328*** (0.082)	0.324*** (0.083)	0.210* (0.113)	0.210* (0.113)	0.384*** (0.092)	0.302*** (0.105)	0.261*** (0.066)	0.456*** (0.073)
Log-likelihood	861	1133	729	705	1148	1066	909	-3713
Cutoff distance	None	None	None	None	None	None	225km	None
Panel III: Model description (for high-tech and manufacturing)								
Cross-sectional units	402 counties	402 counties	402 counties	402 counties	258 labor market regions	96 spatial planning regions	402 counties	402 counties
Time period	1996-2011	1996-2011	2001-2011	2001-2011	1996-2011	1996-2011	1996-2011	1996-2011
Spatial lag of time lag	$\eta = 0$	$\eta = -\tau\rho$	$\eta = 0$	$\eta = 0$	$\eta = 0$	$\eta = 0$	$\eta = 0$	$\eta = 0$
Control variables	Baseline	Baseline	Extended	Baseline	Baseline	Baseline	Baseline	Baseline
Variables in logs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No

Notes: The dependent variable is the log annual start-up rate in the high-tech industry (Panel I) or in the manufacturing industry (Panel II). Level instead of log in specification (8). The spatial weighting matrix is a power inverse distance matrix with exponent $\gamma = 0.9$ in the high-tech and $\gamma = 1.0$ in the manufacturing industry. In all estimations, region and time fixed effects are eliminated. The coefficients of the baseline and extended sets of control variables are shown for specifications (1) and (3) in Table 4.4 in Appendix 4.6. We estimate dynamic Spatial Durbin Models using a Quasi Maximum Likelihood dynamic spatial panel estimator with bias correction (Lee and Yu, 2010b). Standard errors are in parentheses. Stars (*/**/***) indicate significance at the 10%/5%/1% levels.
Sources: Own estimations based on the Mannheim Enterprise Panel and the Regional Statistical Data Catalogue for 1996-2011.

cost of having a shorter time period of data available beginning in 2001 instead of 1996. In (3), the point estimates of τ and ρ are smaller than in (1), but remain positive and significant. At first sight, one might suspect that the influence of characteristics of neighboring counties omitted in (1) are responsible for the larger estimate of ρ in (1). However, as shown in Table 4.4 in Appendix 4.6, all spatial lags of the additional control variables turn out to be insignificant, and among the additional own characteristics of the focal counties, only the share of workers in the services industries is marginally significant at the 10% level for high-tech start-ups. To be sure, in (4) we re-estimate the model using the shorter time period, but without the additional control variables and obtain similar estimates as in (3). The results thus suggest that intertemporal and spatial spillovers became smaller over time. Public start-up subsidies for the unemployed rolled out in Germany in 2003 (Caliendo and Künn, 2011) may play a role, because this policy triggered start-ups out of necessity, whereas intertemporal and spatial spillovers may be more related to opportunity entrepreneurship and may have manifested themselves more strongly during the New Economy period of the late 1990s. Unfortunately, we cannot investigate this in more detail because we cannot further reduce the number of time periods for a consistent estimation of the spatial panel model, and we leave this topic for future research. For our baseline model in this paper, we prefer using the longer estimation period as in (1), which spans several business cycles and is more suitable for a consistent estimation of the general intertemporal and spatial autocorrelation of start-up activity.

How do the estimates change if we use larger regional units for the analysis? When we move from counties to larger labor market regions (column 5 in Table 4.2) and then to the even larger spatial planning regions (column 6) based on the long time period, we observe that the estimated intertemporal autoregressive coefficient τ becomes increasingly larger in comparison to model (1) in both industries. Moreover, in the high-tech industry, the spatial autoregressive coefficient ρ decreases, while it does not change much in the manufacturing industry. As argued in Section 4.3, we prefer the county level for this analysis because of the large differences in startup rates in neighboring counties we observe even within the same labor market regions. Our estimation results thus suggest that using regional units that are too large might lead to an overestimation of intertemporal and underestimation of spatial spillovers. This is in line with Rosenthal and Strange (2003), who conclude from their analysis that spillovers in the context of firm births should be studied at a fine grained geographical level.

Does the introduction of a cutoff distance in the spatial weighting matrix further improve the spatial model of start-up activity? When the distance between two regions exceeds the cutoff distance, their influence on one another is assumed to be zero. To explore the effect on the model fit, we start from model (1) based on the county data and introduce different cutoff distances (see Figures 4.12 and 4.13 in Appendix 4.7 for the two industries). The log-likelihood values reach their maximums when the cutoff distance is 460km in the high-tech and 225km in the manufacturing industry, but they remain similarly high for larger cutoff distances. The estimated coefficients τ and ρ are fairly stable when further increasing the cutoff distances beyond their optimal values. In specification (7) in Table 4.2, we introduce the best cutoff distances in the inverse distance matrices and find that this does not change the estimation results significantly in comparison to (1) without a cutoff distance. Thus, a cutoff distance does not significantly improve the spatial panel model of start-up activity.

As a final sensitivity check in column (8), we include all variables in levels instead of logs. This increases the point estimates of τ and ρ somewhat in both industries in comparison to (1). We prefer the log model because the start-up rates have a dispersed distribution and the log transformation reduces the influence of outliers.[94]

4.4.2 Impulse response functions

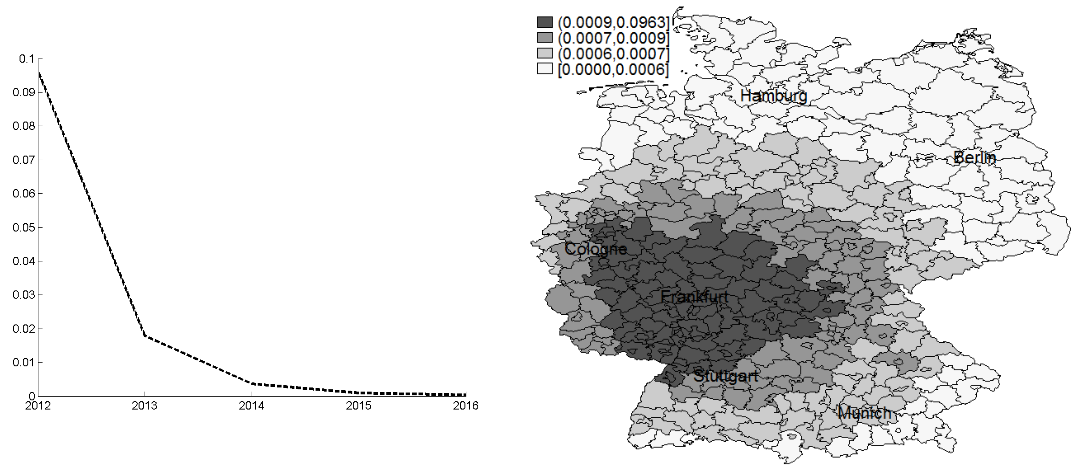
To illustrate the strength and reach of the estimated intertemporal and spatial spillovers, we simulate the response to a local and temporary shock to the start-up rate in Frankfurt/Main in 2012.[95] We first look at the high-tech industry and compare with the manufacturing industry thereafter. In the simulation referring to the high-tech industry, we define an exogenous impulse that has the size of 10% of the observed high-tech start-up rate in Frankfurt/Main in 2011. For the simulations we use the estimated model shown in column (1) of Table 4.2. The response is the relative difference between the simulated high-tech start-up rates in the scenario with the initial impulse and the baseline scenario without the

[94] Fritsch and Aamoucke (2013, 2017) use the number of start-ups (rather than start-up rates) as their dependent variable and negative binomial regression models to deal with the count data nature of this variable. While they include certain covariates from adjacent regions in their regressions, they do not estimate spatial or intertemporal autoregressive models.

[95] As an alternative to simulations, the spatio-temporal diffusion could also be estimated by analytically deriving dynamic space-time response functions (Debarsy et al., 2012).

impulse.[96]

Figure 4.5: Specific impulse responses to a shock in *high-tech* start-up rates in Frankfurt/Main



Notes: The figures show the simulated impulse response to a shock to the start-up rate in the high-tech industry that temporarily hits Frankfurt/Main in 2012. The impulse has the size of 10% of the observed 2011 start-up rate in high-tech in Frankfurt/Main. The left panel shows the impulse response over time for Frankfurt am Main. The right panel shows the spatial impulse response in all municipalities in 2012. In both graphs, the response shown is the relative difference in the high-tech start-up rates between the scenario with the initial impulse in Frankfurt/Main and the baseline scenario without the impulse. The simulations are based on the estimated model shown in column (1) of Table 4.2.

Sources: Own estimations based on county level data from the Mannheim Enterprise Panel and the Regional Statistical Data Catalogue for 1996-2011.

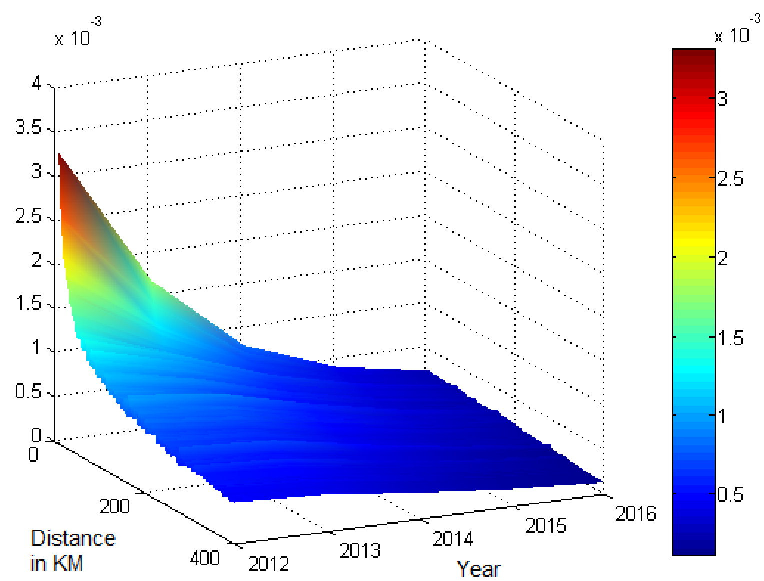
The left panel of Figure 4.5 displays the response to the impulse in Frankfurt/Main over time and shows that in 2013, the year after the exogenous impulse, the start-up rate in high-tech is still higher by about a fifth of the initial impulse in comparison to the scenario without the initial impulse. After that, the response gradually vanishes. The right panel shows the spatial response in 2012 by quartiles. One can see that large parts of south-western Germany respond to the impulse in Frankfurt/Main, including cities like Cologne and Stuttgart.

Figure 4.6 shows the joint intertemporal and spatial impulse response function. To draw this graph we order all counties by their geographical distance to Frankfurt/Main. The figure shows that most of the impulse response in high-tech is limited to counties within a distance of about 200km to Frankfurt/Main and to a period of about two years after 2012, the year of the shock. To be more precise, we sum up all the impulse responses within certain distances around Frankfurt/M. in

[96] E.g., 0.1 means a 10% higher start-up rate in high-tech due to the impulse. In 2011, Frankfurt/Main saw 213 start-ups in high-tech, which translates into a start-up rate of 4.56 per 10.000 working-age inhabitants.

2012 and divide them by the sum of all impulse responses in 2012. We find that 31% of the overall immediate impulse response takes place within a radius of 50km from Frankfurt/M., 41% within 100km, and 53% within 150km. For comparison, the distance between Frankfurt/M. in the north of the Rhein-Main-Neckar region and Walldorf (the location of the software company SAP) in the south of this region is about 100km (62 miles). Note that in the model best describing the data, entrepreneurship still exerts small spillovers to even more distant regions, as shown by the optimal cutoff distance of 225km for the manufacturing and even 460km for the high-tech industry (see above).

Figure 4.6: General impulse response to a shock in *high-tech* start-up rates in Frankfurt/Main



Notes: The figure shows the simulated impulse response to a shock to the start-up rate in the high-tech industry that temporarily hits Frankfurt/Main in 2012. The impulse has the size of 10% of the observed 2011 start-up rate in high-tech in Frankfurt/Main. The vertical axis shows the relative difference in the high-tech start-up rates between the scenario with the initial impulse in Frankfurt/Main and the baseline scenario without the impulse. The axis labeled "Distance in km" shows the distance of counties to Frankfurt/Main. We exclude Frankfurt/Main from the graph because the relatively large local impulse would be dominating. The simulations are based on the estimated model shown in column (1) of Table 4.2.

Sources: Own estimations based on county level data from the Mannheim Enterprise Panel and the Regional Statistical Data Catalogue for 1996-2011.

Figures 4.14 and 4.15 in Appendix 4.7 provide results from analogous simulations for the manufacturing industry. In comparison, both the intertemporal and the spatial impulse responses are substantially smaller than in the high-tech industry (note the different scales). This suggests that knowledge spillovers, which are likely to be more crucial in the high-tech industry than in the general manufacturing industry, may be a key component of intertemporal and spatial entrepreneurial spillovers.

4.5 Conclusion

We provide evidence of significant intertemporal and spatial spillovers of start-up activity in the high-tech and manufacturing industries. The evidence is based on dynamic spatial panel estimators that take into account unobserved regional and time fixed effects and control relevant covariates of the focal regions and their neighbors.

A systematic grid search shows that the spatial weighting matrix that best reflects the spatial structure of start-up activity is a power inverse distance matrix with a distance decay parameter of $\gamma = 0.9$ to $\gamma = 1.0$ and a cutoff distance of more than 200km. This shows that the strengths of entrepreneurial spillovers quickly declines with geographical distance, but is still notable beyond a far distance. This is in line with Rosenthal and Strange (2003) and may indicate that knowledge spillovers by means of face-to-face communication are an important component of interaction. By simulating impulse response functions based on our preferred estimated models, we find that 41% of the immediate response to a shock in the high-tech startup rate at a specific place and time takes place in regions within a distance of 100km (62 miles) from the local origin of the shock. This is about the distance between Frankfurt/M. and Walldorf in the Rhine-Main-Neckar region and more than the distance between San Francisco and San Jose in the Bay Area (80km). Most of the response is observed within two years from the time of the shock. The spatial and intertemporal impulse response is much stronger in the high-tech than in the general manufacturing industry, which gives additional support for knowledge spillovers being an important component of entrepreneurial spillovers.

Our findings demonstrate that entrepreneurship capital is a local and persistent phenomenon. The robust intertemporal and spatial spillovers we document imply positive external effects of investment in entrepreneurship capital by individual entrepreneurs and local governments. For example, local governments investing in entrepreneurship capital by establishing incubators stimulate entrepreneurship not only in their own, but also in neighboring regions. From a social welfare perspective, such positive externalities may lead to underinvestment in entrepreneurship capital by local governments, who do not take into account the social returns to their investment in neighboring jurisdictions. This suggests that higher level governments may increase overall efficiency by supporting local governments in their efforts to promote local entrepreneurship. Further research should investigate specific channels of intertemporal and spatial interactions in entrepreneurship to enhance our understanding of the scope for entrepreneurship policy.

4.6 Appendix - Tables

Table 4.3: Mean characteristics of labor market regions and spatial planning regions

	Labor market regions	Spatial planning regions
Period 1996-2011		
Annual start-ups in high-tech per 10,000 inhabitants at working age	2.71	2.84
Annual start-ups in manufacturing per 10,000 inh. at working age	2.28	2.20
Population in 10,000	31.85	85.61
Share of population at working age in total population (in %)	62.91	63.40
Share employees without apprenticeship (in %)	17.12	16.37
Share employees with apprenticeship (in %)	64.88	63.61
Share employees with university degree (in %)	7.01	7.86
Gross value added in real thousand euro per employee	62.51	63.38
Period 2001-2011		
Share workers in the manufacturing sector (in %)	29.89	27.47
Share workers in the services sector (in %)	67.44	70.18
Share unemployed in the working age population (in %)	7.31	7.55
Average wage per employee in 1,000 real Euro	31.91	32.20

Notes: The table shows unweighted mean characteristics for all 258 labor market regions and all 96 spatial planning regions in Germany. Thus, we have 4128 (1536) annual observations in the period 1996-2011 and 2838 (1056) in the period 2001-2011 for the labor market regions (spatial planning regions). Working age refers to ages 18-65. Real Euro are in prices of 2010. Concerning the education structure of the employees, the omitted base category is the share of employees without information on education. Concerning the industry structure, the omitted base category is the agricultural and mining sector.
Sources: Own calculations based on regional data from the Federal Statistical Office and the Mannheim Enterprise Panel, 1996-2011.

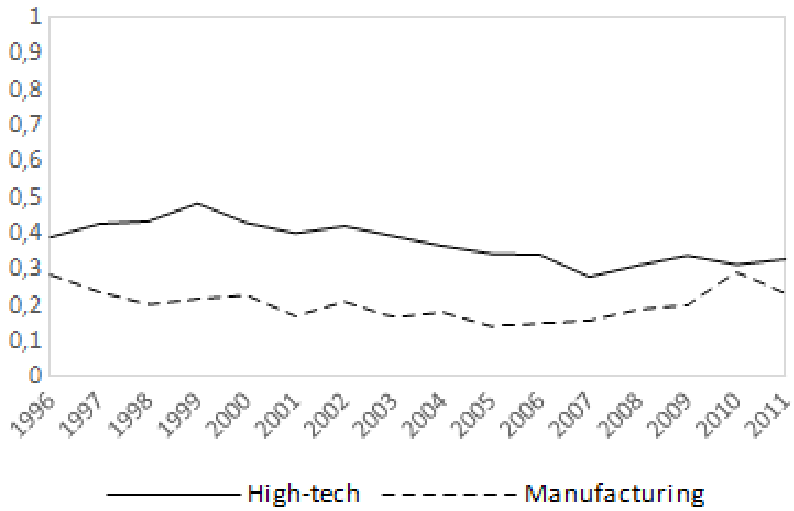
Table 4.4: Regional start-up rates: Coefficients of the control variables

	High-tech		Manufacturing	
	(1)	(2)	(1)	(2)
Intertemporal autoregressive coefficient τ	0.178***	0.109***	0.093***	0.057***
	(0.013)	(0.017)	(0.013)	(0.016)
Spatial autoregressive coefficient ρ	0.719***	0.426***	0.328***	0.210*
	(0.047)	(0.106)	(0.082)	(0.113)
log population size	0.200	1.530***	-0.111	0.561
	(0.160)	(0.361)	(0.180)	(0.393)
log share population at working age	0.561	0.160	-1.283***	-1.476*
	(0.388)	(0.708)	(0.436)	(0.769)
log share employees without apprenticeship	-0.124*	-0.218*	-0.029	-0.210*
	(0.071)	(0.114)	(0.079)	(0.124)
log share employees with apprenticeship	0.175	-0.157	0.002	-0.005
	(0.160)	(0.265)	(0.180)	(0.288)
log share employees with university degree	0.094*	0.018	0.061	-0.026
	(0.054)	(0.095)	(0.060)	(0.103)
log gross value added per employee	-0.149**	-0.138	-0.048	-0.111
	(0.072)	(0.118)	(0.081)	(0.128)
W x log population size	-3.550**	-9.148***	-0.864	-2.172
	(1.474)	(3.241)	(1.343)	(2.830)
W x log share population at working age	-0.019	13.748**	4.810**	5.324
	(2.360)	(5.579)	(2.307)	(5.106)
W x log share empl. without apprenticeship	0.316	1.196	2.465***	3.215***
	(0.739)	(1.255)	(0.705)	(1.141)
W x log share empl. with apprenticeship	0.784	9.058***	0.947	4.013
	(1.105)	(2.905)	(1.039)	(2.520)
W x log share empl. with university degree	1.019*	-0.402	1.328**	3.392***
	(0.592)	(1.217)	(0.536)	(1.032)
W x log gross value added per employee	-0.545	1.116	0.954**	2.572***
	(0.387)	(1.071)	(0.374)	(0.954)
log share workers in manufacturing		-0.233		-0.301
		(0.213)		(0.232)
log share workers in services		-0.892*		0.082
		(0.540)		(0.588)
log unemployed in working age population		0.035		-0.024
		(0.063)		(0.068)
log average wage per employee		0.099		0.234
		(0.236)		(0.257)
W x log share workers in manufacturing		-3.223		-0.955
		(2.959)		(2.513)
W x log share workers in services		7.615		-2.120
		(7.223)		(6.168)
W x log unemployed in working age pop.		-0.219		-0.073
		(0.282)		(0.255)
W x log average wage per employee		-1.188		-2.495
		(2.078)		(1.863)
Log-likelihood	1766	1098	861	729
Cross-sectional units	402 counties	402 counties	402 counties	402 counties
Time period of annual observations	1996-2011	2001-2011	1996-2011	2001-2011
Control variables	Baseline	Extended	Baseline	Extended

Notes: The dependent variable is the log annual start-up rate. In all estimations, region and time fixed effects are eliminated. Descriptions of the variables are provided in Table 4.1. We estimate dynamic Spatial Durbin Models using a Quasi Maximum Likelihood dynamic spatial panel estimator with bias correction (Lee and Yu, 2010c). The spatial weighting matrix W is a power inverse distance matrix with exponent $\gamma = 0.9$ for high-tech and $\gamma = 1.0$ for manufacturing and no cutoff. Standard errors are in parentheses. Stars (*/**/***) indicate significance at the 10%/5%/1% levels.
Sources: Own estimations based on the Mannheim Enterprise Panel and the Regional Statistical Data Catalogue for 1996-2011.

4.7 Appendix - Figues

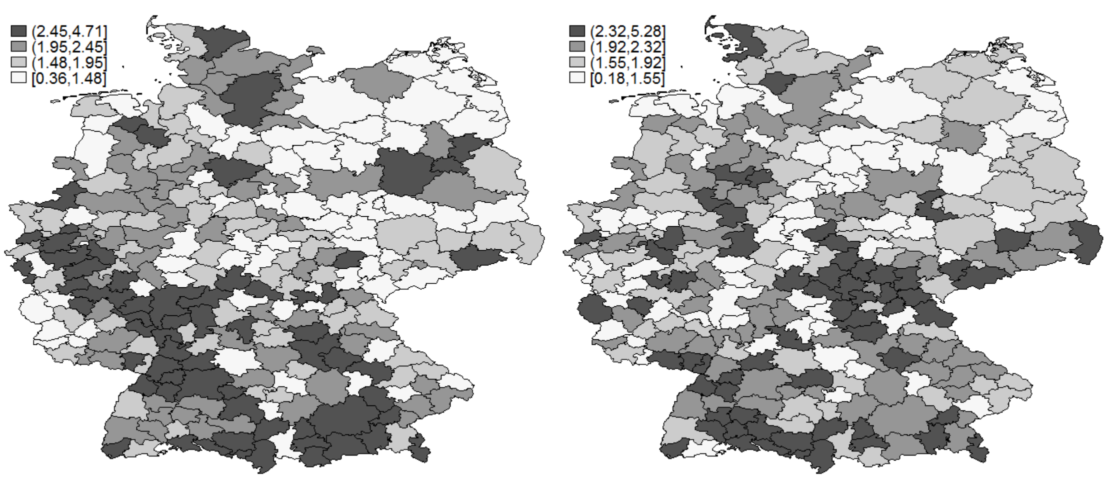
Figure 4.7: Development of Moran's I for start-up rates in German counties from 1996-2011



Notes: To measure spatial autocorrelation in start-up rates in German counties (NUTS 3 regions), we calculate Moran's I for each year from 1996-2011 separately for start-ups in the high-tech and manufacturing industries using a binary contiguity matrix.

Sources: Own calculations based on the Mannheim Enterprise Panel, 1996-2011.

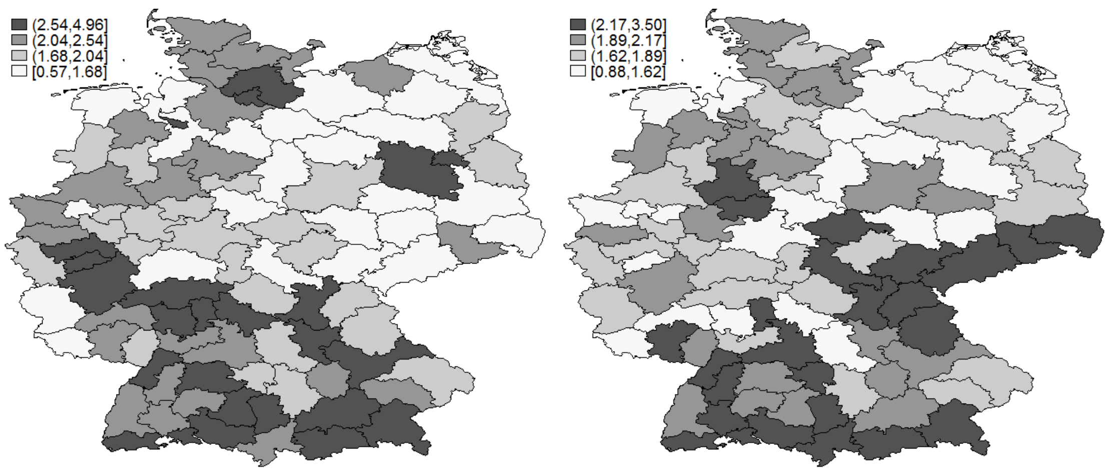
Figure 4.8: Start-up rates in high-tech and manufacturing, *labor market regions* 2011



Notes: Start-ups in the high-tech industry (left panel) and in the manufacturing industry (right panel) per 10,000 inhabitants in 2011. The regions are labor market regions.

Sources: Own illustration based on the Mannheim Enterprise Panel, 1996 and 2011.

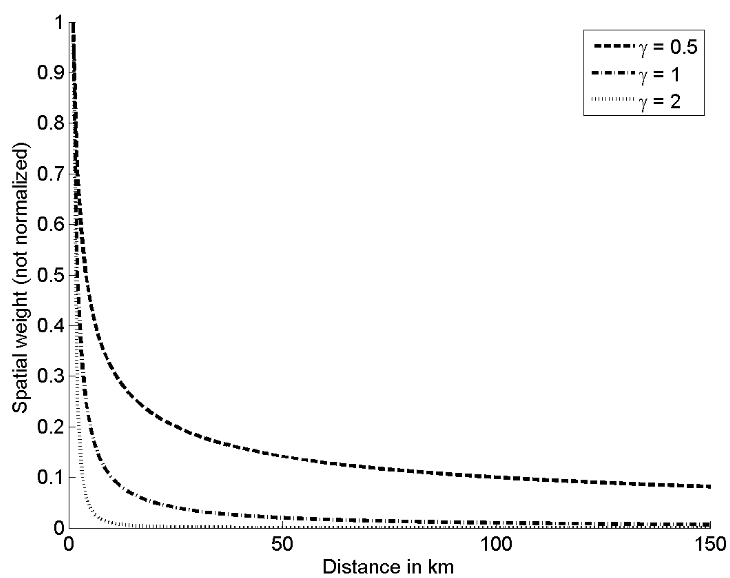
Figure 4.9: Start-up rates in high-tech and manufacturing, *spatial planning regions* 2011



Notes: Start-ups in the high-tech industry (left panel) and in the manufacturing industry (right panel) per 10,000 inhabitants in 2011. The regions are spatial planning regions.

Sources: Own illustration based on the Mannheim Enterprise Panel, 1996 and 2011.

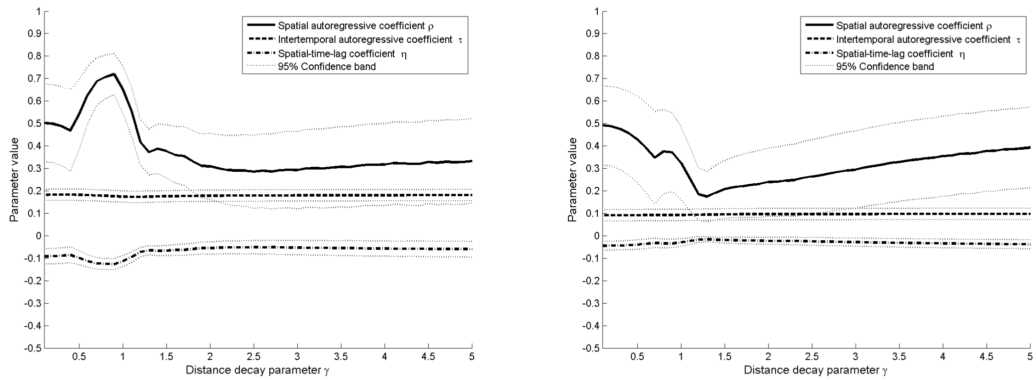
Figure 4.10: Distance decay functions for different values of γ



Notes: The graph illustrates how the spatial weight, i.e., the influence of a region on a neighbor, diminishes with growing distance between the regions depending on the value of the distance decay parameter γ.

Sources: Own illustration adapted from Elhorst and Vega (2015).

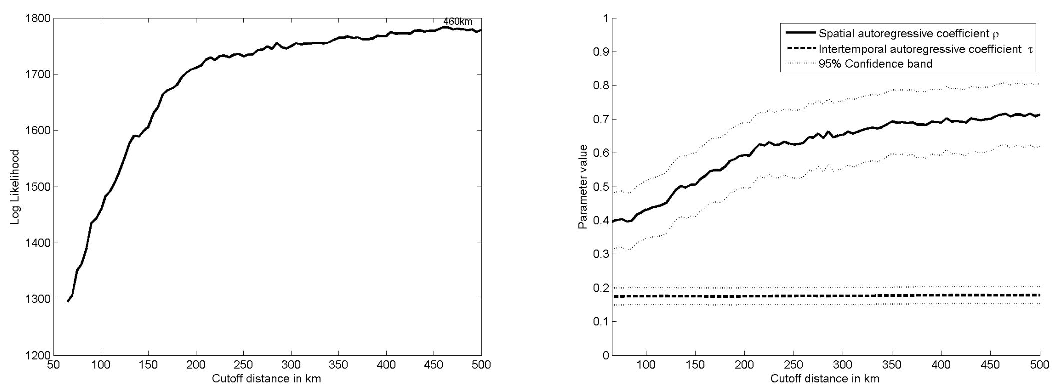
Figure 4.11: Influence of γ on autoregressive coefficients including a spatial-time lag



Notes: The figures show how the intertemporal and spatial autoregressive coefficients ρ and τ and the coefficient of the spatial lag of the time lag η change when increasing the distance decay parameter γ in the weighting matrix. The left panel uses the high-tech and the right panel the manufacturing industry. The difference to the right panels of Figures 4.4 and 4.3 is that we include the spatial lag of the time lag here and restrict its coefficient $\eta = -\tau\rho$ following Parent and LeSage (2012). The standard error of η is calculated using the Delta Method. To draw the figures, we re-estimate model (2) in Table 4.2 fifty times with different values of γ, starting with $\gamma = 0.1$ and then gradually increasing γ in steps of 0.1 until it reaches 5.

Sources: Own estimations based on county level data from the Mannheim Enterprise Panel and the Regional Statistical Data Catalogue for 1996-2011.

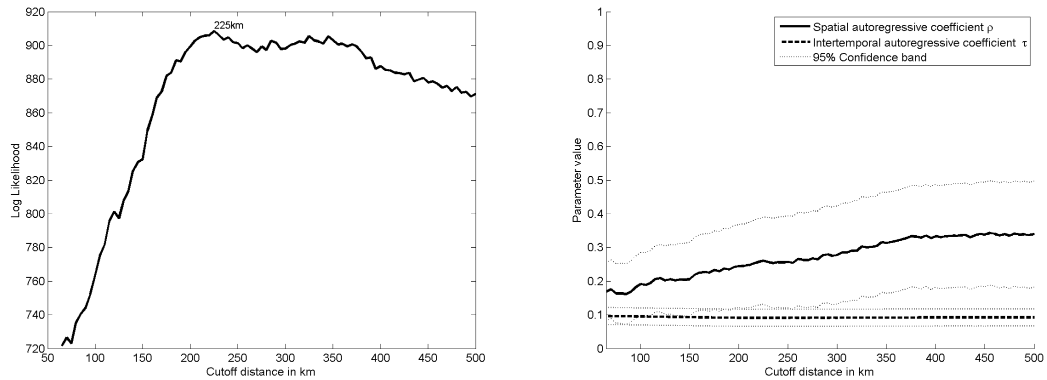
Figure 4.12: Influence of a cutoff distance on estimation results for *high-tech*



Notes: The figures show how the log-likelihood value (left panel) and the intertemporal and spatial autoregressive coefficients (right panel) change in the model of high-tech start-ups when increasing the cutoff distance in the spatial weighting matrix, beyond which neighbors are assumed to have no influence. We re-estimate model (1) repeatedly with different cutoff distances, starting with 50km and then gradually increasing the cutoff distance in steps of 5km until it reaches 500km. In all these estimations we use a power inverse distance matrix with the distance decay parameter $\gamma = 0.9$, as determined for high-tech before. For the high-tech industry, the highest loglikelihood is reached when using a cutoff distance of 460km.

Sources: Own estimations based on county level data from the Mannheim Enterprise Panel and the Regional Statistical Data Catalogue for 1996-2011.

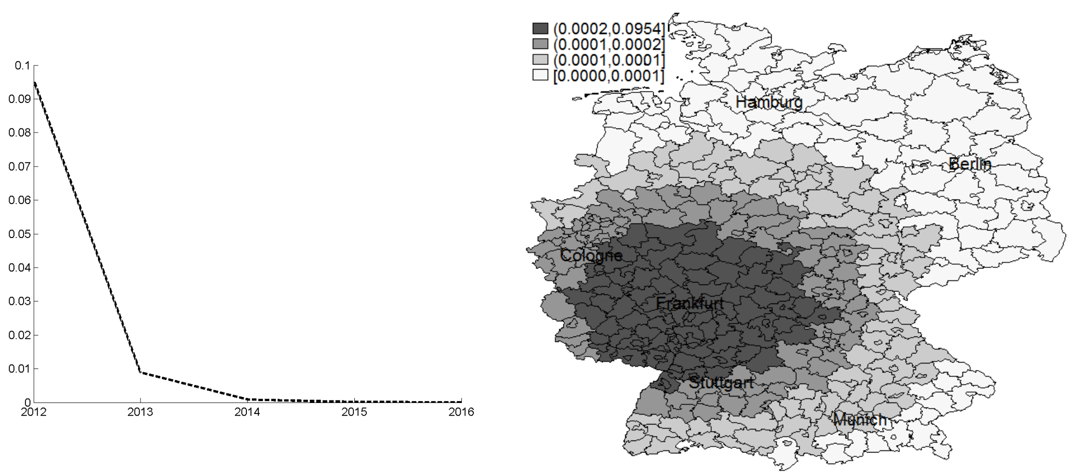
Figure 4.13: Influence of a cutoff distance on estimation results for *manufacturing*



Notes: The figures show how the log-likelihood value (left panel) and the intertemporal and spatial autoregressive coefficients (right panel) change in the model of manufacturing start-ups when increasing the cutoff distance in the spatial weighting matrix, beyond which neighbors are assumed to have no influence. We re-estimate model (1) repeatedly with different cutoff distances, starting with 50km and then gradually increasing the cutoff distance in steps of 5km until it reaches 500km. In all these estimations we use a power inverse distance matrix with the distance decay parameter $\gamma = 1.0$, as determined for manufacturing before. For the manufacturing industry, the highest log-likelihood is reached when using a cutoff distance of 225km.

Sources: Own estimations based on county level data from the Mannheim Enterprise Panel and the Regional Statistical Data Catalogue for 1996-2011.

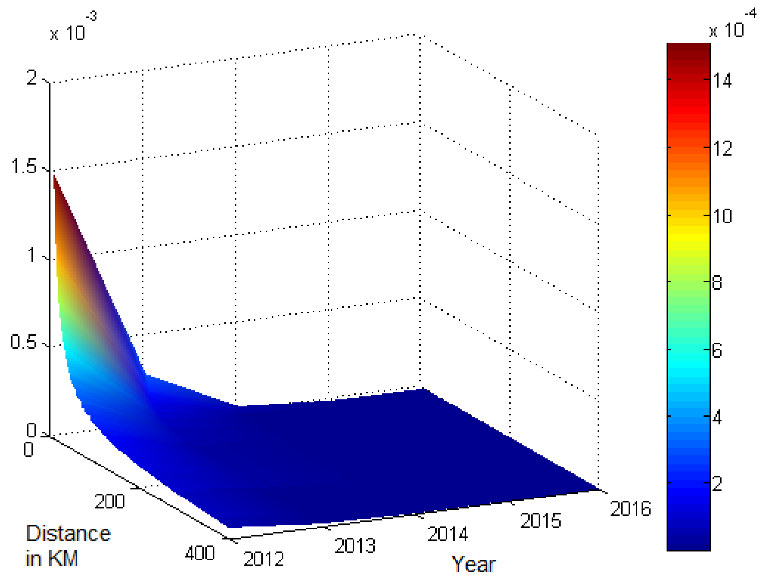
Figure 4.14: Specific impulse responses to a shock in *manufacturing* start-up rates in Frankfurt/Main



Notes: The figures show the simulated impulse response to a shock to the start-up rate in the manufacturing industry that temporarily hits Frankfurt/Main in 2012. The impulse has the size of 10% of the observed 2011 start-up rate in manufacturing in Frankfurt/Main. The left panel shows the impulse response over time for Frankfurt/Main. The right panel shows the spatial impulse response in all municipalities in 2012. In both graphs, the response shown is the relative difference in the manufacturing start-up rates between the scenario with the initial impulse in Frankfurt/Main and the baseline scenario without the impulse. The simulations are based on the estimated model shown in column (1) of Table 4.2.

Sources: Own estimations based on county level data from the Mannheim Enterprise Panel and the Regional Statistical Data Catalogue for 1996-2011.

Figure 4.15: General impulse responses to a shock in *manufacturing* start-up rates in Frankfurt/Main



Notes: The figure shows the simulated impulse response to a shock to the start-up rate in the manufacturing industry that temporarily hits Frankfurt/Main in 2012. The impulse has the size of 10% of the observed 2011 start-up rate in manufacturing in Frankfurt/Main. The vertical axis shows the relative difference in the manufacturing start-up rates between the scenario with the initial impulse in Frankfurt/Main and the baseline scenario without the impulse. The axis labeled "Distance in km" shows the distance of counties to Frankfurt/Main. We exclude Frankfurt/Main from the graph because the relatively large local impulse would be dominating. The simulations are based on the estimated model shown in column (1) of Table 4.2.

Sources: Own estimations based on county level data from the Mannheim Enterprise Panel and the Regional Statistical Data Catalogue for 1996-2011.

Chapter 5

Not in my backyard? – Evaluating the impact of citizen initiatives on housing supply

5.1 Introduction

Recently, channels of citizen participation in governmental processes in Germany are changing. While classical interaction between politicians and citizens still takes place via regular elections, new tools for participation are increasingly applied in politics. On the macro level, German states have used state wide referendums for some time.[97] However, the more dynamic development is on the micro level, specifically at the municipal level. Citizens demand a higher degree of direct political participation and several new tools have been introduced to that regard. This includes direct mayoral elections[98], citizen budgets (the so-called

[97]Since the end of World War II, approximately 300 of such direct democratic procedures took place on the state level, which are often mandatory if the constitution is to be changed. Direct forms of participation have also found their way into political parties: The German Social-Democratic Party (SPD) held a vote amongst their members in 2013 whether they should engage in a grand coalition with the Christian Democrats or not. Furthermore, the Green Party lets their members choose their front runners a in ballot.

[98]Most federal states allowed direct elections of the city mayor only in the 1990s. Bavaria, the state on which we focus in our empirical analysis, allowed direct elections after World War II.

Bürgerhaushalt)[99], or citizen credits.[100] The interest of this paper, however, is the most frequently used channel of direct citizen participation: local level initiative petitions and initiative elections.

The involvement of citizens via initiative petitions (and elections) at the local level has direct consequences for the provision of infrastructure projects and the local policy process for land use. More coordination between the government and the inhabitants is necessary, policy makers have to adjust more to the agenda of the median voter and cannot pursue their own agenda. Nowadays, many of the large infrastructure plans in Germany are supported or contested by local citizen initiatives. Most prominent examples here are the modernization of a major train station in the city of Stuttgart (*Stuttgart 21*), the preservation of parks and open spaces in Berlin (*Tempelhofer Feld*), construction of new electrical power lines in Bavaria (*Südstromtrassen*), the deepening of a canal in a port in Hamburg (*Elbvertiefung*), the fostering of bike traffic in Berlin (*Radverkehrsgesetz*) and local traffic infrastructure projects like cable cars in Hamburg (*Elbseilbahn*).[101]

It is worth noting that some of the these local referendums led to fierce public debates on the political matter at hand and about the use of direct democracy in general. This is best exemplified with the initiative on the land use policy of an open space in Berlin *Tempelhofer Feld*.[102] The local government and proponents argued that there is a lack of developable area in terms of living space and a scarcity of housing in Berlin - especially in prime locations. It would therefore be in the general interest to open the former down-town airport area to land development. Under pure representative democracy, the decision in Berlin would have been to make the Tempelhofer Feld available for new housing as well as public infrastructure (e.g. libraries). The opponents on the other hand wanted

[99] First developed in Brazil and New Zealand, it became a more recent phenomenon as well in Europe and Germany. In the year 2015, 435 (See http://www.buergerhaushalt.org) German cities applied this procedure.

[100] Citizen credits or citizen loans are financing tools of towns that ask (local) citizens to help for specific public goods. In return, the citizens often ask for more transparency and greater influence in the process.

[101] Regarding limits to construction, there is another interesting example from Munich, Bavaria in 2004. Here, the maximum height of any newly approved building was restricted to 100m – the height of the *Frauenkirche*.

[102] For reference, the Tempelhofer Feld is about as large as the central park in New York with more than 370 hectare of land. Unlike New York, Berlin has many other parks and open spaces and the Tempelhofer Feld is a green field that is not utilized in the same way for recreational purposes as is the central park.

to preserve a large open space within the agglomeration of Berlin.[103] The self-interested homevoter cares for local amenities and intends to preserve the value of the own housing. Through the tool of direct democracy, the citizens of Berlin – primarily those close to the park – voted for the preservation of the Tempelhofer Feld as an open space.

Much of the existing empirical literature on the policy effects of citizen initiatives focuses on the fiscal impact (see Section 5.2 for more details). Studies on the political economy of urban development policies are rather scarce (see Solé-Ollé and Viladecans-Marsal (2012, 2013) for Spain, Gerber and Phillips (2004) for the US and Ahlfeldt (2011, 2012); Ahlfeldt and Maennig (2015); Garmann (2014) for Germany, further discussed below). Most of this literature, however, focuses on election processes and/or party preferences. We contribute to the literature by examining citizen initiatives with respect to urban development processes.

For the analysis, we have collected comprehensive data on both the land use patterns (mainly approved residential area, but also and realized residential and non-residential area) as well as information of local citizen initiatives (prepared and distributed by *Mehr Demokratie e.V.*) for all 2031 towns in Bavaria. While data for land use is available since 1983, local citizen initiatives were introduced in 1995. Until the end of our data period in 2010, we observe more than 2000 initiative petitions.

Since both, local land use policies and the presence of initiatives might be caused by (unobserved) changing voter preferences, a simple OLS regression may lead to biased results. Also, we must be aware of reverse causality, in which housing and construction projects are driving the process that lead to an initiative. We use a dual approach to tackle these identification issues. First, we use a matching approach to make towns that had an initiative petitions and those which did not comparable. Here, we can match on the observed pre-trends in the outcome variable, which makes this approach particularly appealing. Second, we also test a difference-in-differences (diff-in-diff) design, that makes use of the timing of the initiative petition.

This paper is organized as follows: Section 5.2 will give a review of the literature on initiatives. Section 5.3 will describe the institutional setting and the data, which is followed by the empirical analysis in Section 5.4. The paper concludes with Section 5.5.

[103] It should also be said, that much of the argument of the opponents was linked to mistrust against the local government. The fear was that few individuals would benefit from the land development, while many would see their opportunities to use the open space decline.

5.2 Literature

Studying citizen initiatives has a long tradition in the public choice literature. From a normative point of view, they have a positive virtue. If the agenda of local politicians does not match the preferences of the median voter, citizen initiatives can correct them (see Romer and Rosenthal (1978); Gerber (1999)).

Most of the empirical literature on initiative effects focuses on fiscal impacts caused by initiatives in the US and Switzerland (see Matsusaka (1995); Feld and Matsusaka (2003); Funk and Gathmann (2011, 2013); Blume et al. (2009)). These studies confirm that the presence of local initiatives decreases the spending of local governments. By contrast, the recent contributions for Germany by Asatryan et al. (2016, 2017) and Asatryan et al. (2016) show that municipalities in Bavaria which experienced a citizen initiative increase their spending. The authors attribute this opposite effect to the cooperative fiscal federalism in Germany, being a different institutional system than that of Switzerland or the US.[104]

Another strand of research focuses on the determinants that lead to the occurrence of local initiatives. Arnold and Freier (2015) examine the effect of signature requirements on the number of initiatives by exploiting discontinuities for signature requirements at population thresholds. They find that higher signature requirements cause a decline of local initiatives. Arnold et al. (2016) show that local support for the introduction of direct democracy is due to a general dissatisfaction with representative democracy rather than with a certain party.

Our theoretical argument linking local land usage and citizen initiatives is based on the idea of the homevoter (Fischel (2001)). Homevoters are characterized by concentrating their investments into their houses which creates strong incentives to retain the value of their homes/houses. Homevoters therefore tend to avoid the costs of urbanization such as construction costs and the negative effects of urbanization (crime, pollution etc.) as these decrease the value of their houses. Another channel how urbanization decreases the house value is the effect of increased supply on the housing price. While the homevoter will influence land use policy making also through representative democracy, the introduction of direct democracy gives the homevoter an additional veto right.

For the US, the paper that is most similar to ours – Gerber and Phillips (2004) – shows that citizen initiatives change the process of urban development. Even though outcomes are the same in the long run, the short run interactions of all

[104] Note also, that Asatryan et al. (2015) shows that municipalities with citizen initiatives have more efficient governments.

actors change substantially when direct democracy comes into play. Developers try to involve citizens more and provide better compensation when changes in land use affect them negatively. The paper highlights that local land use plans are indeed of great political interest. Therefore, it is natural that the voters want to influence the local land use process directly and make use of all democratic tools available.

Garmann (2014) applies the homevoter hypothesis to the introduction of direct mayoral elections in Germany. He uses a gradually implemented policy reform of the nomination scheme of mayors. City mayors were historically nominated by the city council. Following the reform, they were directly elected. The results show a reduced change in urban land area and a lower approval of building licenses for residential buildings when the mayor was directly elected by the citizens. According to the argument, directly elected mayors behave in a more restricted way as they seek the direct support of the homevoters in direct elections.

There is also a small number of additional studies that look at political processes and local land use. Recent contributions by Solé-Ollé and Viladecans-Marsal (2012, 2013) highlight that local land use plans are determined by political variables. The first paper shows that increased political competition leads to less newly developed land and thereby presents evidence for the homevoter channel that we presume to be of interest also with regard to direct local democracy. The second paper illustrates that leftist parties in Spain provide less land for development.

Another possible mechanism that we suspect is the fact that local citizen initiatives cause a damage to the reputation of the local politician. Local initiatives are always a sign of voters disagreeing with the current policies. Politicians fear this loss of reputation because it may endanger reelection. Since citizen initiatives are a rather new tool, it is likely that politicians become more hesitant with their urban development policies after experiencing one initiative to prevent the occurrence of further ones.

Given the reviewed literature, we consider it interesting to study whether local initiatives influence the urban development process. The literature suggests that initiatives have an influence on local politics and that local land use policies are in the interest of politicians and voters. If the median voter in Bavaria is a homevoter, introducing direct democracy should lead to a decline in the urban development process. In addition, politicians want to avoid a loss of reputation. Since the autonomy over the urban development process is in the hand of local politicians, they will be hesitant with this tool to avoid further initiatives after they experienced one.

5.3 Institutions and Data

Bavaria is one of the largest federal states in terms of population, area and economic activity in Germany. About 13 percent of the German population live in Bavaria and the state accounts for almost 20 percent of the entire area of Germany in 2017. Our object of investigation are Bavarian municipalities. The federal state of Bavaria consists of 2,056 municipalities, including 25 independent cities (*Kreisfreie Städte*) and 29 major county towns (*Grosse Kreisstädte*). Municipalities are the lowest administrative tier in Germany. They are responsible for various public affairs, i.e. public order, infrastructure, schooling etc. Some of the tasks are conducted in sole responsibility by the municipalities, while others are conducted jointly with higher-tier administrations.

Regarding the urban development process, municipalities are obliged to provide land development plans (*Flächennutzungsplan*) that have to be approved by a higher administrative authority. In general, this plan depicts the characteristics of land use and is not legally binding. Based on the land development plan, the municipality has to develop a legally binding land use plan (*Bebauungsplan*), which indicates the urban patterns, i.e. construction form, green and traffic areas. It is important to note that municipalities are the decisive actor for implementing urban land use policies. As land use is one of the main tasks of municipalities, this topic is also admissible for local citizen initiatives.

For our research, we combine two different sets of data: regional data on land use patterns and population from the Bavarian State Office for Statistics and a dataset on local petitions for initiative elections in Germany. This latter dataset was assembled by the Universities of Wuppertal and Magdeburg in cooperation with the NGO *'Mehr Demokratie e.V.'* ("More Democracy").

5.3.1 Local initiatives

Local initiatives in Bavaria were introduced on the 1st of November 1995. Before, citizen initiatives were only allowed at the state level (*Volksbegehren* and *Volksentscheid*) and involved high barriers to enter the initiative process (for details see Asatryan et al. (2016)). The resulting public dissatisfaction caused a state-wide referendum to introduce citizen initiatives also at the local level (*Bürgerbegehren* and *Bürgerentscheid*). This referendum was successful and had a state-wide acceptance rate of 57.8 percent (See Arnold et al. (2016)). Since then, local initiatives are frequently used in Bavaria and it has become the state with the highest number of initiative petitions in Germany.

Direct participation of citizens in the political process consists of two steps. If inhabitants are dissatisfied with a certain aspect of politics, they can pronounce their disagreement and start a local initiative petition. During the petition, a certain amount of signatures needs to be collected in order to move on to the second step. If the signature requirement is met and local politics cannot settle the issue, a local initiative election on the proposal is held (*Bürgerentscheid*).

The dataset from "More Democracy" (*Mehr Demokratie e.V.*) consists of 2,155 local initiatives from 1995 until 2011. Regarding the time dimension, an extensive usage of local initiatives occurred directly after the state wide referendum in 1995, see Figure 5.1. However, even after this initial surge, local initiatives are still an often used instrument of citizen participation with around one hundred initiatives per year.

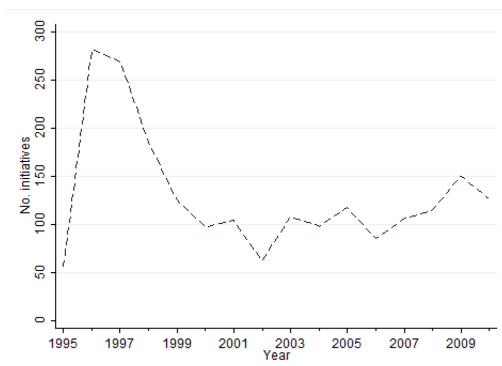


Figure 5.1: Number of initiatives by year. *Source*: Own representation based on the data from *Mehr Demokratie Bayern e.V.*

Regarding the topics of initiatives, we observe a big variety as shown in Table 5.1.[105] Apart from the topic, we also know whether the initiative was successful (either through an initiative election or by negotiation with the council).[106]

By looking at Table 5.1, one can see that only a small share of initiatives is directly related to new housing areas. Therefore, a direct effect of initiatives can

[105] Regarding the assignment of initiatives to topics, we suspect slight measurement errors looking into some detailed descriptions. For example, some initiatives against the demolition of houses may fit into the sections "public infrastructure" or "construction".

[106] Furthermore, we have an outcome "unknown" for initiatives when the exact outcome is not reported.

Table 5.1: Local initiatives petitions by topic 1995 - 2011

Topic	Total	Share	Success	Failure	Unknown
Public infrastructure (*Öff. Infrastruktur / Versorgung*)	277	12,87	112	70	95
Social infrastructure (*Soziale Infrastruktur*)	279	12,96	124	91	64
Culture (*Kultur*)	90	4,18	41	26	23
Disposal (*Entsorgung*)	140	6,51	52	23	65
Economy (*Wirtschaft*)	380	17,66	168	118	95
Traffic (*Verkehr*)	458	21,28	166	157	136
Fees (*Abgaben*)	37	1,72	6	11	20
Constitution (*Hauptsatzung*)	25	1,16	12	4	9
Residence (*Wohngebiete*)	28	1,30	13	12	3
Other land-use planning (*andere Bauleitplanungen*)	211	9,80	89	73	49
Local government reorganization (*Gebietsreform*)	24	1,12	8	12	4
Miscellaneous (*Sonstiges*)	88	4,09	36	10	42
Mobile communications (*Wirtschaft: Mobilfunk*)	115	5,34	59	14	43
$\sum$	2,155	100	886	621	648

Source: Own calculation on the bases of the data from *Mehr Demokratie Bayern e.V.*

only have a small influence. However, we suspect a (potentially more significant) indirect influence, in which local initiatives prevent the necessary infrastructure (i.e. roads, opening of new land) to develop further residential area. Importantly, for this indirect channel to come into effect, we do not need to observe an initiative petition on a topic of land use. In our empirical analysis, we argue that an initiative petition on any topic (be it land use or other issues) affects politicians to become more cautious in land use policy making. Any initiative petition will increase political awareness for the new democratic tool of direct democracy and the potential political harm inflicted by losing an initiative election. Also, politicians will see the need to involve citizens stronger in the coordination process. The result is an increased coordination effort which might cause the residential area supply to decrease.

To show the geographical distribution of the initiatives and the development of initiative activity over time after the introduction of this new political instrument, we highlight a map in Figure 5.3 in the appendix. There are some hot spots in the beginning, i.e. 1995 or 1996. Visual inspection suggests that, over time, new local initiatives gradually evolve around municipalities that already experienced an initiative (also see Asatryan et al. (2015)). Overall, initiative activity is later spread evenly around all areas in Bavaria. However, we can also see that larger towns (population is also related to area here) have more often been in contact with local direct democracy – highlighting the importance to control for population and population growth.

5.3.2 Land use data

For our analysis, we collected information on population as well as the approved and realized residential and non-residential area from 1983 until 2012. Before population numbers changed (in some cases largely) due to the census in 2011, we only use the data until 2010 in the analysis. In addition, we have gathered net migration, the stock of residential and non-residential area. We will use the approved residential area per capita (in square meters) as our main dependent variable. Also, we include the approved residential area per capita.

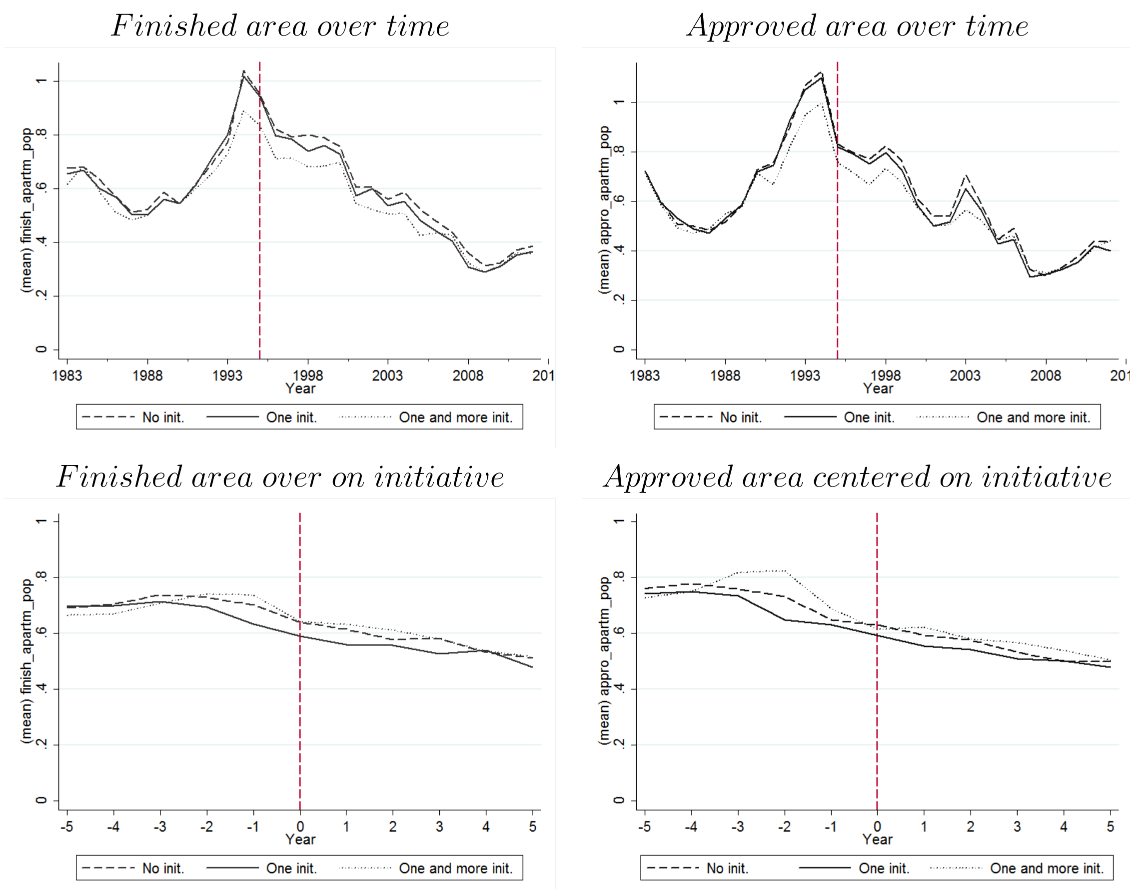


Figure 5.2: Evolution of residential and non-residential area in sqm per capita over time. *Source*: Own calculations.

In the four panels of Figure 5.2, we depict the time pattern of finished and approved residential area. We have organized the graphs in two distinct ways. The upper panels show the time series of our two dependent variables by years for the period 1983 to 2012. The vertical red line here marks the introduction of local initiatives in Bavaria in 1995. For the lower panels, we instead organize the data around the timing of the first initiative in a town (again indicated by a vertical red line). Time zero is the year of the first initiative. The years before and after measure the time to or after this point in time.

In all panels, we distinguish between three groups of towns. Our main group is the towns with exactly one initiative petition. For those observations, the timing of events is trivial. We consider this group to be our treated observations. The second group consists of the towns with more than one initiative. This group is only depicted for comparison and will not be used in the empirical analysis. The reason becomes clear in the panels of Figure 5.2: these towns show apparent differences in the time trend of new area. Towns in this group are also larger on average and show different growth patterns. For this lack of comparability, we chose to not use those observations in the empirical analysis.

The third group of observations is comprised of towns that never saw an initiative election. In the upper panels, we show their time trend using the dashed line. In order to also include this group in the lower panels, we needed to randomly assign a placebo point in time for those towns. We do this by mimicking the actual distribution of time periods within the first group. Note, that this approach is not only used for the graphical analysis, it is also necessary for the implementation of the difference-in-difference design as well as in the matching routine.

Investigating the overall trends, we find that there was steady decline in the number of finished and approved residential area until 1994. Subsequently, the sharp increase is followed by a long lasting continuous decline thereafter. The time series for finished and approved residential area closely mirror each other, except for the fact, that the finished area is always somewhat lagging in time.

Several explanations for these aggregate trends can be named. First, the reunification had Bavaria see massive migration (East Germans and Ethnic Germans), which required substantial expansion of housing supply (see Freier et al. (2016)). After this boom, the economic crisis hit after 1994 and subsidies relevant for housing supply were cut (i.e., social housing programs in the end of the 90s, a grant scheme for first-home buyers (*Eigenheimzulage*) after 2005).

The main finding of the graphical analysis, however, concerns the development of the two groups of towns (treated and control) who are moving in close lock step. While this is to be expected in the period before the treatment started (even desired), the fact that the common trend extends also beyond the start of the treatment previews our later findings that the introduction of local direct democratic instruments had virtually zero effect on land use policy.

In Table 5.5 in the appendix, we highlight the summary statistics for our main variables in the analysis. We show the mean, standard deviation, minimum and maximum for our dependent variables (approved and finished housing area in square meters per capita) and our key independent variables (population, demographics, migration, land prices).

5.4 Empirical methods and results

In this section, we present our empirical estimation results. We first highlight the findings using a simple regression control framework and testing competing fixed and random effects specifications. We then turn to a difference-in-difference setup as well as matching specification. As shown above, the finished and approved residential area variables are closely related and the approved area is more directly linked to political decision making. We therefore present only the results for the approved residential area, noting that the results for the finished area closely mirror those findings.[107]

5.4.1 Regression control framework

We start our empirical analysis using a simple regression control framework (OLS). The dependent variable is approved housing area (per capita and measured in square meter). Our variable of interest is the indicator variable "Initiative" which takes the value one for all time periods starting with the year a municipality first had any initiative petition. In the regression, we also control for the number of population, the share of young inhabitants (below 18 years), the share of elderly (above 65 years), a measure for migration (net percentage population change per year) and information on average land prices within the municipalities (based on actual sales (*Daten der Gutachterausschüsse*)). We also include year fixed effects in all specifications.

Table 5.2 shows the results of this first empirical test. Column 1 uses the base specification, columns 2-4 add county fixed effects, random effects or municipality fixed effects respectively. The findings for our variable of interest are very small (column 2-4) and insignificant in our preferred specification (column 4). Even the largest effect reported in the OLS specification in column 1 would indicate that running an initiative petition in a municipality reduces approved housing area by

[107] Results not shown here, but available upon request.

Table 5.2: OLS Results

	(1) Approved Housing Area	(2) Approved Housing Area	(3) Approved Housing Area	(4) Approved Housing Area
Method	OLS	County FE	Random Effects	Municipal FE
Initiative	-0.028***	-0.026***	-0.021**	-0.012
	(0.009)	(0.009)	(0.009)	(0.009)
Population	-0.001***	-0.000***	-0.001**	0.002**
	(0.000)	(0.000)	(0.000)	(0.001)
Share Young	1.123***	1.099***	0.807***	-1.938***
	(0.126)	(0.130)	(0.200)	(0.357)
Share Elderly	-2.370***	-1.583***	-1.995***	2.946***
	(0.099)	(0.106)	(0.156)	(0.370)
Migration per capita	0.007***	0.006***	0.006***	0.005***
	(0.000)	(0.000)	(0.000)	(0.000)
Land Price	0.297***	0.164***	0.248***	0.178***
	(0.031)	(0.040)	(0.045)	(0.059)
Year FE	Yes	Yes	Yes	Yes
N	31629	31629	31629	31629
R^2	0.25	0.29	0.22	0.23

Notes: * are indicating the usual levels of significance. Standard errors in parentheses are clustered at the level of the municipality.
Source: Own calculations.

less than 1/10 of a standard deviation. Even when the results are statistically significant, they remain economically marginal.

Econometrically, using an OLS design may run into several issues here. Omitted variable bias is always problematic, however, we do use variables of prime importance for approved housing area as controls and at least the municipality fixed effects approach is taking care of all time invariant unobservables. The more severe issue may be reverse causality. If high values for approved housing sets the initial spark to have an initiative in the first place, and if approved housing is correlated over time, we may overestimate the impact of initiatives on housing supply. In order to make more progress on these issues, we follow a dual approach and use a difference-in-difference design as well as a matching approach specifically targeting a close control group that exhibits similar pre-trend in the housing supply.

5.4.2 Difference-in-differences approach

In the diff-in-diff approach, we use the observations with exactly one initiative petition as the treatment group and define the before and after period according to the timing of that local initiative. As a control group, we use all municipalities that never saw a local level initiative. For this control group, we have no natural before and after period, which is particular troublesome as we have shown there to be large aggregate shifts over time. To implement the diff-in-diff, we assign random placebo years to those control observations, such that the distribution in placebo years mirrors the actual distribution of initiatives by year. We can now assign the observations of the control group to a before and after period and set up a standard diff-in-diff estimation.

Table 5.3: Diff-in-Diff estimation results

	(1) Approved Housing Area	(2) Approved Housing Area	(3) Approved Housing Area	(4) Approved Housing Area
Model	Baseline	Time Trend	Year FE	Year + County FE
Treatment*Time	0.021	0.021	0.020	0.021
	(0.014)	(0.014)	(0.013)	(0.013)
Time	-0.054***	-0.029*	-0.012	-0.016*
	(0.009)	(0.017)	(0.009)	(0.009)
Treatment	-0.021*	-0.021*	-0.025**	-0.021**
	(0.011)	(0.011)	(0.010)	(0.010)
Population	-0.000***	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Share Young	0.949***	0.937***	-0.077	-0.182
	(0.209)	(0.209)	(0.208)	(0.210)
Share Elderly	-3.675***	-3.670***	-2.759***	-1.789***
	(0.166)	(0.166)	(0.158)	(0.170)
Migration per capita	8.213***	8.210***	7.214***	6.250***
	(0.499)	(0.499)	(0.496)	(0.487)
Land Price	0.351***	0.351***	0.444***	0.280***
	(0.063)	(0.063)	(0.062)	(0.082)
N	13903	13903	13903	13903
R^2	0.18	0.18	0.23	0.27

Notes: * are indicating the usual levels of significance. Standard errors in parentheses are clustered at the level of the municipality.
Source: Own calculations.

Table 5.3 highlights the results of this empirical analysis. The variable of interest is the interaction term of the treatment and the time (before/after) variable in row 1. We use the baseline specification in column 1, a linear time trend in column 2, full year fixed effects in column 3 and year and county fixed effects in column 4. Our findings closely follow the results above. The estimates are precisely estimated (small standard errors), however, the effects are statistically and economically insignificant.

The diff-in-diff design hinges on a common trend assumption. In the absence of treatment, treated and control observations should develop similarly. To refer our reader back to the graphical analysis above (see Figure 5.2), we here clearly showed that treated and control observation moved in lock-step, before the treatment - reassuring that a common trend assumption is warranted - and after the treatment, indicating that there is little to no effect of the introduction of local direct democratic tools on the approval of housing supply.

5.4.3 Matching

While the diff-in-diff approach provides a causal interpretation of the effects (based on the common-trend assumption), it may still be hampered by the reverse causal-

Table 5.4: Matching Results

	Outcome: Approved housing area post initiative			
	Match on pre-data in levels		Match on pre-data growth	
	3 year average (1)	5 year average (2)	3 year average (3)	5 year average (4)
Average treatment effect on the treated	-0.028 (0.026)	-0.007 (0.025)	-0.041 (0.026)	-0.028 (0.022)
N	1323	1080	1343	1090

Notes: * are indicating the usual levels of significance. Standard errors in parentheses are clustered at the level of the municipality.
Source: Own calculations.

ity issue (even though the co-movement of the pre-trends does not indicate a major issue). One additional approach to tackle the pre-trend issue is to match on just those pre-period data - which we do in the following.

We keep the data structure with treated and controls and also here rely on the randomly distributed placebo years. For each observation, we now define a pre- and a post-period of equal length (3 or 5 years). We apply a standard propensity score matching routine in which we compare treated to control observations with the twist, that we match on the realized pre-period outcome variable on approved housing area (similar to the main idea of synthetic control, see Abadie et al. (2010)). By matching on the predetermined outcome, we make treatment and control variables comparable in this important characteristic.

Table 5.4 shows the results of the matching specifications. In columns 1 and 2, we match on the average approved housing area during the pre-initiative period in levels; in columns 3 and 4 we instead match on the growth in approved housing before the initiative. The results of the matching specification fall inline with the above analysis. Again, we find insignificant small coefficients which are estimated with relatively high precision (small standard errors).

Taken together with the graphical analysis above, the results of the OLS, the diff-in-diff and the matching estimations all indicate that the introduction of direct democratic instruments shows no causal effect on the local housing supply.

5.5 Conclusion

The paper investigates the causal effects of local initiative activity on the land use patterns in Bavarian municipalities from 1983 to 2010. For the analysis, we have put together a unique data set on land use variables (approved and finished residential area) as well as local level information on the use of direct democracy. Throughout the analysis, we show that no direct effect of local initiatives on the land use parameter of the municipalities can be identified. This is previewed in descriptive statistics on the raw data and again shown in OLS, diff-in-diff and matching analyses. The estimates are almost always insignificant, small and relatively precisely estimated (small standard errors). Our findings stand contrary to the homevoter hypothesis, which would predict a decrease in the approved residential land and which has been shown to matter in policy making in other contexts. Our results also hint to the fact that the cleavage between representative and direct democracy on issues regarding land use cannot be too large.

While our data are suitable for the analysis and generally of high quality, we must reckon two issues that may limit the scope of our study. First, we only have few initiatives that touch directly on land use and we decided to use all initiatives (regardless of the topic), as they define a threat point and alert politicians. However, the width of topics is enormous and further analysis (with more data) may be more successful in pinpointing to initiatives of importance to the land use process. Second, while the timing of the initiative is clear, the timing of land use policy is not. Land use plans may have been written beforehand and changes may need varying time to be implemented – making it more difficult to establish a statistical effect.

5.6 Appendix - Tables and figures

Table 5.5: Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	N
Approved Housing Area (m^2 per capita)	0.568	0.44	0.005	21.811	31629
Finished Housing Area (m^2 per capita)	0.592	0.465	0.012	19.692	31648
Initiative	0.049	0.216	0	1	32896
Population	5.995	31.375	0.193	1353.186	32896
Share Young	0.209	0.027	0.079	0.299	32896
Share Elderly	0.164	0.034	0.057	0.414	32896
Migration per capita	2.528	12.379	-199.324	124.022	32896
Land Price	0.057	0.091	0	1.297	32896
No Land Transaction	0.441	0.496	0	1	32896
Year	2002.5	4.61	1995	2010	32896

Source: Own calculations.

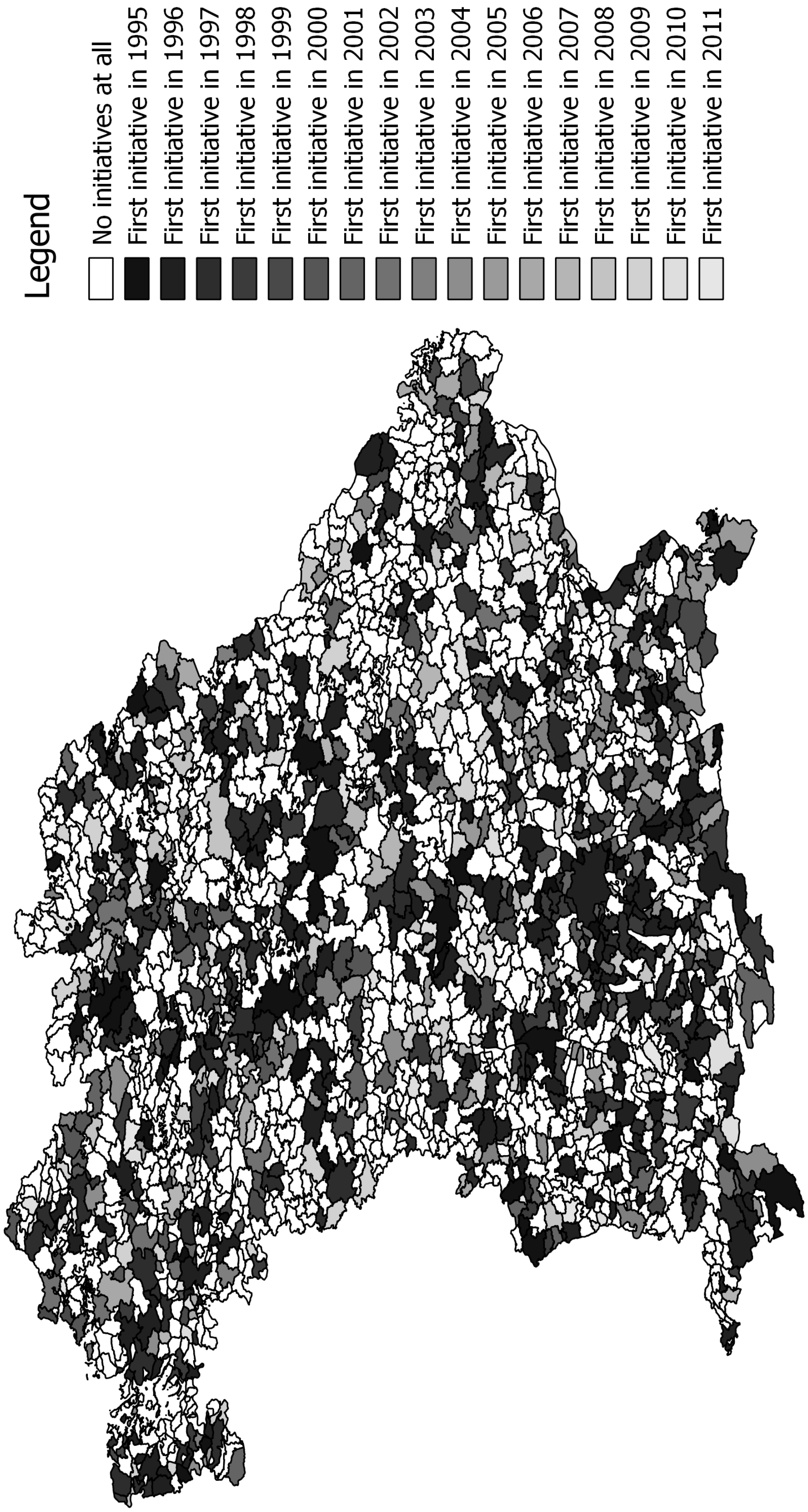


Figure 5.3: Evolution of initiative petitions over time. *Source*: Own calculations

Chapter 6

You shall not build! (until tomorrow) – Electoral cycles and housing policies in Germany)[‖]

6.1 Introduction

Since the beginning of the year 2000, the German Real Estate Market has experienced an increase in both housing prices and rents. However, these developments differ across Germany; agglomerations experience a sharp increase while housing prices and rents in rural areas remain stable or even decrease slightly (BBSR, 2016). This uneven distribution is caused by an emerging reurbanization pattern combined with a low supply of living space in urban areas. The German Government has responded either with stricter rent controls or more relaxed approval policies for new construction projects to increase housing supply as policy tools to counteract this development.

This study contributes to the understanding of the reasons for a lack of housing supply in agglomerations by investigating one of the factors that might have contributed to this issue. Specifically, I examine whether construction approval policies change during election periods. My results show a decrease of housing approvals during municipal elections in West German municipalities. This is

[‖]A prior version has been published as a MPRA Discussion Paper, see Martin (2017).

especially important since these adjustments might delay life cycle related immigration outflows from agglomerations to suburbs. It is likely that these potential homeowners then reside for a longer period in the agglomeration, which would exacerbate the shortage of housing supply and hence might contribute to the recent increase in real estate prices in urban areas.

The German housing market is an interesting study object, since German metropolitan areas still have comparably low rent to income ratios compared to other European metropolitan areas like Paris or London. Nevertheless, the German agglomerations seem to follow the same path as their European counterparts and converge towards similar real estate price levels. This gives an interesting opportunity to study the cause of rent developments in agglomerations and the underlying reasons.

Historically, Germany has a low homeowner rate which has been explained by comparably low subsidies, a strong social housing sector and a low rate of policy interventions. This led to stable prices and relatively high housing quality in Germany. Hence, the incentive to invest in private housing property was not as strong for individuals as in other countries (Voigtländer, 2009). Interestingly, the homeowner rate shows considerable spatial variation, where especially the West German federal states experience higher homeowner rates. Lerbs and Oberst (2014) investigated the spatial variation of homeowner rates and find that the relative price of renting over buying seems to drive this development. Regarding life satisfaction of homeowners in Germany, Zumbro (2014) found that they are slightly happier on average. This finding is explained especially by the quality of dwellings and is more pronounced within lower income groups.

For my analysis, I use a panel of 4,983 West German municipalities between 1,000 and 20,000 inhabitants for the years 2002 to 2010. These study objects are not large entities like Frankfurt or Munich but small to medium sized municipalities located around these agglomerations. Using the approved residence square meters (sqm) per 1,000 inhabitants as dependent variable, I conduct fixed effects regression including municipality and year fixed effects. The timing of local elections across federal states allows me to identify the effect of elections on housing approvals. Evaluated at the mean, my results suggest an 11.4 % decrease of approved housing sqm during election times and a catch up effect of similar magnitude in the years after the election. These results remain economically and statistically significant throughout different robustness checks.

Furthermore, I investigate whether the share of homeowners in a municipality explains this disruption during election times. The negative effect on housing approval during elections seems to be strongest for municipalities above the 90th percentile share of homeowners. Furthermore, this effect only occurs in single and double family houses. This can be interpreted as suggestive evidence that this effect is caused by homevoters (Fischel, 2001), residential homeowners with a strong preference for the status quo in their neighborhood.

My study contributes to two strands of literature. First, it adds an additional policy dimension to the literature about political business cycles. Initially proposed by Nordhaus (1975), his theory suggests that politicians have an incentive to alter their fiscal policies during election times in order to increase their re-election probabilities. Usually, this literature is mostly concerned with governmental budgeting and spending during election times but my research shows that other parameters are also in the interest of politicians. This theory has been applied successfully to different political levels (e.g. national, state, county and municipal elections) and different countries((Alesina et al., 1997; Brender and Drazen, 2005; Drazen and Eslava, 2010; Golden and Poterba, 1980; Klomp and De Haan, 2013; Schuknecht, 1999)).[108]

The literature also shows strong effects on governmental budgeting in Germany. However, it is important to distinguish which administrative level is subject to research in Germany, since governmental layers have different discretionary leeways and are affected by different kinds of elections. In Germany, elections take place on the federal level for the German Parliament, on the state level for the parliaments of the *Länder* and on the municipality level for the local governments. Starting at the state level, Mechtel and Potrafke (2013) show that job creation schemes are more pronounced during election periods. Englmaier and Stowasser (2016) look at the allocation of loans by state owned banks during county elections and find an increase in loans during election times. Schneider (2009) analyses German NUTS1 regions and discusses different channels of manipulating the budgetary business cycle. Using a panel of West German cities, Furdas et al. (2015) examine spending patterns of West German cities during election times and find some evidence for adjustment of spending in different categories. Turning to the municipal level, Foremny and Riedel (2014) show that elections affect the tax setting pattern of municipalities regarding the business tax. Foremny et al. (2014) examine spending patterns on a local level, where they distinguish the election effect of the legislative and the executive. To the best of my knowledge, there is no

[108] For a literature overview of the relationship between election cycles and government spending see Foremny et al. (2014).

literature that shows whether the election cycle directly affects housing policies. Furthermore, it is also the first study for Germany that considers a local policy parameter that is not related to budgeting.

Second, my results also contribute to the ongoing discussion on how political alignment, council composition and the election system affect local housing policies. For Spain, Solé-Ollé and Viladecans-Marsal (2012) show that mayors with a larger majority tend to declare more land for development in a municipality independently of the political affiliation. In addition, Solé-Ollé and Viladecans-Marsal (2013) show that left wing governments in Spain have a tendency to convert less rural land into areas open for development. Garmann (2014) shows that mayors elected by the municipal council assign more land open for development than mayors elected directly by the constituency. The direct effect of local elections on housing policies has not yet been examined.

In assessing electoral effects, the question arises naturally which voters are most sensible to urban policies. The literature identifies the so-called homevoter. First introduced by Brueckner and Lai (1996) in a classic monocentric city model, they model the homevoter as landlord and tenant simultaneously. According to Fischel (2001) the homevoter pools his savings in his house which then serves as his only asset. Hence, homevoters approve policies that increase the value of their house and, of course, try to avoid policy measures that decrease housing prices, which might occur via the supply side or by negative side effects of urbanization like pollution and crime. Therefore, homevoters usually disapprove of policies that increase housing supply and hence decrease the value of their asset[109]. The occurrence of increasing homeownership and resistance to urban growth is also described in Ortalo-Magne and Prat (2014).

So what should be the expected effect of the presence of homevoters during election times? Since homevoters will aim to preserve the value of their houses, all municipal projects that foster urbanization will be opposed. Local politicians should be aware of this occurrence and if homevoters are crucial for reelection it is likely that the homevoters' calculus is taken into account by local politicians during local election times. This should hence be reflected in lower housing approval rates during election periods. Whether this result occurs in the German case is worth exploring since the homeowner rate is lower in Germany than in the US and other European countries.

[109]This statement applies in most cases but might be offset in cases where a certain population threshold enables the municipality to benefit from the provision of public goods like schools, kindergartens or public transport connections.

For the US, the literature finds general support for the homevoter hypothesis (Brunner et al., 2001; Brunner and Sonstelie, 2003; Hilber and Mayer, 2009; Dehring et al., 2008; Gerber, 1999; Gerber and Phillips, 2004). This effect is usually identified via public construction projects such as football stadiums or local school voucher initiatives.

Regarding the German context, the literature discusses different voting patterns between homevoters and leasevoters (citizens that rent instead of own an apartment). Furthermore, the literature finds a tendency against structural with regards to housing and urban development. Based on a referendum against part of the property investment project *Mediaspree* in Berlin (Germany), Ahlfeldt (2011) shows that the resistance against further construction is mostly driven by an expected loss of cultural amenities within cities. Ahlfeldt (2012) examines the pattern for a referendum on a soccer stadium in Munich in 2011 and finds that voters (regardless of being home- or leasevoters) in proximity to the project strongly voted against it, even though there was high support on the aggregated level. Ahlfeldt and Maennig (2015) apply the homevoter hypothesis to a public referendum on an airport concept for Berlin. They show that homevoters, unlike leasevoters, tend to vote more strongly in favor of topics that positively affect amenities surrounding their house. Where the literature on homevoters uses referendums to identify a possible homevoter effect, my study is the first to show that the homevoter might also affect policies during election times.

This paper is organized as follows: section 6.2 describes German municipalities, local elections and the underlying dataset. Section 6.3 describes my empirical strategy, shows my main results and applies robustness tests. Section 6.4 shows how homevoters seems to drive my main results and discusses alternative channels. Section 6.5 concludes my research.

6.2 Institutional setting and data

6.2.1 German municipalities

Municipalities are the lowest administrative tier in Germany. They provide a large variety of public goods (zoning, infrastructure, kindergartens and general public services) of which some are mandatory and other voluntary. Furthermore, some of these public goods are provided in cooperation with higher-tier administrative layers and others are solely the responsibility of the municipality. Most importantly, municipalities have a constitutional right to self-administration (*Kommunale Selb-*

stverwaltung, Art. 28 Abs. 2 GG).

This municipal self-administration is manifested by giving municipalities autonomy over land-use-planning as well as local taxation (business tax and two kinds of property taxes). Furthermore, municipalities have a high degree of autonomy for municipal land use plans, which is only limited by state and federal construction law. Revenues from local taxes account for 20% - 30% of the total municipal revenue. Hence, municipal funding is partly predetermined but municipalities have discretion here via local fees and local taxes and parameters on the spending side.

The permission for a construction project is in the hands of the building control authority (*Bauaufsichtsbehörde*), which checks whether the project fits the municipal zoning plans and if existing construction laws are violated. Therefore, municipalities do not have discretion over the process of approval, although they have powerful tools (like zoning) to lay the framework in which the building control authority may decide. This is important since the building control authority is usually located in the county administration and not in the municipality itself.[110]

In general, municipalities have four tools to shape local land use and the building structure. The Land use plan for the next 15 to 20 years (*Flaechennutzungsplan, §5 Abs. 1 Satz 1 BauGB*) and the municipal construction plan (*Bebauungsplan, §8 Abs. 1 Satz 1 BauGB*) act more as general plans / macro tools, a development freeze (*Veränderungssperre, §14,15 BauGB*) and the municipal accord (*Gemeindliches Einvernehmen, §36 BauGB*) act as micro tools for the urban shape. In the following, these tools will be described in more detail.[111]

The most general tool is the land use plan which assigns the residential, commercial and agricultural land use within municipalities. This plan is usually developed by the local municipality and approved by a higher tier of administration, usually the county. Nevertheless, this plan is merely a planning tool and not legally binding. The construction plan on the other hand is legally binding. Where the land use plan has to be created for the whole municipal area, the construction plan gives specifics about parceled areas open for development. Here, municipalities have full discretion to develop these plans by taking existing laws, like environmental issues, into account.

[110] Some big municipalities also have the building authority within their boundaries. This is, however, more of an exception. Any effects regarding the location of the building authority are negligible, since this paper focuses on smaller municipalities. Furthermore, standard errors are later clustered on a higher administrative level to take this effect into account.

[111] Detailed construction laws differ within West German federal states, although the legal tools described above are the same in all states. The later analysis will take this differences into account by using municipality fixed effects which include state fixed effects as well.

If municipalities strongly oppose certain construction projects even though they fit into the current municipal construction plan, municipalities have micro tools at hand. The most common tool is the development freeze, which allows municipalities to freeze a construction approval process in order to adjust the municipal construction plan. Usually this freeze lasts for two years but can be extended on a yearly basis for up to four years. It is also important to note that a construction freeze requires legal justification, which means it can not be prohibitive without a reason.[112]

A further micro tool is the municipal accord. When the building control authority receives the building application, it informs the municipality about the project. Then the municipality has two months to respond, otherwise the municipal accord is implicitly assumed. A municipality is legally not allowed to refuse consent for other than legal reasons. Furthermore, a municipality may give its consent with certain imposts, which might delay construction projects as well if legal concerns are valid. Nevertheless, it seems that in practice municipalities sometimes withhold consent for political reasons (Dirnberger, 2008), even though they might face legal consequences. Theoretically, the municipality has the best chance to withhold municipal consent if the parceled area does not have a municipal construction plan. Here, a municipality could argue that they need to adjust the construction plan if the building under construction might not fit into neighborhood.

In 2010[113], the federal court ruled that the building control authority was to be held accountable if municipal accord was illegally denied by the municipality. Previously, the accountability in case of an unlawfully declined construction project was not clearly divided between the county administration and the municipality. Hence, neither the building administration, nor the municipality were per se accountable and usually a court had to decide. Since then, municipally accords have not been used with such laxness because the county administration was controlling more strictly. This also justifies the use of the year 2010 as the upper bound of my panel, because it is likely that this effect diminishes.

So how may a municipality limit construction approvals in election times? Since the land use plan is rather imprecise and not legally binding, it does not allow a micro influence on certain projects. The municipal accord might be a potential angle but is also rather costly due to the potential legal consequences if denied

[112] All factors that can be determined in the construction plan are listed in §9 BauGB. In total there are 26 different factors that need to be determined. Specifically interesting for this paper are the amount of maximum apartments in one house, the amount of public housing, as well as environmentally protected areas.

[113] Specifically, the federal court ruled on this under case number *III ZR 29/10 from 16.09.2010*

for illegal reasons. The most promising tool might be the development freeze for specific development projects. Here, municipalities may either delay the approval until the elections are completed or change the municipal construction plan in such a way that inhabitants approve the process more or the project becomes unfeasible. In practice, this might for example mean that a new construction project is not allowed to have as many flats as initially planned or the construction plan should include more green areas in order to make the project unfeasible or at least be delayed.

6.2.2 Municipal elections

In Germany, municipal elections are usually held every five years, with the exception of Bavaria, where elections take place every six years. A summary of all municipal elections and their respective timings in the relevant period can be found in Figure 6.1. As one can see, municipal elections are not held simultaneously across the federal states during the period of my study. In 2014, the elections tend to cluster strongly, but this period is excluded from my research. It is interesting to note that even if local elections take place in the same year across different federal states, the timing within the year still differs. This is indicated by different lengths of the blue bars in Figure 6.1, where the length represents the share of the year until the election takes place.[114]

Local elections serve the purpose of electing the municipal council. The discretion of the mayor and the council varies slightly between the federal states but the municipality itself is always in charge of the local land use process. As pointed out, the timing of the local elections allows me to identify the variation of housing approval during election times and disentangles the election effect from common time effects.

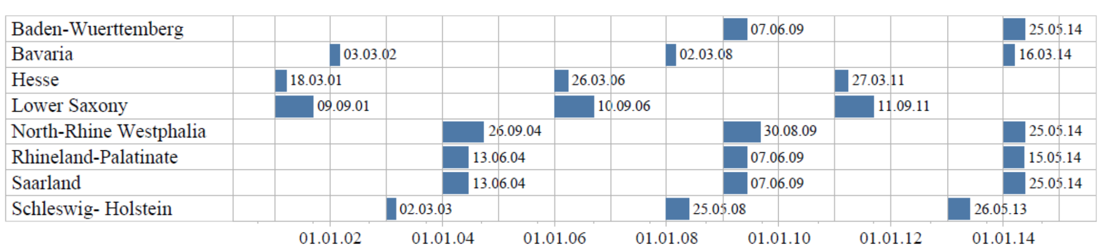


Figure 6.1: Municipal election dates and the intra-year timing in West Germany

[114]This intra-year timing will be taken into account by the econometric specification later on.

6.2.3 Data

The German Federal Statistics Office provides a rich data set on the municipal level, which allows a disaggregated analysis over time. Specifically, yearly data from all municipalities from 2002 until 2014 is provided. This enables me to observe housing approval policies over time and track their changes during election periods. All cross sections from 2008 onward were collected online[115], while all cross sections before 2008 were obtained from *Statistik lokal* CDs from the German Statistics Office. Furthermore, I enriched my dataset with information from the German census in 2011[116].

Regarding housing data, I collected the approved and finished housing and non-housing (*Nutzfläche*) area in square-meters (sqm). Using the approved sqm should provide sufficient variation to observe potential changes during election times. Furthermore, the data allows a distinction regarding finished construction and building licenses. Here, I gathered data on the absolute number of approved and finished houses and apartments. Furthermore the data allows me to track the approvals, completion and stock of houses with one, two or three and more apartments over time. My main dependent variable will be the ratio of approved sqm of housing units per 1,000 inhabitants. This should allow me to take different size effects of municipalities into account[117].

As independent variables over time, I include the population and the population density to take the total size and agglomeration differences into account. My data also allows me to model the demographic structure of the municipality over time by using the share of young (below 15 years) and old (above 65 years) inhabitants in the municipality. In order to proxy income differences across municipalities, I include the municipal share of federal income tax per capita. I collected the local tax rates and their respective tax bases, which are, however, not included. **?** show that business taxes are also manipulated during elections, which would feed into the error term and hence cause endogeneity. The two local property taxes A and B are at the discretion of local politicians, which leads me to the conclusion that these variables should also be excluded[118].

In addition, I gathered the share of homeowners from the national census in 2011 on the municipality level. This data allows a test of the homevoter hypothesis, as

[115] www.regionalstatistik.de

[116] https://ergebnisse.zensus2011.de

[117] In further robustness checks, I will use different normalizations to verify my results.

[118] Initial political budget cycle regressions for the rate of the property tax B shows some adjustment during election times. This encourages me to omit the tax rates from the regressions. The results are available from the author upon request.

outlined above. Unfortunately, information about homeowners is only available for the year 2011, which of course allows only a limited test and implicitly assumes that the relative spatial distribution of homeowners among municipalities is constant over time. I do not think that this assumption is too strict, since the homeowner rate should have a low within variation and for this variable I am mainly interested in the between variation during election times.

My dataset covers all West German municipalities from 2002 until 2014. Nevertheless, I limit my panel to observations until the year 2010 since it is likely that the leeway for municipalities to delay construction approvals decreased from 2011 on due to the ruling of the federal court (see section 6.2.1). In addition, the municipal data was changed by census corrections from 2011 onwards, which would introduce a structural break in my dataset.

Municipalities that were subject to mergers were excluded to avoid confounding factors. Furthermore, I excluded municipalities with less than 1,000 and more than 20,000 inhabitants. I set 20,000[119] inhabitants as my upper boundary for the included municipalities, since it is the official threshold for small towns in Germany. Small municipalities with less than 1,000 inhabitants were excluded as well due to a lack of variation in the housing approvals, as I will explain the following paragraphs.

Using the area of housing approval as dependent variable provides sufficient variation to identify my effect of interest as one can see in Table 6.1. In the raw data, the housing sqm are coded in units of 1000 sqm, where the last two digits are missing (i.e. 1.2 instead of 1233). Therefore, this variable accumulates some measurement error which should be more severe in small municipalities. Furthermore, observations with less than 100 sqm were censored for reasons of data protection. Both types of censoring justify my approach removing the smallest municipalities from my estimation sample. Losing the last digits removes valuable variation, which matters most in small municipalities. Moreover, small municipalities have, in absolute terms, a lower value of approved sqm and are therefore more likely to be affected by the bottom censoring. However, the measurement error that remains within my observations due to the censoring is likely to be randomly distributed and should only increase the confidence intervals.

The descriptive statistics can be found in Table 6.1. Here, one can see that the dependent variable ”Approved housing area in sqm per 1,000 inhabitants” offers a reasonable variation to identify potential election effects. The finished housing

[119]Even when increasing the upper threshold, the results remain similar. The results are available from the author upon request.

area sqm per 1,000 inhabitants offers a similar variation and hence it is natural to use this variable in a placebo test in the empirical section.

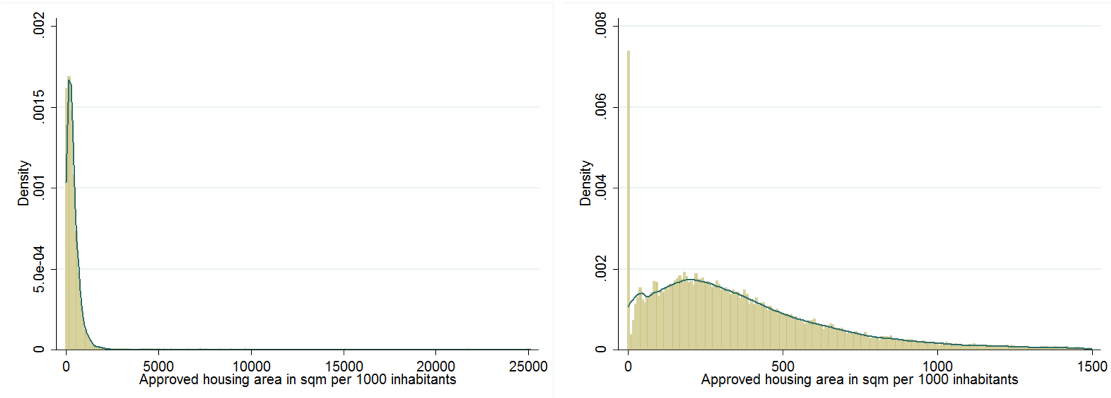
Table 6.1: Descriptive statistics of main dependent variables

	Mean	Std. Dev.	Min	Max	N
Approved housing area in sqm per 1,000 inhabitants	388	395	0	25,092	44,847
Finished housing area in sqm per 1,000 inhabitants	329	322	0	25,302	44,847
Population	5,025	4,286	1000	20,000	44,847
Share young(<15yrs.)	0.161	0.022	0.056	0.275	44,847
Share old(>65yrs.)	0.184	0.034	0.059	0.419	44,847
Population density	21.70	23.53	1.37	306	44,847
Share income tax per capita	295.52	84.15	0	779	44,847
Share of homeowners (0 - 100)	67.28	8.9	30.22	91.26	4,983

Source: Own calculations based on official statistics provided by the Federal Statistical Office.

When visualizing the dependent variable in Figure 6.2, one can see on the LHS in 6.2a, that the sample has some severe outliers which occur, however, in small numbers. Hence, I cut the distribution on the RHS to inspect the densest part of the data in more detail, which is shown in Figure 6.2b. Two features are striking; first, one can see that the dependent variable accumulates a certain amount of zeros (around 2500 observations) and that the distribution is otherwise smooth around the mean. This graphs shows the necessity to cut the distribution at the upper and the lower tail in robustness checks later on. Nevertheless, I keep these observations for my main analysis since this allows me to work with a balanced panel.

In the next step, I visualize the time pattern of my dependent variable. First, I plot the trajectories of the (mean) approved housing area by NUTS1 regions in Figure 6.3. It is apparent that all federal states follow a similar macro pattern of decreasing housing approvals over time which is mainly driven by a decrease in governmental social housing policies during the period. However, sufficient micro variation within the states can also be observed.



(a) Full distribution, bins = 200 **(b)** Outliers excluded, bins = 200

Figure 6.2: Density of the dependent variable

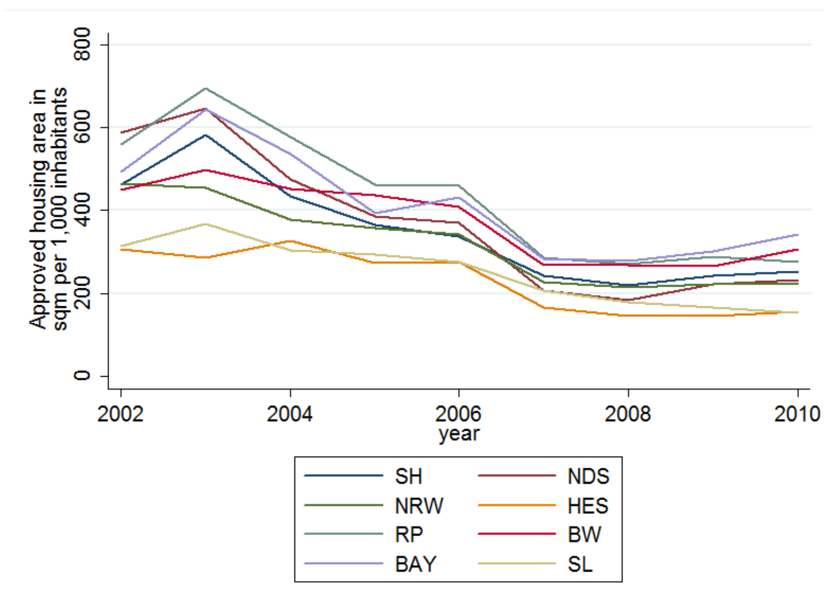


Figure 6.3: Development of approved housing area over time by Nuts1 regions

6.3 Empirical Strategy and Main Results

6.3.1 Empirical Model

My main empirical model looks as follows:

$$y_{i,t} = \sum elec_{i,t}\theta + \beta x_{i,t} + \mu_i + \tau_t + \epsilon_{i,t} \tag{6.1}$$

In (6.1), $y_{i,t}$ denotes the dependent variable, i.e. "Approved housing area in sqm per 1,000 inhabitants". $x_{i,t}$ is a matrix of the covariates as described in section 6.2.3. μ_i and τ_t indicate municipality and year effects, respectively. $\epsilon_{i,t}$ represents a well-behaved error term. The coefficient of interest is θ and defined as follows:

$$elec = \begin{pmatrix} Election_{t-1} \\ Election_t \\ Election_{t+1} \end{pmatrix} \quad and \quad \begin{aligned} &= 1 \text{ in the pre} - \text{election year}, \ 0 \text{ otherwise} \\ &= \tfrac{days\ until\ election}{365} \text{ in the election year}, \ 0 \text{ otherwise} \\ &= 1 \text{ in the post} - \text{election year}, \ 0 \text{ otherwise} \end{aligned}$$

Since municipal election dates vary considerably between the federal states[120] politicians have different time budgets to adjust housing policies in election years. A simple binary dummy for the election year would blur this effect, i.e. an effect for an early election might be overestimated, whereas the effect for a late election could be underestimated. Hence, the dummy is replaced with a weight between zero and unity, given how far the election stretches into the year[121]. For example, if the election takes place on the 31st of December in the given year, this weight will be unity. Otherwise it will be a fraction of the days already passed in the given year. The precise weight of each specific election year dummy can be inferred from Figure 6.1.

This setup of the election dummies allows me to disentangle the election year effects from the post and pre election year effects. Since election dates vary considerably between the federal states, my approach resembles a Difference-in-Difference setting, where I compare municipalities that are subject to local elections to municipalities that do not experience an election in the given year. Furthermore, the timing of the municipal elections should allow me to disentangle the election effect from the common negative time trend.

[120] During the investigated period, the earliest election took place after 60 days had passed, whereas the latest election took place after 269 days had passed in the year

[121] In further robustness tests, I will also compare the performance of this specification with more traditional ones.

Using (6.1), I will try to answer two hypotheses. First, I will verify whether the business cycle indeed exists for housing approval. This should be indicated by a negative coefficient of θ in the election year and a positive coefficient of θ in the year after the election. Second, I will examine whether homeowners in the respective municipality might drive these results. This hypothesis will be investigated in section 6.4. Specifically, I will distinguish between different types of houses (single/double family houses and houses with three and more apartments) and also interact the election year dummies with the share of homeowners in 2011 using the census data. If the adjustment is caused by homeowners, I would expect that the results are more pronounced for single and double family houses and that municipalities with a higher share of homeowners drive these results. Single and double family houses should be most affected by the presence of homevoters because similar buildings tend to cluster together within municipalities. Since homevoters oppose development policies in their direct environment, I would expect that the negative election effect should vanish if the number of apartments per house is increased (which of course increases strongly the likelihood that the inhabitants are not homeowners any more) because these projects are not conducted regularly in the same area of single and double family houses. One might still assume that homevoters should also oppose apartment buildings to the supply side effect on the housing market. But it is less likely that apartments and single / double family houses satisfy the same demand structure on the real estate market due to potentially different qualities of the estate.
Due to the fact that the tools to postpone construction plans need a majority in the municipal council I do not expect that all municipalities show this pattern during election times. I suspect that the effects, which are evaluated at the mean, will be of rather small magnitude since a suitable housing project needs to be present in the election year to conduct this kind of policy.

6.3.2 Main results

The main results can be reviewed in Table 6.2, where all estimated model resemble (6.1). Columns (1) - (5) use the approved housing sqm per 1,000 inhabitants as dependent variable, where (5) is my preferred estimation. Starting with a pooled OLS in Column (1), I subsequently add municipality effects in (2), year effects in (3), a combination of both effects in (4) and control variables in (5). For the remainder of this paper, (5) will serve as my benchmark. Starting with pooled OLS in (1), one can see descriptively that a reduction in approved housing area had already occurred one year before the elections, which remains similar when adding municipality FE in (2). However, when controlling for year fixed effects in (3), one can see that the pattern shifts to a reduction in the election year and a catch up in the post election year. Furthermore, when combining both kinds of fixed effects, this decrease and catch-up process becomes more clear in (4). The covariates add some explanatory power to the model in (5) but do not change the main result, which is a first indication of the main result's robustness.

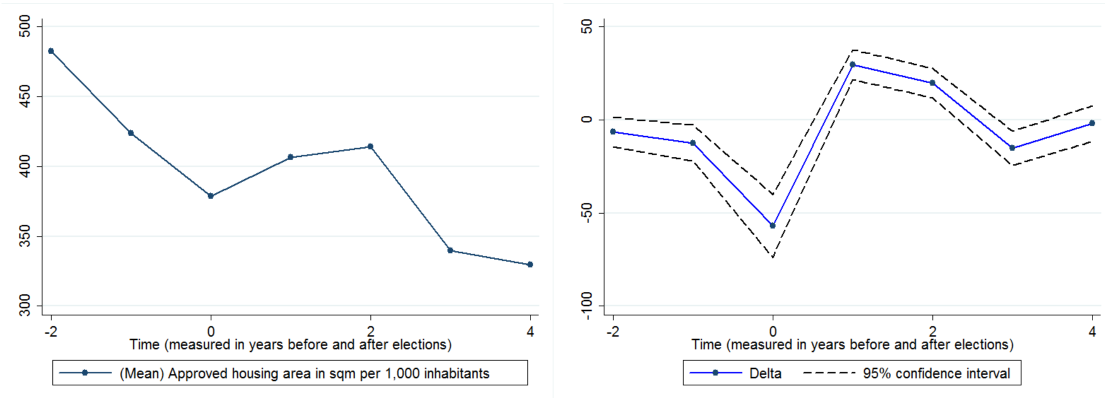
Regarding the economic significance of $Election_t$ in (5), the coefficient explains around 11.4 percent of the mean of approved housing area. For an average sized small town of 5,000 inhabitants, this translates to a decrease of around 200 sqm housing area during election times. This seems intuitive since the coefficient measures an evaluation at the mean. I suspect that not all municipalities use this tool during election times; it is merely a tool used when an application for a building permit is present.

Table 6.2: Main results

	(1)	(2)	(3)	(4)	(5)
$Election_{t-1}$	-49.191***	-41.465***	-17.797***	-6.508	-5.614
	(5.609)	(4.892)	(5.513)	(4.941)	(4.977)
$Election_t$	-63.575***	-66.952***	-42.070***	-44.405***	-44.358***
	(10.594)	(8.502)	(11.194)	(8.931)	(8.909)
$Election_{t+1}$	7.376	8.886**	19.314***	22.367***	24.794***
	(5.094)	(3.694)	(5.184)	(4.187)	(4.137)
Municipality FE	No	Yes	No	Yes	Yes
Year FE	No	No	Yes	Yes	Yes
Control variables	No	No	No	No	Yes
Adjusted R^2	0.003	0.003	0.088	0.125	0.137
N	44,847	44,847	44,847	44,847	44,847

Notes: Dependent variable: Approved housing sqm per 1,000 inhabitants. Robust Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

In the next step, I plot the adjustment of housing approvals during election times. The results can be reviewed in Figure 6.4. Figure 6.4a on the LHS shows descriptive values where the time period is centered for the election year at $t = 0$. In Figure 6.4b on the RHS, I plot the regression results based on Table 6.7 in Appendix 6.6. Here, I regressed only one election coefficient at one point in time but with a longer time horizon regarding the election. By comparing both Figures, one can see the adjustment during election times. Furthermore, by controlling



(a) Raw trend without time effect **(b)** Controlling for time effect

Figure 6.4: Housing approval pattern around election t = 0

for common time effects, the overall negative trend on the LHS is removed on the RHS. Furthermore the results show that the catch-up process from delayed housing approvals persists up to two years after the election.

6.3.3 Robustness

In the following subsection, I will conduct a series of checks to verify the robustness of my results. First, I will apply a series of placebo tests to examine whether my observed variation is caused by actions of municipalities during election times or merely a statistical artifact. This exercise is conducted in Table 6.3. Column (1) displays my main results as a benchmark. In (2), I replace the municipal election dummy with the state federal election dummy and (3) uses the finished, instead of the approved, housing area.

Regarding the state elections, I can not control for the year prior to and following the election since during my observation period some federal elections took place in two consecutive years in the same federal state. (2) verifies that there is no political adjustment of housing policies during state elections. (3) shows that the adjustment of housing approval during election times is not caused by random correlation. This is done by investigating the pattern of finished housing area during election periods. Finished housing area should not be at the discretion of local politicians because once the approval is given, the local landlord will follow his own agenda and schedule for the construction project[122]. Since the

[122] Nevertheless, local politicians might still influence the finished housing area during election via timed housing approvals in the years before the election. Since each housing project follows a unique schedule, it is rather unlikely that housing approvals could be timed with such foresight.

election coefficients are insignificant and of small magnitude in (3), I conclude that there is no willing adjustment of finished housing area but some alterations to the approved housing area. This is especially interesting, since approved and finished housing area have similar distributions as shown by the estimated density for both variables in Figure 6.5 in Appendix 6.7. These results verify that housing approvals are willingly adjusted during election times, where other elections from different tiers or finished housing area, which is not at the discretion of politicians, do not show any effect.[123]

Table 6.3: Placebo tests

	(1)	(2)	(3)
Dependent Variable	Approved housing per 1,000 inh.	Approved housing per 1,000 inh.	Finished housing per 1,000 inh.
Used elections	*Municipal*	*State*	*Municipal*
$Election_{t-1}$	-5.614		-6.343
	(4.977)		(4.321)
$Election_t$	-44.358***	4.919	5.408
	(8.909)	(4.027)	(9.747)
$Election_{t+1}$	24.794***		3.232
	(4.137)		(3.450)
Adjusted R^2	0.137	0.136	0.049
N	44,847	44,847	44,847

Notes: Robust standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

In Table 6.4, I exclude single federal states to verify the stability of my results. One can see that magnitude and significance vary slightly but the sign of the coefficient remains unchanged. My results remain similar and therefore I conclude to observe a general phenomenon.

Afterwards, I cluster my main results on a higher administrative level in Table 6.8 in Appendix 6.6. Specifically, I follow the Classification for Territorial Units for Statistics NUTS (French: *Nomenclature des unités territoriales statistiques*).

[123] I also conducted regressions for finished non-housing area. Here, the confidence intervals of $Election_{t-1}$,$Election_t$ and $Election_{t+1}$ overlap strongly which indicates that there is no change of pattern during election times. I did not show the results since this regression has a very low R^2 of 0.023, where the significant coefficient might actually be misleading. The result is available from the author upon request.

Table 6.4: Exclude single federal states

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Excluded state	SH	NDS	NRW	Hessia	RPF	BW	Bay	SL
$Election_{t-1}$	-5.591	-5.204	-4.187	-12.294**	-9.010*	2.321	-3.312	-5.279
	(5.438)	(5.842)	(5.102)	(5.493)	(4.688)	(5.873)	(5.613)	(5.002)
$Election_t$	-50.666***	-43.272***	-40.702***	-48.893***	-37.420***	-53.734***	-36.590***	-43.624***
	(9.198)	(12.722)	(9.764)	(9.366)	(8.827)	(10.296)	(10.202)	(8.957)
$Election_{t+1}$	27.817***	24.640***	27.145***	20.670***	29.465***	18.399***	21.715***	25.326***
	(4.270)	(4.910)	(4.281)	(4.620)	(3.698)	(5.257)	(5.426)	(4.173)
Adjusted R^2	0.140	0.120	0.135	0.137	0.167	0.138	0.130	0.137
N	41643	39078	43308	41580	38925	36576	28323	44496

Notes: Robust standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

Here, I cluster on the NUTS1, NUTS2 and NUTS3 level.[124] In addition, I also cluster my standard errors on the NUTS2-year and the NUTS1-year level. The results remain significant throughout different kinds of clustering. Nevertheless the results for the NUTS1-year two-way clustering are only significant on the 3.9% level, which might lead to the conclusion that a part of the variation could be driven by federal state specific policies in certain years.

To examine this further, I repeated my baseline regression with linear and squared NUTS1 specific trends to see whether the results change. The results are shown in Table 6.9 in Appendix 6.6. In (1) and (2) I included the NUTS1 specific trends in a linear and quadratic dimension. In (3) and (4) I verified as well whether outliers drive my results and repeated the benchmark regression by omitting all observations with a higher value than 95% quantile in (3) and lower than the 5% quantile (i.e. I removed all zero entries) in (4). Throughout all tests, my results remain significant and of similar magnitude. Therefore I conclude that my results are not driven by federal state-specific policies nor outliers in my dependent variable. This increases the confidence in my results and I conclude that my observed pattern is not driven by some specific effects from a higher degree of administration like the counties or the federal state.

In a last series of robustness tests, I verify the stability of my results with regard to assumptions about the normalization of my dependent variable, the measurement of the election effect and the population threshold to include municipalities in the sample. First, I verify in Table 6.10 in Appendix 6.6 whether different nor-

[124] NUTS units usually follow existing local administrative structures and consist of three levels. Usually the level of NUTS is assigned via population thresholds: NUTS1 corresponds in Germany to the *Länder*, NUTS2 to administrative districts (which do not have any governmental responsibilities in the German context) and NUTS3, which corresponds to German counties where the building authority is located.

malizations of the approved housing area show different patterns. (1) shows the baseline results for comparison, (2) simply shows the level of approved housing area in sqm without normalization, (3) is the log version of my baseline specification, (4) normalizes with the stock of housing area from the land use plan in 2004[125]. Throughout (2) to (4) the sign of the coefficients and the significances remain the same. Second, I change the specification of θ in Table 6.11 in Appendix 6.6. In (1) I use my benchmark regression and add also a state election dummy. In (2), I combine $Election_t - 1$ and $Election_t$ into one coefficient and weight the indicator by the month in which the local election takes place. In (3) θ is weighted by the months that passed into the election year and (4) uses a traditional binary variable whether the given year is an election year or not. Throughout all specifications, my main results remain unchanged. Third and last in Table 6.12 in Appendix 6.6 I vary the lower bound of the population threshold to include or exclude municipalities into the sample. When using any arbitrary population threshold for the lower bound, my main results remain unchanged. When I also include the smallest municipalities, i.e. municipalities with fewer than 500 inhabitants, the effect vanishes and the R-squared drops as well. This reflects the concerns regarding the bottom censoring I expressed in section 6.2.3.

6.4 Mechanism

So far, this paper has shown an adjustment of approved housing construction during election times in small to medium sized West German municipalities. This effect remains the same throughout different robustness tests. In this section, I will shed some light on the underlying mechanism behind this adjustment. I consider two explanations, which might be either an adjustment of politicians towards the homevoter or a general reduction in government activity during election times. An indirect test of political adjustment during election times would be to verify whether the adjustment could be explained by the presence of homeowners. If the results show a significant effect, I would interpret this as the first suggestive evidence in favor of the homevoter hypothesis. This would allow me to rule out reduced political activity during elections as an alternative explanation.

In order to test for my hypothesis, I conduct two empirical exercises. First, I verify whether the adjustment of approved housing licenses is driven by single

[125] Land use data is only available in 4 year intervals before 2008 and on a yearly basis from 2009 on.

family houses, which is more likely a concern of homevoters. Second, I use data from the census in 2011 and check whether the share of homeowners explains the adjustment during election times.

The first exercise is conducted in Table 6.5. (1) replaces the approved sqm with total sum of approved houses per 1,000 inhabitants. (2) uses the approved apartments instead of the houses, (3), (4) and (5) use only houses with one, two or three and more apartments per house, respectively. First, one can see that the approved effect still persists in (1) and (2). Second, the effect is the most significant in economic and statistical terms in (3) and (4) for private residential buildings with only one or two apartment, which are most likely being constructed or owned by homeowners. It is interesting to note that the effect diminishes when housing categories with more than two apartments per house are considered in (5). This is first suggestive evidence for the homevoter hypothesis, since single / double family houses and multi-family houses usually tend to cluster together and homevoters care about their direct environment.

Table 6.5: Housing construction categories

Dependent variable	(1) Approved house licences per 1,000 inh.	(2) Approved apartments per 1,000 inh.	(3) House appr. with 1 app. per 1,000 inh.	(4) House appr. with 2 app. per 1,000 inh.	(5) House appr. with 3+ app. per 1,000 inh.
$Election_{t-1}$	-0.040	-0.050	-0.041	0.002	-0.011
	(0.035)	(0.042)	(0.034)	(0.010)	(0.019)
$Election_t$	-0.275***	-0.329***	-0.229***	-0.098***	-0.002
	(0.055)	(0.075)	(0.052)	(0.024)	(0.045)
$Election_{t+1}$	0.154***	0.189***	0.122***	0.052***	0.015
	(0.030)	(0.037)	(0.028)	(0.010)	(0.018)
Mean effect t	11.7%	11%	11.5%	17%	0.04%
Mean effect $t+1$	6.6%	6.3%	6.1%	9.5%	3.2%
Control variables	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.118	0.122	0.091	0.100	0.008
N	44,847	44,847	44,847	44,847	44,847

Notes: Robust Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

To examine this theory further, I verify whether the effect is more pronounced in municipalities with a higher share of homeowners. Therefore, I will interact the election year dummy with the share of homeowners in 2011 in the respective municipality. The modified version of(6.1) looks as follows:

$$y_{i,t} = \sum elec_{i,t}\theta + (elec_{i,t} \times v_{i,t})\,\eta + \beta x_{i,t} + \mu_i + \tau_t + \epsilon_{i,t} \qquad (6.2)$$

In (6.2), all parameters are defined as before, where $v_{i,t}$ is a dummy variable that defines in which quantile the share of the homeowners from the respective municipality is. Here, I split $v_{i,t}$ at different quantiles (25%,50%,75%,90%). η denotes the estimation coefficient.

The results are shown in Table 6.6. In (1) $v_{i,t}$ indicates whether the respective municipality is in the first quartile of the homeowner shares, in (2) $v_{i,t}$ indicates whether the homeowner share is above the median, in (3) $v_{i,t}$ represents whether a municipality has a homeowner share in the fourth quartile and in (4) $v_{i,t}$ represents whether a municipality is above the 90% quantile. In (5) and (6) I only use municipalities with a share of homeowners in the first or fourth quartile.

The coefficients show the expected sign according to the homevoter hypothesis. Where lowest share of homeowners almost negates the election year effect $Election_t$, the effect becomes negative and increases in magnitude when the homeowner share is relatively high, i.e. in the 90% quantile. The effect for the 25% and 90% also shows statistic significance on the 10% value but the effect of the homevoter becomes more significant when comparing only municipalities with the highest and lowest share of homeowners in (6). Furthermore, the mean effect more than doubles when using only municipalities with a high homevoter share. I interpret these results to mean that my observed negative election year effect is driven by the homeowners share. Combined with the fact that the negative variation stems from single and double family houses, I regard my findings as suggestive evidence for the homevoter hypothesis.

Table 6.6: Interactions with share of homeowners

Dataset	(1) Full Sample	(2) Full Sample	(3) Full Sample	(4) Full Sample	(5) Only 1st and 4th Quartile	(6) Only 1st and 4th Quartile
$Election_{t-1}$	-5.557	-5.602	-5.637	-5.656	-2.494	-2.560
	((4.973)	(4.976)	(4.977)	(4.973)	(8.478)	(8.478)
$Election_t$	-52.341***	-35.538***	-38.492***	-39.366***	-12.250	-21.405
	(10.284)	(10.730)	(9.591)	(8.984)	(16.253)	(13.676)
$Election_{t+1}$	24.858***	24.829***	24.797***	24.794***	31.173***	31.132***
	(4.141)	(4.143)	(4.137)	(4.136)	(6.818)	(6.789)
Homeowner Quantiles interacted with $Elec_t$						
25% $<$	33.337*					
	(17.250)					
$>$ 50%		-18.473				
		(16.389)				
$>$ 75%			-27.593		-46.842*	
			(25.026)		(28.372)	
$>$ 90 %				-66.620*		-77.935**
				(36.935)		(38.869)
Adjusted R^2	0.137	0.137	0.137	0.137	0.107	0.107
N	44,847	44,847	44,847	44,847	22,419	22,419

Notes: Robust Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

My approach provides a straightforward setup to test for the homevoter hypothesis during election cycles. Nevertheless, having only one cross section of homeowner shares provides rather little variation, as can be seen in Table 6.1. A clear direct test of this hypothesis would be to verify whether the amount of development freezes or declined municipal accords for housing construction approval decreases during election times and whether a causal effect of the lower approval rate is linked to the share of homeowners. In terms of a natural experiment it would be useful to randomly assign different shares of homevoters to a municipality and observe whether this causes an increase in measures in municipal councils to postpone construction approvals. However, exhaustive data regarding declined municipal accords and development freezes is not available[126] and the spatial variation of homeowners is rather consistent over time and not available as a time series. Therefore, my results will provide suggestive evidence for the homevoter effect and show that the presence of homeowners actually accompanies political business cycles for housing policies during election times.

6.5 Conclusion

This paper investigated the adjustment of housing policies during municipal election years in West German municipalities. Using a sample of 4,983 West German municipalities from 2002 to 2010, this paper shows that housing construction is reduced during election times and increased afterwards. The results remain unchanged economically and statistically after a variety of robustness tests.

Furthermore, this paper provides suggestive evidence that the magnitude of the effect is driven by the share of homeowners and single and double apartment houses, which are more likely in the hand of homeowners. Homevoters, i.e. citizens who own their house instead of leasing it, have the strongest incentive to oppose development policies in their neighborhood since they have an incentive to maintain the value of their main house. My paper contributes to the existing literature regarding the impact of the homevoter by showing the influence not only through local referendums but during election times as well. This adds an important new policy parameter to the literature on political business cycles, which is so far mostly concerned with governmental spending and budgeting.

The aforementioned mechanism is important because this effect might delay the

[126] Gathering the data manually would require the screening of all municipal council protocols from all 4,983 municipalities over nine years.

natural urbanization /suburbanization processes, which might put even more pressure on the recent rent increase in agglomerations and the surrounding areas. Hence, the results imply the importance of clear responsibilities in housing policies. As this paper clearly shows, a non-transparent division of liability between different layers of governments for housing policies might lead to inefficient low housing approvals during election times. This mechanism is most efficiently dealt with by establishing clear responsibility structures, e.g. through measures like the court ruling in Germany in 2010.

Future research should investigate whether the share of homeowners can explain the use of legal tools by municipal councils to delay housing construction. It is important to investigate whether this construction approval delay manifests itself in higher housing prices and, if this effect exists, the spatial extent. Another fruitful topic for future research is to dig deeper into a causal effect of homeowners on construction approvals. Is the electoral effect actually caused by the homeowner or an unobserved factor that is strongly correlated with the share of homeowners?

6.6 Appendix - Tables

Table 6.7: Different election timing

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Year	$t=-2$	$t=-1$	$t=0$	$t=1$	$t=2$	$t=3$	$t=4$
$Election_t$	-6.445	-12.426**	-57.025***	29.731***	19.737***	-15.215***	-2.047
	(4.012)	(4.911)	(8.645)	(4.074)	(4.069)	(4.691)	(4.880)

Notes: Robust Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

Table 6.8: Standard error clustering of benchmark results

	(1)	(2)	(3)	(4)	(5)	(6)
Clustering	Municipality	NUTS3	NUTS2-year	NUTS2	NUTS1-year	NUTS1
$Election_{t-1}$			-5.614			
(p-value)	(0.259)	(0.344)	(0.567)	(0.413)	(0.709)	(0.525)
$Election_t$			-44.358			
(p-value)	(**0.000**)	(**0.000**)	(**0.005**)	(**0.000**)	(**0.039**)	**(0.008)**
$Election_{t+1}$			24.794			
(p-value)	(**0.000**)	(**0.000**)	(**0.020**)	(**0.002**)	(0.146)	(**0.015**)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes

Notes: P-values in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

Table 6.9: Remove outliers

	(1)	(2)	(3)	(4)
	Nuts1 trend linear	Nuts1 trend squared	Remove top 5%	Remove zeros
$Election_{t-1}$	-0.886	1.790	-5.769**	-5.908
	(5.084)	(5.028)	(2.487)	(5.543)
$Election_t$	-45.265***	-35.390***	-25.737***	-43.867***
	(9.037)	(9.255)	(6.338)	(9.199)
$Election_{t+1}$	20.429***	21.925***	16.530***	24.444***
	(4.321)	(4.403)	(2.461)	(4.252)
Adjusted R^2	0.143	0.145	0.207	0.130
N	44,847	44,847	42,605	42,413

Notes: Robust Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

Table 6.10: Different specifications of dependent variable

	(1) Approved housing sqm per 1,000 inh.	(2) Approved housing sqm	(3) log Approved housing sqm per 1,000 inh.	(4) Approved housing sqm per stock sqm 2004
$Election_{t-1}$	-5.614	-0.022	0.018*	0.249
	(4.977)	(0.021)	(0.011)	(0.385)
$Election_t$	-44.358***	-0.160***	-0.051**	-2.666***
	(8.909)	(0.046)	(0.025)	(0.568)
$Election_{t+1}$	24.794***	0.088***	0.069***	1.890***
	(4.137)	(0.017)	(0.010)	(0.344)
Adjusted R^2	0.137	0.134	0.096	0.080
N	44,847	44,847	44,847	44,847

Notes: Robust Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations.

Table 6.11: Different specifications of election dummy

	(1) State elections	(2) Election month and pre-year	(3) Election month	(4) Binary election dummy
$Election_{t-1}$	-4.798		-5.654	-7.067
	(5.394)		(4.977)	(4.951)
$Election_t$	-44.624***	-12.907*	-44.011***	-17.608***
	(8.822)	(6.777)	(8.819)	(3.764)
$Election_{t+1}$	24.828***	26.535***	24.752***	23.982***
	(4.132)	(4.106)	(4.140)	(4.249)
$State\ Election_t$	2.027			
	(4.339)			
Adjusted R^2	0.137	0.137	0.137	0.137
N	44847	44847	44847	44847

Notes: Robust Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Own calculations.

Table 6.12: Results with different population thresholds

Pop. threshold	(1) -	(2) >500	(3) >1000	(4) >1500	(5) >2000	(6) >2500	(7) >3000	(8) >5000
$Election_{t-1}$	0.850	-3.066	-5.614	-5.120	-3.657	-5.529	-5.740	-6.314
	(5.091)	(4.564)	(4.977)	(5.504)	(6.163)	(6.265)	(6.833)	(4.398)
$Election_t$	-3.664	-30.912***	-44.358***	-39.791***	-36.852***	-33.910***	-28.550***	-29.403***
	(15.921)	(8.805)	(8.909)	(9.187)	(9.406)	(9.576)	(9.610)	(10.298)
$Election_{t+1}$	14.071***	19.530***	24.794***	26.006***	27.086***	25.289***	23.365***	19.694***
	(4.835)	(3.857)	(4.137)	(3.648)	(3.695)	(3.695)	(3.819)	(4.377)
Adjusted R^2	0.065	0.131	0.137	0.156	0.156	0.166	0.163	0.214
N	68938	56113	44847	36873	31275	26910	23841	15408

Notes: Robust Standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. *Source*: Own calculations.

6.7 Appendix Figures

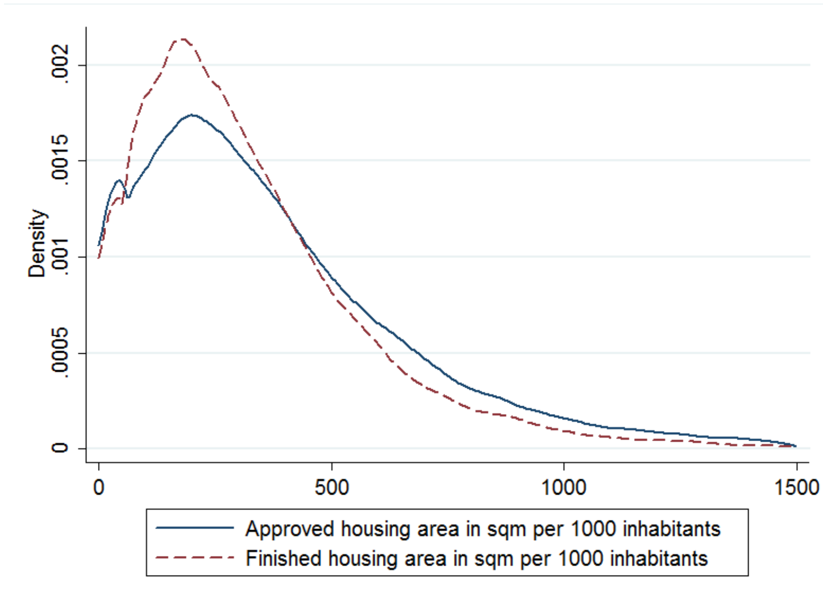


Figure 6.5: Comparison of densities of approved and finished housing sqm. *Source*: Own calculations.

Chapter 7

General Conclusion

This chapter will finish and conclude my dissertation. My goal in the first part was to guide the interested reader through the spatial dependence of German municipal policy, as well as the spillovers of entrepreneurial activity. The second part aimed to show the impact of local petitions and local elections on municipal housing policies in Germany. In the following I intend to show some potential topics for future research based on the chapters of my dissertation.

The first part dealt with spatial econometrics methods in general, where I hoped to learn from the remarks of Gibbons and Overman (2012) for my own research. I took their critique into account by applying extensive robustness checks with different spatial weighing matrices and careful consideration of the empirical specification. Furthermore, in chapter 3, I also included a natural experiment to speculate about the underlying reasons for tax mimicking. In chapter 4 I also applied an extensive grid search algorithm to find the best fitting neighboring weighting matrix from a multitude of neighbor specifications.

Regarding chapter 2, it is important to dig deeper regarding channels and limitations of debt interaction. One possible setting for a natural experiment to learn more about spatial dependence of debt and its potential limits might be the slump of the exchange rate of the Swiss franc to euro at the beginning of 2015. A significant number of cities in NRW took loans in Swiss francs instead of euro. When the fixed exchange rate of the Swiss franc to euro was abolished at the beginning of 2015, the amount of loans to be repayed increased drastically for some municipalities. Hence, this establishes an exogenous and asymmetric increase in the municipal debt and should also influence the potential to incur further debt. This should allow verification of the spatial dependence of municipal debt in a natural experiment. In this event, it might also be fruitful to develop a spatial difference-in-difference estimator to verify whether the increase in debt repayment changed the ability of municipalities to react towards neighboring debt changes that potentially did not have their loans in Swiss francs. With this research, one could investigate more deeply how strong municipalities react towards neighboring debt and which events might change this behavior.

Regarding chapter 3, I think the most important point was to show that local tax interaction does not necessarily imply competition for mobile capital and alternative explanations should be considered as well. One additional parameter, next to social interaction, that could also drive the adjustment process described in chapter 3 is the levy of local funds from municipalities to their respective county. Since the levy from municipalities to counties is also determined by a complex interplay between the normalized business tax rate, the municipal tax rate and the rate of the levy forward to the county this might be an additional driver of municipal tax interactions that remains to be examined.

Chapter 4 provides an interesting approach to simultaneously determine entrepreneurial spillovers in space and time. Since this chapter shows that the aggregation of the research unit (i.e. county or labor market region) determines the magnitude of the time and spatial spillover, future research should first try to increase the granularity of the data. If possible, one could obtain the addresses from the newly founded business in the hightech sector and geocode these, i.e. transform an address into a latitude and longitude. After obtaining several yearly cross sections, one could estimate a spatial lag for each specific business formation and compute a grid search over different k-nearest neighbor matrices (or also the distance decay from chapter 4) to find the optimal neighbor definition. This would not only allow us to track spillovers between regions, but within cities as well.

In chapter 5 and 6 it is interesting to note that housing policies within German policies are not as strongly driven by local referendums as prior research from about the US might suggest. Nevertheless, future research should also explore the effect of voting patterns within cities on housing policies and their development. So far, the literature mostly shows the preferences of the voters, but does not track the development after referendums or elections. One potentially interesting case study might be the local petition on the *Tempelhofer Feld* as described in 5. One could compute the effect of other large scale housing projects in Berlin on the rent / price level of surrounding buildings. This estimated model could be used to construct a counterfactual of the rent development on the *Tempelhofer Feld* if the petition had been declined and the housing project had been allowed.

Since chapter 6 shows that housing policies are adjusted in small municipalities around cities during municipal election times, it is interesting to observe whether this transfers into real changes of land / housing prices in the respective municipality. Furthermore, it is also an interesting topic to verify whether the delay in migration cycles has any effect on the rent and price levels within adjacent agglomerations. Interesting examples could be the cities of Munich, Frankfurt and Stuttgart since they have the same election cycle as their surrounding smaller municipalities. The two city states Berlin and Hamburg might cause identification issues since their election cycles partly differ from the surrounding municipalities in different federal states.

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German Summary

Wie reagieren Gemeinden mit ihrer eigenen Politik auf Schulden- oder Gewerbesteuerhebesatzänderungen von Nachbarsgemeinden? Gibt es Übertragungseffekte von Unternehmensgründungen in benachbarte Regionen? Um diese Fragen zu beantworten, müssen regionale Interaktionseffekte zwischen Gemeinden gemessen werden. Konkret wird gemessen, wie sich die Änderung der abhängigen Variable der geografischen Nachbarn auf die abhängige Variable in der untersuchten Region auswirkt. Wie sich solche Effekte mithilfe von regionalökonometrischen Methoden messen lassen, ist das Hauptthema des ersten Teils dieser kumulativen Dissertation. Kapitel 2 untersucht hierbei die regionale Interaktionen von deutschen Gemeinden über die Schuldenaufnahme, Kapitel 3 die regionale Interaktionen bei der Festlegung der lokalen Gewerbesteuer und Kapitel 4 regionale Ausstrahlungseffekte von neugegründeten Unternehmen in benachbarte Regionen. Diese drei Kapitel bilden den ersten Teil der vorliegenden Dissertation. Der zweite Teil ändert den Betrachtungswinkel weg von Interaktionen zwischen geografischen Nachbarn hin zu dem Einfluss politischer Beteiligungsinstrumente auf die Wohnungspolitik innerhalb von Gemeinden. Kapitel 5 untersucht hierbei den Einfluss von Bürgerbegehren in Bayern, wohingegen Kapitel 6 untersucht, ob sich während der Kommunalwahlen das Bewilligungsverhalten bezüglich Wohnungsbauvorhaben ändert.

Ein Verständnis für die Gründe von Gemeindeinteraktionen bei der Steuersetzung und der Schuldenaufnahme ist wichtig, da die Literatur die Imitation von Steuersätzen üblicherweise als Steuerwettbewerb zwischen Gemeinden interpretiert. Kapitel 2 zeigt jedoch, dass ein Wettbewerb von lokalen Regierungen um mobiles Kapital auch über die Verschuldung stattfinden kann. Kapitel 3 zeigt des Weiteren, dass eine positive Interaktion von Steuersätzen auch auf einen sozialen Lernprozess im Rahmen einer Reform mit ungewissen Konsequenzen hindeuten kann. Diese Einsichten sind wichtig, da Wettbewerb um mobiles Kapital und Lerneffekte bei einem politischen Umfeld mit Unsicherheiten zu diametralen Politikempfehlungen führen können: ein mehr oder weniger an Eingriffen in die Gemeindeautonomie. Außerdem ist es wichtig zu verstehen, inwieweit sich eine hohe Zahl an Unternehmensgründungen in einem Kreis in benachbarte Regionen fortpflanzen kann und wie lange solche Impulse in dieser bestehen bleiben. Das Verständnis für diese Muster erlaubt es, eine kohärentere Wirtschaftspolitik zu gestalten, da Gründungsaktivität eine positive Auswirkung auf das Wachstum des Bruttoinlandsproduktes in einer Region hat. Ferner ist es relevant, wie sich verschiedene politische Beteiligungsinstrumente auf die Wohnungspolitik einer Gemeinde auswirken, da die Flächennutzungspolitik eine der hoheitlichen Aufgaben der Gemeinden ist und lokale Wohnungspolitik über die Angebotsseite einen starken Einfluss auf Immobilienpreise hat.

Im Detail beschäftigt sich Kapitel 2 mit regionalen Übertragungseffekten der Verschuldung von Gemeinden in der geografischen Nachbarschaft. Basierend auf den theoretischen Überlegungen kann, unter gewissen Annahmen, gezeigt werden, dass Gemeinden im Rahmen eines Steuerwettbewerbes um mobiles Kapital einen Anreiz haben, ihre Schulden zu erhöhen. Anschließend wird für die Bundesländer Bayern und Nordrhein-Westfalen (NRW) eine Analyse über die regionalen Abhängigkeiten der Verschuldung durchgeführt. Die Hauptanalyse wird mithilfe eines "Spatial Durbin" Modelles vorgenommen. Diese Spezifikation erlaubt es, neben der abhängigen Variable der Nachbarn ebenfalls die Kovariaten der Nachbarsgemeinden in die Regression aufzunehmen, welche ebenfalls die Verschuldung treiben könnten.

Die Hauptergebnisse zeigen, dass sich bei einer Erhöhung der Verschuldung der benachbarten Gemeinden um durchschnittlich 100 Euro pro Kopf, die pro-Kopf-Verschuldung der jeweiligen Gemeinde zwischen 16 und 33 Euro steigt. Diese Ergebnisse bleiben auch erhalten, nachdem verschiedene Robustheitstests durchgeführt wurden. Interessanterweise treten regionale Interaktionen nur bei Schuldenkategorien auf, welche sich in dem Ermessen von Lokalpolitikern befinden. Z.B. zeigt die Verschuldung von öffentlichen Unternehmen keine signifikanten Effekte und der Interaktionskoeffizient sinkt sehr stark. Darüber hinaus zeigen weitere Analysen, dass die regionale Interaktion in Gemeinden in NRW über Kassenkredite die stärkste Signifikanz und den höchsten Koeffizienten aufweist, während in Bayern die Interaktionen über Verschuldungen im Kernhaushalt den stärksten Effekt zeigen. Gegeben der unterschiedlichen ökonomischen Situationen in den beiden Bundesländern ist dieser Unterschied nachvollziehbar, da Kassenkredite in NRW häufiger verwendet werden. Diese Effekte bleiben auch signifikant, wenn eine dynamische Variante des ”Spatial Durbin” Modelles verwendet wird.

Kapitel 3 analysiert mithilfe detaillierter Daten über lokale politische und sozio-ökonimsche Netzwerke regionale Interaktionseffekte bei Gewerbesteuerhebesätzen zwischen Gemeinden in NRW. Als natürliches Experiment für die Untersuchung wird eine Erhöhung des hypothetischen Gewerbesteuerhebesatzes in NRW im Jahr 2003 zugrunde gelegt. Gemeinden haben einen Anreiz haben ihren eigenen Gewerbesteuerhebesatz nicht unter den hypothetischen fallen zu lassen, weil sie sonst fürchten, Zuweisungen aus dem kommunalen Finanzausgleich zu verlieren. Dadurch erzeugt diese Reform eine exogene Variation der Steuersätze, damit Steuerinteraktion zwischen Gemeinden gemessen werden können.

Basierend auf einem ’Spatial Lag’ Schätzer wird der von dieser Steuerreform ausgehende Anreiz als Instrument verwendet, um Steuerinteraktionen zwischen Gemeinden zu messen. Interessanterweise sind die Interaktionen zwischen Gemeinden im selben Kreis ökonomisch und statistisch signifikant, während Interaktionen zwischen geografischen Nachbarn keine statistische Signifikanz und einen geringeren Interaktionskoeffizienten aufweisen. Darüber hinaus sind die Effekte von kurzer Dauer und signifikante Interaktionen werden ebenfalls bei Gemeinden gemessen, welche sich Verwaltungsinstitutionen und Zugang zu denselben lokalen Medien teilen. In einem letzten Schritt wird der gemessene Effekt gegen traditionelle Erklärungsmechanismen falsifiziert. Hierbei zeigt sich, dass der Erklärungsansatz des Steuerwettbewerbes ausgeschlossen werden kann, da die Steuerbasis einer Gemeinde nicht von der benachbarten Steuersetzung beeinflusst wird. Darüber hinaus haben knappe Wahlen ebenfalls keine Auswirkungen auf die Interaktionen, womit der sogenannte ”Messlattenwettbewerb”, die Ausrichtung der gemeindlichen Steuerpolitik auf Nachbargemeinden aufgrund von politischer Verwundbarkeit, ausgeschlossen werden kann. Demnach lässt sich das Ergebnis als ein sozialer Lerneffekt in einer Umgebung mit unsicheren Ergebnissen des eigenen Handelns interpretieren. Durch die Erhöhung des hypothetischen Hebesatzes können Gemeinden die Konsequenzen der Festlegung des eigenen Hebesatzes auf die Zuflüsse aus der Gemeindeumlage schwer abschätzen. Deswegen orientieren sie sich an Gemeinden im selben Kreis, da sich Gemeinden über dieses Netzwerk wahrscheinlich abstimmen.

In Kapitel 4 wechselt der Fokus der Forschungsfrage von Interaktionen zwischen Gemeinden zu Übertragungseffekten zwischen regionalen Einheiten bei einer hohen Unternehmensgründungsaktivität. Die bisherige Forschung hat diese Übertragungseffekte entweder nur in der zeitlichen, oder in der räumlichen Dimension gemessen. Beide Dimensionen wurden bisher jedoch nicht gemeinsam in einer Forschungsarbeit gemessen, was mit der vorliegenden Dissertation durchgeführt wird.

Die Untersuchung basiert auf einem Längsschnitt von 402 deutschen Kreisen der Jahre 1996 bis 2011. Hierbei werden die Übertragungseffekte von Gründungen in der Hightechbranche und im verarbeitenden Gewerbe geschätzt. Als empirisches Modell wird ein dynamisches "Spatial Durbin" Modell verwendet, welches erlaubt, die regionale und zeitliche Komponente sowie die Kovariaten der Nachbarn miteinzubeziehen. Um der Kritik über arbiträre Definitionen von Nachbarspezifikationen zu begegnen, wird zur korrekten Spezifikation eine Rastersuche über verschiedene Nachbarsmatrizen angewendet. Diese erlaubt es die optimale Nachbarschaftsspezifikation zu identifizieren. Zudem wird mithilfe des final geschätzten Modelles eine "Impuls-Antwort-Analyse" durchgeführt, bei der die Anzahl der Unternehmensgründungen in Frankfurt am Main um zehn Prozent erhöht wird. Anschließend wird die Entwicklung der Gründungen mithilfe des finalen ökonometrischen Modelles mit und ohne Schock simuliert. Hierbei zeigt sich, dass der Übertragungseffekt im Hightechbereich stärker ist als im verarbeitenden Gewerbe und nach zwei Jahren abebbt.

Wurden bisher Übertragungseffekte zwischen Gemeinden betrachtet, so wechselt der Fokus der Forschungsarbeit von der Messung von Nachbarschaftseffekten auf die Auswirkungen politischer Beteiligungsinstrumente auf die Genehmigung von Wohnungsbauvorhaben innerhalb von Gemeinden. In Kapitel 5 wird untersucht, ob lokale Bürgerbegehren Auswirkungen auf die Bewilligung von Bauvorhaben haben. Kapitel 6 hingegen zeigt, wie die Bewilligung von Bauvorhaben im Wohnbereich während der Kommunalwahlen fluktuiert.

Kapitel 5 fokussiert sich auf das Bundesland Bayern, um den Effekt lokaler Volksabstimmungen auf den bewilligten Wohnraum zu messen. Theoretisch ist zu erwarten, dass Eigenheimbesitzer, sogenannte "homevoter" mit einer starken Präferenz für den Status quo in diesen Abstimmungen so stimmen, dass eine weitere Urbanisierung verhindert wird. Der Großteil der bisherigen Forschung hierzu untersucht den amerikanischen Raum und findet den vorher beschriebenen Effekt. In dieser Forschungsarbeit werden die Daten von *Mehr Demokratie e.V.* verwendet, welche alle lokalen Initiativen seit 1995 beinhaltet. Diese Daten werden mit einem detaillierten Datensatz der bayerischen Gemeinden zur Bevölkerungsentwicklung und zur Wohnraumbewilligung und - fertigstellung angereichert.

Im empirischen Teil wird untersucht, wie sich die Entwicklung der bewilligten

Wohnfläche in einer Gemeinde entwickelt, nachdem dort eine lokale Initiative stattgefunden hat. Hierbei findet ein Vergleich zwischen Gemeinden statt, die niemals eine Initiative und die genau eine Initiative in der Beobachtungsperiode hatten. Gemeinden mit mehr als einer Initiative werden von der Untersuchung ausgeschlossen. Um die potentielle Endogenität zwischen unbeobachteten Wählerpräferenzen und dem Aufkommen von lokalen Bürgerbegehren zu berücksichtigen, wird neben einem Kleinstquadratschätzer (KQ) ebenfalls ein Differenzen-in-Differenzen-Ansatz und ein "Propensity Score Matching" verwendet. Bei dem Propensity Score Matching werden die Untersuchungs- und Kontrollgruppe auf Basis der Trends der Wohnraumbewilligung vor der lokalen Initiative zusammengeführt. Um die Entwicklung der Wohnraumbewilligung nach einer lokalen Initiative zu evaluieren, wird allen Gemeinden der Kontrollgruppe eine Placeboinitiative zugewiesen. Beim Verteilen der Placeboinitiativen wurde die gleiche Verteilung der Begehren über die Jahre hinweg wie bei der Behandlungsgruppe beibehalten. Unabhängig von der Methode, KQ-Schätzer mit fixen Effekten, Differenzen-in-Differenzen und "Propensity Score", zeigt sich keine signifikante Änderung von Wohnraumbewilligungen, nachdem eine Initiative stattgefunden hat. Interessant ist hierbei, dass die Schätzkoeffizienten und Standardfehler von einem kleinen Ausmaß sind, womit die Ergebnisse auf einen Nulleffekt hindeuten. Dieses Ergebnis steht im Gegensatz zur bisherigen Forschung, da insbesondere gemäß der Homevoter-Hypothese erwartet werden kann, dass Eigenheimbesitzer Initiativen nutzen würden, um die Urbanisierung der Gemeinde zu verhindern.

Das nachfolgende Kapitel 6 beschäftigt sich ebenfalls mit der Bewilligung von Wohnraumprojekten in Gemeinden. Dieses mal wird jedoch die Variation im Laufe von Kommunalwahlen in westdeutschen Gemeinden untersucht. Hierbei wird ein Längsschnitt von allen Gemeinden zwischen 1.000 und 20.000 Einwohnern in Westdeutschland verwendet. Anschließend wird ein KQ-Schätzer mit fixen Effekten für Jahre und Gemeinden verwendet, um zu messen, ob während des Wahljahres die bewilligten Wohnraumquadratmeter zurückgehen. Durch die unterschiedliche Terminierung der Kommunalwahlen zwischen den Bundesländern ähnelt dies einem Differenzen-in-Differenzen-Schätzer.

Die Ergebnisse zeigen, dass im Wahljahr die bewilligten Wohnraumquadratmeter um ca. elf Prozent, evaluiert am Durchschnitt, zurückgehen. Dieser Rückgang scheint in den anschließenden zwei Jahren aufgeholt zu werden. Interessant hierbei ist, dass dieser Rückgang nur bei der Bewilligung von Ein- und Zweifamilienhäusern signifikant und dem Anteil der Eigenheimbesitzer positiv korreliert ist. Dieses

wird als Evidenz für die Homevoter-Hypothese interpretiert, da Eigenheimbesitzer im Gegensatz zu Mietern einen stärkeren Anreiz haben, die Wohnungspolitik in Ihren Sinne zu beeinflussen, um den Wert ihres Eigenheims zu erhalten.
Dass der Bebauungszyklus während der Kommunalwahlen verzögert wird, ist eine neue Einsicht für die Literatur. Gerade im Lichte der zuletzt stark anziehenden Immobilienpreise in Agglomerationen könnte dieser Mechanismus einen Teil des bisherigen Preisanstieges erklären, da so die Angebotssituation in städtischen Gebieten noch etwas verschärft wird.
Der zweite Teil der Dissertation hat die Auswirkungen von lokalen politischen Beteiligungsinstrumenten auf die Wohnungspolitik deutscher Gemeinden untersucht. Hierbei hat sich gezeigt, dass lokale Bürgerbegehren keine Auswirkungen auf die lokale Wohnungsbaupolitik haben. Ferner scheinen Wohnungsgenehmigungen in Wahljahren reduziert zu werden. Dieses ist ein neuer Aspekt zur bisherigen Forschung, welche sich bislang hauptsächlich auf die Vereinigten Staaten fokussiert, wo lokale Bürgerbegehren einen starken Einfluss auf die Stadtpolitik haben. Eine mögliche Erklärung hierfür wäre, dass im deutschen Kontext Bürgerbegehren noch ein relativ neues Instrument im Vergleich zu den USA sind. Deswegen können Eigenheimbesitzer in Deutschland ihren Einfluss im Rahmen von Wahlen geltend machen und benötigen nicht Instrumente der direkten Demokratie, um Einfluss auf die Wohnungspolitik zu nehmen.

Declaration of Co-authorship

May 1, 2017

Declaration of co-authorship of Chapter 2 of the Thesis of Thorsten Martin

We, Rainald Borck, Frank Fossen and Ronny Freier, co-authored Chapter 2 of this thesis and the corresponding paper which is published in the Journal *Regional Science and Urban Economics*. Our contribution to this paper consisted of many formal and informal discussions about the topic of debt spillovers in the German context, the institutional setting, the empirical findings and their implications, as well as deriving a theoretical model and taking part in writing the paper.

Thorsten Martin made many important and independent contributions to all parts of the paper including, but not limited to, the implementation of the Spatial Econometrics routines, the implementation of the estimation strategy, as well as various sensitivity checks. Moreover, he conducted all the data work and a substantial part of writing the first draft and finalizing the manuscript.

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Declaration of co-authorship of Chapter 3 of the Thesis of Thorsten Martin

I, Sebastian Blesse, co-authored Chapter 3 of this thesis and the corresponding paper which is published as a *ZEW Discussion Paper*. My contribution to this paper consisted of many formal and informal discussions about the topic of tax mimicking in the German context, the institutional setting, the theoretical background, the empirical findings and their implications, as well as authoring the final draft.

Thorsten Martin made many important and independent contributions to all parts of the paper including, but not limited to, the implementation of the Spatial Lag IV estimator that takes advantage of a natural experiment, gathering and implementing additional geographical data as well as deep discussion regarding the scope of the paper. Moreover, he conducted all the data work and a substantial part of writing the first draft and finalizing the manuscript.

Mannheim, 5. Mai 2017

S. Blesse

Sebastian Blesse

Declaration of co-authorship of Chapter 4 of the Thesis of Thorsten Martin

I, Frank Fossen, co-authored Chapter 4 of this thesis and the corresponding paper that is currently prepared for submission to a scientific journal. My contribution to this paper consisted of many formal and informal discussions about the topic of entrepreneurial spillovers in the German context, the institutional setting, the theoretical background, the empirical findings and their implications, and taking part in writing the paper.

Thorsten Martin made many important and independent contributions to all parts of the paper including, but not limited to, the implementation of the Quasi Maximum Likelihood grid search algorithm, the impulse response analysis, unique changes of the underlying Matlab code to better adapt to the research question as well as various sensitivity checks. Moreover, he conducted all the data work and some parts of the writing.

Reno, NV (USA), May 1, 2017

Frank Fossen

Declaration of co-authorship of Chapter 5 of the Thesis of Thorsten Martin

We, Felix Arnold and Ronny Freier, co-authored Chapter 5 of this thesis and the corresponding paper that is currently prepared for submission to a scientific journal. Our contribution to this paper consisted of many formal and informal discussions about the topic of local petitions and housing supply in the German context, the institutional setting, the empirical findings and their implications. Furthermore, we prepared the local initiative data.

Thorsten Martin made many important and independent contributions to all parts of the paper including, but not limited to, the empirical implementation of the pseudo treatment, the implementation of the estimation strategy, as well as different sensitivity checks. Moreover, he conducted a substantial part of the data work and a major part of the writing the first draft and finalizing the manuscript.

Berlin, 02.05.17

Felix Arnold

Berlin, 3.05.17

Ronny Freier

Statutory declaration

Eidesstattliche Erklärung

Ich versichere an Eides statt, dass meine hinsichtlich der früheren Teilnahme an Promotionsverfahren gemachten Angaben richtig sind und, dass die eingereichte Arbeit oder wesentliche Teile derselben in keinem anderen Verfahren zur Erlangung eines akademischen Grades vorgelegt worden sind. Ich versichere darüber hinaus, dass bei der Anfertigung der Dissertation die Grundsätze zur Sicherung guter wissenschaftlicher Praxis der DFG eingehalten wurden, die Dissertation selbständig und ohne fremde Hilfe verfasst wurde, andere als die von mir angegebenen Quellen und Hilfsmittel nicht benutzt worden sind und die den benutzten Werken wörtlich oder sinngemäß entnommenen Stellen als solche kenntlich gemacht wurden. Einer Überprüfung der eingereichten Dissertation bzw. die an diese Stelle eingereichten Schriften mittels einer Plagiatssoftware stimme ich zu.

Berlin
3. Juni, 2017
Thorsten Martin