Empirical Essays on Fiscal Federalism

Inauguraldissertation

zur

Erlangung des Doktorgrades

der

Wirtschafts- und Sozialwissenschaftlichen Fakultät

der

Universität zu Köln

2016

vorgelegt

von

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aus

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Tag der Promotion: 13. Mai 2016

Acknowledgment

To answer the question "What are you proud of?" will be easy from now on. Undertaking a PhD pushes one to ones limits and is a life-changing experience. I would not be where I am today without my amazing family, friends, flatmates and colleagues. ***Thanks a million*** to all of you!!!

Most of all, I would like to thank **Caroline-Antonia Hummel** for immersing herself in the world of academics with me (and all what came along with it). I love working with you. Without our friendship completing this thesis would have been so much harder! **Philipp Dörrenberg**, I owe you so much. I know how lucky I am to have you as friend and co-author. Thanks for writing a paper with me. I cannot imagine how the last years would have been without my office partner, **Eva Gerhards**. Thanks for all the fun we had sharing an office, for always listening and providing valuable advice and emotional support. You are my sparring partner – not only in academics.

I am deeply indebted to **Michael Thöne** for giving me the opportunity to work and learn at the FiFo. To work in policy consulting while doing your PhD is not the easiest thing to do, but – as the following chapters will show – it can be a fruitful symbiosis. Thank you so much for always having faith in my skills and for giving me so many responsibilities and liberties. These last five years enabled me to grow personally, professionally, and academically.

I also would like to express my gratitude to **Felix Bierbrauer** for supervising my thesis. I truly appreciate that you always found time for valuable academic advice and guidance. Thank you for supporting my idea to study applied topics of public economics. I am also grateful to **Andreas Peichl** for agreeing to be the second supervisor of this thesis and for providing valuable advice. Besides those particularly mentioned, there are various other people (academic peers, referees and employees of various German ministries) who left their footprint on this thesis. My thanks belongs to all of them!

The biggest thanks goes to my parents, **Barbara** and **Walter Rauch** – I am what I am mostly because of you. To know that I can always count on you, made and makes my life so much easier. Thank you so much for your endless love and support. Last but not least, **Johannes**, you are the best brother imaginable. Thank you for going through live with me!!!

Contents

List of Tables

List of Figures

Chapter 1

Introduction

Fiscal federalism. Among public economists there is virtual unanimity that fiscal federalism is the "optimal form of government" (Oates 1972, page 15). According to the theory of fiscal federalism a federal government structure tackles the trade-offs between the two poles of extreme centralized and decentralized government. It assigns the three central economic functions of the public sector – resolving the distribution, stabilization and allocation problem – to the level of government which performs best: The higher levels of government are responsible for maintaining economic stabilization and attaining equitable distribution. The lower levels of government are responsible for establishing efficient allocation by the decentralized provision of public goods, among others ("decentralization theorem") (Oates 1968, 1972; Musgrave 1959). However, employing a federal system of government does not only demand the divisions of functions to the appropriate level of government, but also the fiscal instruments to fulfill them. The most important revenue instruments being taxes, debt and – what is unique to a federal system – intergovernmental transfers. It lies at the core of the theory of fiscal federalism to provide a normative framework to evaluate the appropriate division of functions and fiscal instruments between the different levels (e.g., McLure 1983). In practice, employing a federal government structure triggers an ongoing *assignment problem*. Nevertheless, this finds its justification in its claim for superiority. All these issues render research on fiscal federalism vital.

It does not come as a surprise that the German system of federalism is far from being optimal from a theoretical point of view. A good example for this is that the local level has the autonomy over the volatile and mobile business tax. The *mature* German system of federalism is not designed from scratch on the drawing board by economists. It is the

reflection of ongoing social, political and economic change with the European integration towards a supranational level of government merely representing the tip of the iceberg. The actual division of functions and fiscal instruments between the different levels of government and the layers of government itself are subject to an ongoing political debate and reformation. But this is inherent to a system of fiscal federalism. For empirical economists this inherence is attractive: With its 16 federal states and more than 10,000 municipalities the German fiscal federalism is an ongoing laboratory with great institutional variety and frequent reforms.

The aim of this dissertation is *not* to employ the theory of fiscal federalism on general grounds to the German system of federalism and to provide a comprehensive review on potential ideas of improvements in the form of policy recommendations. This dissertation acknowledges that a radical reform is rather unlikely. This is why it employs established economic principles and tools to the lowest federal level of government in Germany – the municipalities – to pointedly answer economic questions and – if possible – to derive and provide adequate policy implications and recommendations. It draws on the full spectrum of economic theory and does not concentrate on the theory of fiscal federalism. The major keywords being: Race to the bottom (Chapter 2), inverse-elasticity rule (Chapter 3), common pool problem (Chapter 4) and tax smoothing (Chapter 5). In short, the *main objective* of this dissertation is to put conventional economic concepts into the context of a federal environment.

Data. After having established the main motivation to focus this dissertation on the German federal landscape, let me address another reason: data. Germany has a detailed statistical reporting system on all levels of government. Comprehensive reporting rules lie the basis for a common statistical framework in all German federal states. There is a broad spectrum of demographic, social and economic indicators which is gathered annually for all German municipalities and counties. This enables empirical research on the lower federal levels. The data is accessible for the public. However, it is not provided ready for use in a single dataset. It has to be collected from a variety of sources. In particular, if one wants to use time series data with a long panel dimension. This leads over to the next reason: existing body of applied literature.

Applied literature. When we started this research, the existing body of applied academic literature employing research in this arena, i.e., analyzing data of German municipalities empirically, was rather scarce. Major exemptions were, e.g., Becker et al. (2012), Büttner (2003, 2005, 2006) and Egger et al. (2010). Potential explanations for this are

twofold: The practical challenges on data availability might pose one. On the other hand, the number of academic researchers having a solid background on the complex German institutional system and its ongoing reforms is rather limited. In the course of writing this dissertation a rising academic interest in this topic has evolved. More and more literature is emerging (e.g., Baskaran 2013, 2014, 2015; Blesse and Baskaran 2014; Borck et al. 2015; Fuest et al. 2016). Against this backdrop, this dissertation provides four further contributions belonging to the field of applied literature on German municipalities.

1.1 Key issues

This section comments on key issues: It shortly introduces the research questions, remarks on the common empirical background of the following four chapters and discusses the related literature. Last, the policy implications are summarized.

1.1.1 Research questions

The common background of the four main chapters of this dissertation is that they put established economic principles and tools in the federal environment. All of them study research questions of practical economic and political relevance at the local level empirically using German administrative data. All chapters have their *starting point* in economic theory. **Chapter 2** studies the incentive effects of municipal fiscal equalization systems. The hypothesis of standard tax multipliers as a driver of local tax policy has long been discussed in the applied literature on the evaluation of and reform options for fiscal equalization systems. However, an explicit test of this hypothesis was missing from the academic literature. We close this gap by approaching the following research questions:

- Do standard business tax multipliers have an effect on actual business tax multipliers?
- Have practitioners at the state level devised a clever mechanism to circumvent common pitfalls of local tax policy?

Chapter 3 studies the real-world tax policy at the local level. From a theoretical perspective the seminal contribution of Ramsey (1927) shows that optimal tax rates on goods depend inversely on their elasticity of demand. However, it is an open question whether

real-world policy makers apply optimal taxation rules. This paper employs the German case – municipalities have the autonomy on the business and the property tax – as empirical testbed. This leads to the following research questions:

- Do real-world policy makers apply the rules of Ramsey by imposing different tax rates on goods with different tax elasticities?
- In other words: does tax policy set higher tax rates on goods that are less responsive to taxation relative to more responsive goods?

Chapter 4 applies the common pool problem to the political environment. It studies whether municipalities affected by a municipal territorial reform exploit their common pool and behave opportunistically by strategically taking on debt to finance (inefficient) public projects. Empirical research on the "tragedy of the commons" (Hardin 1968) is, in particular, compelling since the veracity of this claim is successfully challenged by recent developments in the literature of the "commons." My research questions are:

- Do municipalities free-ride and exploit their common pool in the event of a territorial reform?
- Is the size of the effect greater the smaller the municipality's own size relative to its common pool?
- Are there factors mitigating or preventing opportunistic behavior?

Chapter 5 puts Barro's (1979) tax smoothing in the federal context. It studies whether ruling out debt finance could prevent subnational governments that enjoy tax autonomy over certain taxes from engaging in welfare-enhancing tax smoothing. This paper challenges credit rationing at the local level on these grounds. It analyzes the empirical relationship between credit access and tax smoothing using the case of municipalities in Germany with the focus on the following question:

- Are local tax rates less volatile in federal states where municipalities have easy access to credits?

1.1.2 Empirical background

All research questions are answered empirically. All chapters have in common that they employ administrative data on German municipalities. Meaning, in all chapters, the unit of

observation is the municipality. Chapter 3 and 5 use data on municipalities of all German federal states. Chapter 2 focuses on an institutional reform in North Rhine-Westphalia and Chapter 4 on a municipal territorial reform in Saxony-Anhalt.

Data sources and structure. We draw on a variety of official statistical sources. Data is provided by the statistical offices of the individual federal states, the Regional Database Germany (*Regionaldatenbank Deutschland*), the Statistical Local (*Statistik Lokal*) publications, the Federal Employment Agency (*Bundesagentur für Arbeit*), the Federal Statistical Office (*Statistisches Bundesamt*) and the Research Data Centres of the Federal Statistical Office and the statistical offices of the Länder (*Forschungsdatenzentren der Statistischen Ämter des Bundes und der Länder*).

Chapter 2 to 4 employ annual panel data with a long time dimension (in Chapter 5 we employ a simple aggregated cross section). Panel datasets contain a time series for each cross-sectional unit, i.e., the municipality. Having repeated observations on the same municipality comes along with various benefits (see, e.g., Wooldridge 2006). Most importantly, it allows to control for unobserved but time-invariant omitted municipal-level characteristics by employing fixed effects estimation techniques. The datasets provide information on a broad spectrum of demographic, social and economic indicators for the individual municipalities and counties over time. The panel datasets are of a multi-level nature in so far that we combine municipal-level and county-level information (in Chapter 5 we even add state-level information in the form of the credit access index). There is a base set of data which is drawn on in all chapters. Depending on the research focus of the individual chapters we acquire further information. This is in particular true for the two chapters which concentrate on individual federal states (Chapter 2 on North Rhine-Westphalia and Chapter 4 on Saxony-Anhalt). For Chapter 5 we even conduct an institutional analysis with respect to local credit access which flows into the credit access index.

Identification strategy. For the identification of the causal relationship we draw on difference-in-differences designs by exploiting policy reforms in all but the last chapter. We employ them in a panel data environment by using fixed effects. In particular, in Chapter 2 we make use of a very convincing quasi-experiment being the result of a ruling of North Rhine-Westphalia's state constitutional court on the municipal fiscal equalization scheme. Quasi-experiments are well-liked in applied economics to analyze non-experimental data in an experimental spirit (see Angrist and Pischke 2009, page 15 ff. and 227 ff.). If all identifying assumptions are met (key assumption is the common trend assumption),

employing a difference-in-differences design solves the common endogeneity concerns and allows to credibly identify the causal effect of interest. As opposed to the other chapters, in Chapter 5 we mainly work with descriptive statistics. The econometric approach is based on ordinary least squares. The reason for this is that our research interest demands aggregation.

1.1.3 Related literature

What comes along with the various dimensions of the research questions is that this dissertation does not belong to a **closed body of research**: First and foremost, this literature pushes the body of applied research working with German administrative data. As already stated above there is a rising academic interest on this regard. Examples include Baskaran (2013, 2014, 2015), Becker et al. (2012), Blesse and Baskaran (2014), Borck et al. (2015), Büttner (2003, 2005, 2006), Egger et al. (2010), Foremny and Riedel (2014) and Fuest et al. (2016). Besides that, this dissertation adds to the significant body of fiscal federalism on general grounds (see, e.g., Oates [1972] for an introduction in the traditional theory and Oates [2005] for a review on its second generation).

Chapter 2. The findings of Chapter 2 contribute to the strand of research studying fiscal equalization systems and tax competition. See, e.g., Becker et al. (2012), Bucovetsky and Smart (2006), Köthenbürger (2002) and Smart (2007). We add to the literature by empirically analyzing the impact of standard business tax multipliers on municipal business tax policy. We furthermore adapt an established theoretical model used in Smart (2007) and Egger et al. (2010) to illustrate the interaction of local taxation and fiscal equalization. Closest to our work are Büttner (2006) and Egger et al. (2010). They also employ German data. They investigate the incentive effects of equalization systems on tax policy and demonstrate the positive impact of capacity equalization on local tax rates. Baskaran (2014) is closely related to our work in so far that he takes the hypothesis of standard tax multipliers as a driver of local tax policy as a given in his analysis of local tax mimicking by municipalities in Germany.

Chapter 3. The work of this chapter speaks about and contributes to three lines of literature. It studies if implemented tax policy at the local level is consistent with the seminal inverse-elasticity rule. First, we relate to a small literature that studies whether actual tax policy follows the requirements of optimal-taxation theory. See, for instance, Saez (2001) and Mankiw et al. (2009). As opposed to our paper, the few studies on the

optimality of actually implemented taxes are mostly on the personal income tax. We provide some first evidence in this regard for other taxes. Second, we contribute to the literature on the elasticity of the corporate tax base and the elasticity of tax revenues. While the literature acknowledges that firms respond to tax incentives and adjust to taxes along various different dimensions, the literature studying the tax-sensitivity of the overall tax bases and tax revenues empirically is scarce (e.g., Devereux et al. 2014 and Dwenger and Steiner 2012). A few papers study the sensitivity of the business-tax base in the same institutional setting that we use. Most importantly, Baskaran (2015), Büttner (2003) and Fossen and Steiner (2014). Our analysis intends to complement the few empirical papers, and adds to the important understanding of behavioral responses of firms to profit taxation. Third, our research adds to a very small literature on the sensitivity of property to taxes. One recent study is by Baskaran (2015), older research is by Stine (1988).

Chapter 4. The research of Chapter 4 contributes to the significant body of literature on the "commons" on a general level (see, e.g., Ostrom et al. [2002] for a review). In this context, particular mention should be made on this chapter's relation to the sizable empirical literature focusing on the validity of the "law 1/n" with regard to the impact of legislature size on spending (Chapter 4 employs this formalization of the common pool theory derived by Weingast et al. [1981] as a theoretical starting point). Examples include Bradbury and Crain (2001), Gilligan and Matsusaka (2001) and Pettersson-Lidbom (2012). Studying free-riding on public debt for the case of a municipal territorial reform in the German federal state of Saxony-Anhalt this work is directly related to the just emerging empirical literature on opportunistic behavior in municipal territorial reforms. Notable other studies with a focus on free-riding on public debt with evidence from Scandinavian countries are by Hinnerich (2009), Jordahl and Liang (2010) and Saarimaa and Tukiainen (2015). Contributions with a somewhat different angle are by Blom-Hansen (2010), Hansen (2014) and Nakazawa (2013). As opposed to the other papers, this research does not only focus on capturing the implications of the "law 1/n," but broadens the horizon on the underlying incentive effects.

Chapter 5. This chapter advances the research on tax smoothing and the literature on budget constraints (see, e.g., Barro 1979 and Oates 2005). It is motivated by the sizable empirical and theoretical literature on tax smoothing following the pioneering work of Barro (1979) and Bohn (1990). This chapter investigates the empirical relationship between credit access and tax smoothing using the case of municipalities in Germany. Our approach allows us to use tax rate data and investigate tax rate volatility directly.

This is in contrast to the indirect tax smoothing tests prevalent in existing literature. See, for example, Adler (2006), Barro (1979, 1995), Bohn (1990), Olekalns (1997) and Strazicich (1997). We establish a link between tax smoothing and actual credit access that is to the best of our knowledge almost entirely missing from the literature to date. Most existing studies focus on tax smoothing at the national level, where free credit access can be assumed. Notable exceptions are Strazicich (1997) and Reitschuler (2010). This work also contributes to the branch of the fiscal federalism literature that objects to granting credit access to lower federal levels by supporting the notion that strict credit rationing of the local level may be the best institutional choice for higher-level governments even if there is substantial tax autonomy at the local level (e.g., Oates 2005).

1.1.4 Policy implications at a glance

To answer the research questions outlined above empirically lies at the heart of this dissertation. The empirical results also have valuable policy implications. To do them justice I provide the key policy implications at a glance:

- *Implications for the practical design of fiscal equalization schemes.* Our results highlight the importance of the parameters of equalization systems for shaping local tax policy. Through its choice of the standard tax multiplier, German federal states can influence the level of municipal tax rates and the weight of competitive downward forces. Standard tax multipliers should be regarded as a tool for governments to shape lower-level tax policy, with important consequences for their own competitiveness.

- *Implications for tax policy at the local level.* Our results suggest that efficiency gains could be realized through reducing (or even reversing) the difference in levels between business and property taxation at the German municipal level.

- *Implications for municipal territorial reforms.* The findings imply that governments should consider that municipal territorial reforms trigger opportunistic behavior in the form of free-riding on public debt for investment purposes. They should take into account that the incentives structure differs depending on relative common pool size and further characteristics (voluntary vs. mandatory mergers and accountability). Governments should take precautions to prevent or at least mitigate opportunistic behavior such as, e.g., employing spending restrictions, giving municipalities more

leeway to merge or incorporate according to their preference or reducing the number of free-riding municipalities by fostering incorporations and mergers where one municipality keeps its name. If liquidity credit rationing is enforced strictly, it, per se, rules out free-riding on debt for consumption purposes and limits it to investment purposes.

- *Implications for local-level public debt for current expenditure.* Our results make a case for more credit rationing, at least at the local level. Otherwise calls for bail-outs become more and more likely. In Germany, federal states with loose attitudes towards local debt should then follow the example of states that never expanded local credit access in the first place. Making such a change would necessarily require revisiting local spending responsibilities and the adequacy of state transfers to the local level. Beyond Germany, our findings suggest that higher-level governments should think twice before allowing subnational governments, and local governments in particular, access to credit to fund current expenditures. Chances are local debt will not be used in a welfare enhancing way.

1.2 Chapters and main findings

This section provides an overview of the proceeding chapters. All summaries follow the same structure: After providing information on co-authorship and prior publication, all of them begin with the motivation and explain how the research adds to the existing literature. Next, the empirical approach is outlined and the findings are presented. The summaries close with policy implications.

1.2.1 Chapter 2: How to stop the race to the bottom?

Chapter 2 examines whether municipal fiscal equalization systems prevent a race to the bottom of business tax multiplier. It is a joint work with *Caroline-Antonia Hummel* and is published as "How to stop the race to the bottom. Empirical evidence from North Rhine-Westphalia" in International Tax and Public Finance (Rauch and Hummel 2015).

Motivation. Conventional economic wisdom suggests that decentralized business taxation and a common pool of equalization transfers among local jurisdictions should lead to a race to the bottom in local business tax rates. In this paper, we argue that the so-called

"standard tax multipliers" (*fiktive Hebesätze* or *Nivellierungshebesätze*) help to prevent both the race to the bottom and the raiding of the commons. Standard tax multipliers are employed in municipal fiscal equalization schemes in all German territorial states to calculate the fiscal capacity from taxes for which subnational governments enjoy tax autonomy such as the business tax. Their use has the following effect: If the municipality's actual business tax multiplier is smaller than the standard tax multiplier, the accounted standardized business tax revenue is greater than actual business tax revenue (and vice versa). While the effects of most mechanisms within the equalization system are unknown to the municipalities, they are well aware of the impact of the standard tax multiplier (i.e., the "overestimation" of tax revenue if the standard tax multiplier exceeds their business tax multiplier). Standard tax multipliers prevent municipalities from neglecting their own tax sources. They provide a signal for an "appropriate" tax rate level, which municipalities are incentivized to follow in order to maximize their revenue. Thus, practitioners at the state level have devised a clever mechanism to circumvent common pitfalls of local tax policy. Consistent with this argument, local business tax rates in Germany hardly appear to have been driven by a race to the bottom. Instead, they have exhibited a steady upward trend over the past three decades. This paper presents further evidence of the upward pressure that fiscal equalization exerts on local tax rates.

Contribution to the literature. We add to the literature by focusing the analysis on standard tax multipliers and using an innovative identification strategy with a new dataset. Standard tax multipliers are an institutional feature of any equalization scheme that relies on so-called "representative tax systems," as well as being present in municipal fiscal equalization in all thirteen German territorial states. The hypothesis of standard tax multipliers as a driver of local tax policy has long been discussed in the applied literature on the evaluation of and reform options for fiscal equalization systems. To the best of our knowledge, an explicit test of this hypothesis is absent from the academic literature to date. We adapt an established theoretical model to illustrate the interaction of local taxation and fiscal equalization. This allows us to derive the optimal business tax multiplier as well as the incentive effect of a change in the standard business tax multiplier. Beyond the mechanics exposed in the model, we believe that standard tax multipliers provide an easy-to-read signal to local policymakers. They view standard tax multipliers as a reference for an appropriate and politically feasible tax multiplier. Thus, we argue that changes in standard tax multipliers are more obvious and potentially a more powerful trigger of local tax responses than previously analyzed fiscal equalization parameters.

Empirical approach. Standard tax multipliers are often equal, or at least related, to the average of actual tax multipliers, creating an endogeneity problem in empirical analysis. A quasi-experiment in North Rhine-Westphalia allows us to solve this problem. Until 1995, North Rhine-Westphalia's equalization scheme featured standard tax multipliers that were differentiated according to municipal population size. In 1993, the state constitutional court ruled that this arbitrary differentiation was not permissible. As a result, North Rhine-Westphalia's state legislature had to adjust its municipal fiscal equalization scheme. The court ruling thus led to exogenous variation in the standard tax multiplier for small municipalities. The strict exogeneity of this reform is in contrast to other reforms where standard tax multipliers are adjusted to better reflect actual average tax multipliers. Our empirical analysis is based on a balanced panel dataset of annual administrative data for all 396 municipalities of North Rhine-Westphalia. The dataset covers the time period from 1987 to 2002, thereby containing information on the pre-reform, reform and post-reform periods. It draws on a variety of official statistics data sources. The rich and unique dataset includes a number of municipal- and county-level control variables. Our research design combines a municipal-level fixed effects model with a difference-in-differences approach, where local business tax multipliers are regressed on interaction terms between treatment groups and treatment points. Our identification strategy exploits the exogenous (quasi-experimental) variation of the standard tax multiplier for "small" municipalities induced by the reform of the North Rhine-Westphalian municipal fiscal equalization system in the mid-1990s.

Findings. The results show that upward shifts in standard business tax multipliers lead to immediate upward adjustments in actual business tax multipliers. This is true for all affected municipalities. The reaction is more pronounced for municipalities whose business tax multipliers are below post-reform standard tax multipliers. The findings are robust to a number of alternative specifications. They also reflect the positive incentive effect derived from theoretical considerations.

Policy implications. Our results have important implications for the practical design of fiscal equalization schemes. They highlight the importance of the parameters of equalization systems for shaping local tax policy. Through its choice of the standard tax multiplier, a state can influence the level of municipal tax rates and the weight of competitive downward forces. Standard tax multipliers should be regarded as a tool for governments to shape lower-level tax policy, with important consequences for their own competitiveness.

1.2.2 Chapter 3: Does tax policy follow the inverse-elasticity rule?

Chapter 3 studies whether implemented tax policy is consistent with the seminal inverse-elasticity rule. This chapter is written in collaboration with *Philipp Dörrenberg*.

Motivation. The seminal contribution of Ramsey (1927) shows that optimal tax rates on goods depend inversely on their elasticity of demand. While the optimal-tax literature has made progress since Ramsey's initial contribution, the general idea of his framework has not lost ground: the excess burden of taxation is positively related to the sensitivity of the tax base, and the tax rate should be inversely related to a tax base's tax sensitivity. In the case of two tax bases, for example, Ramsey taxation suggests to impose a higher tax rate on the tax base that is relatively less responsive to tax-rate changes. Empirical evidence on this question is scarce.

Contribution to the literature. Our paper speaks about and contributes to three strands of literature. First, we relate to a small literature that studies whether actual tax policy follows the requirements of optimal-taxation theory. The few studies on the optimality of actually implemented taxes are mostly on the personal income tax. We provide some first evidence in this regard for other taxes. Second, we contribute to the literature on the elasticity of the corporate tax base and the elasticity of tax revenues. While the literature acknowledges that firms respond to tax incentives and adjust to taxes along various different dimensions, only a few papers explicitly study the tax-sensitivity of the overall tax bases and tax revenues. One reason for scarce evidence presumably is that in most countries, corporate taxes are levied on the national or state level, which hinders the causal identification of credible estimates. Comparisons of countries, and even federal states within a country, are critical and common trend assumptions in country-level analyses over time are often hard to establish. We aim to overcome these shortcomings by relying on the case of German local business and property taxes in a difference-in-differences research design with many independent tax-setting municipalities which operate in a homogeneous economic environment. Third, our work adds to a very small literature on the sensitivity of property to taxes.

Empirical approach. We explore the unique case of local taxation on the municipal level in Germany, where more than 11,000 municipalities have autonomy to set taxes on business profits and property values. This empirical playground allows us to identify how changes in business and property taxes affect the respective tax revenues, and hence makes

it possible to evaluate local taxation in Germany with respect to Ramsey taxation. Our dataset covers the full universe of municipalities in a very long panel of 16 years, 1995-2010. We employ a difference-in-differences research design and for identifying variation we rely on many (business and property) tax-rate reforms that were implemented by the municipalities. The long panel dimension, along with the fact that the municipalities operate in a very homogeneous economic environment, allows us to control for many confounding factors that present potential threats to common trend assumptions. As opposed to many other empirical tax studies, we do not have to deal with changes in the tax base definitions that often come along with tax reforms, because the tax base, as well as a basic tax rate, is defined on the federal level and municipalities only set a multiplier on the exogenous basic rate and base.

Findings. We find that business-tax revenue does not significantly increase in response to tax hikes. Property-tax revenue, on the other hand, increases by almost 1% in response to a 1% increase in the tax rate. These results imply that the business-tax base responds strongly to tax-rate changes whereas the property-tax base is not tax sensitive. If tax policy was consistent with the inverse-elasticity rule, our estimates would suggest that tax rates are significantly higher on immobile property than on mobile business profits. However, this is not what we observe. In almost all years of our sample period, the share of municipalities with a higher business tax than property tax is considerably greater than 50%. In addition, the average business-tax rate is greater than the average property-tax rate in all years of our sample period.

The inverse-elasticity rule depicts that goods should be taxed in inverse proportion to their price elasticities. We can combine this rule with our elasticity estimates and conduct back-of-the-envelope calculations to determine the optimal relation between the business and property tax rates. Given our elasticity estimates of 0.129 for business-tax revenues and 0.886 for property-tax revenues, such calculations yield that the property tax rate should, from an optimal-tax perspective, approximately be seven-times higher on average than the business tax rate. This is in contrast to actual rates; we observe that throughout our sample period the business-tax multiplier is on average 1.08 times higher than the property-tax multiplier.

Policy implications. This suggests that German municipalities are not compliant with the inverse-elasticity rule and presumably leave efficiency gains on the table. Our results suggest that efficiency gains could be realized through reducing (or even reversing) the difference in levels between business and property taxation.

1.2.3 Chapter 4: Common pool problems and territorial reforms

Chapter 4 studies the common pool problem in the context of a municipal territorial reform. It is the only single-author paper.

Motivation. The common pool problem is a well-known and widely studied phenomenon among public economists. This paper applies the notion of the common pool problem to the event of a municipal territorial reform: Municipalities being designated for the same merger or incorporation form a temporary common pool with respect to debt during the time window between the announcement and actual execution of a reform, because they generally still have the autonomy to take on debt individually. Responsible for repayment, however, are not the individual municipalities themselves, but the newly founded municipality they become part of. According to the "tragedy of the commons" (Hardin 1968) this situation results in exploitation and overuse of the resource. This implies that the municipalities affected by the reform strategically take on debt to finance (inefficient) public projects.

The previous applied literature on common pool problems in territorial reforms generally employs a formalization of the common pool theory derived by Weingast et al. (1981) as a theoretical starting point for the analysis. Broadly interpreted, the so-called "law 1/n" proposes that the size of the incentive effect is greater the smaller the municipality's own size relative to the size of its common pool. However, in the aftermath of Hardin (1968) a growing body of interdisciplinary research on the "commons" has successfully challenged the general validity of Hardin's pessimistic view. In this spirit one can propose that there might be other (implicit) mechanisms, mitigating or even preventing free-riding, which are not covered by the "law 1/n." Against this backdrop, there is demand for empirical clarification.

Contribution to the literature. Econometric research on free-riding in municipal territorial reforms has evolved only recently. This paper is among the first to study empirically free-riding on public debt before the execution of a territorial reform. The existing small strand of literature focuses on capturing the implications of the "law 1/n" and reports mixed results. The contribution to the literature of this paper is threefold: First, since the existing empirical evidence is inconclusive, future research is required. Second, exploring the free-riding incentives for the case of a municipal territorial reform in Germany, this is the first notable study for a non-Scandinavian country. This paper, hence, contributes to the generalization of results. Third, in the light of the recent developments

in the literature on the "commons" this paper goes one step further and takes a broader perspective on the underlying incentive mechanisms. It emphasizes that there may as well be factors which reduce or prevent free-riding.

Empirical approach. A promising case for a strong difference-in-differences design is the recent municipal territorial reform in Saxony-Anhalt. It reduced the number of municipalities from 1,039 to 219 between 2007 and 2011. This reform has an array of interesting facets that allow me to study all hypotheses in a coherent environment. This study exploits data that has not been used by academic research so far. Apart from the vast number of observations, it is the long panel dimension that makes this dataset particularly interesting from an econometric point of view. Therefore, I am able to closely investigate the validity of the main identifying assumption (common trend assumption). Furthermore, due to the specific settings of the reform I can perform an array of robustness checks (e.g., alternative control group), strengthening the econometric design.

Findings. My empirical results, indeed, confirm that being in a common pool, indeed, triggers an incentive to free-ride (Hypothesis 1a). However, the size of the incentive is linked to the theoretical predictions based on the "law 1/n." I find a statistically and economically significant free-riding effect with the incentive to free-ride being greater, the smaller the municipality's own size is relative to its common pool (Hypothesis 1b). What distinguishes my paper is that I carefully consider differences in incentive structures by approaching three further hypotheses: I can report a statistically insignificant free-riding effect for the group of surviving municipalities. This confirms the accountability prediction (Hypothesis 2a). Furthermore, municipalities which merge or incorporate in the non-mandatory phase behave less opportunistically than municipalities being forced to merge or incorporate in the mandatory phase (Hypothesis 2b). Last, I cannot provide evidence in favor of timing (Hypothesis 3). The sizes of the effects are of economic relevance.

Policy implications. My results support the notion that governments should consider the opportunistic behavior when announcing a municipal territorial reform. They should take into account that the incentives structure differs depending on relative common pool size and further characteristics (voluntary vs. mandatory mergers and accountability). The results have important policy implications: First, to prevent (or at least mitigate) opportunistic behavior, governments could constrain municipal autonomy after a municipal territorial reform is made public. However, at least in Germany the implementation of spending restrictions might prove to be difficult due to the guarantee of municipal autonomy granted by the Basic Law. Second, governments should be aware of the fact

that free-riding also occurs in the case of voluntary mergers, but to a lesser extent. This is a particularly important point for Germany. Aside from extensive state-wide municipal territorial reforms, there are constantly voluntary mergers or incorporations of individual municipalities. Governments could encourage self-binding inter-municipal contracts to reduce free-riding. Third, they could mitigate the incentives to free-ride by giving the municipalities more leeway to merge or incorporate according to their preference. Last, they could reduce the number of free-riding municipalities by fostering incorporations and mergers where one municipality keeps its name. If liquidity credit rationing is enforced strictly, it, per se, rules out free-riding on debt for consumption purposes and limits it to investment purposes.

1.2.4 Chapter 5: Tax smoothing and credit access

Chapter 5 studies if credit access has an impact on tax smoothing at the local level. It is co-authored with *Caroline-Antonia Hummel* and *Eva Gerhards*.

Motivation. In the context of recent debt crises, political practitioners emphasize risks associated with high public debt levels, and try to design effective measures for limiting debt build-up. Public debate often portrays debt as something that is best avoided or at least strictly limited. Otherwise, countries might one day have to suffer the consequences in the form of debt crises, recessions, fiscal austerity and painful reforms to clean up the mess. The theory of tax smoothing suggests that giving governments access to debt financing could be welfare enhancing as allowing governments to smooth their taxes over time can reduce distortionary costs from taxation. While it is clear from a theoretical viewpoint that tax smoothing is beneficial, it is unknown whether governments with credit access do in fact engage in tax smoothing. Political decision-makers might not realize the potential benefits from tax smoothing and instead take advantage of debt to finance unsustainable levels of current expenditures. The possible relationship between credit access and tax smoothing is particularly important where lower level governments enjoy tax autonomy. The standard fiscal federalism literature objects to granting credit access to lower federal levels. However, ruling out debt finance could prevent subnational governments that enjoy tax autonomy over certain taxes from engaging in welfare-enhancing tax smoothing. This paper investigates the empirical relationship between credit access and tax smoothing using the case of municipalities in Germany.

Contribution to the literature. We contribute to the literature, first, by devel-

oping and using an innovative and intuitive approach to investigating the strength of tax smoothing behavior. In contrast to existing studies, we do not ask whether governments do or do not engage in tax smoothing. Instead, we analyze whether tax rates become smoother and thus closer to Barro's ideal if the institutional setting becomes more accommodating. We define "smoother tax rates" as tax rates that are subject to a smaller number of changes within a given time period. This measure constitutes an intuitive and straightforward translation of theoretical tax smoothing definitions found in the literature into the context of local tax policy. Our approach also allows us to use tax rate data and investigate tax rate volatility directly. This is in contrast to the indirect tax smoothing tests prevalent in existing literature, which rely on the behavior of the budget balance or government expenditure over time. Second, we establish a link between tax smoothing and actual credit access that is to the best of our knowledge almost entirely missing from the literature to date. While credit access is typically not an issue at the national level, it becomes crucial once we move to subnational levels of government. Third, we contribute to the literature that objects to granting credit access to lower federal levels by supporting the notion that strict credit rationing of the local level may be the best institutional choice for higher-level governments even if there is substantial tax autonomy at the local level.

Empirical approach. This paper uses the unique institutional setting of German fiscal federalism to study the behavior of municipalities in Germany and to test whether credit access is associated with lower tax rate volatility. Germany is a promising case to study given that municipalities enjoy autonomy over property and business tax rates and differ in the degree of credit access allowed by the respective federal state. To operationalize local credit access, the institutional environment and empirical level of local indebtedness in each of the 13 territorial states were examined in detail to derive an index of local credit access. We propose the number of tax rate changes within a 16 year-time frame as a measure of tax rate volatility. The descriptive and econometric modeling is based on a sample of more than 10,000 municipalities in all German territorial states. We employ a line of argument based on four steps of data analysis using descriptive statistics (Step 1 to 3) and econometric analysis (Step 4).

Findings. We start by showing that the development of liquidity credit stocks has not been cyclical (Step 1). In fact, easier credit access coincides with dramatic increases in local per capita short-term liquidity credits in some federal states. In Step 2 we pinpoint that tax rate changes are rather rare in all federal states regardless of credit access. We find no cyclical behavior involving tax reductions and jumps. Instead, we find a rise in tax

rate volatility towards the end of our time horizon, despite easing credit access. Next, we show graphically that there is a possible positive relationship between tax rate volatility and credit access, contradicting the notion that credit access might induce less volatile tax rates (Step 3). We suggest spending pressure as a potential explanation. Last, we employ an econometric approach in which we account for spending pressure (Step 4). However, the econometric results also suggest a positive link, which would point to an improper use of local debt. Hence, we cannot reject the positive relationship between credit access and tax rate volatility. It therefore appears that whether federal states allow their municipalities access to debt or not has no impact on the stability of their tax rate choices. Local tax rates are not less volatile in federal states which grant their municipalities ample access to debt. While local tax rates in Germany are generally rather stable over time, this still gives cause for concern. Further research will be required to validate this conclusion. If it is confirmed, important implications follow.

Policy implications. If the major theoretical justification for public debt for consumption expenditures crumbles in practice, there is a case for more credit rationing, at least at the local level. Otherwise calls for bail-outs become more and more likely. In Germany, federal states with loose attitudes towards local debt should then follow the example of states that never expanded local credit access in the first place. Making such a change would necessarily require revisiting local spending responsibilities and the adequacy of state transfers to the local level. Beyond Germany, the findings of this paper suggest that higher-level governments should think twice before allowing subnational governments, and local governments in particular, access to credit to fund current expenditures. Chances are local debt will not be used in a welfare enhancing way.

Chapter 2

How to stop the race to the bottom?

2.1 Introduction

It is a well-known normative principle among public economists that business taxation should not be decentralized to subnational levels of government. Otherwise, so the argument goes, local governments would engage in a harmful "race to the bottom" where they constantly try to undercut their neighbors' business tax rates. Resulting tax rates would be inefficiently low (Oates 1972). A similarly widespread insight is that problems of overspending and reduced tax effort arise whenever budgeting involves a common pool of resources (see Raudla [2010] for a review on the use of the "budgetary commons" metaphor in existing literature).

Germany's institutional setting involves business tax autonomy for local governments and a common pool of fiscal equalization transfers from the state to the local level. As a result, conventional wisdom points to overly low business tax rates as a likely outcome: Competition for mobile capital presumably pushes tax rates downwards. At the same time, one might suspect that the common pool of equalization transfers further reduces tax effort. Indeed, Köthenbürger (2002) shows that equalization schemes that rely on revenue equalization tend to reinforce tax competition. In contrast, fiscal equalization in the form of tax base or capacity equalization increases subnational tax rates and thus attenuates competitive forces, which may be efficiency-enhancing when competition effects are strong enough (Köthenbürger 2002; Bucovetsky and Smart 2006; Smart 2007). Municipal fiscal equalization in Germany adheres to the capacity equalization principle, which is also employed in the transfer systems of countries such as Canada and Australia. In such

systems, jurisdictions' tax bases are evaluated at a standard tax rate and compared to a benchmark level of spending or revenue to determine the size of the transfer.[1] The transfer to each jurisdiction decreases in its "fiscal capacity."

In this paper, we argue that the so-called "standard tax multipliers" (*fiktive Hebesätze* or *Nivellierungshebesätze*) help to prevent both the race to the bottom and the raiding of the commons. Standard tax multipliers are employed in fiscal equalization schemes to calculate the fiscal capacity from taxes for which subnational governments enjoy tax autonomy. In the case of German municipalities, business tax is one of the most important components of fiscal capacity. The use of standard tax multipliers has the following effect: If the municipality's actual business tax multiplier is smaller than the standard tax multiplier, the accounted standardized business tax revenue is greater than actual business tax revenue (and vice versa). While the effects of most mechanisms within the equalization system are unknown to the municipalities, the municipalities are well aware of the impact of the standard tax multiplier (i.e., the "overestimation" of tax revenue if the standard tax multiplier exceeds their business tax multiplier). Standard tax multipliers prevent municipalities from neglecting their own tax sources. They provide a signal for an "appropriate" tax rate level, which municipalities are incentivized to follow in order to maximize their revenue. Thus, practitioners at the state level have devised a clever mechanism to circumvent common pitfalls of local tax policy. Consistent with this argument, local business tax rates in Germany hardly appear to have been driven by a race to the bottom. Instead, they exhibited a steady upward trend over the past three decades.[2]

A growing body of empirical literature investigates the incentive effects of equalization systems on tax policy and demonstrates the positive impact of capacity equalization on local tax rates. Egger et al. (2010) exploit a change of the equalization formula in the state of Lower Saxony and show that this reform had a significant impact on municipalities' business tax rates for four consecutive years. Büttner (2006) provides evidence that there is a positive relationship between the marginal contribution rate, defined as the rate at which an increase in the tax base reduces equalization transfers, and local business tax rates in the state of Baden-Württemberg. Smart (2007) investigates the effect of equal-

[1] In contrast to most such systems, German municipal fiscal equalization schemes rely on a comparison between "fiscal need" and "fiscal capacity." Moreover, the sum of all equalization transfers is typically fixed by the state level and not endogenous.

[2] Weighted average business tax multipliers in Germany increased from 330 in 1980 and 364 in 1990 to 390 in 2010 (Federal Statistical Office 2014c).

ization among Canadian provinces, showing that an expansion of transfers leads to higher provincial tax rates.

This paper presents further evidence of the upward pressure that fiscal equalization exerts on local tax rates. We add to the literature by focusing the analysis on standard tax multipliers and using an innovative identification strategy with a new dataset. Standard tax multipliers are an institutional feature of any equalization scheme that relies on so-called "representative tax systems," as well as being present in municipal fiscal equalization in all thirteen German territorial states.[3] The hypothesis of standard tax multipliers as a driver of local tax policy has long been discussed in the applied literature on the evaluation of and reform options for fiscal equalization systems (e.g., Büttner et al. 2008; Parsche and Steinherr 1995; Goerl et al. 2013). Baskaran (2014) even takes this hypothesis as a given in his analysis of local tax mimicking by municipalities in Germany. He views a reform of standard tax multipliers in the state of North Rhine-Westphalia in 2003 as the cause of observable adjustments in actual tax multipliers. This is despite the fact that, to the best of our knowledge, an explicit test of this hypothesis is absent from the academic literature to date. We adapt the theoretical models used in Smart (2007) and Egger et al. (2010) to illustrate the interaction of local taxation and fiscal equalization. This allows us to derive the optimal business tax multiplier as well as the incentive effect of a change in the standard business tax multiplier. Beyond the mechanics exposed in the model, we believe that standard tax multipliers provide an easy-to-read signal to local policymakers. They view standard tax multipliers as a reference for an appropriate and politically feasible tax multiplier. In contrast to changes in eligibility criteria, adjustment levels, or marginal contributions rates, which may also influence local tax multipliers as shown in the previous literature, standard tax multipliers have the same magnitude as actual multipliers. As a result, changes in standard tax multipliers are easily translated into perceived necessary adjustments of local multipliers. As stated by Baskaran (2015), hikes in standard tax multipliers also provide a window of opportunity for local officials to raise tax multipliers while deflecting the blame to the state level. Thus, we argue that changes in standard tax multipliers are more obvious and potentially more powerful trigger of local tax responses than previously analyzed fiscal equalization parameters.

Standard tax multipliers are often equal, or at least related, to the average of actual

[3]Moreover, fiscal equalization between federal states in Germany also employs standard tax rates to standardize property transfer tax revenue since the introduction of state tax rate autonomy for this tax in 2006.

tax multipliers, creating an endogeneity problem in empirical analysis. A quasi-experiment in North Rhine-Westphalia allows us to solve this problem. Until 1995, North Rhine-Westphalia's equalization scheme featured standard tax multipliers that were differentiated according to municipal population size. In 1993, the state constitutional court ruled that this arbitrary differentiation was not permissible. As a result, North Rhine-Westphalia's state legislature had to adjust its municipal fiscal equalization scheme. The court ruling thus led to exogenous variation in the standard tax multiplier for small municipalities. The strict exogeneity of this reform is in contrast to other reforms where standard tax multipliers are adjusted to better reflect actual average tax multipliers.

Our empirical analysis is based on a balanced panel dataset of annual administrative data for all 396 municipalities of North Rhine-Westphalia. The dataset covers the time period from 1987 to 2002, thereby containing information on the pre-reform, reform and post-reform periods. It draws on a variety of official statistics data sources. The rich and unique dataset includes a number of municipal- and county-level control variables.

Our research design combines a municipal-level fixed effects model with a difference-in-differences approach, where local business tax multipliers are regressed on interaction terms between treatment groups and treatment points. Our identification strategy exploits the exogenous (quasi-experimental) variation of the standard tax multiplier for "small" municipalities induced by the reform of the North Rhine-Westphalian municipal fiscal equalization system in the mid-1990s. We find a positive effect of the standard business tax multiplier on local business tax multipliers, as predicted by theoretical considerations. The findings are robust to a number of alternative specifications.

Section 2.2 clarifies the institutional features of the German business tax and municipal fiscal equalization. Section 2.3 introduces the theoretical model. Section 2.4 explains our empirical approach and data. Section 2.5 presents the results of the empirical analysis. Section 2.6 concludes.

2.2 Institutional background

Germany's federal structure is a key determining factor of the country's fiscal landscape. The federal level, the three city-states and 13 territorial states, and the more than 11,000 municipalities each have differing degrees of tax autonomy over different taxes. For German municipalities, the business tax (*Gewerbesteuer*) and the equalization transfers

(*Schlüsselzuweisungen*) provided to them by their federal state are two of the most important income sources.[4] In 2013, municipal net revenue from the business tax and fiscal equalization transfers accounted for 15.8 and 14.3% of aggregate municipal income, respectively (Federal Statistical Office 2014b).

2.2.1 Business taxation

It is a particularity of the German tax system that municipalities enjoy business tax autonomy. Each municipality sets its own local business tax multiplier (*Gewerbesteuerhebesatz*). In contrast, the business tax base and the basic tax rate (*Steuermesszahl*)[5] are defined at the federal level. The resulting tax rate is determined by multiplying the local business tax multiplier with the basic federal tax rate. The business tax is charged on operating profits of corporate and non-corporate firms. In 2013, gross business tax revenue amounted to 43 billion (bn) euros, making it Germany's third most revenue-generating tax (Federal Statistical Office 2014a).

2.2.2 Municipal fiscal equalization

In 2013, municipal fiscal equalization transfers in Germany totaled 29.4 bn euros (Federal Statistical Office 2014b). These transfers serve a double purpose. First, most German municipalities lack sufficient own revenue sources to fund their tasks. The transfers they receive from their respective federal state via its municipal fiscal equalization system thus serve a fundamental financing function. Second, the transfers are designed to reduce differences in municipalities' capacities to provide public goods.

Municipal fiscal equalization systems function similarly in all German states. All of them employ the same basic mechanism of comparing a fictitious measure of "fiscal need" with a standardized measure of "fiscal capacity." Total fiscal equalization transfers $\sum_{i=1}^{I} T_i$ (*Schlüsselmasse*, i.e., the sum of all equalization transfers paid out in one year in the state in question) are predetermined. The fiscal equalization transfer T_i of municipality i equals

$$T_i = \alpha(\beta N_i - C_i) \; \forall \, i \text{ with } \beta N_i > C_i. \tag{2.1}$$

[4]Other relevant sources are the local property tax, the municipal shares of value-added tax (VAT) and income tax, as well as duties and charges.

[5]The basic federal tax rate was set at 5% (with lower rates for operating profits below 48.000 euros) during our sample period. It was reduced to a uniform rate of 3.5% in 2007.

T_i depends on the combined effect of the following factors: adjustment level α, i.e., the degree to which the difference between fiscal need and fiscal capacity is equalized; fictitious measure of fiscal need, which is calculated by multiplying a fiscal need number N_i by the basic amount β; standardized measure of fiscal capacity C_i.[6]

Municipalities whose fiscal capacity exceeds their fiscal need are called "abundant" and do not benefit from fiscal equalization transfers. The basic amount is determined via an iterative process and equals[7]

$$\beta = \frac{\sum_{i=1}^{I} T_i + \alpha \sum_{i=1}^{I} C_i}{\alpha \sum_{i=1}^{I} N_i} \; \forall \, i \text{ with } \beta N_i > C_i. \tag{2.2}$$

While the derivation of the fictitious measure of fiscal need is negligible with respect to the focus of this paper, the derivation of the standardized measure of fiscal capacity is not. Fiscal capacity is the sum of standardized business and property tax revenue and the (unstandardized) municipal share of VAT and income tax revenue. To assure local tax multiplier autonomy, municipal fiscal equalization systems employ so-called standard tax multipliers to evaluate tax revenue from taxes for which the municipalities set tax multipliers (business and property tax). Standard tax multipliers are set by the respective federal states. Standardized business tax revenue R_i^{std} equals

$$R_i^{std} = s \times \frac{R_i}{m_i} \tag{2.3}$$

with R_i :=business tax revenue, m_i :=business tax multiplier and s :=standard tax multiplier.[8]

If the actual business tax multiplier is smaller than the standard tax multiplier, the accounted standardized tax revenue is greater than the actual business tax revenue (and vice versa). While the effects of most mechanisms within the equalization system are unknown to the municipalities, they are well aware of the impact of the standard tax

[6]In addition to such "common" fiscal equalization transfers, some states employ special transfers to municipalities suffering from a very low standardized tax revenue to ensure that they achieve a pre-defined level of fiscal resources. However, this is not the case in North Rhine-Westphalia.

[7]Due to the endogeneity of the basic amount, the comparative statics of municipal fiscal equalization are not straightforward and unknown to municipalities.

[8]Standardized property tax revenue is determined equivalently.

multiplier (i.e., the "overestimation" of tax revenue if the standard tax multiplier exceeds their business tax multiplier).

2.3 A simple theoretical model

To understand the incentive effect of standard tax multipliers, we develop a simple theoretical model of local taxation and fiscal equalization with two revenue-maximizing local jurisdictions. It is a version of the models employed by Egger et al. (2010) and Smart (2007), which we extend to include the standard tax multiplier as well as the basic amount. It allows us to derive the optimal business tax multiplier and the incentive effect of a change in the standard business tax multiplier. Suppose there are two municipalities i and j whose sole income sources are business taxation and fiscal equalization transfers. The business tax base B_i of municipality i does depend not only on its own business tax rate m_i, but also on the one of municipality j, m_j:

$$B_i = B_i^0 + \gamma m_j - \delta m_i \tag{2.4}$$

where $B_i^0 \geq 0$ and $\delta > \gamma \geq 0$. Tax revenue R_i thus becomes

$$R_i = m_i(B_i^0 + \gamma m_j - \delta m_i). \tag{2.5}$$

Fiscal capacity C_i is

$$C_i = \frac{sR_i}{m_i} = s(B_i^0 + \gamma m_j - \delta m_i) \tag{2.6}$$

where s again denotes the standard tax multiplier.

Assuming that both municipalities are non-abundant, the respective fiscal equaliza-

tion transfers T_i are derived by inserting Equation (2.2)[9] into Equation (2.1):

$$T_i = \alpha \left[\frac{\sum T_{i,j} + \alpha s \left(B_i^0 + \gamma m_j - \delta m_i + B_j^0 + \gamma m_i - \delta m_j \right)}{\alpha \left(N_i + N_j \right)} N_i - s \left(B_i^0 + \gamma m_j - \delta m_i \right) \right] \tag{2.7}$$

As an auxiliary assumption, suppose that both municipalities seek to maximize their revenue from taxes and transfers:

$$\max_{m_i} R_i + T_i \tag{2.8}$$

The reduced-form equation for the optimal tax rate of municipality i then becomes:

$$m_i* = \frac{1}{4\delta^2 - \gamma^2} \left[2\delta B_i^0 + \gamma B_j^0 + \alpha s \left\{ \gamma\delta + 2\delta^2 + \frac{1}{N_i + N_j} \left(\gamma N_j(\gamma - \delta) + 2\delta N_i(\gamma - \delta) \right) \right\} \right] \tag{2.9}$$

This leads to the following first derivative with respect to the standard tax multiplier s:

$$\frac{\partial m_i^*}{\partial s} = \frac{1}{4\delta^2 - \gamma^2} \left[3\delta\gamma N_i + N_j(2\delta^2 + \gamma^2) \right] > 0 \tag{2.10}$$

Proposition: *An increase in the standard tax multiplier increases the optimal tax multiplier chosen by the municipalities.*

Given this relationship, the use of standard tax multipliers prevents municipalities from neglecting their own revenue sources and provides a clever way to circumvent the common pool problem. What is more, many municipalities consider the standard tax multiplier as a signal for their own tax policy. Even if – as is likely the case – local policymakers do not fully understand the intricacies of fiscal equalization and the effect of the basic amount, they recognize intuitively that they should respond to changes in the standard tax multiplier to avoid transfer losses. In contrast to changes in the adjustment level α, which also induces tax multiplier reactions, changes in s are easily translated into appropriate adjustments of the local tax multiplier, as both have the same magnitude.

[9]The first-round basic amount becomes:

$$\beta_1 = \frac{\sum_{i=1}^{I} T_i + \alpha s(B_i^0 + \gamma m_j - \delta m_i + B_j^0 + \gamma m_i - \delta m_j)}{\alpha(N_i + N_j)}.$$

Therefore, a race to the bottom in local business tax rates does not occur when standard tax multipliers are used in equalization.

2.4 Empirical approach

In this section we first provide background information on the quasi-experiment exploited in the empirical analysis, before we present our data sources and outline our empirical model. Last, we discuss our identification strategy.

2.4.1 Quasi-experiment

We exploit a quasi-experiment in the state of North Rhine-Westphalia for an empirical test of our proposition. North Rhine-Westphalia offers a promising case to study given that it is the most populous German state with over 17 million inhabitants. Moreover, it stands out as a state where municipalities' business tax multipliers are high relative to those found elsewhere in Germany. The same applies to its standard tax multipliers.

Like those in the other 12 territorial states, the 396 North Rhine-Westphalian municipalities receive state transfers through a municipal fiscal equalization system. Each year, several billion euros (8 bn in 2014) are paid out as equalization transfers. North Rhine-Westphalia currently sets a single standard tax multiplier with respect to the business tax. Until 1995, the equalization scheme featured standard tax multipliers that were differentiated according to population size. The fiscal capacity of municipalities with up to 150,000 inhabitants ("small" municipalities) was calculated using a standard tax multiplier of 350. The fiscal capacity of municipalities whose population size exceeded this threshold ("big" municipalities) was evaluated with a standard tax multiplier of 380. In 1993, the state constitutional court ruled that this arbitrary differentiation was not permissible (VerfGH 9/92, 22/92).[10] As a result, North Rhine-Westphalia's state legislature was required to adjust its municipal fiscal equalization scheme. Standard tax multipliers for municipalities with less than 150,000 inhabitants were increased in three equal steps between 1996 and 1998 to reach the larger cities' multiplier. This 30-points change amounted to an increase of 8.5% in the standardized tax multiplier. The court ruling thus led to sizable exogenous variation in the standard tax multiplier for small municipalities. To the best of our

[10]The differentiation was found to be arbitrary as long as the legislator had not established why it was warranted for objective reasons.

knowledge, this quasi-experiment has not been used in the literature to date.

2.4.2 Data sources

Our empirical analysis is based on a balanced panel dataset of annual administrative data for all 396 municipalities of North Rhine-Westphalia. The dataset covers the time period from 1987 to 2002, thereby containing information on the pre-reform, reform, and post-reform periods. It draws on a variety of official statistics data sources, namely North Rhine-Westphalia's statistical office (*IT.NRW*), the Regional Database Germany (*Regionaldatenbank Deutschland*) and the Federal Employment Agency (*Bundesagentur für Arbeit*). The rich and unique dataset includes municipal business tax multipliers, inhabitants, income tax and VAT shares, employees at place of employment, gross domestic product (GDP) (at county level), disposable income of private households (at county level), municipal debt, tax bases and revenues from property and business tax, commuters and municipal surface area. There are 375 "small" and 21 "big" municipalities up until 1999. From 2000 onwards, one additional city has more than 150,000 inhabitants.

Table 2.1: Summary statistics, 1987-2002

Variable	Mean	Std. Dev.	Min.	Max.	N
Business tax multiplier	373.284	35.652	250	490	6336
Income tax share	0.286	0.05	0.17	0.484	4356
GDP	20.732	4.013	13.066	62.922	3960
Inc. of priv. households	16.645	1.642	13.136	21.456	3168
Employees	0.259	0.093	0.048	0.625	3960
Surface area	0.429	0.345	0.028	2.36	4356

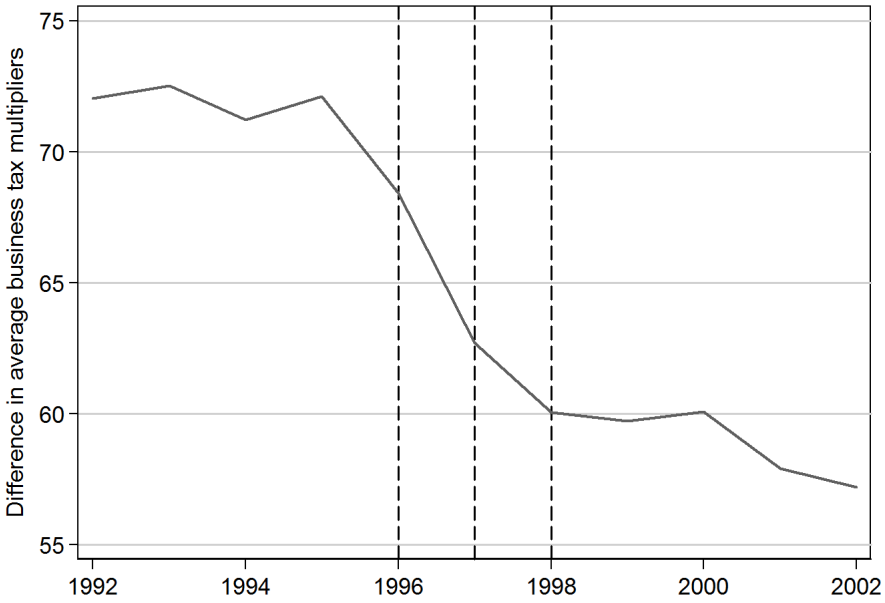
Notes: Business tax multiplier (in %), surface area (in hectare (ha) p.c.), income tax share (in 1,000 euro p.c.), employees (p.c.) (municipal level) and income of private households (in 1,000 euro p.c.), GDP (in 1,000 euro p.c.) (county level); number of observations: 396 municipalities per year.

Table 2.1 provides summary statistics for the most important variables, which are reported in per capita (p.c.) terms with the exception of business tax multipliers. Between 1987 and 2002, business tax multipliers in North Rhine-Westphalia varied between 250 and 490, with an unweighted average of 373.28. Table 2.1 also illustrates some data availability issues. None of the tabulated control variables are available for all years. Municipal income

tax shares, GDP and surface area have only been reported since 1992.[11] There are no data on the disposable income of private households before 1995 or on the number of employees before 1993. More detailed summary statistics are provided in Tables 2.6, 2.8 and 2.7 in Appendix 2.7.

Figure 2.1 depicts the difference between average business tax multipliers of "big" and "small" municipalities between 1992 and 2002.[12] The three dashed vertical lines mark the three reform years where standard business tax multipliers for small municipalities were raised. As shown, the average business tax multiplier of big municipalities was more than 70 percentage points higher than that of small municipalities at the outset. During the three reform years, the difference in averages dropped sharply, to a level of 60 percentage points and below.

Figure 2.1: Difference in average business tax multipliers between "big" and "small" municipalities, 1992-2002



Source: IT.NRW, own calculations.

[11] GDP is also missing in 1993.

[12] In 2003, the standard tax multiplier was increased to 403 for all North Rhine-Westphalian communities.

2.4.3 Empirical model

Our research design combines a municipal-level fixed effects model with a difference-in-differences approach. The dependent variable is the business tax multiplier $m_{i,t}$ of municipality i in year t. Our independent variables of interest are the interaction terms $TG_i \times TP_t$, $t = 1996, ..., 1998$ between treatment groups ($TG_i = 1$ if population $\leq 150,000$ and 0 otherwise)[13] and treatment points ($TP_{1996} = 1$ if $t = 1996$, $TP_{1997} = 1$ if $t = 1997$, $TP_{1998} = 1$ if $t = 1998$ and 0 otherwise).

We include two types of control variables to adjust for observable time-variant differences between municipalities: $\mathbf{X_{i,t}}$ and $\mathbf{Z_{c,t}}$ represent column vectors of municipal-level variables (debt p.c., share of income tax etc.) and county-level variables (GDP p.c. etc.), respectively. Furthermore, we control for municipal and year fixed effects (λ_i, Φ_t). The municipal fixed effects account for unobserved but time-invariant omitted municipal-level factors that may influence business tax multipliers. By adding year fixed effects to the regression equation we are able to control for common shocks affecting tax rates across all municipalities in a given year. We use the following regression model with $t = 1995, ..., 1998$:

$$\begin{aligned} m_{i,t} = \alpha TG_i + \delta_{1996} TG_i \times TP_{1996} + \delta_{1997} TG_i \times TP_{1997} + \delta_{1998} TG_i \times TP_{1998} \\ + \beta \mathbf{X_{i,t}} + \theta \mathbf{Z_{c,t}} + \lambda_i + \Phi_t + \varepsilon_{i,t} \end{aligned} \tag{2.11}$$

where the error term $\varepsilon_{i,t}$ is clustered on the county level.

Our coefficients of interest δ_{1996}, δ_{1997} and δ_{1998} measure how the business tax multiplier differential between "small" municipalities (treatment group) and "big" municipalities (control group) changed ceteris paribus (c.p.) between the reference year 1995 and 1996, and 1997 and 1998, respectively.

2.4.4 Discussion of identification

Our identification strategy exploits the exogenous (quasi-experimental) variation of the standard tax multiplier for "small" municipalities induced by the reform of the North Rhine-Westphalian municipal fiscal equalization system in the mid-1990s. In contrast

[13] Population size in 1995 determines assignment to treatment groups for the one municipality that grows beyond 150,000 inhabitants in 2000.

to later changes to standard business tax multipliers, this reform was prompted by a court ruling and is therefore truly exogenous. The chosen identification strategy thus circumvents typical endogeneity concerns.[14] The validity of identification hinges on the assumption of a common trend between treatment and control groups.[15] We assume that business tax multipliers would have evolved in parallel in the absence of treatment (conditional on other included independent variables). Without treatment, δ_{1996}, δ_{1997} and δ_{1998} would have to be zero.

Differences in administrative status between the treatment and the control groups might pose a potential concern regarding this identifying assumption. All 21 cities in the control groups are cities with county status. Of the 375 municipalities in the treatment group, only two have county status, while the remaining 373 belong to a county. If there had been systematic differences in or changes to the financing structure or spending responsibilities of municipalities with as opposed to without county status during our sample period, this might constitute a violation of the common trend assumption. We know of no such major shifts during the period of interest. Moreover, the revenue sources of cities with county status are equivalent to those of municipalities belonging to a county: Both rely on the same types of taxes, fees and charges, transfers, etc. In contrast, counties are financed solely through state transfers and the *Kreisumlage*, a financial contribution levied from municipalities within the county. Through this levy, municipalities belonging to a county share the responsibility for financing county-level spending. Given this administrative and fiscal setup, we believe that our treatment and control groups are sufficiently comparable.

Systematic differences in the degree to which both groups suffer from fiscal distress and find themselves under the supervision of regulatory authorities might also bias our estimation results. In recent years, regulatory authorities have been bound by official decrees to ensure that local tax multipliers of municipalities operating under budget consolidation plans are higher or at least equal to average state-wide tax multipliers of municipalities in their population size range. This might induce upward movements in tax multipliers which are unrelated to standard tax multipliers. There are unfortunately no official records on municipalities with budget consolidation plans in the mid-1990s. However, according to

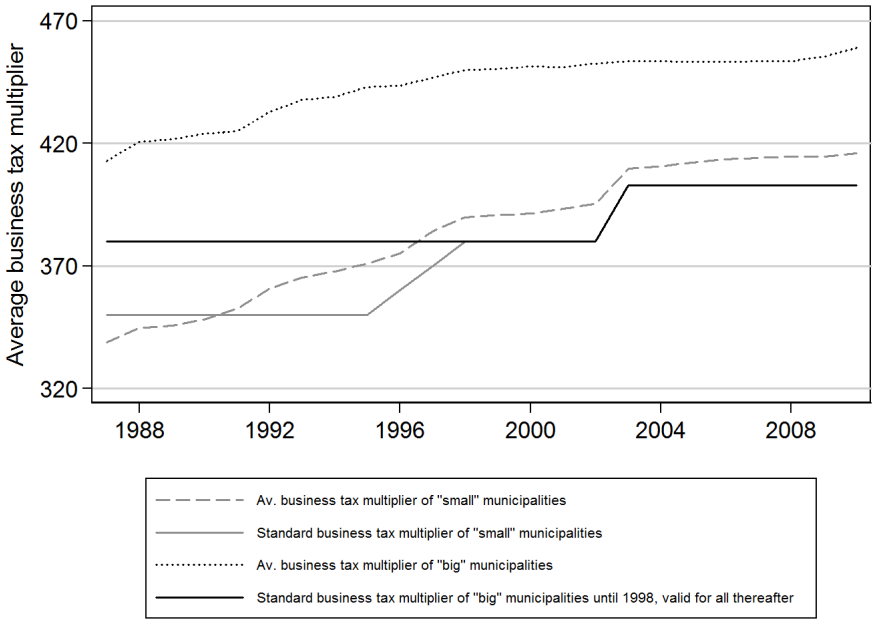
[14] Standard tax multipliers were not set exogenously in later reforms (2003, 2011).

[15] Although we distinguish "treated" and "untreated" municipalities, it is important to note that transfer payments to all municipalities were affected by the reform: The sum of all transfers is fixed and the change in the standard tax multiplier affects how this sum is distributed among all municipalities.

the Ministry of the Interior, the practice of actively influencing tax multipliers is a relatively new phenomenon. To the best of their knowledge, no official decrees existed during our sample period that would have mandated regulatory authorities to make higher tax rates a precondition for the approval of budget consolidation plans. What is more, budget consolidation plans were much less widespread during our sample period than they are today. Thus, we are confident in the validity of the common trend assumption.

We investigate the common trend by plotting the development of the average business tax multipliers of "small" municipalities (treatment group) and "big" municipalities (control group) between 1987 and 2010 (Figure 2.2). The former is represented by the gray dashed and the latter by the black dotted line. The corresponding standard tax multipliers are shown in gray ("small" municipalities) and black ("big" municipalities/ all municipalities). Figure 2.2 supports the common trend assumption. Both groups have seen a gradual upward trend since 1987. The development of their business tax multipliers has been similar for most of the time period. Visible exceptions with some convergence of averages occurred during the reform years 1996-1998 and 2003 (see also Figure 2.1).[16]

Figure 2.2: Development of average business tax multipliers and standard business tax multipliers, 1987-2010



Source: IT.NRW, own calculations.

[16] As mentioned above, there was another reform in 2003. The incentive effect was stronger for the group of "small" municipalities due to their lower business tax multipliers.

2.5 Results

In this section we first present our main results. Next, we perform a number of robustness checks to validate our results. Last, we provide an extension to test if "small" municipalities' reactions to the reform differed systematically depending on their pre-reform business tax multipliers.

2.5.1 Main results

Table 2.2 shows results for two regressions where $t = 1995, ..., 1998$. We restrict our main analysis to the reform period as we expect municipalities to react instantaneously to changes in applicable standard tax multipliers. Specification *I* contains baseline results for a regression without any control variables apart from the usual municipal and year fixed effects. The regression displayed in Specification *II* includes income tax shares, GDP and disposable income of private households, surface area and the number of employees at place of work (each per capita) as additional controls.

Table 2.2: Regression results

	I Baseline	II With controls
Treatment group × 1996	3.694***	3.527***
	(0.755)	(1.077)
Treatment group × 1997	9.406***	6.772***
	(1.974)	(2.296)
Treatment group × 1998	12.047***	8.471***
	(2.321)	(2.883)
Income tax share		92.080**
		(41.175)
GDP		-1.479*
		(0.765)
Inc. of priv. households		4.056
		(3.661)
Surface area		-54.038
		(44.459)
Employees		48.119
		(33.958)
N	1584	1584
R^2	0.498	0.512

Notes: Fixed effects estimates based on Equation (2.11). Balanced panel of all 396 municipalities for the period 1995 to 1998. Dependent variable: business tax multiplier (in %) (municipal level). Independent variables of interest: interaction terms between treatment group and treatment points. Treatment group: "small" municipalities, whose standard tax multiplier was increased in three equal steps between 1996 and 1998. Control group: "big" municipalities, whose standard tax multiplier was not affected by the reform. Treatment points: 1996, 1997 and 1998. Base year: 1995. Base group: control group. Both specifications control for municipal and year fixed effects. Specification *II* additionally controls for income of private households (in 1,000 euro p.c.), GDP (in 1,000 euro p.c.) (county level) and surface area (in ha p.c.), income tax share (in 1,000 euro p.c.), employees (p.c.) (municipal level). Standard errors in parentheses are clustered by county. Significance levels: * 0.10, ** 0.05, *** 0.01.

In line with our expectations, the interaction terms $TG_i \times TP_t,\ t = 1996, ..., 1998$, between treatment group and treatment point dummies are highly significant with positive estimated coefficients in both regression specifications. According to the baseline specification, business tax multipliers of small municipalities were about 3.7 percentage points

higher in 1996 than in 1995 ($\delta_{1996} = 3.694$), c.p. They rose by another 5.7 percentage points in the following year ($\delta_{1997} - \delta_{1996} = 5.712$). A smaller adjustment of about 2.6 percentage points took place in 1998, the final year of the reform ($\delta_{1998} - \delta_{1997} = 2.641$). Given the annual increase of the standard tax multiplier of 10 percentage points, the degree of adjustment of small municipalities' tax multipliers is remarkable.

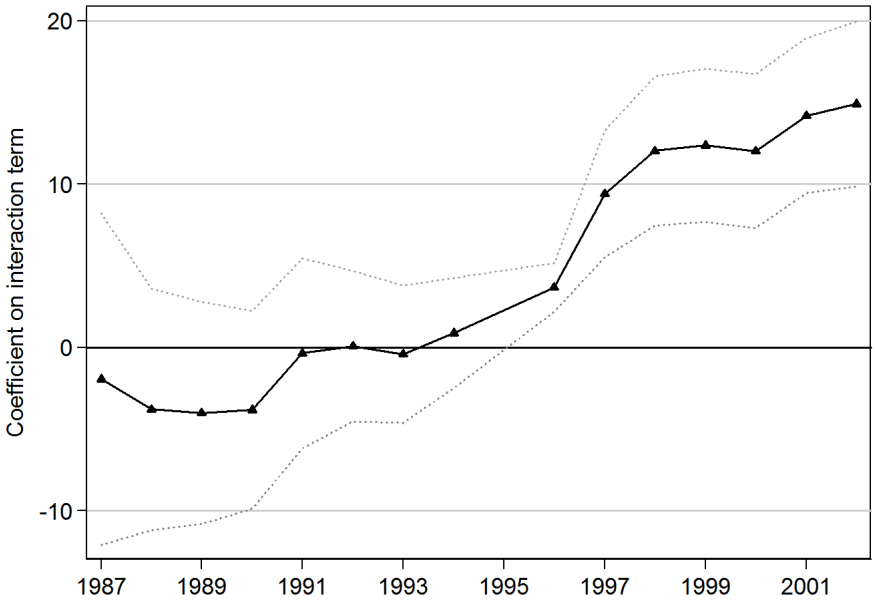
Adding time-variant controls slightly affects the coefficients of interest (Specification II). Per capita income tax shares and GDP each turn out to be individually significant covariates. Per capita disposable income of private households, surface area and employees at place of employment are jointly significant with the remaining controls and further improve the goodness of fit as measured by the R^2. The results of Specification *II* support the general magnitude and the direction of the reform effect. However, they also suggest that the development of business tax multipliers is affected by time-variant factors aside from the reform. We expect the common trend between treatment and control groups to hold conditional on these time-varying factors. Our results are stable across all tested model specifications.[17]

2.5.2 Robustness checks

To validate our results, we perform equivalent regressions using the full dataset where t runs from 1987 to 2002 and corresponding interactions terms $TG_i \times TP_t$, $t = 1987, ..., 1994, 1996, ..., 2002$ and year fixed effects are added. Specification *III* of Table 2.3 shows the results of such a regression without any further control variables.

[17]Further potential controls were tested (e.g., debt p.c. and employees p.c.), but were not significant and did not improve goodness of fit.

Figure 2.3: Coefficients on interaction terms



Notes: Dotted lines mark 95% confidence intervals around point estimates.
Source: IT.NRW, own calculations.

The coefficients of interest, δ_t, belonging to this regression are also illustrated in Figure 2.3. Importantly, the interaction terms $TG_i \times TP_t$, $t = 1987, ..., 1994$ belonging to the pre-reform period are all individually and jointly statistically insignificant. In contrast, the interaction terms $TG_i \times TP_t$, $t = 1996, ..., 2002$ of the reform and post-reform period are all highly significant with positive coefficients, indicating an upward shift of business tax multipliers triggered by the reform. The estimated adjustment during the reform years 1996 to 1998 is exactly the same as in Specification I of Table 2.2. In the years following the reform, estimated coefficients δ_{1999} to δ_{2002} remain fairly stable. This lends support to the notion of an immediate response to each annual change of the standard tax multiplier.[18] Due to limitations in data availability (see Section 2.4.2), a regression using pre-reform data and a set of control variables is not possible. However, the analysis can be extended to post-reform years. This is done in Specification IV of Table 2.3 where t runs from 1995 to 2002 and per capita income tax shares, GDP, disposable income, surface area and employees again have been included as control variables. Again, the reform effects are significant and their magnitude and direction are in line with our expectations.

[18]The slightly higher coefficients in 2001 and in 2002 might be due to anticipating reactions to the 2003 reform.

Table 2.3: Regression results (extended time period)

	III	IV
	Pre- and post-reform	Post-reform
Treatment group × 1987	-1.927	
	(5.167)	
Treatment group × 1988	-3.782	
	(3.762)	
Treatment group × 1989	-4.001	
	(3.457)	
Treatment group × 1990	-3.808	
	(3.081)	
Treatment group × 1991	-0.347	
	(2.955)	
Treatment group × 1992	0.089	
	(2.343)	
Treatment group × 1993	-0.402	
	(2.138)	
Treatment group × 1994	0.888	
	(1.715)	
Treatment group × 1996	3.694***	3.533***
	(0.755)	(0.879)
Treatment group × 1997	9.406***	7.578***
	(1.975)	(1.940)
Treatment group × 1998	12.047***	10.291***
	(2.323)	(2.298)
Treatment group × 1999	12.387***	11.092***
	(2.383)	(2.457)
Treatment group × 2000	12.035***	9.824***
	(2.398)	(2.611)
Treatment group × 2001	14.203***	11.754***
	(2.416)	(2.777)
Treatment group × 2002	14.916***	12.554***
	(2.562)	(2.927)
Income tax share		100.505***
		(31.279)

	III Pre- and post-reform	IV Post-reform
GDP		-0.418
		(0.409)
Inc. of priv. households		1.269
		(1.755)
Surface area		-15.049
		(24.808)
Employees		25.773*
		(15.359)
N	6336	3168
R^2	0.760	0.551

Notes: Specification *III* and *IV* are based on a balanced panel of all 396 municipalities for the period 1987 to 2002 and 1995 to 2002, respectively. Dependent variable: business tax multiplier (in %) (municipal level). Independent variables of interest: interaction terms between treatment group and treatment points. Treatment group: "small" municipalities, whose standard tax multiplier was increased in three equal steps between 1996 and 1998. Control group: "big" municipalities, whose standard tax multiplier was not affected by the reform. Treatment points: 1987 to 1994 (only Specification *III*), 1996 to 2002. Base year: 1995. Base group: control group. Both specifications control for municipal and year fixed effects. Specification *IV* additionally controls for income of private households (in 1,000 euro p.c.), GDP (in 1,000 euro p.c.) (county level) and surface area (in ha p.c.), income tax share (in 1,000 euro p.c.), employees (p.c.) (municipal level). Standard errors in parentheses are clustered by county. Significance levels: * 0.10, ** 0.05, *** 0.01.

As an additional robustness check, we rerun Specifications *I* and *II* of Table 2.2, this time excluding municipalities that were abundant, i.e., did not receive any equalization transfers, at any point between 1995 and 1998. This reduces the number of municipalities in the treatment group to 316. There are 19 municipalities left in the control group. Municipalities that did not benefit from equalization transfers presumably faced weaker incentives to raise their tax multipliers following the increase in their standard tax multiplier. Some incentive effect remains as it is very hard, if not impossible, for most municipalities to predict whether their fiscal capacity might exceed their fiscal need in a given year. Nonetheless, we expect the estimated treatment effect to be stronger than in our baseline specification.

Table 2.4 shows the corresponding regression results. The estimated treatment effect is very similar and slightly more pronounced than in our baseline specifications, confirming our expectations.

Table 2.4: Regression results (excluding abundant municipalities)

	V Baseline	VI With controls
Treatment group × 1996	3.695***	3.386***
	(0.847)	(1.179)
Treatment group × 1997	10.227***	7.325***
	(2.151)	(2.361)
Treatment group × 1998	12.921***	9.024***
	(2.526)	(2.872)
Income tax share		85.131*
		(49.956)
GDP		-1.337
		(0.824)
Inc. of priv. households		6.250*
		(3.517)
Surface area		-36.957
		(39.868)
Employees		47.738
		(33.627)
N	1340	1340
R^2	0.535	0.553

Notes: Fixed effects estimates based on Equation (2.11). Balanced panel of 335 municipalities, which received transfers in all years from 1995 to 1998, for the period 1995 to 1998. Dependent variable: business tax multiplier (in %) (municipal level). Independent variables of interest: interaction terms between treatment group and treatment points. Treatment group: "small" municipalities, whose standard tax multiplier was increased in three equal steps between 1996 and 1998. Control group: "big" municipalities, whose standard tax multiplier was not affected by the reform. Treatment points: 1996, 1997 and 1998. Base year: 1995. Base group: control group. Both specifications control for municipal and year fixed effects. Specification *VI* additionally controls for income of private households (in 1,000 euro p.c.), GDP (in 1,000 euro p.c.) (county level) and surface area (in ha p.c.), income tax share (in 1,000 euro p.c.), employees (p.c.) (municipal level). Standard errors in parentheses are clustered by county. Significance levels: * 0.10, ** 0.05, *** 0.01.

2.5.3 Extension

Lastly, we adapt our model to test if "small" municipalities' reactions to the reform differed systematically depending on their pre-reform business tax multipliers. We expect to find a more pronounced effect for "small" municipalities with a "low" pre-reform business tax multiplier. We operationalize these considerations by distinguishing two groups within our original treatment group: Treatment group 1 consists of the 217 "small" municipalities whose business tax multiplier was smaller than 380 in 1995 ($TG1_i = 1$ if population $\leq 150,000$ and $m_{i,1995} < 380$ and 0 otherwise). Treatment group 2 refers to the 158 "small" municipalities with business tax multipliers greater than or equal to 380 in 1995 ($TG2_i = 1$ if population $\leq 150,000$ and $m_{1995} \geq 380$ and 0 otherwise). The corresponding interaction terms are defined as $TG1_i \times TP_t$ and $TG2_i \times TP_t$, $t = 1996, ..., 1998$. We estimate the following regression equation with $t = 1995, ..., 1998$:

$$m_{i,t} = \alpha_1 TG1_i + \alpha_2 TG2_i + \sum_{t=1996}^{1998} \delta_{1,t}\, TG1_i \times TP_t + \sum_{t=1996}^{1998} \delta_{2,t}\, TG2_i \times TP_t + \beta \mathbf{X_{i,t}} + \theta \mathbf{Z_{c,t}} + \lambda_i + \Phi_t + \varepsilon_{i,t} \quad (2.12)$$

Table 2.5 shows the results for differentiated treatment groups, with Specification *V* displaying the regression without controls (except for the usual municipal and year fixed effects) and Specification *VI* including the same time-variant controls as Specifications *II* and *IV*.

In line with our expectations, we find a much stronger reform effect on the business tax multipliers of treatment group 1 than on those of treatment group 2. All interaction terms between treatment group 1 and treatment point dummies are highly statistically significant with positive estimated coefficients $\delta_{1,t}$. The size of the estimated coefficient on the interaction term between treatment group 1 and treatment point 1996 is rather low and close to the one of treatment group 2 (Specification *V*: $\delta_{1,1996} - \delta_{2,1996} = 0.951$). This is not the case in 1997 and 1998: According to Specification *V*, the business tax multipliers of treatment group 1 were about 13.4 percentage points higher in 1997 than in 1995 ($\delta_{1,1997} = 13.375$) and continued rising in 1998 ($\delta_{1,1998} = 17.284$).

In contrast, the estimated effects of the interaction terms between treatment group 2 and the treatment point dummies are rather stable ($\delta_{2,1996} = 3.144, \delta_{2,1997} = 3.956$ and $\delta_{2,1998} = 4.855$). Moreover, statistical significance of treatment group 2's interaction terms is low compared with those of treatment group 1 and in case of $\delta_{2,1998}$ depends on the

specification used.

Table 2.5: Regression results (two treatment groups)

	V Two treatment groups	VI With controls
Treatment group (1) × 1996	4.095***	4.782***
	(1.046)	(1.339)
Treatment group (2) × 1996	3.144***	3.360***
	(0.897)	(0.961)
Treatment group (1) × 1997	13.375***	14.279***
	(2.113)	(2.588)
Treatment group (2) × 1997	3.956*	3.945*
	(2.080)	(2.282)
Treatment group (1) × 1998	17.284***	16.836***
	(2.209)	(2.815)
Treatment group (2) × 1998	4.855*	3.811
	(2.525)	(2.844)
Employees		43.035
		(28.682)
Income tax share		-56.122
		(42.267)
GDP		-1.436**
		(0.606)
Inc. of priv. households		2.603
		(2.663)
Surface area		20.049
		(42.173)
N	1584	1584
R^2	0.560	0.565

Notes: Fixed effects estimates based on Equation (2.12). Balanced panel of all 396 municipalities for the period 1995 to 1998. Dependent variable: business tax multiplier (in %) (municipal level). Independent variables of interest: interaction terms between treatment groups and treatment points. Treatment group 1: "small" municipalities whose business tax multiplier was smaller than 380 in 1995, and whose standard tax multiplier was increased in three equal steps between 1996 and 1998. Treatment group 2: "small" municipalities whose business tax multiplier was greater than or equal to 380 in 1995, and whose standard tax multiplier was increased in three equal steps between 1996 and 1998. Control group: "big" municipalities, whose standard tax multiplier was not affected by the reform. Treatment points: 1996, 1997 and 1998. Base year: 1995. Base group: control group. Both specifications control for municipal and year fixed effects. Specification *VI* additionally controls for income of private households (in 1,000 euro p.c.), GDP (in 1,000 euro p.c.) (county level) and surface area (in ha p.c.), income tax share (in 1,000 euro p.c.), employees (p.c.) (municipal level). Standard errors in parentheses are clustered by county. Significance levels: * 0.10, ** 0.05, *** 0.01.

In summary, we find that the rise of the standard business tax multiplier had an effect on the business tax multipliers of all "small" municipalities, but this effect was particularly strong for municipalities with a "low" pre-reform business tax multiplier (i.e, pre-reform business tax multiplier below post-reform standard tax multiplier).

2.6 Conclusion

Conventional economic wisdom suggests that decentralized business taxation and a common pool of equalization transfers among local jurisdictions should lead to a race to the bottom in local business tax rates. In practice, however, a simple institutional device, standard tax multipliers, is used to counteract downward pressure on municipal tax rates and tax effort. Standard tax multipliers are employed in fiscal equalization schemes in all German territorial states to assess a municipality's fiscal capacity independently of its actual tax multiplier.

Using the case of North Rhine-Westphalia in the mid-1990s, this paper empirically analyzes the impact of standard business tax multipliers on municipal business tax policy. The results show that upward shifts in standard business tax multipliers lead to immediate upward adjustments in actual business tax multipliers. This is true for all affected municipalities. The reaction is more pronounced for municipalities whose business tax multipliers are below post-reform standard tax multipliers. The findings are robust to a number of alternative specifications. They also reflect the positive incentive effect derived from theoretical considerations.

Our results have important implications for the practical design of fiscal equalization schemes. They highlight the importance of the parameters of equalization systems for shaping local tax policy. Through its choice of the standard tax multiplier, a state can influence the level of municipal tax rates and the weight of competitive downward forces. Some states choose to set standard tax multipliers that are so low that they have virtually no signaling effect while others induce a race to the top in local taxation through regular adjustments of standard multipliers. This partially explains why there is far greater heterogeneity in business tax multipliers across federal states than within states in Germany.

By consequence, standard tax multipliers should be regarded as a tool for governments to shape lower-level tax policy, with important consequences for their own competitiveness.

2.7 Appendix

Table 2.6: Summary statistics (means) by year, 1987-2002

Year	Business tax multiplier	Inc. tax share	GDP	Inc. of priv. households	Employees	Surface area
1987	342.737	.	.	.	.	.
1988	348.838	.	.	.	.	.
1989	349.583	.	.	.	.	.
1990	352.146	.	.	.	.	.
1991	356.376	.	.	.	.	.
1992	364.646	0.294	18.849	.	.	0.450
1993	369.182	0.294	.	.	0.266	0.445
1994	371.593	0.296	19.471	.	0.261	0.440
1995	374.801	0.294	20.066	15.480	0.261	0.434
1996	378.775	0.268	20.288	15.686	0.257	0.430
1997	387.518	0.270	20.603	16.018	0.253	0.426
1998	393.114	0.282	20.992	16.339	0.254	0.423
1999	393.912	0.294	21.287	16.647	0.257	0.420
2000	394.530	0.296	21.699	17.204	0.260	0.418
2001	396.346	0.281	21.884	17.923	0.261	0.416
2002	398.449	0.278	22.181	17.866	0.259	0.414
Total	373.284	0.286	20.732	16.645	0.259	0.429

Notes: Business tax multiplier (in %), surface area (in ha p.c.), income tax share (in 1,000 euro p.c.), employees (p.c.) (municipal level) and income of private households (in 1,000 euro p.c.), GDP (in 1,000 euro p.c.); number of observations: 396 municipalities p.a.

Table 2.7: Summary statistics (means) for treatment group by year, 1987-2002

Year	Business tax multiplier	Inc. tax share	GDP	Inc. of priv. households	Employees	Surface area
1987	338.811	.	.	.	.	.
1988	344.813	.	.	.	.	.
1989	345.547	.	.	.	.	.
1990	348.120	.	.	.	.	.
1991	352.533	.	.	.	.	.
1992	360.827	0.291	18.505	.	.	0.472
1993	365.336	0.291	.	.	0.259	0.466
1994	367.816	0.294	19.124	.	0.255	0.461
1995	370.976	0.292	19.690	15.482	0.255	0.455
1996	375.147	0.266	19.935	15.693	0.251	0.451
1997	384.192	0.269	20.239	16.036	0.247	0.447
1998	389.928	0.281	20.604	16.363	0.248	0.444
1999	390.744	0.292	20.899	16.673	0.250	0.440
2000	391.344	0.294	21.289	17.233	0.254	0.438
2001	393.275	0.279	21.471	17.960	0.254	0.436
2002	395.416	0.276	21.755	17.895	0.253	0.435
Total	369.676	0.284	20.351	16.667	0.253	0.450

Notes: Business tax multiplier (in %), surface area (in ha p.c.), income tax share (in 1,000 euro p.c.), employees (p.c.) (municipal level) and income of private households (in 1,000 euro p.c.), GDP (in 1,000 euro p.c.); number of observations: 375 "small" municipalities p.a.

Table 2.8: Summary statistics (means) for control group by year, 1987-2002

Year	Business tax multiplier	Inc. tax share	GDP	Inc. of priv. households	Employees	Surface area
1987	412.857	.	.	.	.	.
1988	420.714	.	.	.	.	.
1989	421.667	.	.	.	.	.
1990	424.048	.	.	.	.	.
1991	425.000	.	.	.	.	.
1992	432.857	0.338	24.986	.	.	0.055
1993	437.857	0.342	.	.	0.382	0.055
1994	439.048	0.334	25.658	.	0.373	0.055
1995	443.095	0.335	26.778	15.444	0.368	0.055
1996	443.571	0.308	26.597	15.554	0.363	0.055
1997	446.905	0.298	27.105	15.704	0.360	0.056
1998	450.000	0.315	27.930	15.915	0.360	0.056
1999	450.476	0.331	28.205	16.189	0.366	0.056
2000	451.429	0.326	29.037	16.692	0.372	0.056
2001	451.190	0.311	29.245	17.260	0.373	0.056
2002	452.619	0.308	29.786	17.344	0.370	0.056
Total	437.708	0.323	27.533	16.263	0.369	0.056

Notes: Business tax multiplier (in %), surface area (in ha p.c.), income tax share (in 1,000 euro p.c.), employees (p.c.) (municipal level) and income of private households (in 1,000 euro p.c.), GDP (in 1,000 euro p.c.); number of observations: 21 "big" municipalities p.a.

Chapter 3

Does tax policy follow the inverse-elasticity rule?

3.1 Introduction

Motivation and research question. The seminal contribution of Ramsey (1927) shows that optimal tax rates on goods depend inversely on their elasticity of demand. While the optimal-tax literature has made progress since Ramsey's initial contribution, the general idea of his framework has not lost ground: the excess burden of taxation is positively related to the sensitivity of the tax base, and the tax rate should be inversely related to a tax base's tax sensitivity. In the case of two tax bases, for example, Ramsey taxation suggests to impose a higher tax rate on the tax base that is relatively less responsive to tax-rate changes.[1] One important question that arises in this context is: do real-world policy makers apply the rules of Ramsey by imposing different tax rates on goods with different tax elasticities? In other words: does tax policy set higher tax rates on goods that are less responsive to taxation relative to more responsive goods?

Our approach. Evidence on this question is scarce, and the reasons for this scarcity are twofold. First, in order to ensure comparability between the tax rates on different

[1] Ramsey's original framework is about minimizing the deadweight loss of indirect taxes. However, the general rationale also applies to other type of taxes. For example, the recent literature on the elasticity of taxable income (surveyed by Saez et al. 2012) concludes that optimal labor income taxes decrease with the mobility of the income-tax base. In a similar vein, optimal corporate taxes depend on the mobility of the firm tax base (Devereux et al. 2014; Kawano and Slemrod 2015). Mankiw et al. (2009) and Piketty and Saez (2013) survey the current status of the optimal-tax literature.

goods, one requires an institutional setting where tax rates on distinct taxable goods are set by the same institutional players and the taxed goods are subject to similar economic conditions. Second, since tax elasticities are to be estimated empirically, a set-up is required that allows credible empirical identification of the tax sensitivity of the taxed goods of interest. In this paper, we address the research question in an empirical set-up that meets both requirements. We explore the unique case of local taxation on the municipal level in Germany, where more than 11,000 municipalities have autonomy to set taxes on business profits and property values. This empirical playground allows us to identify how changes in business and property taxes affect the respective tax revenues,[2] and hence makes it possible to evaluate local taxation in Germany with respect to Ramsey taxation. We particularly study whether different elasticities of the business-tax and property-tax revenue, which we estimate empirically, translate into actual tax-policy following the inverse-elasticity rule (which is an advancement of the standard Ramsey result; see Baumol and Bradford [1970]).

Our dataset covers the full universe of municipalities in a very long panel of 16 years, 1995-2010. We employ a difference-in-differences research design and for identifying variation we rely on many (business and property) tax-rate reforms that were implemented by the municipalities. The long panel dimension, along with the fact that the municipalities operate in a very homogeneous economic environment, allows us to control for many confounding factors that present potential threats to common trend assumptions. As opposed to many other empirical tax studies, we do not have to deal with changes in the tax base definitions that often come along with tax reforms, because the tax base, as well as a basic tax rate, is defined on the federal level and municipalities only set a multiplier on the exogenous basic rate and base.[3]

[2]Note that there is a direct link between tax-revenue elasticities and tax-base elasticities: a high tax-revenue elasticity with respect to (w.r.t.) tax rates implies a low (in absolute terms) elasticity of the tax base w.r.t. taxes, and vice versa (see Section 3.4 for a detailed exposition of this rationale). We estimate the effect on tax revenues, rather than tax bases, because of data restrictions; the statistical offices do not provide long panel data on tax bases. Calculating the base as tax revenue divided by tax rate is not an option because of unobserved deductions and credits (Fossen and Steiner 2014) and because we do not have information on the distribution of property types in the municipalities (see Section 3.2.2).

[3]Due to data-access limitations and the non-existence of the long panel dimension out of a single data source, these data have not been used extensively by researchers. Notable exceptions include Büttner (2003) on the sensitivity of the business tax base, Becker et al. (2012) on the effect of the local tax on the activity of multinational enterprises, Baskaran (2014) for tax-mimicking effects, Foremny and Riedel (2014) for the effect of elections on tax-rate changes, and Fuest et al. (2016) on the incidence of the tax on wages.

Findings. In our empirical analysis we relate business-tax revenue and property-tax revenue in a municipality to the business-tax rate and property-tax rate, respectively, in this municipality, and exploit within-municipality variation over time to study how elastically business-tax and property-tax revenues respond to tax changes. We find that business-tax revenue does not significantly increase in response to tax hikes. Property-tax revenue, on the other hand, increases by almost 1% in response to a 1% increase in the tax rate. These results imply that the business-tax base responds strongly to tax-rate changes whereas the property-tax base is not tax sensitive (see Section 3.4 on the relationship between the tax-base elasticity and the tax-revenue elasticity). If tax policy was consistent with the inverse-elasticity rule, our estimates would suggest that tax rates are significantly higher on immobile property than on mobile business profits. However, this is not what we observe. In almost all years of our sample period, the share of municipalities with a higher business tax than property tax is considerably greater than 50%. In addition, the average business-tax rate is greater than the average property-tax rate in all years of our sample period. Our elasticity estimates are not driven by municipalities with higher property-tax rates than business-tax rates: municipalities which impose higher rates on business profits than on property also face much more tax responsive business profits than property.

The inverse-elasticity rule depicts that goods should be taxed in inverse proportion to their price elasticities.[4] We can combine this rule with our elasticity estimates and conduct back-of-the-envelope calculations to determine the optimal relation between the business and property tax rates. Given our elasticity estimates of 0.129 for business-tax revenues and 0.886 for property-tax revenues, such calculations yield that the property tax rate should, from an optimal-tax perspective, approximately be seven-times higher on average than the business tax rate. This is in contrast to actual rates; we observe that throughout our sample period the business-tax multiplier is on average 1.08 times higher than the property-tax multiplier. This suggests that German municipalities are not compliant with the inverse-elasticity rule and presumably leave efficiency gains on the table.

Rationalizing the results. We propose three reasons for this result. The deviation

[4]The inverse-elasticity rule is about minimizing deadweight loss in the presence of an exogenous revenue requirement. Note that municipalities arguably do face such a revenue requirement. For example, they are obligated by the federal level to finance large parts of child care and social assistance to unemployed individuals. The inverse-elasticity rule is given by $\tau_1\eta_1 = \tau_2\eta_2$, where τ and η are the tax rates and base elasticities of the two goods. See Section 3.4 for a formal argument on the relationship between the tax-base and tax-revenue elasticity.

between actual and optimal rates are either rooted in *(i)* a lack of knowledge of optimal-tax rules among local policy-makers, *(ii)* distributional (equity) concerns where property is taxed low for social-policy reasons, *(iii)* political-economy arguments in which politicians seek to maintain office, or a combination of these three points. The first possible reason may be prevalent because the optimal-tax literature is highly theoretical and there is a disconnect to applied public finance (see also Footnote 5). The second point may play a role because it is often a declared goal of social-policy to incentivize people to buy and own property in order to build up wealth and be able to absorb economic shocks such as job losses. The rationale for the third reason is that, in contrast to property owners, firm-owners and workers of local firms are more likely to live in other municipalities and therefore do not vote in the municipality where their work place is located. It is therefore more attractive for office-seeking politicians to tax non-residents through business taxes rather than residents through the property tax.

Contribution to the literature. Our paper speaks about and contributes to three strands of literature. First, we relate to a small literature that studies whether actual tax policy follows the requirements of optimal-taxation theory.[5] For example, Saez (2001) develops an optimal income tax model that can be applied based on empirically estimated earnings elasticities. Numerical simulations show that the pattern of actual tax schedules is close to the U-shaped pattern of optimal tax rates that he derives based on his model. The optimal top marginal tax rate, however, is higher than the actual top rate in the United States (U.S.) (Diamond and Saez 2011). Mankiw et al. (2009) discuss the extent to which the most important lessons from the optimal-tax literature are consistent with actual policies. They conclude that "tax policy has moved in the directions suggested by theory along a few dimensions, even though the recommendations of theory along these dimensions are not always definitive." (Mankiw et al. 2009, pages 147/148). For example, they argue that the observed trend towards flatter income-tax rates in Organisation for Economic Co-operation and Development (OECD) countries may suggest that tax-policy is affected by optimal-tax theory. The paper mostly reports cross-country trends in taxation and does not provide a test of optimal-tax rules based on empirically estimated elasticities. Creedy and Gemmell (2015) build on empirical estimates

[5] According to Saez (2001), the optimal-tax literature mostly interests theorists and does not make an impact on applications. This disconnect between the theoretic optimal-tax literature and the applied-empirical literature may be among the reasons why actual tax policies are rarely evaluated in the light of optimal-tax laws. Sørensen (2007) discusses the political relevance of the theory of optimal taxation.

of the elasticity of taxable income to show that, for many individuals actual income-tax rates in the U.S. are well in line with optimal revenue-maximizing tax rates. As opposed to our paper, these few studies on the optimality of actually implemented taxes are mostly on the personal income tax. We provide some first evidence in this regard for other taxes.

Second, we contribute to the literature on the elasticity of the corporate tax base and the elasticity of tax revenues. While the literature acknowledges that firms respond to tax incentives and adjust to taxes along various different dimensions,[6] only a few papers explicitly study the tax-sensitivity of the overall tax bases and tax revenues. One reason for scarce evidence presumably is that in most countries, corporate taxes are levied on the national or state level, which hinders the causal identification of credible estimates. Comparisons of countries, and even federal states within a country, are critical and common trend assumptions in country-level analyses over time are often hard to establish. We aim to overcome these shortcomings by relying on the case of German local business and property taxes in a difference-in-differences research design with many independent tax-setting municipalities which operate in a homogeneous economic environment.

There are a few papers that identify the effect of business/corporate taxes on tax revenues or tax bases. Devereux et al. (2014) examine the elasticity of corporate taxable income in the United Kingdom. Using kinks in the corporate tax schedule, the paper finds moderate elasticities that differ depending on which kink is examined. Dwenger and Steiner (2012) study a reform of the federal German corporate tax to estimate an elasticity that is slightly larger than the ones in Devereux et al. (2014). In contrast to these papers, our estimations are not based on a short time period or a single tax reform but exploit various tax rate changes in a panel over many years.[7]

A few papers study the sensitivity of the business-tax base in the same institutional setting that we use. Büttner (2003) finds a very large effect on the tax base, suggesting that the municipalities are on the downward-sloping part of the business-tax Laffer curve.

[6]For example, it has been shown that corporate tax policies affect location decisions (e.g., Devereux and Griffith 1998), investment behavior (e.g., Feld and Heckemeyer 2011), financing decisions (e.g., Graham 2003) or choice of organizational form (e.g., de Mooij and Nicodème 2008). Devereux et al. (2014) present a more detailed overview of this literature.

[7]Additional papers on the effect of corporate taxation on the tax base and revenues in non-German countries include Gruber and Rauh (2007) who use accounting data that are subject to the usual concerns with these type of data. Clausing (2007), Devereux (2007) and Brill and Hassett (2007) employ country-level regressions but do not include country fixed effects, which makes the results questionable. Mintz and Smart (2004) and Dahlby and Ferede (2012) use within-country variation in Canada. Kawano and Slemrod (2015) study corporate-tax-base responses in a panel of OECD countries.

Baskaran (2015)[8] does not find any significant effects of the business tax on tax revenues; this is in line with our findings. These studies of Büttner (2003) and Baskaran (2015) are based on data from only one (out of 16) federal states while we use country-wide data. Fossen and Steiner (2014) use firm-level data to study how firms respond to changes in municipality-level tax rates. Their findings suggest that firms reduce their taxable income by about 0.5% in response to a 1% increase in the tax rate, suggesting that municipalities can raise extra tax revenues through higher tax rates despite a significant tax-base response. In contrast to the long panel we are able to rely on, Fossen and Steiner (2014) only use data from two years which complicates standard common trend assumptions. In general, our analysis intends to complement the few empirical papers, and adds to the important understanding of behavioral responses of firms to profit taxation.

Third, our work adds to a very small literature on the sensitivity of property to taxes. One recent study is Baskaran (2015) (see Footnote 8), which uses the same institutional set-up as we do, though only for one federal state, and finds that the tax-revenue elasticity of property taxes is close to unity. Stine (1988) uses local-level panel data from New York State and finds that the property tax base does not respond strongly to tax-rate changes. The findings in both these studies are consistent with our findings.

Structure of the paper. The paper proceeds as follows. Section 3.2 illustrates the institutional background. Section 3.3 describes the data sources and provides summary statistics. Section 3.4 lays out our empirical model and identification strategy. Section 3.5 presents and discusses the results. Section 3.6 concludes the chapter.

3.2 Institutional background

While most taxes are set at the federal level in Germany, German municipalities are granted the right to set a business tax (*Gewerbesteuer*, short: BT) and a property tax (*Grundsteuer*, short: PT) autonomously. This implies that the business and property tax are set locally by each of more than 11,000 municipalities (*Gemeinden*) that are spread around the entire country. Business and property tax revenue are two of the most relevant sources of income of German municipalities.[9] The business tax is the most abundant tax

[8]This paper, developed independently of this work, focuses solely on the revenue elasticities of the local taxes, while we focus on the implications of our revenue-elasticity estimates for the inverse-elasticity rule.

[9]Other major municipal income sources are: municipal share of value-added tax and income tax, duties and charges, municipal fiscal equalization transfers.

on profits in Germany. Total (gross) business tax revenue amounted to 43 billion (bn) euros in 2013, this corresponds to about 7% of total national tax revenues. Property tax income summed up to 12.4 bn euros in 2013.

3.2.1 Business taxation

The business tax is levied on operating profits and applies to both corporate and non-corporate firms.[10] The tax base for the local business tax is legally defined by the federal government and cannot be affected by the municipalities. The federal government also sets a basic federal tax rate (*Steuermesszahl*, τ_{fed}^{BT}), but the local municipalities choose the multiplier (*Hebesatz*, m_i^{BT}). The actual business tax rate is derived by multiplying the local tax multiplier with the basic federal tax rate.[11] The local tax rate τ_i^{BT} on business profits in a municipality i equals: $\tau_i^{BT} = \tau_{fed}^{BT} \times m_i^{BT}$. The basic federal tax rate, τ_{fed}^{BT}, was set at 5% until 2007 and was decreased to 3.5% since 2008. That is, a multiplier m_i^{BT} of 300% implied a local business-tax rate of 15% before 2008 and 10.5% since 2008.[12] There are no kinks in the tax schedule and the tax rate is not graduated so that marginal tax rates equal average tax rates. With regard to the period of interest, two relevant reforms took place in Germany on the federal level: First, the tax on business capital was abolished in 1998, reducing the tax base of the business tax. Second, since 2008 the business tax is no longer deductible as a business expense widening the tax base. These two reforms affected all municipalities and firms in the country equally, and therefore should not confound our results. Business tax multipliers varied between 30 and 600 during the time period 1995 to 2010 and averaged about 325 (Figure 3.1 in Section 3.5.3 provides an overview of the development of average multipliers over time, more summary statistics can be found in Section 3.3.2).

[10] In addition to the local business tax, corporate firms and non-corporate firms are also subject to federal corporate and income taxes, respectively. Subject to certain restrictions, non-corporate firms may deduct the local business tax from the federal income tax. Most companies in the agricultural and public sector as well as self-employed and freelancers are exempted from the local business tax.

[11] Strategic profit shifting of corporations with multiple establishments to exploit differences in local business tax multipliers are illegal and very difficult.

[12] A minimum multiplier (floor) of 200% was implemented in 2004 to avoid detrimental tax competition. However, only about 18 out of more than 11,000 municipalities had set a multiplier below 200% before this reform.

3.2.2 Property taxation

The property tax works much in the same way as the business tax; the tax base and the basic federal rate are defined at the federal level, and the municipalities only decide upon the property-tax multiplier. The German property tax system distinguishes two categories of property: property used for agricultural and forestry (property tax A) and constructable property or property with buildings (property tax B). In this paper, we focus on the latter (property tax on buildings and constructable property) because the tax on agricultural and forestry property is of minor importance for a municipality's tax revenue. The tax base is a standardized value (*Einheitswert*) of the property (and not the market value).[13] As with the business tax, the property tax in a municipality i depends on the federal rate and the multiplier, i.e., $\tau_i^{PT} = \tau_{fed}^{PT} \times m_i^{PT}$. The basic federal tax rate depends on the type of building and also varies between West and East Germany. Depending on the type of property, the rates vary between 0.26 and 0.35% in West Germany and between 0.5 and 1% in East Germany. The differences between East and West Germany account for the different definitions of the standardized value (which is the tax base) in the two parts of the country and are intended to make the multiplier rates comparable between East and West Germany.[14] Property tax multipliers varied between 50 and 810 during the time period 1995 to 2010 and averaged about 313 (Figure 3.1 in Section 3.5.3 provides an overview of the development of the average multipliers over time, more summary statistics can be found in Section 3.3.2).

3.2.3 Comparability of business and property tax rates

In evaluating whether German municipalities apply the inverse-elasticity-rule when setting the multiplier tax rates for the property and business tax, we imply that the multipliers are comparable between these two types of taxes. At first glance, this seems to be a strong assumption because the tax bases appear to be very different: operating profits of firms are the tax base in case of the business tax, and standardized property values are the base for the property tax. However, in this paper we do not compare the effective tax rates

[13]The standardized values do not fairly reflect today's real property value (although they are supposed to according to the legal basis). This is only one of the reasons why the reform of the property tax is subject to frequent debate.

[14]Since our data is on the municipality level, we do not have information on the distribution of property types, hence do not know the respective federal tax rates. This is one reason why we use the multipliers rather than the effective tax rates in our empirical analyses.

but rather use the multiplier rates for comparison. These are not directly applied to the respective tax bases (profits or property). Instead, the base is multiplied with the federal tax rate (*Steuermesszahl*) and then the multipliers are applied (recall: $\tau_i = \tau_{fed} \times m_i$). We argue that the substantially different levels of the federal tax rate (recall: between 3.5 and 5% for the business tax and between 0.26 and 1% for the property tax) account for the different nature of the tax bases, and make the multiplier rates comparable between the two types of taxes. This is supported by the levels of multipliers that we observe. Although the tax bases are so different in nature, the multipliers for both taxes range between very similar levels. During the time period that we examine, on average multipliers ranged between 312 and 338 for the business tax and 292 and 329 for the property tax. Hence, the multipliers appear to be on similar and comparable levels.[15]

3.3 Data

In this section we provide an overview of our data, present summary statistics and summarize the identifying variation in tax rates that we use in our empirical analyses.

3.3.1 Data sources

We construct a unique dataset on German municipalities using and combining different sources of administrative data. Our municipality-level data on tax rates, tax revenues and population come from the statistical offices of the federal states.[16] Municipality-level employment statistics are provided by the Federal Employment Agency (*Bundesagentur für Arbeit*). We further add variables on the county-level (gross domestic product (GDP), level of debt, disposable income of private households) for the years 1995-2010, which come from a different data source at the statistical offices of the federal states (note that each of about 400 counties either comprises several municipalities or constitutes a single

[15]The view that the federal rates are adjusted to make the tax multipliers comparable is supported by the fact that the federal rates for the property tax are different for East and West Germany due to different definitions of the standardized property values.

[16]The most recent data, since 2009, are available online at the so-called Regional Database Germany (*Regionaldatenbank Deutschland*). Data for the years between 2001 and 2008 are made available via the Statistical Local *(Statistik Lokal)* publications. Earlier data before 2001 are retrieved on request directly from the respective statistical offices of the federal states. This is the reason why the time dimension of our dataset varies by state. For example, while our oldest data on the PT and BT go back to the 1970s (Bavaria), Mecklenburg-Vorpommern could only provide data since 1998.

municipality).

While some federal states provide data that range back to the 1970s, the majority of states make data available since the early 1990s. In addition, the county-level variables are as well only available since 1995. This is the reason why we base our baseline estimations on the period 1995-2010. We also run sensitivity checks with a longer time-series but fewer federal states. To obtain comparable elasticity measures, we restrict our empirical analysis to those municipalities for which we have complete information on the business-tax and property-tax rates and revenues. We further exclude municipalities with negative business or property tax revenue in a certain year from our empirical analysis since actual revenues cannot be negative but are only reported to be negative due to accounting procedures. In total, the data in our baseline analyses contain about 170,000 municipality-level observations in the 16-year panel, 1995-2010 (see Table 3.5 in Appendix 3.7 for more information). The next section provides summary statistics.

3.3.2 Summary statistics and identifying variation

Table 3.1 provides descriptive statistics for our baseline sample during the period 1995 to 2010. Average tax revenue from the business tax (BT) is much higher than from the property tax (PT), suggesting that the business tax is more important for the municipalities. The table also shows that there is great heterogeneity across municipalities w.r.t. to population size (between 10 and 3.4 million inhabitants). The fact that the average BT multiplier is higher than the average PT multiplier is at the center of this paper, and will be discussed in more detail below. Tables 3.6 to 3.11 in Appendix 3.7 report summary statistics (mean, standard deviation, numbers of observation) by year and state.

Our difference-in-differences identification strategy rests on within-municipality variation in tax rates over time. Table 3.2 provides an overview of variation in the business-tax and property-tax multipliers. For each year of your baseline sample, it depicts the share of municipalities which change their BT and PT rates in year t compared to year $t-1$. On average, 8% and 10% of the municipalities change their BT and PT multipliers per year, respectively. Thereby, the BT tends to be more stable than the PT and the share of municipalities that changed the PT is higher each year.

Table 3.1: Summary statistics, 1995-2010

Variable	Mean	Std. Dev.	Min.	Max.	N
Revenue BT	2651.616	28204.096	0.005	2078955	174670
Revenue BT (log)	4.91	2.501	-5.276	14.547	174670
Multiplier BT	324.885	37.755	30	600	174670
Multiplier BT (log)	5.776	0.121	3.401	6.397	174670
Revenue PT	808.975	8033.088	0.511	747238	174670
Revenue PT (log)	4.799	1.694	-0.671	13.524	174670
Multiplier PT	313.119	45.87	50	810	174670
Multiplier PT (log)	5.736	0.146	3.912	6.697	174670
GDP	3773758.17	3354230.616	671848	98751797	158540
Population	7.183	46.607	0.01	3471.418	174418
Population (sq)	2223.819	117169.293	0	12050743	174418
Debt	157390.836	119370.435	0	3414334	162730
Private income	2845050.66	1934041.044	488465	31811513	150270
Employees	2549.232	18909.947	0	1158925	158700

Notes: Summary statistics. Municipal level variables BT and PT revenue (in 1,000 euro) and multiplier (in %), population (in 1,000) and number of employees. County level variables: level of debt (in 1,000 euro), income of private households (in 1,000 euro) and GDP (in 1,000 euro). Administrative data on the universe of German municipalities collected from the statistical offices of the German federal states. Years 1995 to 2010. Restricted to observations for which information on BT and PT were available. Observation excluded if revenue is reported negative.

Tables 3.12 and 3.13 in Appendix 3.7 provide further detailed information on the size and direction of the changes that we exploit for identifying variation. Most of the changes are multiplier increases in the range between 1 and 50 points. There are more tax increases than decreases, and only a small share of municipalities implement multiplier changes that exceed 50 points. The variation over time is visualized in Appendix 3.7 Figures 3.2 and 3.3. The figures depict maps of Germany showing the levels of business taxes and property taxes in the years 2001 and 2010, respectively. The differences in colors between the years provide a visualization of the variation over time for both type of taxes.

Table 3.2: Share of municipalities with changing BT and PT multipliers (in %), 1995-2010

Year	Change BT	Change PT
1995	14.40	18.97
1996	8.45	10.60
1997	8.83	12.94
1998	8.96	12.40
1999	4.63	6.25
2000	5.02	5.95
2001	8.01	8.30
2002	7.96	9.97
2003	9.69	13.35
2004	8.81	13.26
2005	11.12	14.98
2006	8.20	9.98
2007	4.50	5.70
2008	4.29	5.00
2009	4.84	5.07
2010	9.79	12.88
Total	7.82	10.14

Notes: Share of municipalities which change their BT and PT multipliers between years.

3.4 Empirical strategy and identification

To estimate the effect of local taxes on tax revenues, we employ the following difference-in-differences (DiD) panel-regression model:

$$\log Y^j_{i,c,s,t} = \epsilon^j \log m^j_{i,c,s,t} + \beta^j_1 \mathbf{X}_{i,c,s,t} + \beta^j_2 \mathbf{Z}_{c,s,t} + \lambda_i + \phi_t + \phi_t \times \eta_s + \mu^j_{i,s,c,t}, \tag{3.1}$$

where i stands for a municipality in county c in federal state s in year t, and j indicates the type of tax; either property tax (PT) or business tax (BT). The dependent variable, $Y^j_{i,c,s,t}$, is tax revenue generated from the (business or property) tax of type j (in euro) in municipality i. The explanatory variable of interest is the (business or property) tax multiplier $m^j_{i,c,s,t}$. $\mathbf{X}_{i,c,s,t}$ is a vector containing several control variables on the municipality-level (population, number of employees), $\mathbf{Z}_{c,s,t}$ contains control variables

on the county-level (GDP, level of debt, disposable income of private households), λ_i are municipality fixed effects, ϕ_t are year fixed effects, $\phi_t \times \eta_s$ are federal-state specific year effects, and $\mu_{i,c,s,t}$ is an error term. Tax revenues and tax rates enter the regression in logs to derive an elasticity interpretation of the effect of tax rates on revenues. The elasticity of interest for tax of type j is denoted ϵ^j. Standard errors are clustered on the municipality level.

Each of the about 11,000 municipalities annually sets the tax rate multiplier for year t in the last months of year $t-1$, and our data cover the universe of all municipalities and span a long time period (see Section 3.3.2 for details on the variation in tax rates). This institutional set up generates many differential tax rate changes, motivating the use of the DiD research design. The identifying assumption behind our DiD design is that municipalities with tax rate changes would have had the same development of tax revenues as municipalities without tax rate changes, had they not implemented a tax rate change. This assumption is violated if, for example, local economic conditions force certain municipalities to change rates, whereas other municipalities face other, less pressing local economic conditions. We take two steps to control for this possible threat to identification (as in Fuest et al. [2016]; also see Footnote 17): First, we include federal-state (*Länder*) specific year effects that control for any economic shocks on the federal-state level. Since federal states constitute homogeneous economic environments, it is unlikely that a certain economic shock hits municipalities within a federal state differently. Second, in addition to wiping out time-invariant municipality effects, we control for time-varying municipality characteristics such as size as well as economic conditions such as GDP, level of debt or number of employees. This implies that we only compare municipalities with similar characteristics and economic conditions. In addition, previous evidence shows that tax changes are usually implemented for political, rather than economic, reasons (such as upcoming elections), suggesting that local economic conditions play a minor role in explaining tax changes (see, e.g., Castanheira et al. 2012; Foremny and Riedel 2014; Fuest et al. 2016).[17]

One other concern with many empirical tax studies is that tax reforms often come with changes in tax rates *and* tax base definitions (e.g., tax-rate-cut-cum-base-broadening type reforms. Kawano and Slemrod [2015] provide empirical evidence that tax changes are usually accompanied by tax-base changes). This is not a concern in our set-up: any changes in tax base definitions are legislated on the federal level and are accounted for by the year

[17]Fuest et al. (2016) use the same variation in business taxes that we use in a similar empirical design. They provide careful tests showing that local economic conditions do not drive tax changes.

fixed effects. Reverse causality is not likely to bias our results either. The timing of events is such that municipalities usually set the multiplier in the last months of year $t-1$; that is, before they are aware of tax revenues in year t. Reverse causality, however, remains a potential problem if municipalities have precise expectations in $t-1$ regarding tax revenue in year t. We argue that such expectations will usually be based on economic conditions and hence should be captured by the control variables and state-specific year effects. A further identification concern relates to tax competition effects and the potential dependency of tax policies across neighboring municipalities. Recent empirical evidence using the same institutional set-up as ours by Baskaran (2014), however, does not find any sort of tax mimicking and therefore neglects this possible source of bias. His paper shows that following an exogenous tax reform that provided municipalities in one federal state with an incentive to change rates, neighboring municipalities in other federal states did not adjust their rates in response. Isen (2014) finds a similar result for local taxation in the US.

In general, this empirical set-up is very advantageous in light of the purpose of this paper: we aim to compare tax responses of two different taxable goods. This can be done cleanly in the described empirical framework since we look at the same municipalities over the same time period using the exact same identification strategy and data. If any sources of endogeneity remain despite our carefully chosen research design, the resulting bias will be similar for the property and business tax elasticities, hence ensuring that the relative magnitudes of the estimates are comparable.

Tax-base versus tax-revenue elasticities. The inverse-elasticity rule is regarding the responsiveness of tax bases, rather than the responsiveness of tax revenues. However, data restrictions do not allow us to study the effect of taxes on tax bases and we instead estimate the effect on tax revenues (see Footnote 2 and Section 3.2.2). To see the relationship between the tax-base elasticity and the tax-revenue elasticity, consider the simple inverse-elasticity rule:

$$\tau_1 \eta_1 = \tau_2 \eta_2, \tag{3.2}$$

where τ_j and η_j (with $j \in 1, 2$) are the tax rate and the tax-base elasticity of goods 1 and 2 (i.e., property and business), respectively. The tax base is denoted B_j and the term for the tax-base elasticity is: $\eta_j = \frac{\Delta B_j}{\Delta \tau_j} \frac{\tau_j}{B_j}$. Tax revenue TR_j is given by $TR_j = \tau_j B_j$, and changes in tax revenue are then:

$$\Delta TR_j = \Delta\tau_j B_j + \tau_j \Delta B_j = \Delta\tau_j B_j + \eta_j B_j \Delta\tau_j = B_j(1+\eta_j)\Delta\tau_j \tag{3.3}$$

To derive the elasticity of tax revenue, we divide Equation 3.3 by $TR_j = \tau_j B_j$ and rearrange the equation:

$$\frac{\Delta TR_j}{\Delta\tau_j}\frac{\tau_j}{TR_j} = (1+\eta_j) \tag{3.4}$$

The left-hand-side in this equation is the elasticity of tax revenue w.r.t. the tax rate, which is the parameter that we identify from our empirical estimates (denoted ϵ^j in the regression equation above). The equation shows that our empirical estimate equals $(1+\eta_j)$, and hence has a direct relationship with the elasticity of the tax base. In particular, a high elasticity of tax revenues is associated with a low (in absolute terms)[18] elasticity of the tax base, and vice versa. In the extreme case where our revenue elasticity would be estimated to be 1 (0), the associated tax-base elasticity is 0 (-1). This exercise of course neglects the existence of deductions and credits, but illustrates the general relation between the elasticities of the tax base and tax revenue.

3.5 Results and discussion

In this section we first present our main regression results. Next we provide some additional results. Last we discuss our findings and provide possible explanations.

3.5.1 Main results

Table 3.3 depicts our main regression results. Columns (I) and (II) show that business profits are very sensitive to tax-rate changes. The elasticity of tax revenues with respect to a change in the tax multiplier is about 0.12 and not different from zero in a statistical sense. That is, municipalities are not able to raise significant amounts of extra tax revenue by increasing business tax rates. The results are robust to the control variables included (Columns (I) vs. (II)). Our elasticity estimates suggest that, because firms are tax responsive, tax rates in German municipalities are placed close to the peak of the Laffer curve. While these elasticities appear to be very low, they are still higher than the results in

[18]Note that we expect the tax-revenue elasticity to be positive and the tax-base elasticity to be negative.

Büttner (2003) who finds that municipalities in the federal state of Baden-Württemberg even loose tax revenue in response to tax-rate hikes.

Columns (III) and (IV) of Table 3.3 present the results for the property-tax elasticities. We find that, in contrast to business income, property is almost not responsive to tax-rate changes. A 1% increase in the property tax multiplier leads to a tax-revenue increase of about 0.9%. The results are practically independent of the control variables we include (Columns (III) vs. (IV)). The property tax elasticities are in line with intuition: physical property cannot be as mobile as business income and only responds slightly through, for example, long-term responses (see next subsection) or avoidance/evasion channels. In light of the fact that we estimate property-tax and business-tax elasticities in the exact same institutional set-up using an identical research design, the plausibility of the property-tax estimates also lends credibility to our estimates for the business-tax elasticities.

Table 3.3: Effects of business and property taxes on tax revenues

	(I)	(II)	(III)	(IV)
	Revenue business tax		Revenue property tax	
m^{BT}	0.129	0.116		
	(0.0900)	(0.0966)		
m^{PT}			0.886***	0.896***
			(0.0115)	(0.0109)
Observations	158,288	136,614	158,288	136,614
R^2	0.148	0.136	0.671	0.685
Year FE	yes	yes	yes	yes
Municip. FE	yes	yes	yes	yes
State × Year FE	yes	yes	yes	yes
Add. Controls	no	yes	no	yes

Notes: Fixed-effects regressions based on Equation (3.1) with standard errors in parentheses. Standard errors clustered on the municipality level. Significance levels are * < 0.10, ** < 0.05, *** < 0.01. Administrative data on the universe of German municipalities. Years 1995 to 2010. The unit of observation is a single municipality. Dependent variable in Columns (I) and (II) is municipality-level tax revenue generated from the local business tax (BT), and municipality-level tax revenue from the local property tax (PT) in Columns (III) and (IV). The independent variable of interest is the municipality-level tax multiplier m for the business or property tax. Reported coefficients can be interpreted as elasticities identified from tax reforms. All specifications include year fixed effects, municipality fixed effects, and federal-state specific year fixed effects. Columns (I) and (III) include controls for population and GDP. Columns (II) and (IV) additionally add controls for the level of debt, number of employees, and disposable income of private households.

3.5.2 Additional results

Our results are robust to a number of further sensitivity checks presented in Table 3.14 in Appendix 3.7. Panel A of the table depicts the results of regressions estimated on a balanced panel of municipalities. The results are almost unchanged. In Panel B, we present regressions estimated on a sample of less federal states for which we have data for more years. In particular, these regressions include the years 1984 to 2010 for the federal states of North Rhine-Westphalia, Bavaria, and Lower Saxony. Compared to the baseline estimates, both elasticities become smaller. However, a negative coefficient for the business tax multiplier remains insignificant. Panels C and D present long-run estimates where we lag the independent tax variable one and two years, respectively. Still insignificant, the property tax coefficient moves even closer to zero. The results for the property tax are interesting. As one would expect, property is more mobile in the long-run. The coefficient drops to 0.66 when we lag the tax rate one year, and drops even further, to 0.46, in the two-year-lag specification. The result that property becomes more mobile in the long-run is in line with the findings of Stine (1988).

In order to examine if differences in elasticities are associated with different tax rates, Table 3.4 reports regression results separately for municipalities that have higher property-tax rates than business-tax rates (Panel A) and municipalities that impose a higher rate on business profits than on property (Panel B). The coefficients in both panels reveal the same pattern as before for both types of municipalities. Business profits are very tax sensitive with imprecisely measured elasticities close to zero, whereas the tax-revenue elasticity for the property tax is always close to unity. These results show that business-profits are more responsive to taxes even in those municipalities that impose higher taxes on business than on property.

Table 3.4: Tax-revenue elasticities. Heterogeneity w.r.t. tax-rate relation

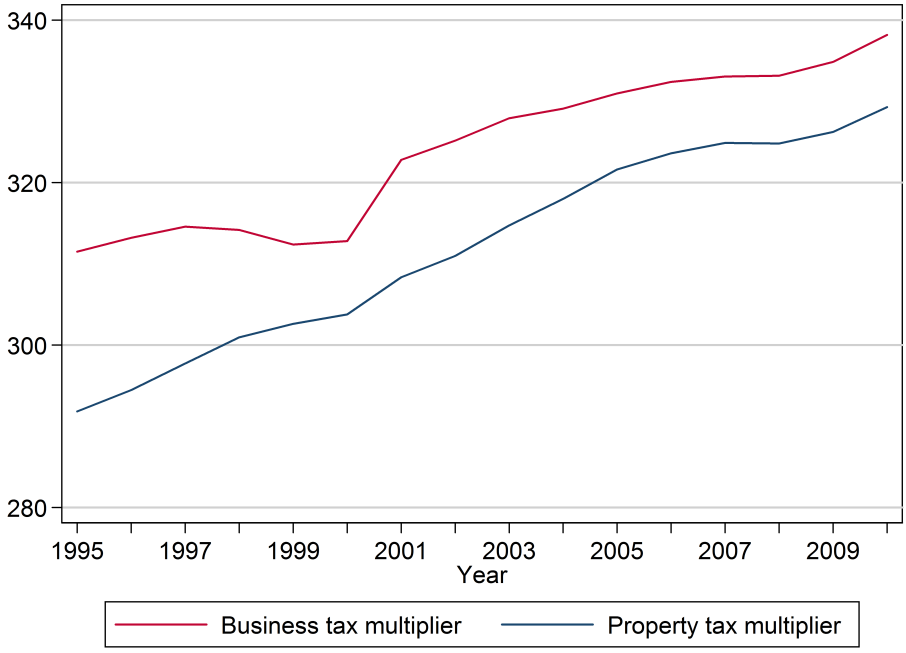
	(I)	(II)	(III)	(IV)
	Revenue business tax		Revenue property tax	
Panel A: Municipalities with $m^{BT} \leq m^{PT}$				
m^{BT}	0.191	0.234*		
	(0.125)	(0.138)		
m^{PT}			0.894***	0.908***
			(0.0245)	(0.0221)
Observations	66242	55461	66242	55461
R^2	0.167	0.154	0.544	0.491
Panel B: Municipalities with $m^{BT} > m^{PT}$				
m^{BT}	0.0714	0.0445		
	(0.154)	(0.160)		
m^{PT}			0.912***	0.917***
			(0.0127)	(0.0127)
Observations	92046	81153	92046	81153
R^2	0.122	0.115	0.753	0.775
Year FE	yes	yes	yes	yes
Municip. FE	yes	yes	yes	yes
State × Year FE	yes	yes	yes	yes
Add. Controls	no	yes	no	yes

Notes: Fixed-effects regressions based on Equation (3.1) with standard errors in parentheses. Standard errors clustered on the municipality level. Significance levels are * < 0.10, ** < 0.05, *** < 0.01. Administrative data on the universe of German municipalities. Years 1995 to 2010. The unit of observation is a single municipality. Dependent variable in Columns (I) and (II) is municipality-level tax revenue generated from the local business tax (BT), and municipality-level tax revenue from the local property tax (PT) in Columns (III) and (IV). The independent variable of interest is the municipality-level tax multiplier m for the business or property tax. Reported coefficients can be interpreted as elasticities identified from tax reforms. All specifications include year fixed effects, municipality fixed effects, and federal-state specific year fixed effects. Columns (I) and (III) include controls for population and GDP. Columns (II) and (IV) additionally add controls for the level of debt, number of employees, and disposable income of private households. Panel A (B) is restricted to municipalities whose business-tax multiplier is smaller or equal (greater) than the property-tax multiplier.

3.5.3 Discussion of results

Comparing implemented business-tax and property-tax rates. Our empirical findings are consistent across all specifications: property-tax revenue responds significantly to tax-rate changes whereas business-tax revenue does not. These results imply that the property-tax base is significantly less responsive than the business-tax base. Following the simple inverse-elasticity rule, this would suggest that average property taxes should be considerably higher than average business taxes. However, this is not what we observe in the data. Figure 3.1 depicts the evolution of the (unweighted) average business-tax multiplier and the (unweighted) average property-tax multiplier over time. The figure clearly shows that property-tax multipliers are lower than business-tax multipliers throughout our entire sample period. While the difference becomes slightly smaller over time, it has been considerably large and stable since the early 2000s. Figures 3.2 and 3.3 in the Appendix 3.7 provide an overview of business-tax and property-tax multipliers in all German municipalities in 2001 and 2010, respectively. The maps confirm the previous figure; there seem to be more darker areas – which indicate high multipliers – in the left maps sketching business-tax rates compared to the right map where we display the property-tax rates in both years. Table 3.15 in the Appendix 3.7 presents more evidence in this regard. For each year between 1995 and 2010, it displays the share of municipalities which have a higher business-tax multiplier than property-tax multiplier. On average, 54% of all municipalities taxed business profits higher than property, and this share was larger than 50% in all years except 1999 and 2000. Our results show that our main elasticity estimates are not driven by those municipalities with higher property-tax rates than business-tax rates: the business-tax elasticities are not different for municipalities which set higher taxes on business profits relative to those with higher property-tax rates (see Table 3.4).

Figure 3.1: Average BT and PT multipliers over time, 1995-2010



Notes: The figure depicts the evolution of business-tax and property-tax multipliers over time. Unweighted country-wide averages for the years 1995-2010 based on municipality-level data. Own compilation. Administrative data as described in the text.

Given our empirical elasticity findings, the descriptive observation that business profits are taxed at higher rates than property stands in contrast with the inverse-elasticity rule, which depicts that goods should be taxed in inverse proportion to their tax sensitivity. Considering the simplest version of this theoretical insight,[19] our baseline revenue-elasticity estimates of 0.129 for business-tax revenues and 0.886 for property-tax revenues would imply that the multiplier on property should be approximately seven times higher than the multiplier on business profits. This ratio is very high and likely not feasible in practice, but it reveals that in this set-up, taxing business profits at higher rates than property is not in line with one of the basic insights of optimal taxation. As opposed to the "optimal" ratio, we observe that in reality throughout our sample period the business-tax multiplier is on average 1.08-times higher than the property tax multiplier. In support of the claim that municipalities do not set tax rates in line with inverse-elasticity-rule ideas, we find

[19]Note that the inverse-elasticity rule reads $\tau_1\eta_1 = \tau_2\eta_2$, where our estimated tax-revenue elasticities equal $(1+\eta_j)$, with η_j being the elasticity of the tax base (see Section 3.4).

that even those municipalities that set higher business-tax rates face highly tax-responsive business-tax bases and non-responsive property tax-bases.

Possible explanations. Why do we find that policy-makers in German municipalities do not set tax rates according to the inverse-elasticity-rule? We propose three possible explanations for the fact that municipalities tax business profits heavier although they are much more sensitive to tax rate changes than the property-tax base:[20] *(i)* one obvious explanation may be that policy makers in German municipalities lack knowledge of economic optimal-tax rules. That is, they may simply not know that it may be more efficient to tax the less mobile tax base higher relative to another more mobile tax base. *(ii)* Another explanation is that property is taxed low for social-policy reasons. It may be an explicit goal of tax policy to encourage individuals to own property and buy real-estate. Low taxes on property could help to serve this goal. *(iii)* A third possible explanation is based on a political-economy argument where politicians seek to stay in office and maximize votes. Business owners and employees, who would benefit from low business taxes, can live either in the respective municipality or in neighboring regions. Property owners, on the other hand, usually live in the municipality where they pay taxes. As a result, an office-seeking politician may be more encouraged to serve property owners through lower taxes than business owners and employees who may not have the opportunity to reward low business taxes through votes in elections.

We acknowledge the possibility that no single argument explains our results, but that our findings may be driven by a combination of explanations. We also do not claim that our list of possible explanations is exhaustive. However, these three explanations seem to be most plausible and we argue that they can help to rationalize our empirical findings.

3.6 Conclusion

Using rich panel data from German municipalities, this paper estimates how tax revenue generated from business and property taxes responds to variation in the respective tax rates. The results indicate that business profits are quite tax sensitive, and property

[20] These explanations are plausible ad-hoc explanations for which we do not provide empirical evidence. Testing these explanations is arguably not possible with the available data, and is also beyond the scope of this paper.

does not respond significantly in the short-run. A 1% increase in business taxes does not have a significant effect on business-tax revenue, while a 1% increase in the property tax increases tax-revenue from the property tax by almost 1%. Applying basic Ramsey-type rules of optimal taxation, these elasticity estimates would suggest that average tax rates are significantly higher on property relative to business-tax rates. However, this is not what we observe in the data: on average, business profits are taxed at lower rates than property.

Our results suggest that efficiency gains could be realized through reducing (or even reversing) the difference in levels between business and property taxation. However, we propose three possible explanations for this deviation between empirical facts and optimal-taxation theory: *(i)* a lack of knowledge of optimal-tax rules among local policy-makers, *(ii)* distributional (equity) concerns where property is taxed low for social-policy reasons, *(iii)* political-economy arguments in which politicians seek to maintain office, or a combination of these three points. These explanations for lower property taxes could offset the negative welfare effects that stem from non-compliance with the inverse-elasticity rule.

3.7 Appendix

Table 3.5: Number of observations by year, 1995-2010

Year	N
1995	8980
1996	8964
1997	8956
1998	9507
1999	10522
2000	10551
2001	12363
2002	11890
2003	11622
2004	11654
2005	11693
2006	11874
2007	11935
2008	11605
2009	11470
2010	11084
Total	174670

Notes: Number of municipalities in each year for which data are available. Administrative data on the universe of German municipalities. Baseline sample: Years 1995 to 2010. Restricted to observations for which information on BT and PT were available. Observation excluded if revenue is reported negative.

Table 3.6: Summary statistics by year, BT, 1995-2010

Year	Statistics	Revenue BT	Revenue BT (log)	Multiplier BT	Multiplier BT (log)
1995	Mean	2311.504	4.789	311.508	5.734
	Std. Dev.	21598.333	2.574	35.831	0.120
1996	Mean	2517.189	4.840	313.197	5.740
	Std. Dev.	23946.445	2.573	35.473	0.117
1997	Mean	2666.805	4.898	314.575	5.745
	Std. Dev.	25103.648	2.567	35.663	0.117
1998	Mean	2615.059	4.753	314.179	5.743
	Std. Dev.	28113.074	2.564	36.638	0.121
1999	Mean	2473.443	4.467	312.369	5.736
	Std. Dev.	27460.430	2.738	37.969	0.129
2000	Mean	2498.550	4.542	312.806	5.737
	Std. Dev.	27183.304	2.643	38.460	0.131
2001	Mean	1980.573	4.571	322.796	5.770
	Std. Dev.	21136.883	2.463	37.065	0.120
2002	Mean	1973.761	4.653	325.167	5.778
	Std. Dev.	20979.255	2.440	36.693	0.120
2003	Mean	2063.060	4.766	327.932	5.786
	Std. Dev.	21187.696	2.430	37.039	0.119
2004	Mean	2433.847	4.937	329.102	5.790
	Std. Dev.	25367.947	2.402	36.802	0.116
2005	Mean	2725.474	5.015	330.980	5.796
	Std. Dev.	29118.591	2.439	36.948	0.114
2006	Mean	3209.349	5.069	332.403	5.800
	Std. Dev.	35787.475	2.475	36.694	0.113
2007	Mean	3360.294	5.174	333.072	5.802
	Std. Dev.	36408.183	2.484	36.567	0.112
2008	Mean	3460.794	5.270	333.159	5.803
	Std. Dev.	35740.545	2.453	36.114	0.110
2009	Mean	2827.814	5.233	334.865	5.808
	Std. Dev.	28937.175	2.318	36.271	0.110
2010	Mean	3223.486	5.485	338.181	5.818
	Std. Dev.	32704.606	2.266	35.769	0.107
Total	Mean	2651.616	4.910	324.885	5.776
	Std. Dev.	28204.096	2.501	37.755	0.121

Notes: Summary statistics for the business-tax (BT) multiplier (in %) and revenue (in 1,000 euro) by year. Municipal level variables. Administrative data on the universe of German municipalities. Baseline sample: Years 1995 to 2010. Restricted to observations for which information on BT and PT were available. Observation excluded if revenue is reported negative.

Table 3.7: Summary statistics by year, PT, 1995-2010

Year	Statistics	Revenue PT	Revenue PT (log)	Multiplier PT	Multiplier PT (log)
1995	Mean	699.414	4.535	291.834	5.665
	Std. Dev.	6605.621	1.756	43.523	0.150
1996	Mean	750.503	4.654	294.460	5.674
	Std. Dev.	7002.196	1.723	43.632	0.148
1997	Mean	793.135	4.743	297.729	5.685
	Std. Dev.	7268.107	1.706	44.344	0.149
1998	Mean	780.040	4.741	300.949	5.697
	Std. Dev.	7321.814	1.678	43.122	0.144
1999	Mean	739.522	4.588	302.623	5.703
	Std. Dev.	7252.905	1.732	42.203	0.140
2000	Mean	767.201	4.631	303.771	5.707
	Std. Dev.	7346.548	1.733	42.409	0.140
2001	Mean	700.672	4.664	308.354	5.722
	Std. Dev.	6943.614	1.674	41.820	0.136
2002	Mean	741.952	4.779	310.991	5.731
	Std. Dev.	7337.267	1.641	42.527	0.136
2003	Mean	788.204	4.818	314.718	5.742
	Std. Dev.	7785.709	1.661	44.213	0.140
2004	Mean	813.757	4.867	317.981	5.752
	Std. Dev.	7922.311	1.658	44.740	0.140
2005	Mean	837.927	4.891	321.609	5.764
	Std. Dev.	8197.625	1.662	45.198	0.139
2006	Mean	838.376	4.876	323.611	5.770
	Std. Dev.	8124.914	1.681	45.443	0.140
2007	Mean	864.923	4.893	324.894	5.774
	Std. Dev.	9090.562	1.691	45.945	0.142
2008	Mean	880.519	4.930	324.814	5.774
	Std. Dev.	9173.143	1.674	45.347	0.138
2009	Mean	918.787	4.986	326.247	5.778
	Std. Dev.	9487.170	1.678	46.421	0.141
2010	Mean	986.964	5.064	329.297	5.787
	Std. Dev.	9950.598	1.696	47.874	0.143
Total	Mean	808.975	4.799	313.119	5.736
	Std. Dev.	8033.088	1.694	45.870	0.146

Notes: Summary statistics for the property-tax (PT) multiplier (in %) and revenue (in 1,000 euro) by year. Municipal level variables. Administrative data on the universe of German municipalities. Baseline sample: Years 1995 to 2010. Restricted to observations for which information on BT and PT were available. Observation excluded if revenue is reported negative.

Table 3.8: Summary statistics by year, control variables, 1995-2010

Year	Statistics	GDP	Population	Population (sq)	Debt	Private income	Employees
1995	Mean	3433375.467	8.161	2738.396	154590.008	2559801.562	3392.857
	Std. Dev.	2992882.061	51.692	133011.641	134945.719	1708455.063	19784.457
	N	7938	8938	8938	8957.000	8008	5971
1996	Mean	3510728.703	8.202	2726.095	158206.215	2640317.645	3364.361
	Std. Dev.	3018219.672	51.567	132198.592	135206.793	1738892.703	19582.060
	N	7944	8922	8922	8941	7942	5961
1997	Mean	3614216.533	8.222	2690.078	160783.372	2711941.942	2762.070
	Std. Dev.	3092953.040	51.213	129854.608	132244.958	1786663.019	20632.247
	N	7901	8914	8914	8933	7899	8845
1998	Mean	3552825.841	7.673	2483.495	161337.703	2658979.930	2775.957
	Std. Dev.	3148427.497	49.243	124107.002	129722.133	1810624.593	20605.496
	N	8492	9465	9465	8774	8490	8634
1999	Mean	3527194.437	7.019	2244.355	156819.311	2659090.049	2508.717
	Std. Dev.	3113521.643	46.854	117236.048	116646.174	1803386.496	19448.556
	N	9286	10480	10480	10517	9284	9968
2000	Mean	3650911.875	7.130	2292.638	156161.253	2736173.901	2573.133
	Std. Dev.	3252803.952	47.350	116967.243	116836.173	1860942.762	19948.901
	N	9315	10509	10509	10546	9313	9852
2001	Mean	3488908.559	6.578	1936.406	151317.177	2701554.879	2306.019
	Std. Dev.	3139471.379	43.512	108303.265	106505.708	1829176.586	18421.301
	N	11257	12363	12363	12358	11255	11758
2002	Mean	3578562.150	6.837	2017.697	153618.009	2755024.182	2348.554
	Std. Dev.	3180820.932	44.397	110712.973	108458.270	1864328.874	15771.237
	N	10864	11890	11890	11885	10862	11140
2003	Mean	3632078.053	6.929	2064.488	158100.160	2818249.333	2448.032
	Std. Dev.	3288927.110	44.907	111824.507	112820.871	1935913.382	18679.377
	N	10558	11622	11622	11617	10556	10798
2004	Mean	3716333.846	6.966	2061.225	158834.803	2867310.957	2413.446
	Std. Dev.	3312489.813	44.865	111649.361	116399.798	1959544.225	18286.146
	N	10591	11654	11654	11649	10589	10839
2005	Mean	3756930.674	6.951	2067.278	158908.250	2918179.425	2329.475
	Std. Dev.	3420770.792	44.935	112029.001	117897.918	2008268.102	17851.621
	N	10681	11693	11693	11688	10679	11100
2006	Mean	3873972.221	6.848	2053.458	157288.312	2963959.723	2330.555
	Std. Dev.	3529823.499	44.797	111904.650	117044.792	2049399.469	17934.164
	N	10894	11874	11874	11869	10892	11173
2007	Mean	4059444.476	6.862	2064.035	160899.491	3016112.699	2414.218
	Std. Dev.	3595740.818	44.912	112547.357	116166.022	2040731.186	18383.795
	N	11433	11935	11935	11930	11431	11063
2008	Mean	4320020.496	6.941	2131.338	160290.078	3179064.384	2403.170
	Std. Dev.	3721436.981	45.644	115119.503	114832.111	2100182.080	18719.445
	N	10789	11605	11605	11600.	11602	10992
2009	Mean	4190919.295	7.091	2171.336	155181.566	3195175.741	2485.998
	Std. Dev.	3580753.216	46.057	116519.711	123711.417	2100534.889	19219.118
	N	10688	11470	11470	11466	11468	10866
2010	Mean	4203950.315	7.359	2273.719	.	.	2832.661
	Std. Dev.	3669922.485	47.114	119863.577	.	.	20537.384
	N	9909	11084.000	11084	0	0	9740
Total	Mean	3773758.170	7.183	2223.819	157390.836	2845050.660	2549.232
	Std. Dev.	3354230.616	46.607	117169.293	119370.435	1934041.044	18909.947
	N	158540	174418	174418	162730	150270	158700

Notes: Summary statistics for control variables by year. Population (in 1,000) and employees are municipal-level variables. GDP, debt and private income (in 1,000 euro) are county-level variables. Administrative data on the universe of German municipalities. Baseline sample: Years 1995 to 2010. Restricted to observations for which information on BT and PT were available. Observation excluded if revenue is reported negative.

Table 3.9: Summary statistics by state, BT, 1995-2010

State	Statistics	Revenue BT	Revenue BT (log)	Multiplier BT	Multiplier BT (log)
Schleswig-Holstein	Mean	745.956	3.904	307.727	5.726
	Std. Dev.	4780.849	2.132	25.273	0.084
	N	16900	16900	16900	16900
Hamburg	Mean	1453423.938	14.168	468.750	6.150
	Std. Dev.	315091.655	0.212	5.000	0.011
	N	16	16	16	16
Lower Saxony	Mean	2409.766	5.616	328.742	5.791
	Std. Dev.	13799.286	2.073	31.028	0.093
	N	16095	16095	16095	16095
Bremen	Mean	153736.063	11.391	406.250	6.005
	Std. Dev.	132640.105	1.161	24.984	0.062
	N	32	32	32	32
North Rhine-Westphalia	Mean	18935.870	8.819	402.546	5.995
	Std. Dev.	60610.971	1.272	29.995	0.076
	N	6326	6326	6326	6326
Hesse	Mean	7129.824	7.101	323.426	5.775
	Std. Dev.	58395.945	1.534	31.129	0.092
	N	6785	6785	6785	6785
Rhineland-Palatinate	Mean	799.836	3.780	348.266	5.852
	Std. Dev.	7028.456	2.101	16.163	0.045
	N	20887	20887	20887	20887
Baden-Württemberg	Mean	4000.077	6.737	337.275	5.820
	Std. Dev.	19726.553	1.752	15.688	0.046
	N	17572	17572	17572	17572
Bavaria	Mean	2424.219	5.964	323.094	5.775
	Std. Dev.	29041.222	1.630	24.409	0.073
	N	32562	32562	32562	32562
Saarland	Mean	6513.212	7.737	387.417	5.957
	Std. Dev.	14464.098	1.336	28.487	0.074
	N	518	518	518	518
Berlin	Mean	917270.500	13.711	396.250	5.979
	Std. Dev.	187300.175	0.194	31.385	0.088
	N	16	16	16	16
Brandenburg	Mean	459.642	3.203	298.419	5.688
	Std. Dev.	2346.693	2.468	39.828	0.147
	N	11357	11357	11357	11357
Mecklenburg-Vorpommern	Mean	318.373	3.212	286.741	5.649
	Std. Dev.	2070.543	2.037	37.871	0.142
	N	11060	11060	11060	11060
Saxony	Mean	2224.769	5.864	379.783	5.938
	Std. Dev.	12614.080	1.671	21.139	0.056
	N	4975	4975	4975	4975
Saxony-Anhalt	Mean	403.616	3.275	310.850	5.730
	Std. Dev.	2621.768	2.117	40.703	0.141
	N	15653	15653	15653	15653
Thuringia	Mean	382.072	3.496	305.438	5.717
	Std. Dev.	2212.053	2.157	28.279	0.098
	N	13916	13916	13916	13916
Total	Mean	2651.616	4.910	324.885	5.776
	Std. Dev.	28204.096	2.501	37.755	0.121
	N	174670	174670	174670	174670

Notes: Summary statistics for the business-tax (BT) multiplier (in %) and revenue (in 1,000 euro). Municipal level variables. Administrative data on the universe of German municipalities. Baseline sample: Years 1995 to 2010. Restricted to observations for which information on BT and PT were available. Observation excluded if revenue is reported negative.

Table 3.10: Summary statistics by state, PT, 1995-2010

State	Statistics	Revenue PT	Revenue PT (log)	Multiplier PT	Multiplier PT (log)
Schleswig-Holstein	Mean	261.172	4.027	262.492	5.560
	Std. Dev.	1409.915	1.475	35.796	0.146
	N	16900	16900	16900	16900
Hamburg	Mean	342835.938	12.734	508.125	6.229
	Std. Dev.	51732.922	0.153	28.570	0.057
	N	16	16	16	16
Lower Saxony	Mean	915.927	5.470	323.970	5.771
	Std. Dev.	4231.042	1.464	45.563	0.139
	N	16095	16095	16095	16095
Bremen	Mean	66628.938	10.774	535.625	6.282
	Std. Dev.	47974.725	0.873	26.873	0.050
	N	32	32	32	32
North Rhine-Westphalia	Mean	5651.991	7.788	361.174	5.878
	Std. Dev.	14609.484	1.091	54.834	0.154
	N	6326	6326	6326	6326
Hesse	Mean	1488.887	6.249	253.259	5.520
	Std. Dev.	8584.838	1.090	44.141	0.166
	N	6785	6785	6785	6785
Rhineland-Palatinate	Mean	226.609	3.854	317.264	5.758
	Std. Dev.	1295.508	1.430	17.584	0.058
	N	20887	20887	20887	20887
Baden-Württemberg	Mean	1096.189	5.993	310.575	5.730
	Std. Dev.	4431.207	1.279	41.337	0.129
	N	17572	17572	17572	17572
Bavaria	Mean	616.879	5.323	321.709	5.763
	Std. Dev.	5531.124	1.130	49.290	0.144
	N	32562	32562	32562	32562
Saarland	Mean	1953.108	7.043	296.236	5.686
	Std. Dev.	4201.607	0.818	30.477	0.096
	N	518	518	518	518
Berlin	Mean	561261.625	13.217	671.250	6.502
	Std. Dev.	118161.662	0.210	86.862	0.124
	N	16	16	16	16
Brandenburg	Mean	222.674	3.913	317.220	5.755
	Std. Dev.	718.923	1.562	31.716	0.094
	N	11357	11357	11357	11357
Mecklenburg-Vorpommern	Mean	172.862	3.801	310.819	5.737
	Std. Dev.	887.033	1.225	23.489	0.072
	N	11060	11060	11060	11060
Saxony	Mean	989.607	5.691	381.666	5.941
	Std. Dev.	5071.448	1.103	34.597	0.086
	N	4975	4975	4975	4975
Saxony-Anhalt	Mean	181.519	3.776	323.926	5.775
	Std. Dev.	1085.329	1.281	33.473	0.100
	N	15653	15653	15653	15653
Thuringia	Mean	172.063	3.818	307.587	5.727
	Std. Dev.	804.378	1.335	20.247	0.064
	N	13916	13916	13916	13916
Total	Mean	808.975	4.799	313.119	5.736
	Std. Dev.	8033.088	1.694	45.870	0.146
	N	174670	174670	174670	174670

Notes: Summary statistics for the property-tax (PT) multiplier (in %) and revenue (in 1,000 euro) by year. Municipal level variables. Administrative data on the universe of German municipalities. Baseline sample: Years 1995 to 2010. Restricted to observations for which information on BT and PT were available. Observation excluded if revenue is reported negative.

Table 3.11: Summary statistics by state, control variables, 1995-2010

State	Statistics	GDP	Population	Population (sq)	Debt	Private income	Employees
Schleswig-Holstein	Mean	4151959.319	2.629	136.084	141758.697	3233055.944	824.533
	Std. Dev.	1411577.813	11.366	2244.043	45722.093	1062259.438	4818.204
	N	16900	16900	16900	15844	15844	15503
Hamburg	Mean	77652983.813	1735.383	3012321.516	.	.	761869.500
	Std. Dev.	8631321.696	28.618	99654.676	.	.	27185.624
	N	16	16	16	0	0	16
Hamburg	Mean	4147178.013	7.851	559.628	205605.520	3173531.716	2189.902
	Std. Dev.	4778508.583	22.316	8571.394	221902.167	2661481.347	9222.710
	N	16095	15971	15971	15083	15083	15795
Bremen	Mean	11783102.906	332.674	155980.600	.	6382527.533	140741.125
	Std. Dev.	8699017.505	216.264	143875.283	.	4470902.396	97590.495
	N	32	32	32	0	30	32
North Rhine-Westphalia	Mean	7897634.143	45.466	9674.618	394951.670	6246073.520	14544.072
	Std. Dev.	3483560.578	87.228	59297.087	237495.983	2312022.953	36759.705
	N	5910	6326	6326	5918	5918	6315
Hesse	Mean	5608189.936	14.257	1581.257	297602.351	3963672.880	5034.391
	Std. Dev.	3056734.695	37.124	21033.399	170384.191	1549557.950	24805.684
	N	6785	6785	6785	6359	6359	6785
Rhineland-Palatinate	Mean	2587942.122	2.193	98.422	131467.494	2203686.899	859.843
	Std. Dev.	1168709.906	9.676	1366.475	53722.009	849730.922	5368.050
	N	20887	20887	20887	18687	18687	16986
Baden-Württemberg	Mean	7000051.079	9.658	759.300	164742.411	4909596.943	3454.437
	Std. Dev.	3678634.304	25.808	11330.876	80415.282	2493096.545	14132.474
	N	17572	17513	17513	16479	16479	17436
Bavaria	Mean	3173542.509	6.034	1030.700	114760.582	2245023.083	2131.064
	Std. Dev.	2947775.516	31.533	35588.741	73209.247	1116155.683	16828.717
	N	32562	32562	32562	30515	30515	32533
Saarland	Mean	5031352.753	19.800	937.644	166496.749	3236104.318	6496.625
	Std. Dev.	3293402.906	23.381	4180.278	80139.137	1365070.208	14191.731
	N	518	518	518	466	466	518
Berlin	Mean	82571546.750	3414.457	11659372.688	.	.	1091742.923
	Std. Dev.	7345400.529	30.175	206653.771	.	.	48429.357
	N	16	16	16	0	0	13
Brandenburg	Mean	2551389.586	2.967	76.613	117349.308	2137335.396	995.508
	Std. Dev.	648835.130	8.235	759.802	32190.568	532045.352	3579.122
	N	11357	11357	11357	10942	10942	8561
Mecklenburg-Vorpommern	Mean	1561827.773	2.178	100.252	137164.534	1406583.310	703.014
	Std. Dev.	343133.316	9.773	1409.169	23460.862	305885.368	4033.748
	N	8690	11024	11024	9495	10277	9429
Saxony	Mean	3245178.340	10.577	2041.929	157940.584	2532636.486	3271.035
	Std. Dev.	1881977.352	43.937	20273.134	78572.691	1311561.468	16876.904
	N	4474	4975	4975	4492	3991	4946
Saxony-Anhalt	Mean	3307163.489	2.539	153.559	144848.383	2533005.833	862.667
	Std. Dev.	1037183.168	12.129	2665.248	51927.618	675492.733	5287.916
	N	2882	15653	15653	15436	2665	12568
Thuringia	Mean	1741294.057	2.437	74.130	134660.253	1480964.214	940.747
	Std. Dev.	500440.343	8.258	1187.627	33156.792	347946.319	4600.958
	N	13844	13883	13883	13014	13014	11264
Total	Mean	3773758.170	7.183	2223.819	157390.836	2845050.660	2549.232
	Std. Dev.	3354230.616	46.607	117169.293	119370.435	1934041.044	18909.947
	N	158540	174418	174418	162730	150270	158700

Notes: Population (in 1,000) and employees are municipal level variables. GDP, debt and private income (in 1,000 euro) are county level variables. Administrative data on the universe of German municipalities. Baseline sample: Years 1995 to 2010. Restricted to observations for which information on BT and PT were available. Observation excluded if revenue is reported negative.

Table 3.12: Size of changes in BT multipliers, 1995-2010

Year	I	II	III	IV	V	VI	VII	VIII	IX
1995	0.03	0.09	0.50	0.10	85.60	5.35	7.85	0.46	0.03
1996	0.04	0.14	0.43	0.18	91.55	2.82	4.45	0.38	0.01
1997	0.04	0.15	0.33	0.20	91.17	2.33	5.40	0.32	0.05
1998	0.01	0.13	0.28	0.19	91.04	3.16	4.75	0.39	0.05
1999	0.06	0.14	0.48	0.25	95.37	1.37	2.14	0.18	0.00
2000	0.07	0.16	0.43	0.28	94.98	1.61	2.21	0.24	0.02
2001	0.01	0.10	0.44	0.30	91.99	3.65	3.11	0.34	0.06
2002	0.04	0.11	0.27	0.24	92.04	2.98	4.04	0.28	0.01
2003	0.03	0.04	0.22	0.30	90.31	2.72	6.14	0.22	0.02
2004	0.04	0.07	0.30	0.24	91.19	3.06	4.84	0.22	0.05
2005	0.02	0.07	0.33	0.28	88.88	4.16	5.96	0.29	0.01
2006	0.03	0.04	0.42	0.37	91.80	3.91	3.12	0.25	0.06
2007	0.02	0.04	0.40	0.38	95.50	1.69	1.78	0.15	0.04
2008	0.00	0.09	0.45	0.32	95.71	1.12	1.88	0.40	0.03
2009	0.00	0.05	0.57	0.50	95.16	1.11	2.28	0.30	0.03
2010	0.03	0.04	0.30	0.48	90.21	2.29	5.82	0.80	0.04
Total	0.03	0.09	0.38	0.30	92.18	2.65	4.02	0.32	0.03

Notes: Share of municipalities which change their business-tax multiplier in the range of the following nine categories, I - IX, by year. I: < -100; II: $-100 \leq$ change < -50; III: $-50 \leq$ change < -10; IV : $-10 \leq$ change < 0; V: no change; VI: $0 <$ change ≤ 10; VII: $10 <$ change ≤ 50; VIII: $50 <$ change ≤ 100; IX: change> 100. Municipal level variable. Administrative data on the universe of German municipalities. Baseline sample: Years 1995 to 2010. Restricted to observations for which information on BT and PT were available. Observation excluded if revenue is reported negative.

Table 3.13: Size of changes in PT multipliers, 1995-2010

Year	I	II	III	IV	V	VI	VII	VIII	IX
1995	0.00	0.10	0.22	0.04	81.03	3.44	13.99	1.07	0.10
1996	0.00	0.11	0.20	0.10	89.40	2.15	7.49	0.56	0.00
1997	0.00	0.04	0.24	0.21	87.06	2.56	9.09	0.80	0.01
1998	0.01	0.05	0.20	0.14	87.60	3.03	8.35	0.56	0.05
1999	0.01	0.04	0.21	0.16	93.75	1.57	4.05	0.18	0.03
2000	0.00	0.07	0.34	0.13	94.05	1.37	3.90	0.14	0.01
2001	0.00	0.26	0.28	0.20	91.70	3.45	3.82	0.25	0.02
2002	0.01	0.02	0.15	0.12	90.03	2.35	6.62	0.66	0.04
2003	0.01	0.03	0.12	0.07	86.65	2.17	8.72	2.15	0.07
2004	0.00	0.00	0.15	0.13	86.74	2.57	9.62	0.79	0.02
2005	0.00	0.03	0.13	0.15	85.02	3.53	10.34	0.77	0.04
2006	0.05	0.04	0.13	0.19	90.02	2.62	6.38	0.50	0.07
2007	0.01	0.03	0.33	0.23	94.30	1.28	3.51	0.28	0.03
2008	0.01	0.08	0.53	0.30	95.00	1.20	2.68	0.18	0.01
2009	0.01	0.11	0.51	0.44	94.93	1.00	2.80	0.19	0.03
2010	0.02	0.05	0.26	0.51	87.12	2.16	8.64	1.11	0.12
Total	0.01	0.06	0.25	0.20	89.86	2.23	6.71	0.64	0.04

Notes: Share of municipalities which change their property-tax multiplier in the range of the following nine categories, I - IX, by year. I: < -100; II: $-100 \leq$ change < -50; III: $-50 \leq$ change < -10; IV : $-10 \leq$ change < 0; V: no change; VI: $0 <$ change ≤ 10; VII: $10 <$ change ≤ 50; VIII: $50 <$ change ≤ 100; IX: change> 100. Municipal level variable. Administrative data on the universe of German municipalities. Baseline sample: Years 1995 to 2010. Restricted to observations for which information on BT and PT were available. Observation excluded if revenue is reported negative.

Table 3.14: Robustness checks

	(I) Tax revenue business	(II)	(III) Tax revenue property	(IV)
***Panel A**: Balanced Panel*				
m^{BT}	0.0949	-0.0225		
	(0.126)	(0.132)		
m^{PT}			0.878***	0.890***
			(0.0117)	(0.0116)
Observations	79,238	73,172	79,238	73,172
R^2	0.209	0.178	0.802	0.801
***Panel B**: Longer panel, less federal states*				
m^{BT}	-0.106	/		
	(0.108)	/		
m^{PT}			0.774***	/
			(0.0224)	/
Observations	92,631	/	92,631	/
R^2	0.242	/	0.905	/
***Panel C**: Tax rate lagged 1 year*				
m^{BT}_{t-1}	0.0897	0.0446		
	(0.0903)	(0.0931)		
m^{PT}_{t-1}			0.659***	0.656***
			(0.0119)	(0.0117)
Observations	138,714	122,370	138,714	122,370
R^2	0.150	0.140	0.635	0.637
***Panel D**: Tax rate lagged 2 years*				
m^{BT}_{t-2}	0.0986	0.0728		
	(0.0954)	(0.0997)		
m^{PT}_{t-2}			0.467***	0.454***
			(0.0143)	(0.0143)
Observations	124,568	110,743	124,568	110,743
R^2	0.146	0.139	0.584	0.574

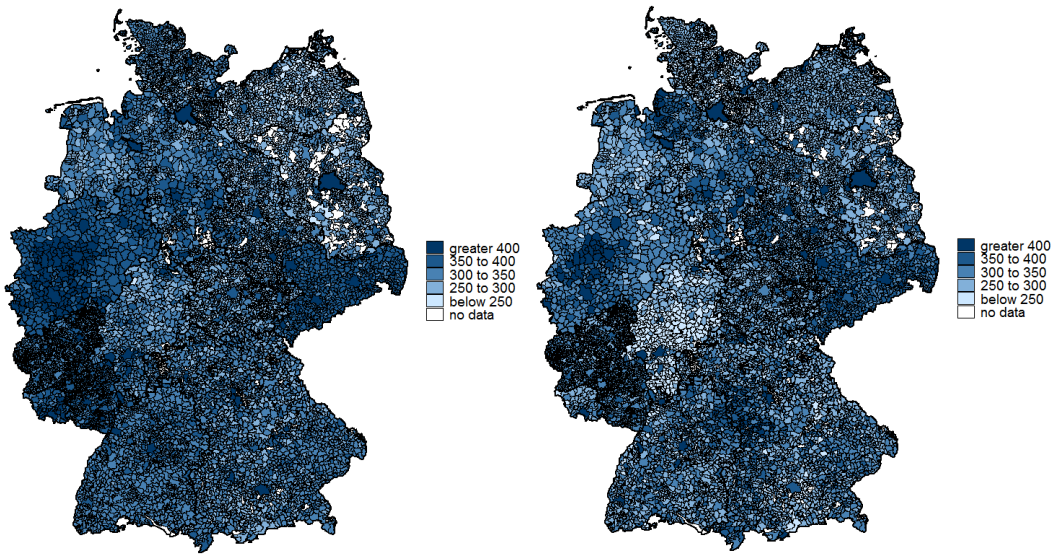
Notes: Fixed-effects regressions based on Equation (3.1) with standard errors in parentheses. Standard errors clustered on the municipality level. Significance levels are * < 0.10, ** < 0.05, *** < 0.01. Administrative data on the universe of German municipalities. Years in baseline: 1995 to 2010. The dependent variable in Columns (I) and (II) is municipality-level tax revenue generated from the local business tax (BT), and municipality-level tax revenue from the local property tax (PT) in Columns (III) and (IV). The independent variable of interest is the municipality-level tax multiplier. Reported coefficients can be interpreted as elasticities identified from tax reforms. All specifications include year fixed effects, municipality fixed effects, and federal-state specific fixed effects. Columns (I) and (III) include controls for population and GDP. Columns (II) and (IV) additionally add controls for the level of debt, number of employees, and private income. Panel A is identical to the baseline results in Table 3.3 but estimated on a balanced sample of municipalities. Panel B is for three federal states for which data are available between 1983 and 2010, and uses this long time span. Panel C (Panel D) is on the same unbalanced panel of municipalities as the baseline but uses the tax rate lagged one (two) year(s).

Table 3.15: Share of municipalities with higher rates on business profits than on property by year, 1995-2010

Year	Mean
1995	0.567
1996	0.565
1997	0.555
1998	0.513
1999	0.472
2000	0.468
2001	0.574
2002	0.582
2003	0.582
2004	0.562
2005	0.546
2006	0.539
2007	0.534
2008	0.532
2009	0.537
2010	0.547
Total	0.543

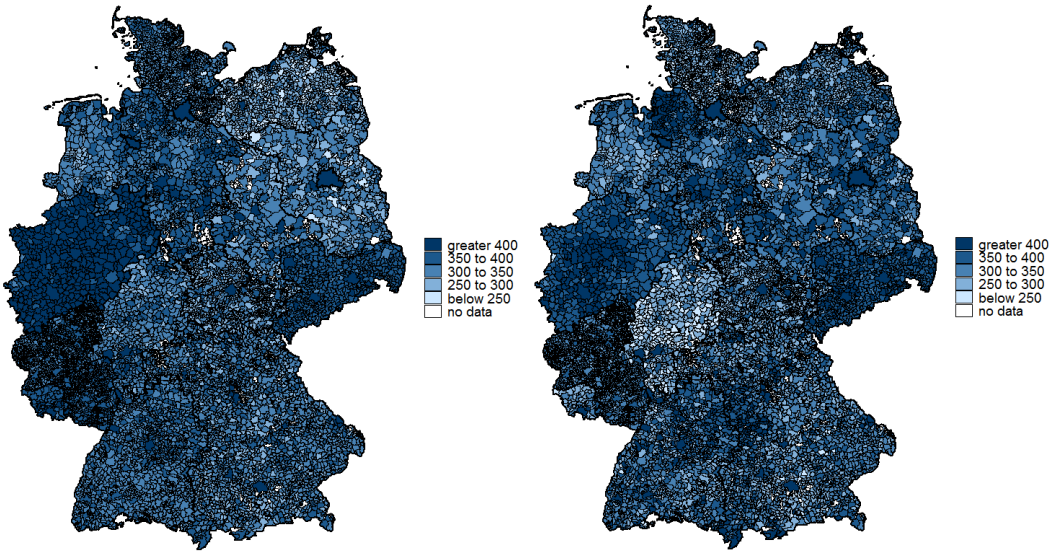
Notes: The table depicts for each year the share of municipalities that have a higher multiplier on business-profits than on propety. Municipal level variables. Administrative data on the universe of German municipalities. Baseline sample: Years 1995 to 2010. Restricted to observations for which information on BT and PT were available. Observation excluded if revenue is reported negative.

Figure 3.2: BT (left) and PT (right) multipliers in 2001



Notes: The figure depicts the spatial distribution and levels of business-tax (left) and property-tax (right) multipliers in 2001. Own compilation with geoinformation provided by the GeoDataCenter (Federal Agency for Cartography and Geodesy).

Figure 3.3: BT (left) and PT (right) multipliers in 2010



Notes: The figure depicts the spatial distribution and levels of business-tax (left) and property-tax (right) multipliers in 2010. Own compilation with geoinformation provided by the GeoDataCenter (Federal Agency for Cartography and Geodesy).

Chapter 4

Common pool problems and territorial reforms

4.1 Introduction

The common pool problem is a well-known and widely studied phenomenon among public economists. A common pool occurs if an individual (or a smaller group) is the beneficiary of a common resource, while the costs involved are paid collectively. According to the "tragedy of the commons" (Hardin 1968), this situation results in exploitation and overuse of the resource. A common pool may arise in many contexts and forms – the most prominent case certainly is the example of fishery (e.g., Gordon 1954; Scott 1955). It also manifests itself in the political environment (see Tullock 1959; Buchanan et al. 1967).

This paper applies the notion of the common pool problem to the event of a municipal territorial reform: Municipalities being designated for the same merger or incorporation form a temporary common pool with respect to debt during the time window between the announcement and actual execution of a reform. The reason for the formation of a common pool is that the municipalities generally still have the autonomy to take on debt individually. Responsible for repayment, however, are not the individual municipalities themselves, but the newly founded municipality they become part of. Conventional economic theory suggests that members of a common pool behave opportunistically. This implies that the municipalities affected by the reform strategically take on debt to finance (inefficient) public projects.[1] The motives explaining the political decision-making process

[1]There is anecdotal evidence implying that in view of a municipal territorial reform (some) municipal-

driving the opportunistic behavior are complex: foreseeable loss of political power, municipal independence and future accountability. Politicians might act in their private interest, e.g., pursue flagship projects associated with the politician's name or in the interest of the municipality.[2]

The previous applied literature on common pool problems in territorial reforms generally employs a formalization of the common pool theory derived by Weingast et al. (1981) as a theoretical starting point for the analysis (most importantly for this research Hinnerich [2009], Jordahl and Liang [2010] and Saarimaa and Tukiainen [2015]).[3] Broadly interpreted, the so-called "law 1/n" proposes that the size of the incentive effect is greater, the smaller the municipality's own size relative to the size of its common pool. However, in the aftermath of Hardin (1968) a growing body of interdisciplinary research on the "commons" has successfully challenged the general validity of Hardin's pessimistic view (see, e.g., Ostrom et al. [2002] for a survey). In this spirit one can propose that there might be other (implicit) mechanisms, mitigating or even preventing free-riding,[4] which are not covered by the "law 1/n." This is why this paper does not only examine if being in a common pool triggers an incentive to free-ride (Hypothesis 1a) and if the size of the effect is greater, the smaller the municipality's own size relative to its common pool (Hypothesis 1b). It rather broadens the horizon and explicitly addresses three fur-

ities affected engage in public projects, for instance, building a public swimming pool or playground, they would not have engaged in otherwise. As my concluding remarks point out, whether or not these projects are efficient or inefficient from a welfare perspective can only be determined by deeper analysis.

[2]Analyzing and modeling the political decision making process would go beyond the scope of this paper. It must remain open for further research (note that Bruns et al. [2015] provide an interesting study on the determinants of merging decisions for the German federal state of Brandenburg). See Section 4.2.2 for more information on the impact of the reform on the politicians at the local level.

[3]In a model of distributive policy Weingast et al. (1981) derive, among others, that the inefficiency of public projects rises with the number of decision makers n (districts). The related literature commonly refers to this as "law 1/n." Applied to the case of a municipal territorial reform, this would suggest that the incentive to free-ride is increasing in the number of municipalities being part of the common pool. However, for the event of a territorial reform it seems to be more adequate to interpret the "law 1/n" more broadly by referring to the size of a municipality relative to the size of its common pool (note that this line of argument was also put forward by Hinnerich [2009] and Jordahl and Liang [2010], among others). Note that Hansen (2014) advocates the original formulation of the "law 1/n." I take this into account by testing some specifications which make direct use of the original formulation (see coalition size C_i in Section 4.3).

Note that there is a sizable related empirical literature focusing on the validity of the "law 1/n" with regard to the impact of legislature size on spending. The results are inconclusive (e.g., Bradbury and Crain 2001; Gilligan and Matsusaka 2001; Pettersson-Lidbom 2012).

[4]This strand of literature understands "free-riding" as a synonym for "behaving opportunistically by exploiting (the other members of) the common pool."

ther questions: Do municipalities which remain accountable after the execution of the reform engage in free-riding at all? Do municipalities which identify more with a municipal territorial reform free-ride less? Does timing affect the free-riding behavior?[5] First, I suggest that municipalities (and their politicians) considered accountable (for handling public debt and infrastructure) after the reform in public perception will not engage in free-riding (Hypothesis 2a). This applies to municipalities which are only absorbing or keep their name when merging.[6] Second, the more the members of the common pool identify with the amalgamation, the lower, presumably, the incentive to behave opportunistically is. Consequently, voluntary mergers or incorporations might be less affected by opportunistic behavior (Hypothesis 2b). Third, it seems reasonable to assume that the timing of the merger or incorporation has an effect on free-riding: The bigger the time window between announcement and execution, the more time the municipalities have to coordinate opportunistic behavior and exploit the common pool (see Hypothesis 3).

Against this backdrop there is demand for empirical clarification. Econometric research on this topic has evolved only recently.[7] I am among the first to empirically study free-riding on public debt before the execution of a territorial reform. Closest to my paper are the two papers on boundary reforms in Sweden (Hinnerich 2009; Jordahl and Liang 2010) and a recent Finnish paper (Saarimaa and Tukiainen 2015). Besides that, there are the papers by Blom-Hansen (2010), Hansen (2014) and Nakazawa (2013) with a somewhat different angle.[8] Common to all previous papers is that they rely on a difference-in-

[5]One further aspect is also of importance: To behave opportunistically is not sustainable in the long run. Thus, municipalities forming a common pool might not engage in free-riding if all members (or rather the respective politicians) convincingly commit themselves to not engaging in it. Unfortunately, this effect is not measurable in my empirical design.

[6]Note that when a municipality stays in existence by keeping its name, this does not per se guarantee that its politicians remain in power after the reform (see Section 4.2.2). However, it outlasts the reform and its politicians continue to be associated with its performance.

[7]There is another branch of empirical research on municipal territorial reforms. This branch examines if they bring economic benefits after amalgamation through taking advantages of economies of scale by creating larger municipalities. This is of importance, because many countries around the world (Australia, Belgium, Canada, Denmark, Germany, Great Britain, New Zealand and Norway to name but a few) initiated large scale territorial reforms in the last decades to increase municipality size with the objective of saving costs and increasing efficiency. Empirical evidence on their success is inconclusive with most studies lacking a convincing empirical strategy (noteworthy exceptions are, e.g., Reingewertz [2012] and Hansen et al. [2014]).

[8]Blom-Hansen (2010) studies the common pool problem with a focus on (capital) budget overruns for the case of the Danish local government reform in 2007. What makes his research interesting is that a number of spending restrictions were applied prior to the reform. His estimates are derived in a cross-section environment. He reports overspending due to free-riding for capital budgets as well as

differences design (in a broader sense) to identify the causal effect of interest. Besides, all of them also employ a continuous free-ride measure along the lines of the "law 1/n." The concrete operationalization differs.[9] Hinnerich (2009) explores an extensive mandatory boundary reform taking place in the early 1970s in Sweden. His dependent variable is the difference between pre-reform and post-reform debt. His independent variable of interest is the continuous free-ride measure itself (and not an interaction term). He finds statistically and economically significant results in favor of opportunistic behavior. Jordahl and Liang (2010) study a previous municipal amalgamation reform in Sweden (1948 to 1952). They exploit the time dimension of their dataset and use a conventional difference-in-differences design. They are the first distinguishing a discrete effect from the continuous effect (both are applied in form of an interaction term with the treatment period). They find a discrete free-riding effect, which – contrary to the implications of the "law 1/n" – does not depend positively on relative common pool size. Saarimaa and Tukiainen (2015) focus exclusively on voluntary mergers fostered by the Finnish government (in particular through subsidization) between 2007 and 2009. This raises selection issues. Furthermore, their research suffers from a low number of treated municipalities. However, their research is particularly interesting, since it approaches the aforementioned questions. Saarimaa and Tukiainen (2015) also employ a discrete beside a continuous effect and make use of the panel structure in their research design. They find a strong free-riding effect consistent with the "law 1/n" for debt and the spending of cash reserves. Furthermore, they report that the additional revenue was mainly spent on investment and current expenditures.

certain areas of current budgets in the year before the reform with the effect not depending on common pool size (despite the restrictions). Hansen (2014) explores the same reform as Blom-Hansen (2010). Her study is the only one that focuses directly on the impact of the number of decision makers in a common pool (operationalized as number of municipalities) on expenditures. She reports a statistically significant positive effect of the availability of a common pool as well as its size in the last year of the treatment period. Nakazawa (2013) studies a large wave of amalgamations after 1999 in Japan with the focus on the relationship between free-riding behavior and regulation on municipal-level debt. Unlike the other countries, in Japan, municipal borrowing is strictly monitored (and restricted) by the government. Nakazawa (2013) finds that regulation, indeed, controls opportunistic behavior.

[9]The prevailing literature generally distinguishes between a discrete and a continuous free-riding effect. The aim is to, on the one hand, tackle whether the availability of a common pool alone incentivizes free-riding and, on the other hand, whether this effect adheres to the suggestions of the "law 1/n." The "discrete effect" refers to a standard dummy variable specification that takes the value 1 if the municipality is affected by the reform and hence part of a common pool and 0 if not. The "continuous effect" refers to specifications with a continuous variable capturing the free-riding effect along the lines of the "law 1/n" (municipality's size relative to the size of its common pool). For the exact specification of the continuous free-ride measure in this paper, see F_i in Section 4.3.

Since prior evidence is mixed and focuses on capturing the implications of the "law 1/n," future research is required. A promising case for a strong difference-in-differences design is the recent municipal territorial reform in Saxony-Anhalt, which between 2007 and 2011 reduced the number of municipalities from 1,039 to 219. This reform has an array of interesting facets: First, German municipalities have a large degree of autonomy. This makes a territorial reform vulnerable to free-riding. Second, the reform was split in a non-mandatory and a mandatory phase. In the former the municipalities had a certain degree of leeway with regard to their merging or incorporation decision. In the latter the state government forced the municipalities to merge or incorporate. This makes it possible to test in a coherent environment if municipalities which identify more with their partners behave less opportunistically. Third, not all municipalities amalgamated at the same time. This allows me to study if the timing of the merger or incorporation affected the size of the effect. Fourth, I am able to capture the accountability mechanism. Besides mergers leading to the foundation of a municipality with a new name (as is commonly the case), there were incorporations as well as mergers where one municipality stayed in existence and kept its name. Last, unlike in other studies the pre-reform and the post-reform period can be clearly separated. This makes the identification of the causal effect reliable.

This study exploits data that has not been used by academic research so far. Retrieving the data was complicated. The statistical office of Saxony-Anhalt in Germany could only provide data (ready for use) on past municipal debt reflecting the latest post-reform territorial status. This makes it unsuitable for my research question. I managed to retrieve the data by digitizing old paper reports. Apart from the vast number of observations, it is the long panel dimension that makes this dataset particularly interesting from an econometric point of view. Thus, I am able to closely investigate the validity of the main identifying assumption (common trend assumption). Furthermore, due to the specific settings of the reform I can perform an array of robustness checks (e.g., alternative control group). This strengthens the econometric design.

This is the first notable study for a non-Scandinavian country exploring the free-riding incentives for the case of a municipal territorial reform. One motivation for this paper is the aim to contribute to the generalization of results. However, of more importance is its aim to broaden the perspective on the underlying incentive structure. My empirical results confirm that being in a common pool, indeed, triggers an incentive to free-ride (Hypothesis 1a). However, the incentive effect is linked to the theoretical predictions based on the "law 1/n." I find a statistically and economically significant free-riding

effect with the incentive to free-ride being greater, the smaller the municipality's own size relative to its common pool (Hypothesis 1b). What distinguishes my paper is that I carefully consider differences in incentive structures by approaching the three questions stated above (Hypotheses 2a and b, 3). I can report a statistically insignificant free-riding effect for the group of surviving municipalities. This confirms the accountability prediction. Furthermore, municipalities which merge or incorporate in the non-mandatory phase behave less opportunistically than municipalities being forced to merge or incorporate in the mandatory phase. Last, I cannot provide evidence in favor of timing (Hypothesis 3).

The remainder of the paper is organized as follows: Section 4.2 gives background information on the institutional framework of German municipalities and describes the municipal territorial reform in the German federal state of Saxony-Anhalt. Section 4.3 presents the hypotheses. Section 4.4 explains the data sources, reports descriptive statistics, outlines the research strategy and provides a discussion on identification and robustness checks. Section 4.5 first explores the relevant graphical evidence and then presents results. Section 4.6 concludes.

4.2 Institutional background

In this section I first introduce the German institutional landscape with a focus on municipalities. Then I present background information on the municipal territorial reform in the federal state of Saxony-Anhalt.

4.2.1 Municipalities in Germany

After the federal government and the states, municipalities are the lowest level of administrative government in Germany. The Basic Law for the Federal Republic of Germany (*Grundgesetz*) grants the municipalities the right to manage their own affairs.[10] This

[10]Most municipalities are affiliated to a county, only a minority is organized as a municipality with county status. The major difference between these two types is that the latter are responsible for municipal tasks (*Gemeindeaufgaben*) and county tasks (*Kreisaufgaben*), while the former are only responsible for municipal tasks (the county is then responsible for county tasks). County-affiliated municipalities can either be independent (*Einheitsgemeinde*), or – depending on the federal state – they might be associated on a below-county level (*Verwaltungsgemeinschaft*, *Amtsgemeinde*, *Samtgemeinde* or *Verbandsgemeinde* to name the most important German terms). With respect to this chapter only two of these lower level layers are of relevance: 1) Association of administration (*Verwaltungsgemeinschaft*): Associations of administration represent a form of intercommunal administrative cooperation. 2) Municipal association

guarantees a high degree of autonomy with regard to municipal task execution and borrowing for municipalities.[11] The local government law of the individual federal states (for Saxony-Anhalt *Kommunalverfassungsgesetz*) defines the concrete legislative framework for the municipalities. The local government law differs between federal states, but it is the same for all municipalities within one federal state. The basic principles, however, are common to all German municipalities. This allows a generalization of the results of this study. All German municipalities are in charge of a variety of tasks for which they enjoy ultimate decision-making authority (voluntary tasks). Consequently, the municipalities are also responsible for a wide array of public infrastructure projects (e.g., public kindergartens or swimming pools). The local government laws allow municipalities to take on debt for investment purposes. They rule out debt for consumption purposes. This, in general, restricts possible opportunistic behavior materializing in excessive borrowing to investment expenditures.[12]

4.2.2 Municipal territorial reform

Background.[13] Before the municipal territorial reform between 2007 and 2011, in 2007, there were 1,039 municipalities in Saxony-Anhalt. Most of them were very small municipalities with little administrative and financial power. There were only three municipalities with county status. Around 70 percent of the county-affiliated municipalities had less than 1,000 inhabitants and 40 percent even less than 500 inhabitants. 996 of these municipalities were affiliated to 94 associations of administration. All efforts in the early 2000s to induce voluntary mergers were unsuccessful. Saxony-Anhalt's negative demographic trend and diminishing financial resources (expiring of reconstruction aid for the New Länder) further increased the pressure to act.[14]

(*Verbandsgemeinde*): Municipal associations have a common elected council and mayor and are by law obliged to perform certain tasks for their member municipalities (for more information see Ministry of the Interior and Sports Facilities Saxony-Anhalt 2007a, page 76 f.).

[11] To the best of my knowledge being a member of an association of administration or a municipal association does not limit a municipality in taking on debt in Saxony-Anhalt.

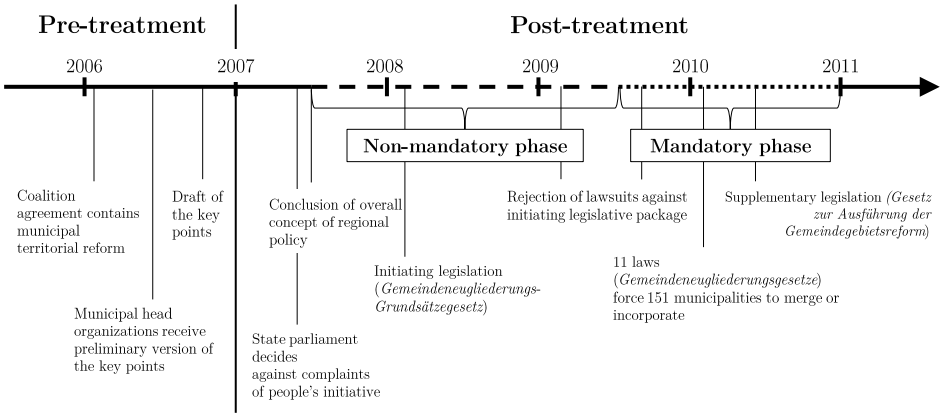
[12] See Section 4.6 for a deeper discussion on further channels of opportunistic behavior.

[13] Unless indicated otherwise, the overall concept of regional policy (Ministry of the Interior and Sports Facilities Saxony-Anhalt 2007a) is the source of information on the municipal territorial reform.

[14] Before the municipal territorial reform described here, there had already been other endeavors to increase the administrative municipal power in Saxony-Anhalt (e.g., introduction of the administrative layer association of administration). Furthermore, two county territorial reforms (*Kreisgebietsreformen*)

Timeline. In response, the coalition agreement on April 24, 2006 contained a municipal territorial reform to reduce the number of municipalities and increase their size. Already on November 17, 2006 the state parliament of Saxony-Anhalt decided to draft the key points of the overall concept of regional policy (*Leitbild*) until December 2006. The municipal head organizations were closely involved. They got a short preliminary version of the key points already in mid 2006. On August 7, 2007 the overall concept of regional policy was concluded. It was the basis for the relevant law (*Gemeindeneugliederungs-Grundsätzegesetz*) which came into force on February, 21 2008. See Figure 4.1 for a clear display of the time line.

Figure 4.1: Time line of the municipal territorial reform



Notes: Illustration by author.

The reform was divided into two phases: The non-mandatory phase lasted from mid 2007 to June 30, 2009. During the non-mandatory phase municipalities could territorially restructure according to their preferences as long as it was within the scope of the guidelines of the regional policy. The mandatory phase, in which the state passed laws to force municipalities to merge or incorporate, began on July 1, 2009. The government aimed for voluntary mergers by creating public awareness, working closely with the respective municipalities (and their citizens) and incentivizing them financially.[15] The reform was

preceded the municipal territorial reform.

[15]The overall concept of regional policy scheduled around 45 million euros for financial incentives and already outlined the distribution mechanisms. Two circular decrees in 2007 provided the legal basis (Ministry of the Interior and Sports Facilities Saxony-Anhalt 2007b, 2007c). Municipalities taking part in the restructuring in accordance with the overall concept of regional policy received 20 euro per capita (p.c.)

concluded in 2011.

The municipal territorial reform met some resistance.[16] Most mergers took place during the voluntary phase (2007 to mid 2009). However, in the mandatory phase 151 municipalities were, through 11 laws (*Gemeindeneugliederungsgesetze*), forced into regional-policy-consistent mergers and incorporations. 53 of them filed a lawsuit at the constitutional court (Ministry of the Interior and Sports Facilities Saxony-Anhalt 2015). Some municipalities withdrew their lawsuit later on. All others were rejected except for two due to procedural errors. Only in 2014 were all lawsuits settled at the constitutional court.

At the beginning of 2011, there were 219 municipalities in Saxony-Anhalt. 104 were non-associated municipalities. All others were associated with the 18 municipal associations. Figure 4.2 presents the change in pre- and post-reform territorial status. It highlights the extent of the territorial reform (additionally, it already refers to the econometric specifications by pointing out the (spatial) distribution of control, treatment and alternative control group (for more information see Section 4.4.3). Figure 4.7 in Appendix 4.7 also provides the evolution of the municipal territorial reform.

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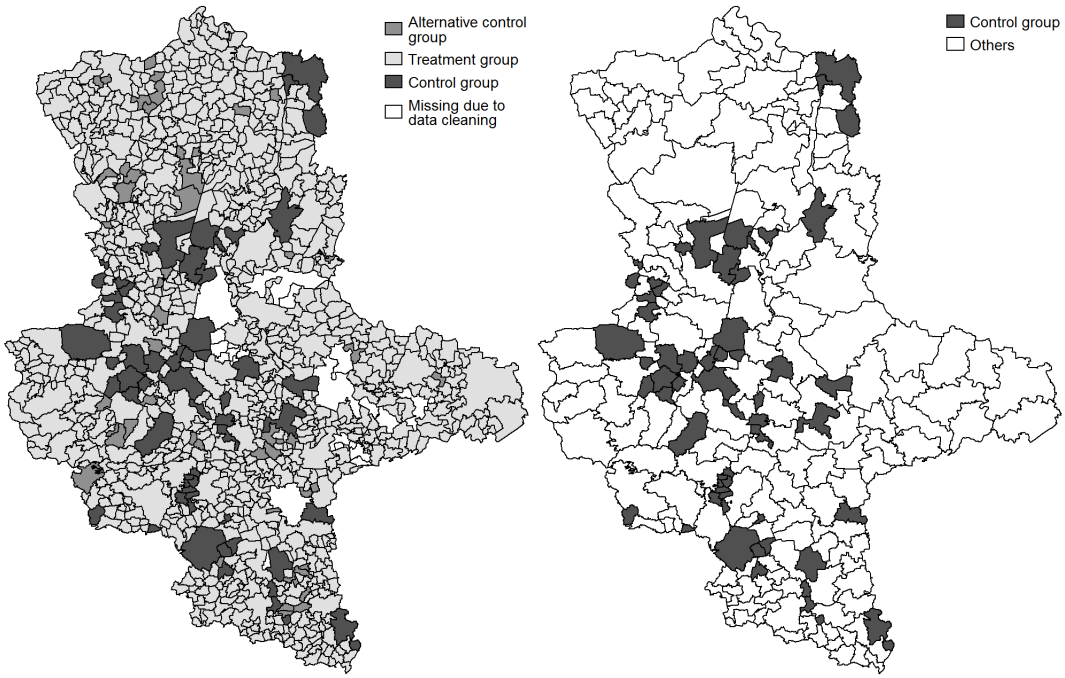
of municipal transfers for investment (with a limit of 5,000 inhabitants being considered). The maximum amount provided accumulated to 10,000 euros. Furthermore, the new municipalities (and newly founded municipal associations) received transfers for investment equal to 100,000 euros and consolidation transfers depending on their needs. According to the Ministry of Finance Saxony-Anhalt (2015) the financial incentives provided in the non-mandatory phase summed up to 61.1 million euros.

[16]Immediately after announcing the reform, a people's initiative (*Volksinitiative gegen flächendeckende Einheitsgemeinden und Zwangseingemeindungen im Umfeld von Ober- und Mittelzentren-Sachsen-Anhalt 2011*) to prevent the reform was founded. It was joined by over 170 municipalities. However, on July 13, 2007 the state parliament decided against their complaints. 178 municipalities filed a lawsuit with the constitutional court against the initiating legislative packages (*Begleitgesetz zur Gemeindegebietsreform* from February 14, 2008). It was rejected by the constitutional court on April 21 and September 15, 2009. The plan to launch a petition for a referendum (*Volksbegehren*) of 26 municipalities in response to the first rejection in April 2009 failed the minimum number of signatures required (Gundlach 2013, page 133).

Figure 4.2: Pre-reform (left) and post-reform territorial status (right)



Notes: The figure depicts the pre-reform 2006 territorial status (left) and post-reform 2011 territorial status (right). Control and treatment group refers to the municipalities which were not and were affected by reform, respectively. Alternative control group refers to those municipalities which were forced to merge and fought against this in court. Own compilation with geoinformation provided by the GeoDataCenter (Federal Agency for Cartography and Geodesy).

Principles and criteria. The reform effort was guided by the following principles: It favored the amalgamation of non-associated municipalities. It abolished the administrative layer of associations of administration. It implemented a new local administrative unit (municipal associations). However, municipal associations were supposed to be an exception and were only permitted during the voluntary phase. The reform protected the already existing 37 non-associated municipalities. Amalgamations (into non-associated municipalities and municipal associations) were to be consistent with the boundaries of former associations of administration. This extremely limited the autonomy of decision during the non-mandatory phase. Amalgamations were supposed to consider long-standing economic, ecological and historic relationships as well as aspects concerning the general regional-planning policy.

The most important formal criteria were: Non-associated municipalities should not

have less than 10,000 inhabitants. Municipal associations should have a minimum of three and a maximum of eight associated municipalities. Municipal associations should have a minimum of 10,000 inhabitants, all of their associated municipalities not less than 1,000 inhabitants. Certain exceptions applied with regard to the minimum population size, for example, in particular geographic positions. Only the 61 associations of administration with a joint administrative unit (*Verwaltungsgemeinschaft nach dem Modell "Gemeinsames Verwaltungsamt"*) could vote to amalgamate into a municipal association (but only if they had no dominating municipality). The 33 associations of administration with a leading municipality (*Verwaltungsgemeinschaft nach dem Modell "Trägergemeinde"*) should be transformed into non-associated municipalities.

Politicians. The local government law of Saxony-Anhalt provides detailed transitional arrangements for the mayors and municipal councils of the affected municipalities until the time of the next election.[17] At the next elections all voters of the new municipalities elected a new mayor and municipal council; the distribution of voters, hence, was the important factor for the political power constellation. Municipalities associated with a municipal association remained legally independent. Mayor and municipal council continued their term.[18]

4.3 The free-riding effect

In this section I propose the five hypotheses underpinning the empirical approach and their operationalization. Hypotheses 1a and b are at the center of previous empirical research. Hypothesis 1a picks up the implications of the "tragedy of the commons." It addresses whether the mere availability of a common pool already triggers an incentive to free-ride.

[17]In case of a merger electing a new municipal council was obligatory. If there was only one salaried mayor in the merging municipalities, he or she became the new mayor. In the unlikely case that there was more than one salaried mayor in the merging municipalities the procedure was more complicated: Either the merging agreement already designated one of them to become the new mayor or one of them was elected by the new municipal council. In case of an incorporation the incorporated municipality could either transform its council into a council of locality (*Ortschaftsrat*)(merging or incorporating municipalities could form a locality (*Ortschaft*)). Alternatively, there were elections for a new municipal council. If the absorbing municipality had a salaried mayor, he or she staid in power, if it had only an honorary one there were re-elections.

[18]Besides that, the municipal association also has its own mayor (*Verbandsgemeindebürgermeister*) and council (*Verbandsgemeinderat*).

Hypothesis 1a: *Being in a common pool triggers an incentive to free-ride.*

However, in view of the previous literature Hypothesis 1b and not 1a may be seen as the actual starting point.

Hypothesis 1b: *The incentive to free-ride is greater, the smaller the municipality's own size relative to the size of its common pool.*

The first study on this research interest (Hinnerich 2009) concentrates on the theoretical predictions in the spirit of the "law 1/n." It suggests that the incentive to behave opportunistically is increasing, the smaller the municipality's own size relative to its common pool (note, as explained before, the original formulation relates the number of common pool members to public project size). Hinnerich (2009) operationalizes this – also called continuous – incentive to free-ride of municipality i as:

$$F_i = 1 - n_i/n_j \in [0, 1) \tag{4.1}$$

with n_i being the population size of municipality i and n_j being the population size of the incorporating municipalities and the absorbing municipality or the merging partners.[19] I follow his definition. See Figure 4.3 in Section 4.4.2 for the distribution of the free-riding measure F_i.

De facto, Hinnerich (2009) abstains from studying Hypothesis 1a by only referring to the continuous measure. In line with Jordahl and Liang (2010) and Saarimaa and Tukiainen (2015), I argue that solely employing the continuous measure hinges on various assumptions: On the assumption that the theoretical predictions of the "law 1/n" are interpreted correctly. On the assumption that the municipalities understand the underlying incentives fully. On the assumption that they anticipate their free-ride incentive correctly. And, last, on the assumption that they are not limited in their ability to exploit the common pool. To account for the fact that the simple formation of a common pool

[19]Due to the complexity of the municipal territorial reform in Saxony-Anhalt the definition of the common pool n_j was not straightforward. Most incorporations took place at one point in time, but there were certain cases of multiple mergers or incorporations. As far as the latter is concerned, the definition of the common pool refers to the last (and not the first) amalgamation status. Given the level of detail of the overall concept of the regional policy and the short time horizon, it seems reasonable to assume that municipalities were aware of the additional mergers and incorporations. Nevertheless, this decision is subject to a robustness check. I calculate the free-riding effect by using the population size of the year 2006.

might trigger free-riding independently of its size (as stated by Hypothesis 1a) Jordahl and Liang (2010) add a so-called discrete effect besides the continuous effect. Saarimaa and Tukiainen (2015) follow their approach. This discrete effect is modeled as a dummy variable which takes the value 1 if the municipality is affected by the reform (merged, incorporated or absorbing) and 0 otherwise. All municipalities which were not affected by the reform (neither merging nor incorporating nor absorbing) constitute the control group (note they have no incentive to free-ride and hence $F_i = 0$).[20]

Hypotheses 1a and 1b are captured by the econometric model specified in Equation 4.3 outlined in Section 4.4.3 with the operationalization of the discrete and continuous effect being based upon previous reserach.

Hypothesis 2a: *Municipalities which remain accountable after the execution of the reform do not engage in free-riding.*

Hypothesis 2b: *The more municipalities identify with the reform, the lower is the free-riding effect.*

However, to allege that all municipalities being part of a common pool engage in free-riding is shortsighted in the light of the recent developments in the literature on the "commons." There might exist implicit control mechanisms attenuating opportunistic behavior or keeping municipalities from behaving opportunistically at all. This leads to Hypotheses 2a and b. I suggest that municipalities (and politicians) which (and politicians who) are in public perception considered to be accountable (for handling public debt and infrastructure) after the reform will not engage in free-riding. This applies for municipalities which are only absorbing or are keeping their name when merging. The more the members of the common pool identify with the amalgamation, the lower presumably is the incentive to free-ride. Consequently, it seems reasonable to believe that voluntary mergers or incorporations are less affected by opportunistic behavior than mandatory mergers. Saarimaa and Tukiainen (2015) present evidence supporting opportunistic behavior in case of voluntary

[20] In light of Hansen's (2014) advocation of the original formulation I expand my analysis and also examine if the size of the free-riding effect is greater the more municipalities are in a common pool as originally proposed by the "law 1/n." In accordance with Saarimaa and Tukiainen (2015) I call it coalition size. I define it as:

$$C_i = 1 - 1/p_m \in [0, 1) \tag{4.2}$$

with p_m being the number of merging partners (or the sum of the incorporating municipalities plus the absorbing municipality).

mergers. However, their research design does not allow a distinction between voluntary and mandatory mergers.

I capture these hypotheses by dividing the treated municipalities into three treatment groups. I define dummy variables to compute the difference between the treatment and the control groups. Treatment group 1 (group of survivors): This group of municipalities consists of absorbing municipalities and merging municipalities which keep their name. This treatment group refers to Hypothesis 2a while the two other treatment groups refer to Hypothesis 2b. Treatment group 2 (group of non-survivors with weak incentives): It consists of municipalities which were incorporated or merged during the non-mandatory phase. Treatment group 3 (group of non-survivors with strong incentives): This group comprises all municipalities which were incorporated or merged mandatorily and did not dispute this at the constitutional court. Hypotheses 2a and b refer to Equation 4.4 in Section 4.4.3.

Hypothesis 3: *The size of the free-riding effect is greater, the later the incorporation or merger takes place.*

The last hypothesis refers to the timing of the merger or incorporation. I suggest that the timing of the merger or the incorporation has a positive effect on the size of the free-riding effect. The later it takes place, the more time the municipalities have to behave opportunistically and to coordinate opportunistic behavior. A decisive factor is that there are public projects which do not qualify to be financed through free-riding on debt at all if there is a strong time limitation. In particular, cost-intensive public projects (for which the municipalities take on debt for investment purposes) are time-sensitive. They may involve planning, tendering and adjustment of the budget-bye laws. Evidence in favor of this hypothesis has already been brought forward by Hinnerich (2009). Hypothesis 3 is reflected in Equation 4.5 (see Section 4.4.3).

4.4 Empirical approach

This section starts by explaining the data sources. Next, I report the relevant descriptive statistics. Last, I outline the econometric model and provide a discussion on identification and robustness.

4.4.1 Data

Data sources. I collected a panel dataset on the full universe of municipalities in Saxony-Anhalt from 2000 to 2010 to study the free-riding incentive in the course of the municipal territorial reform.[21] All data comes from official public administrative statistics, but from different sources. My research interest hampers the data collection. In Germany, the statistical offices typically only provide data (ready for use) on the latest territorial status after a territorial reform took place. This implies that all data is aggregated to post-reform boundaries. However, for my research interest using data which reflects the territorial status of the respective year is crucial. Information on municipal debt comes from the annual statistical reports of local debt for Saxony-Anhalt, which is published by the Statistical Office of Saxony-Anhalt.[22] While the latest reports (2006 to 2010) are available online, the ministry retains only paper versions for the earlier years. To the best of my knowledge, this data has not been used for academic research so far. I obtained data for all usual municipal-level controls except for employees from the Regional Database Germany (*Regionaldatenbank Deutschland*) for the year 2009. Data for the years from 2001 to 2008 emanates from the Statistical Local (*Statistik Lokal*) publications. Data for the year 2000 was requested from the Statistical Office of Saxony-Anhalt. Data on employees is provided by the Federal Employment Agency (*Bundesagentur für Arbeit*). All data on variables which are only available at county level are also provided online by the Regional Database Germany.[23]

Detailed information on the distinct features of the territorial reform were provided by the Ministry of the Interior and Sports Facilities and the Ministry of Finance of Saxony-Anhalt at request. Information on the municipalities which were merged or incorporated on a mandatory basis were gathered directly from the respective laws. The overall concept of regional policy is a further important data source. The Federal Statistical Office provides information to connect pre- and post-county-reform identifiers. The Statistical Office of Saxony-Anhalt makes available detailed information on the municipal territorial reform (type of change in territorial status and respective change of identifier).

[21] However, data for 2010 is only provided for the territorial status of 2011. This is why this year is missing in the empirical analysis.

[22] The municipal debt comprises credit market debt and debt from public households.

[23] Data on outlays and revenues is missing for the municipalities of Saxony-Anhalt due to their ongoing reform of municipal accounting procedures. This prevents me from complementing my research with an empirical analysis of the impact of this reform on fiscal outcomes.

Data cleaning. Due to the reporting rules of the German administrative statistic there is the need for data cleaning: A municipality which is incorporated is no longer reported. The absorbing municipality, on the other hand, remains in the sample, constitutes the aggregate of the two and does not change its identifier. The same holds for municipal mergers if one of the municipalities' names is maintained. In case of a merger, a new identifier is only assigned if the merging municipalities choose a new name. In the pre-reform period I drop all absorbing municipalities in the years before the incorporation. The same holds for merging municipalities which maintain the name of one municipality.[24] All merging municipalities as well as municipalities which were incorporated in the pre-reform period (2000 to 2006) are excluded.[25] I also exclude mergers and incorporations which took place in 2007. In the post-reform period (2007 to 2009) I drop absorbing municipalities after their first incorporation and merging municipalities which maintained their name after their first mergers. I also exclude new municipalities from the analysis.

4.4.2 Descriptive statistics

Table 4.1 shows descriptive statistics for the baseline sample for the period 2006 to 2009. Table 4.8 in the Appendix 4.7 complements it by presenting the equivalent statistics for the full sample (2000 to 2009). It first lists the dependent variable debt in euro p.c. (all nominal variables are deflated to 2000 prices). The average municipal debt in the sample is 575.56 (in euro p.c.) (for the period 2006 to 2009). While the minimum is 0 (not having any debt at all), there is a municipality with 17,254.84 euro p.c. debt. The next four variables refer to the treatment dummies: On average there are 7% of the municipalities in the control group (CG) and 93% of the municipalities in the treatment group (TG). To be more concrete 11%, 72% and 11% are in treatment group 1 ($TG1$), treatment group 2 ($TG2$), and treatment group 3 ($TG3$), respectively. Table 4.7 in Appendix 4.7 provides supplementary information on the absolute number of municipalities by group and year.

The average free-riding measure (F_i) is 0.786 with a minimum of 0 (municipalities of

[24] Alternatively, I could have assigned a new identifier for artificial separation. However, since I am only interested in municipalities existing in the pre- and post-reform period, dropping was the most practical solution.

[25] There were some mergers and incorporation before the state-wide reform between mid-2007 and 2011. I considered to include the incorporations and mergers during the pre-reform period as further little reforms in the difference-in-differences design. However, modeling the actual reform is challenging and there is reason to believe that free-riding behavior of these municipalities differs. Including them without dealing with this aspect could affect the estimates.

the control group) and a maximum of almost 1 (0.9998714) (see Equation 4.1 and 4.2 for the definition of F_i and C_i). The coalition size (C_i) averages around the same value (0.784), again with a minimum of 0 (municipalities of the control group), but with a maximum of 0.967. This means that the greatest common pool consists of 30 members ($0.967 = 1 - \frac{1}{30}$). Table 4.1 furthermore displays descriptive statistics for the most important municipal-level control variables (population, employees (p.c.), business and property tax revenue (both in euro p.c.)) and county-level control variables (disposable income of private households and gross domestic product (GDP) (both in euro p.c.)).

Table 4.1: Descriptive statistics, 2006-2009

	Mean	Std. Dev.	Min.	Max.	Observations
Debt	575.559	909.506	0	17254.838	3595
TG	0.934	0.248	0	1	3595
TG1	0.106	0.308	0	1	3595
TG2	0.717	0.451	0	1	3595
TG3	0.111	0.315	0	1	3595
F	0.786	0.296	0	1	3595
C	0.784	0.241	0	0.967	3595
Population	1707.117	4263.638	47	46384	3595
Employees	0.173	0.238	0.009	5.003	3071
Business tax rev.	182.498	1881.666	-6977.196	53784.978	3595
Property tax rev.	58.762	29.104	0	575.853	3595
Priv. income	13321.462	418.565	12513.075	14324.152	3595
GDP	17248.617	2142.751	13177.412	22673.803	3595

Notes: Main sample for the period 2006 to 2009 with 3,595 municipalities. F_i and C_i are the continuous free-riding and coalition size measures, respectively. Debt (in euro p.c.), population, employees (p.c.), business and property tax revenue (both in euro p.c.) are municipal-level variables. Income of private households and GDP (both in euro p.c.) are county-level variables. TG_i is a dummy variable and equals 1 if the municipality is treated and 0 if not. $TG1_i$, $TG2_i$, and $TG3_i$ are equal to 1 if the municipality is part of the group of survivors, group of non-survivors (weak incentives) and the group of non-survivors (strong incentives), respectively, and 0 otherwise. All nominal values are deflated to 2000 prices.

Table 4.2 allows to compare the mean of the pre-reform and post-reform period for

the control and treatment group. The pre-reform mean refers to the years 2000 to 2006 and concerns 6,175 and 374 municipalities of treatment and control group, respectively. The post-reform period is shorter (2007 to 2009) and consists only of 2,450 (treatment group) and 177 (control group) municipalities.[26]

It catches one's eye that the mean debt (in euro p.c.) for control and treatment group is higher in the pre-reform period than in the post-reform period. This is rather unexpected (note that I work with 2000 prices). However, the mean of the control group falls more (17%) than the mean of the treatment group (14%). This is in line with my expectations.

Table 4.2: Descriptive statistics (mean) for control and treatment group differentiated by pre- and post-reform period

	Debt	F	C	Population	Employees	Business tax rev.	Property tax rev.	Priv. income	GDP
	Pre-reform (2000-2006)								
Control group	1077.50	0.00	0.00	3708.45	0.20	146.38	56.71	13255.60	16129.32
Treatment group	619.24	0.85	0.84	1389.92	0.17	116.49	55.61	13170.82	16015.21
Total	645.41	0.80	0.79	1522.33	0.17	118.20	55.67	13175.66	16021.73
	Post-reform (2007-2009)								
Control group	897.11	0.00	0.00	4243.45	0.21	195.12	60.23	13462.01	17288.54
Treatment group	531.51	0.84	0.84	1477.60	0.17	179.80	58.85	13365.71	17306.48
Total	556.14	0.79	0.78	1663.95	0.18	180.83	58.94	13372.20	17305.28

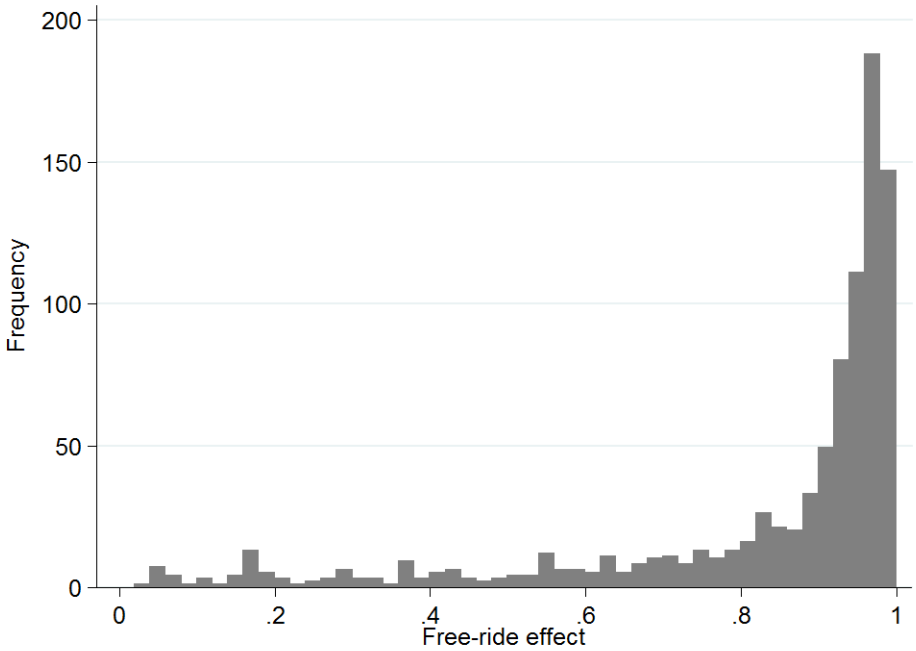
Notes: Main sample for the period 2000 to 2009 with 9,176 municipalities (except for employees: 8,090 observations). F_i and C_i are the continuous free-riding and coalition size measures, respectively. Debt (in euro p.c.), population, employees (p.c.), business and property tax revenue (both in euro p.c.) are municipal-level variables. Income of private households and GDP (both in euro p.c.) are county-level variables. Pre-reform and post-reform period refer to 2000 to 2006 and 2007 to 2009, respectively. All nominal values are deflated to 2000 prices.

Figure 4.3 shows the distribution of the free-riding measure F_i for all 909 municipal-

[26]Except for employees there are no missings. The number of observations for employees is 5,514 and 2,026 (treatment group), and 374 and 176 (control group) for pre- and post-reform period, respectively.

ities of the treatment group in 2006. It highlights that the distribution is right skewed. This implies that there are more small municipalities than big municipalities in the common pools. The median is rather high (.94). The 15th percentile and the 85th percentile equals .63 and .98, respectively. By definition none of the municipalities of the treatment group has a free-ride measure $F_i = 0$ or $F_i = 1$ (keep in mind $F_i = 0$ for all municipalities of the control group).

Figure 4.3: Distribution of the free-riding effect for treatment group, 2006



Notes: Distribution of the free-ride measure for all 909 municipalities of the treatment group in 2006.

Figure 4.8 in Appendix 4.7 provides the equivalent depiction for the coalition size. Note that the coalition size is less scattered since all municipalities of one common pool have the same effect.

4.4.3 Econometric model

My identification strategy relies on a difference-in-differences approach in a panel data environment.

Equation 4.3. I start with Equation 4.3. It specifies the free-riding incentive as it

prevails in previous empirical research. It refers to Hypotheses 1a and 1b. It is presented in its basic form:[27]

$$\begin{aligned} d_{i,t} = \alpha + \textstyle\sum_{t=2007}^{2009} \alpha_t TG_i \times TP_t + \sum_{t=2007}^{2009} \beta_t F_i \times TG_i \times TP_t \\ + \eta \boldsymbol{X_{i,t}} + \theta \boldsymbol{Z_{c,t}} + \lambda_i + \Phi_t + \varepsilon_{i,t} \end{aligned} \tag{4.3}$$

where i stands for a municipality in county c in year t for the period 2006 to 2009 with 2006 being the base year (pre-treatment point) and 2007 to 2009 being the post-treatment points.[28] The dependent variable $d_{i,t}$ is debt in euro p.c. All nominal values are deflated to 2000 prices. This specification combines the discrete and the continuous free-riding effect. My independent variables of interest are: the interaction terms $TG_i \times TP_t$ between the treatment group TG_i and the treatment points TP_t (discrete effect) and the triple interaction term $F_i \times TG_i \times TP_t$[29] between the municipality-specific continuous free-riding measure F_i, the treatment group TG_i and the treatment points TP_t with $t = 2007$, 2008 and 2009 (continuous effect). TG_i is equal to 1 if the municipality is affected by the reform (merged, incorporated or absorbing) and 0 otherwise. TP_t is equal 1 if $t = 2007$, 2008 and 2009, respectively, and 0 otherwise. I also run some regression where I replace the continuous free-riding effect F_i by coalition size C_i.

I further add $\boldsymbol{X_{i,t}}$, representing a vector of municipal-level variables (population, employees p.c., property and business tax in euro p.c.), and $\boldsymbol{Z_{c,t}}$, representing a vector of county-level variables (GDP and income of private households in euro p.c.), to account for observable time-variant differences between municipalities. Furthermore, I add municipality and year fixed effects (λ_i, Φ_t). The municipality fixed effects control for unobserved but time-invariant omitted municipal-level factors that may influence the dependent variable. By employing year fixed effects I account for common shocks affecting the dependent variable across all municipalities in a given year. The group of municipalities which were merged or incorporated mandatorily, but filed a lawsuit against this, is excluded from the

[27] Empirically, I also test specifications only referring to Hypothesis 1a: $d_{i,t} = \alpha + \sum_{t=2007}^{2009} \alpha_t TG_i \times TP_t + \eta \boldsymbol{X_{i,t}} + \theta \boldsymbol{Z_{c,t}} + \lambda_i + \Phi_t + \varepsilon_{i,t}$ (Equation 4.3, Specification (I)) and Hypothesis 1b: $d_{i,t} = \alpha + \sum_{t=2007}^{2009} \beta_t F_i \times TG_i \times TP_t + \eta \boldsymbol{X_{i,t}} + \theta \boldsymbol{Z_{c,t}} + \lambda_i + \Phi_t + \varepsilon_{i,t}$ (Equation 4.3, Specification (II)). However, the previous literature generally combines both effects.

[28] This may be hard to grasp: The treatment period refers to the time between the announcement of the reform and the execution (and not the time after the execution as one might expect), since I am interested in the chance of behavior after the announcement. Accordingly, the pre-treatment period refers to 2006 (and the years prior to 2006).

[29] $F_i \times TG_i \times TP_t$ equals $F_i \times TP_t$.

analysis. Standard errors are clustered on the municipality level.

Equation 4.4. Next I present Equation 4.4, which refers to Hypotheses 2a and 2b and is specified as follows:

$$d_{i,t} = \delta + \sum_{t=2007}^{2009} \delta_{1,t} TG1_i \times TP_t + \sum_{t=2007}^{2009} \delta_{2,t} TG2_i \times TP_t + \sum_{t=2007}^{2009} \delta_{3,t} TG3_i \times TP_t + \eta \boldsymbol{X_{i,t}} + \theta \boldsymbol{Z_{c,t}} + \lambda_i + \Phi_t + \varepsilon_{i,t} \tag{4.4}$$

Treatment group $TG1_i$ is equal to 1 if the municipality is part of the survivor group and 0 otherwise. Treatment groups $TG2_i$ and $TG3_i$ are equal to 1 if the municipality is part of the group of non-survivors (weak incentives) and the group of non-survivors (strong incentives), respectively, and 0 otherwise. The independent variables of interest are the interaction terms $TGj_i \times TP_t$ between the treatment groups TGj_i with $j = 1$, 2 and 3 and treatment point $t = 2007$, 2008 and 2009, respectively. Treatment group $TG1_i$ refers to Hypothesis 2a, while treatment group $TG2_i$ and $TG3_i$ refer to Hypothesis 2b. Employing Equation 4.4 is necessary to tackle Hypothesis 2a and 2b. Equation 4.3 does not consider how the municipality was affected by the territorial reform (surviving, voluntary or mandatory merger). It focuses on relative common pool size.

Equation 4.5. The last equation refers to Hypothesis 3 and employs aggregated data. I aggregate the pre-reform (2000 to 2006) and the post-reform (2007 to 2009) observations to one observation each. The main purpose of the aggregation is the sensitivity analysis later. It is also convenient with respect to Hypothesis 3. It allows the analysis of the effect of the year in which the merger or incorporation actually took place. I employ a fixed effects model with only two time points ($p = pre$ and $post$). $Post_p$ consequently is the post-reform period dummy and is equal to 1 if $p = post$ and 0 if not. To explore if the free-riding effect depends on the timing of the merger or the incorporation, I use the following specification:

$$d_{i,p} = \gamma + \sum_{t=2008}^{2011} \gamma_t Change_{t,i} \times Post_p + \eta \boldsymbol{X_{i,p}} + \theta \boldsymbol{Z_{c,p}} + \lambda_i + \Phi_p + \varepsilon_{i,p} \tag{4.5}$$

$Change_{t,i}$ equals 1 if the municipality was merged or incorporated in $t = 2008, ..., 2011$, respectively, and 0 if it was not. Independent variables of interest are the interaction terms between $Change_{t,i} \times Post_p$ between the timing of the merger or in-

corporation $Change_{t,i}$ with $t = 2008, ..., 2011$ and the post-reform period dummy $Post_p$.

4.4.4 Identification and robustness

The municipal territorial reform in Saxony-Anhalt can be interpreted as a quasi-experiment. The decision by the state government to carry out a municipal territorial reform represents an (exogenous) shock to the individual municipalities. The assignment to treatment itself is not at random, but is clearly defined in the overall concept of regional policy and is enforced in the mandatory phase. It mainly depends on population size besides geographical and administrative characteristics. This is why population is an important control variable. The implementation of a non-mandatory phase leaves the municipalities a very limited degree of leeway regarding their decision with whom to merge or incorporate (within the close limits of the overall concept of regional policy). This might raise minor endogeneity concerns, but only in relation to potential partners (and only in the non-mandatory phase) and not in relation to whether or not the municipalities are affected by the reform at all.[30] However – and this is what is the important point for eliminating selection bias concerns – there is no indication that the decisions related to the leeway granted in the non-mandatory phase are correlated with the future trend of the dependent variable in the absence of treatment. The same holds for the issue of self-selection into mandatory and non-mandatory treatment. Nevertheless, I add property and business tax revenue (local level) and GDP (county level, all three in euro p.c.) to account for potential group-specific economic shocks coinciding with the reform.

To address even minor potential selection issues I, furthermore, perform a robustness check where the treatment group only consists of municipalities which definitely could not evade treatment and which are easily identifiable (see robustness check for self-selection). These are municipalities with less than 1,000 inhabitants, municipalities that are part of associations of administration with a leading municipality and municipalities that are part of treatment group 3.

The main advantage of the difference-in-differences design is that it solves potential identification issues (due to unobserved heterogeneity) by comparing the control group – all municipalities which are not affected by the reform – to the treated municipalities

[30] Except for the small group of municipalities (municipalities being part of associations of administration with a joint administrative unit which had no dominating municipality) which could become part of a municipal association in the non-mandatory phase (and thereby remain independent). All already existing non-associated municipalities were protected from the reform.

before and after the reform. The validity of this approach is based on the assumption that the trend in the dependent variable of the control and treatment groups would have been the same in the absence of the treatment. Of course, this assumption cannot be tested explicitly. A common way to explore the so-called common trend assumption is to check whether the trends of control and treatment group developed in parallel in the past. Given that all municipalities enjoy the same institutional environment, there is no reason to believe these trends would not continue in the future in the absence of treatment. This is why this paper relies heavily on the graphical analysis conducted in Section 4.5.1. There are further ways to tackle the common trend assumption such as Placebo tests or non-equivalent dependent variables (see, e.g., St. Clair and Cook [2015] for a discussion of difference-in-differences methods in public finance).[31] One pitfall of a difference-in-differences approach with a long panel dimension – as pointed out by Bertrand et al. (2004) – is the serial correlation problem, which can lead to a wrong rejection of the null hypothesis. This problem is frequently disregarded by applied literature. I address it by employing clustered standard errors and performing a robustness check where I ignore the time series dimension of the data.

It is inherent to the study of municipal territorial reforms that treatment and control groups differ with respect to certain characteristics, in particular, population size. The treatment group specific fixed effects serve to capture average permanent differences between treatment and control groups. Nevertheless, the difference-in-differences design is stronger, the closer the control group is to the treatment group. This is why I excluded the three municipalities with county status. As part of my sensitivity analysis I also use the group of municipalities which were merged or incorporated mandatorily, but filed a lawsuit against this, as an alternative control group. Since those municipalities were designated for a merger or an incorporation in the overall concept of regional policy, their relevant characteristics (population size, geographical and administrative characteristics) are very similar to the treatment group. Given that they tried to reverse their mandatory merger or incorporation it is unlikely that they behaved opportunistically (see robustness check for alternative control group). However, one drawback is that they selected themselves in this group.

One particular strength of the municipal territorial reform in Saxony-Anhalt is that

[31] The latter is not an option because there is no variable which is available in public administrative statistic, which is conceptually similar to the dependent variable and which is not affected by the territorial reform.

I can clearly separate the pre- and post-reform period. Pre-reform and post-reform refers to the time before and after the reform was made public. The reform was announced by the coalition agreement in April 2006 with the municipal head organizations having information on the preliminary version of the key points in August. 2007 was therefore the first year for which the budget bye-laws could be adjusted in response to the reform.

A possible limitation of this research lies in the calculation of the continuous free-ride measure F_i (which is only employed in Equation 4.3). As outlined above, the formulation of the common pool refers to the whole post-reform period (2007 to 2011). This implies that early mergers or incorporations were aware of further, later mergers or incorporations. However, it might be the case that early mergers or incoporations only considered their current common pool. If this is the case the current definition would overestimate the incentive to free-ride and downward bias the estimated coefficient. For a robustness check I exclude all observations from the regression which are part of a common pool with multiple mergers or incorporations (see robustness check b). A further concern relates to the 178 municipalities which lost their lawsuit against the initiating legislative packages in 2009. They might not have behaved opportunistically since they hoped to bring the reform to an end (see robustness check a). This could also involve a downward bias. Of minor importance but nevertheless subject to a robustness check are the few territorial changes in the pre-reform period. It might be that the newly founded municipalities or the absorbing municipalities were subject to a post-reform trend. This raises concerns regarding the validity of the common trend assumption (see robustness check c).

For the sake of completeness I report a further minor drawback of my research: There is no adequate data available for the year 2010.[32] This concerns only municipalities which were first dissolved mandatorily in 2011 (the last year of the reform). Municipalities which were dissolved in 2010 were reported regularly for the last time in 2009. Lacking this information mainly implies that we cannot estimate the interaction $TG3_i \times TP_{2010}$.

4.5 Graphical evidence and results

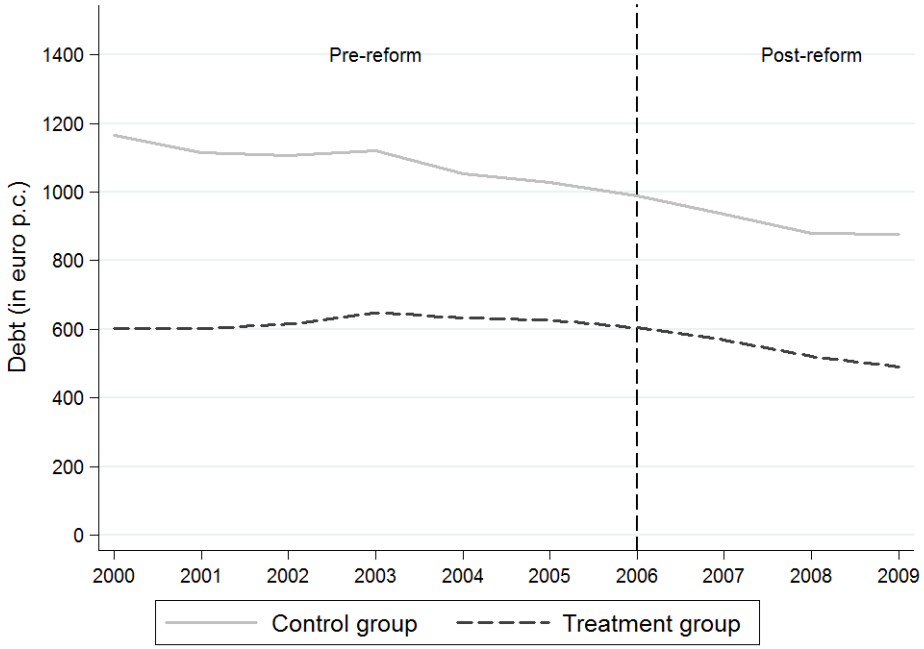
In this section I first explore the relevant graphical evidence. Then I present the main results. Last, I provide a number of robustness checks to validate my results.

[32] I could not retrieve data for 2010 reflecting the territorial status of 2010. See discussion in Section 4.4.1.

4.5.1 Graphical evidence

I start by exploring the validity of the common trend assumption graphically. Figure 4.4 presents the development of average debt (in euro p.c., deflated to 2000 prices) of the control group (gray line) and the treatment group (dashed black line) between 2000 and 2009. The dashed black vertical line separates the pre-reform (2000 to 2006) and the post-reform (2007 to 2009) period. 2010 is missing due to data availability.

Figure 4.4: Development of average debt (in euro p.c.) by control and treatment groups, 2000-2009



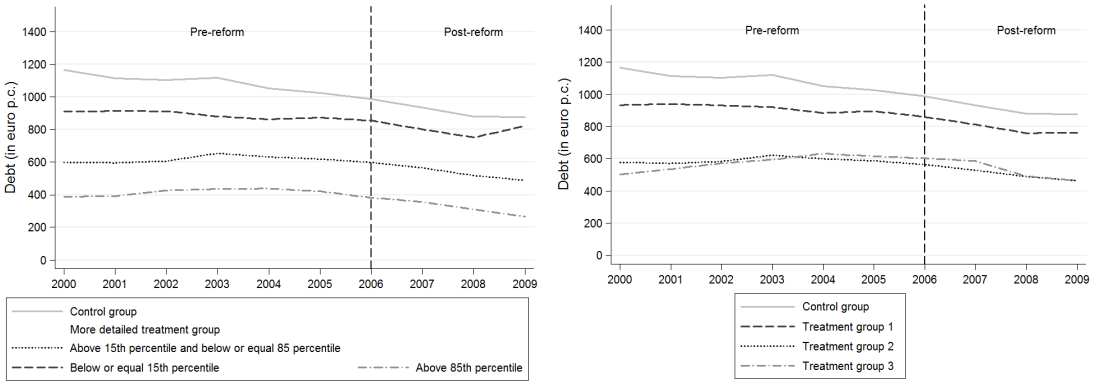
Notes: Unweighted mean, deflated to 2000 prices.

At first glance treatment and control groups seem to experience different pre-reform trends between 2000 and 2006. The average public debt of the treatment group remains rather stable. The average public debt of the control group is subject to a downwards trend. At second glance, however, there seems to be a turning point in 2003. Before 2003 average debt of the control group is indeed decreasing, while that of the treatment group is not. This changes after the hump in 2003, which is visible for both groups. Average debt for both – treatment and control – groups is descending and evolves almost in parallel in 2004, 2005, and 2006.

What is surprising is that there is no break in the development of debt for the

treatment group in the post-reform period, as one would have expected. One explanation might be that the crude separation into control and treatment groups clouds the actual free-riding effect. The development of average debt of municipalities with a low incentive to behave opportunistically should be more akin to municipalities which have no incentive to free-ride (control group) than to municipalities with a high incentive. This is accounted for in Figure 4.5.

Figure 4.5: Development of average debt (in euro p.c.) by control and differentiated treatment groups, 2000-2009



Notes: Unweighted mean, deflated to 2000 prices. Left figure refers to Equation 4.3. Treatment group is divided into three groups depending on their value of F_i for illustrative purposes only (value of F_i below or equal to the 15th percentile, above the 15th percentile and below or equal to the 85th percentile and above the 85th percentile). Right figure refers to Equation 4.4. Treatment group 1, 2, and 3 refers to group of survivors, non-survivors (weak incentives) and the group of non-survivors (strong incentives), respectively.

Figure 4.5 (left) relates to Equation 4.3 and refers to F_i. For illustrative purposes only, I divided the treated municipalities into three groups depending on their incentive to free-ride. I assign municipalities with a value of F_i below or equal to the 15th percentile $F_i \in (0, 0.63]$, above the 15th percentile and below or equal to the 85th percentile $F_i \in (0.63, 0.98]$ and above the 85th percentile $F_i \in (0.98, 1)$ to separate groups. Figure 4.5 (right) relates to Equation 4.4. Treatment group 1, treatment group 2, and treatment group 3 refer to the group of survivors, the group of non-survivors with weak incentives and the group of non-survivors, respectively. A similar pattern as above emerges: After 2003 there seems to be a rather parallel evolution of average debt of the control and treatment groups for 2004, 2005, and 2006 on the one hand. On the other hand there

is no obvious change in average debt for the treatment groups in the post-reform period. Figure 4.10 in Appendix 4.7 (left) complements the graphical analysis by presenting the evolution of the differences in average debt between the control and treatment groups (and all subversions).

The graphical assessment casts some doubt on the validity of the common trend assumptions. However, I have four attenuating comments: First, I present a rather long pre-reform period with a much better picture in the later years (after 2003). As discussed above in 2004, 2005, and 2006 the difference between treatment and control group remains rather stable. The major problem related to a pre-reform trend is that this might violate the identifying assumption of a common trend. Since I employ a difference-in-differences design with a base year approach (2006) parallelism with respect to years directly before the base year is of more importance than with respect to the years further away. Second, the assessment of the pre-reform trends in the related literature (e.g., Hinnerich 2009 and Saarimaa and Tukiainen 2015) is very optimistic with respect to the confirmation of the common trend assumption. In my opinion, the graphical evidence presented by them is not superior to mine. While I acknowledge that my findings are not ideal, I add to the literature, because they are arguably and relatively better than those of the previous papers in the literature. Third, the overall picture improves if we consider the alternative control group instead of the control group (see Figure 4.9 and 4.10 (right)). Fourth, nonparallel trends exposed visually might be due to confounding factors. However, exploring the unconditional common trend assumption graphically enhances the overall transparency. Nevertheless, my econometric strategy weakens the overall importance of the graphical analysis. To tackle the conditional common trend assumption in an environment where I control potential confounding factors, I run regressions using the extended dataset for the years 2000 to 2009. Comparing 2006 (base year) and the other pre-reform years allows me to assess if there are differences between the treatment and control period in the pre-reform period. If such differences exist, this would be a conclusive argument against the validity of the common trend assumption.

4.5.2 Main results

I start with specifications including only the base year (2006) and the three post-reform years (2007 to 2009).

Hypothesis 1a and b. Table 4.3 depicts the main regression results for

Equation 4.3. Specification (I) begins by controlling only for the discrete effect $TG_i \times TP_t$ (for $t = 2007, ..., 2009$) while Specification (II) only considers the continuous effect $F_i \times TG_i \times TP_t$ (for $t = 2007, ..., 2009$). Specification (III) combines the discrete and the continuous effect (as it prevails in the previous literature). The left column of each specification presents the baseline regression without any further controls except for the usual municipality and year fixed effects. The right column includes municipal-level controls for population, employees (p.c.), business and property tax revenue and county-level controls for income of private households and GDP (all in euro p.c.). Note that the additional controls are the same across all specifications (add. controls). See Appendix 4.7 for the unabridged version of all tables of this section.

Table 4.3: Regression results for Equation 4.3, short, 2006-2009

	I		II		III	
	Baseline	Add. controls	Baseline	Add. controls	Baseline	Add. controls
TG×2007	20.156***	5.620			-11.063	-16.427
	(4.72)	(10.17)			(8.91)	(13.09)
TG×2008	39.697**	34.316**			-20.567	-25.685
	(15.44)	(15.76)			(23.20)	(23.14)
TG×2009	21.221	19.225			-50.813	-55.823
	(32.29)	(32.64)			(40.18)	(41.32)
F×TG×2007			31.270***	16.695*	37.340***	27.574**
			(5.42)	(9.87)	(8.92)	(12.72)
F×TG×2008			60.211***	58.663***	71.832***	77.079***
			(15.37)	(16.87)	(22.56)	(24.90)
F×TG×2009			50.236*	54.312*	84.799***	97.060***
			(26.82)	(30.04)	(31.00)	(36.90)
Observations	3595	3071	3595	3071	3595	3071
R^2	.0752869	.075696	.0774994	.0777302	.0780647	.0783733
Municip. FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Add. controls	no	yes	no	yes	no	yes

Notes: Fixed-effects regressions based on Equation 4.3 with standard errors in parentheses. Standard errors clustered on the municipality level. Significance levels are $^{*} < 0.10$, $^{**} < 0.05$, $^{***} < 0.01$. Administrative data on municipalities of Saxony-Anhalt. Years 2006 to 2009. The unit of observation is a single municipality. Dependent variable is municipal-level debt (in euro p.c.). The independent variables of interest are the interaction terms $TG_i \times TP_t$ between the treatment group TG_i and the treatment points TP_t with $t = 2007, ..., 2009$ and the triple interaction term $F_i \times TG_i \times TP_t$ between the municipality-specific continuous free-ride measure F_i, the treatment group TG_i and the treatment points TP_t with $t = 2007, ..., 2009$. Base year is 2006. All specifications include year fixed effects and municipality fixed effects. Add. controls include municipal-level controls for population, employees (p.c.), business and property tax revenue and county-level controls for income of private households and GDP (all in euro p.c.). All nominal values are deflated to 2000 prices. For unabridged version see Table 4.9 in Appendix 4.7.

Specification (I) generally supports Hypothesis 1a. The estimated effects for the interaction terms $TG_i \times TP_{2007}$ and $TG_i \times TP_{2008}$ for the baseline are positive and statistically significant ($\alpha_{2007} = 20.156$ and $\alpha_{2008} = 39.697$). This implies that compared to 2006, the treatment group has 20.156 euros p.c. more debt in 2007 and 39.697 euros p.c. more debt in 2008 than the control group, ceteris paribus (c.p.). From an economic perspective the size of the effect is also not negligible. The size effect corresponds to around 3% and 7% pre-reform debt stock in 2007 and 2008, respectively (the average debt of the treatment group equals 604.813 euro p.c. in 2006). Adding controls changes the size of the coefficient of $TG_i \times TP_{2008}$ slightly and results in a statistically insignificant coefficient for 2007 ($TG_i \times TP_{2007}$). The interaction term $TG_i \times TP_{2009}$ is statistically insignificant in both specifications. Specification (II) speaks in favor of Hypothesis 1b. All triple interaction terms $F_i \times TG_i \times TP_t$ (for $t = 2007, ..., 2009$) have positive and statistically significant coefficients. Based on the baseline regression, having a value of F_i close to 1 instead of 0 implies a rise in debt of 31.270, 60.211 and 50.236 (in euro p.c.) in 2007, 2008 and 2009, respectively, in comparison to 2006, c.p. Again, the sizes are of economic importance. It corresponds to up 10% (2008) pre-reform debt stock. Adding controls again changes the coefficient size, but not the overall economic and statistical significance. Specification (III) combines the discrete and the continuous effect – this approach is at the center of the previous literature. What stands out is the performance of the (triple) continuous interaction terms $F_i \times TG_i \times TP_t$. Their estimated coefficients are positive and statistically significant for all treatment points TP_t (for $t = 2007, ..., 2009$). Specification (III) suggests the superiority of the triple interaction term specification over the discrete interaction terms. The three discrete interaction terms $TG_i \times TP_t$ (for $t = 2007, ..., 2009$) are

neither individually nor jointly statistically significant. My results stress the importance of Hypothesis 1b by showing that the incentive effect is actually linked to the relative common pool size.[33] According to the baseline estimates of Specification (III), having a value of F_i close to 1 instead of 0 increases per capita debt by 37.340 (6% of pre-reform debt stock), 71.832 (12% of pre-reform debt stock), and 84.799 (14% of pre-reform debt stock) euros p.c. in 2007, 2008, and 2009, respectively, in comparison to 2006, c.p. In line with my expectations the free-riding effect is lowest in 2007 and highest in 2009. The additional controls are mainly individually insignificant (in Specification (I), (II) and (III)) and jointly statistically significant only in Specification (III).

Hypothesis 2a and b. Table 4.4 shows the main results for Equation 4.4 with the left two columns referring to the short panel (2006 to 2009). As expected, the coefficients for the interaction terms $TG1_i \times TP_t$ (for $t = 2007, ..., 2009$) are individually statistically insignificant. This supports Hypothesis 2a. Municipalities which are only absorbing or which keep their name when merging do not behave opportunistically. Next, I tackle Hypothesis 2b: In 2007 and 2008 in comparison to 2006 the municipalities of treatment group 2 – these are the municipalities which merged (but did not keep their name) or incorporated voluntarily – had 19.692 and 42.335 higher debt (in euro p.c.) than the control group, c.p.[34] As expected, the effect was even higher for treatment group 3, which comprises the municipalities which were forced to merge or incorporate ($\delta_{3,2007} = 37.683$ and $\delta_{3,2008} = 58.073$). With an average pre-reform debt stock of 564.564 and 603.920 euro p.c. for treatment groups 2 and 3 (in 2006), the sizes of the effects again are of economic relevance. For treatment groups 2 respective 3 the size of the effect corresponds to 3 respective 6% (2007) and 7 respective 10% (2008) average pre-reform debt stock. What surprises is that the coefficients of the interactions terms $TG2_i \times TP_{2009}$ and $TG3_i \times TP_{2009}$ are statistically insignificant, even though 2009 was the last year of independence for many municipalities affected by the municipal territorial reform. Again, adding controls almost does not alter the coefficients of the interactions terms $TG2_i \times TP_{2008}$ and $TG3_i \times TP_{2008}$, but affects the significance of $TG2_i \times TP_{2007}$ and $TG3_i \times TP_{2007}$. All in all, my results are well in line with Hypothesis 2b. Municipalities which identified more with the reform behave less opportunistically. While the additional controls are mainly not individually

[33] A potential explanation for the lack of significance lies in multicollinearity, which raises doubts about combining the continuous and the discrete effect. However, this approach is established by the literature.

[34] Note that these municipalities also received financial incentives, which may have reduced the degree of opportunistic behavior.

statistically significant, they are jointly statistically significant.

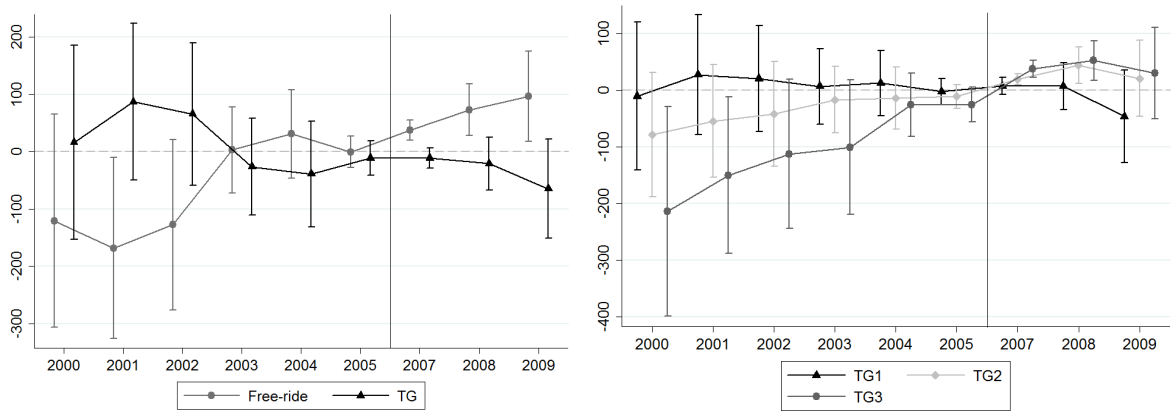
Table 4.4: Regression results for Equation 4.4

	Short (2006-2009)		Extended (2000-2009)	
	Baseline	Add. controls	Baseline	Add. controls
TG1×TP$_t$ for $t = 2000, ..., 2005$			indiv. insign.	
TG1×2007	7.005	-6.020	7.005	3.350
	(7.67)	(11.76)	(7.67)	(8.66)
TG1×2008	4.581	-4.474	7.220	2.998
	(20.84)	(21.26)	(21.14)	(21.11)
TG1×2009	-33.925	-40.324	-46.164	-52.498
	(39.00)	(39.89)	(41.69)	(41.95)
TG2×TP$_t$ for $t = 2000, ..., 2005$			indiv. insign.	
TG2×2007	19.692***	6.334	19.692***	19.548***
	(4.87)	(10.66)	(4.87)	(7.13)
TG2×2008	42.335***	41.890**	43.861***	39.609**
	(16.28)	(16.86)	(16.31)	(17.10)
TG2×2009	26.106	32.467	20.829	20.493
	(33.01)	(34.20)	(34.19)	(35.98)
TG3×TP$_t$ for $t = 2000, ..., 2005$			partly indiv. sign.	
TG3×2007	37.683***	18.297	37.683***	7.166
	(7.83)	(15.41)	(7.83)	(29.15)
TG3×2008	58.073***	57.224***	52.162***	55.765***
	(16.25)	(16.72)	(17.68)	(18.73)
TG3×2009	35.929	38.273	30.018	24.109
	(40.52)	(42.51)	(41.06)	(43.23)
Observations	3595	3071	9176	8090
R^2	.0782382	.0792284	.0226043	.024872
Municip. FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Add. controls	no	yes	no	yes

Notes: Fixed-effects regressions based on Equation 4.4 with standard errors in parentheses. Standard errors clustered on the municipality level. Significance levels are * < 0.10, ** < 0.05, *** < 0.01. Administrative data on municipalities of Saxony-Anhalt. Years 2006 to 2009 (left two columns) and 2000 to 2009 (right two columns). The unit of observation is a single municipality. Dependent variable is municipal-level debt (in euro p.c.). The independent variables of interest are the interaction terms $TGj_i \times TP_t$ between the treatment groups TGj_i with $j = 1$, 2 and 3 and the treatment points TP_t with $t = 2007$, ..., 2009 of the post-reform period. Pre-reform period 2000 to 2006 and post-reform period 2007 to 2009. Base year is 2006. All specifications include year fixed effects and municipality fixed effects. Add. controls include municipal-level controls for population, employees (p.c.), business and property tax revenue and county-level controls for income of private households and GDP (all in euro p.c.). All nominal values are deflated to 2000 prices. For unabridged version see Table 4.4 in Appendix 4.7.

Extended dataset. Next, I run equivalent regressions using the extended dataset for the years 2000 to 2009 to validate my results. Presenting a dataset with a longer pre-reform period gives insight into possible differences between treatment and control group when comparing 2006 (base year) and the other pre-reform years. If such differences exist, this would be an argument against the validity of the common trend assumption. I start by presenting the results for Equation 4.4, because they are provided in the right two columns of Table 4.4. For the baseline model of Equation 4.4 Figure 4.6 (right) graphically depicts the estimated coefficients $\delta_{1,t}$, $\delta_{2,t}$ and $\delta_{3,t}$ for the interaction terms of interest $TGj_i \times TP_t$ with $j = 1$, 2 and 3 graphically (for $t = 2000$, ..., 2005 and $t = 2007$, ..., 2009). The capped spikes display the 95% confidence intervals. The interaction terms $TGj_i \times TP_t$ between the treatment groups TGj_i with $j = 1$ and 2 and the treatment points with $t = 2000$, ..., 2005 are all individually statistically insignificant. The interaction terms $TG3_i \times TP_t$ (for $t = 2000$, ..., 2005) are in parts individually statistically significant. This raises doubt about the validity of the common trend assumption. However, what is most important is that they are not jointly statistically significant (see Table 4.12 in Appendix 4.7 for a summary on the tests for joint significance of Equation 4.3 and 4.4). The coefficients of the interaction terms of interest for the years of the post-reform period remain rather stable.

Figure 4.6: Coefficients on interaction terms for Equation 4.3 (left) and Equation 4.4 (right)



Notes: Left figure refers to the estimated coefficients α_t and β_t for the interaction terms of interest $TG_i \times TP_t$ and $TG_i \times F_i \times TP_t$, respectively (for $t = 2000, ..., 2005$ and $t = 2007, ..., 2009$) (Equation 4.3, Specification (III), Baseline). Right figure refers to the estimated coefficients $\delta_{1,t}$, $\delta_{2,t}$ and $\delta_{3,t}$ for the interaction terms of interest $TG1_i \times TP_t$, $TG2_i \times TP_t$, and $TG3_i \times TP_t$ (for $t = 2000, ..., 2005$ and $t = 2007, ..., 2009$) (Equation 4.4, Baseline). Base year is 2006. Capped spikes display the 95% confidence intervals.

Table 4.5 depicts the results when running the specifications of Equation 4.3 with the full panel (2000 to 2009). Figure 4.6 (left) graphically presents the respective estimates α_t and β_t for the interaction terms of interest $TG_i \times TP_t$ and $TG_i \times F_i \times TP_t$ (for $t = 2000, ..., 2005$ and $t = 2007, ..., 2009$), respectively, for the baseline model of Specification (III). Again, the size of the coefficients remains rather stable with those of the triple interaction terms $F_i \times TG_i \times TP_t$ (for $t = 2007, ..., 2009$) being affected the most. In Specification (I) and (III) none of the pre-reform interaction terms $TG_i \times TP_t$ (for $t = 2000, ..., 2005$) are statistically significant, which is not the case with respect to $F_i \times TG_i \times TP_t$ (for $t = 2000, ..., 2005$) in Specification (II) and (III). This is why I take a closer look at the joint significance: Specification (I) and (II) are without concern when it comes to the joint significance of the interaction terms of interest of the pre-reform period $TG_i \times TP_t$ or $F_i \times TG_i \times TP_t$ (for $t = 2000, ..., 2005$). Specification (III) suffers from jointly statistically significant interaction terms $F_i \times TG_i \times TP_t$ (for $t = 2000, ..., 2005$) for the pre-reform period. This is not unproblematic with respect to the validity of the common trend assumption. The severity of the problem might be mitigated by the fact that the two individually statistically significant interaction terms triggering this are from

early years $F_i \times TG_i \times TP_t$ (for $t = 2001$ and 2002).

Table 4.5: Regression results for Equation 4.3, extended, 2000-2009

	I		II		III	
	Baseline	Add. controls	Baseline	Add. controls	Baseline	Add. controls
TG×TP$_t$ for $t = 2000, ..., 2005$	indiv. insign.				indiv. insign.	
TG×2007	20.156***	14.962**			-11.063	-9.917
	(4.72)	(7.08)			(8.91)	(10.19)
TG×2008	40.652***	35.393**			-20.832	-16.904
	(15.49)	(15.87)			(23.65)	(23.69)
TG×2009	18.257	14.754			-64.646	-62.017
	(33.05)	(34.31)			(44.27)	(45.10)
F×TG×TP$_t$ for $t = 2000, ..., 2005$			partly indiv. sign.		partly indiv. sign.	
F×TG×2007			31.270***	25.329**	37.340***	32.244**
			(5.42)	(9.97)	(8.92)	(13.92)
F×TG×2008			61.390***	53.769***	73.230***	65.384**
			(15.56)	(17.44)	(23.16)	(25.71)
F×TG×2009			50.016	45.398	96.276**	93.235**
			(30.85)	(35.30)	(40.17)	(46.01)
Observations	9176	8090	9176	8090	9176	8090
R^2	.0191573	.0206718	.0207753	.0223042	.0211508	.0227885
Municip. FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Add. controls	no	yes	no	yes	no	yes

Notes: Fixed-effects regressions based on Equation 4.3 with standard errors in parentheses. Standard errors clustered on the municipality level. Significance levels are $^{*} < 0.10$, $^{**} < 0.05$, $^{***} < 0.01$. Administrative data on municipalities of Saxony-Anhalt. Years 2000 to 2009. The unit of observation is a single municipality. Dependent variable is municipal-level debt (in euro p.c.). The independent variables of interest are the interaction terms $TG_i \times TP_t$ between the treatment group TG_i and the treatment points TP_t with $t = 2000, ..., 2005$ and $2007, ..., 2009$ and the triple interaction term $F_i \times TG_i \times TP_t$ between the municipality-specific continuous free-ride measure F_i, the treatment group TG_i and the treatment points TP_t with $t = 2000, ..., 2005$ and $2007, ..., 2009$. Base year is 2006. Pre-reform period 2000 to 2006 and post-reform period 2007 to 2009. Base year is 2006. All specifications include year fixed effects and municipality fixed effects. Add. controls includes municipal-level controls for population, employees (p.c.), business and property tax revenue and county-level controls for income of private households and GDP (all in euro p.c.). All nominal values are deflated to 2000 prices. For unabridged version see Table 4.10 in Appendix 4.7.

Coalition effect. Table 4.13 in the Appendix considers the coalition size measure which refers to the original formulation of the "law 1/n" (see Equation 4.2). Specification (IV) includes the triple interaction terms $C_i \times TG_i \times TP_t$ between the municipality-specific continuous coalition measure C_i, the treatment group TG_i, and the treatment points TP_t with $t = 2007, ..., 2009$ as the only variables accounting for opportunistic behavior. Specification (V) adds them to Specification (III). While coalition size performs well in Specification (IV) without controls, this does not hold after adding controls. Specification (V), furthermore, points out that $F_i \times TG_i \times TP_t$ performs much better. In Specification (V) the triple interaction terms $C_i \times TG_i \times TP_t$ are jointly statistically insignificant for the post-reform period ($t = 2007, ..., 2009$) (in the short and in the extended panel). The triple interaction terms $F_i \times TG_i \times TP_t$, on the other hand, are all individually statistically significant for the post-reform period ($t = 2007, ..., 2009$) (in the short and in the extended panel). This is why this research focuses on the continuous free-ride measure referring to population size (and not, as the original formulation would suggest, to the number of municipalities).

Hypothesis 3. Last I approach Hypothesis 3. After addressing it implicitly in the previous specifications, I also explicitly examine it by using the aggregated dataset (as explained in the outline to Equation 4.5). When estimating Equation 4.5, I exclude the observations belonging to treatment group 1, since the previous results suggest that these municipalities do not engage in opportunistic behavior.[35] Table 4.6 presents the results for Equation 4.5. The interaction term $Change2008_i \times Post_p$ – which refers to

[35]This also accounts for the fact that a number of municipalities absorb or merge in more than one year.

the municipalities which merged or incorporated in 2008 – is not statistically significant in the baseline regression, but only in the one with controls. The interaction terms $Change2009_i \times Post_p$, $Change2010_i \times Post_p$ and $Change2011_i \times Post_p$ are statistically significant. Municipalities incorporated or merged in 2009, 2010 and 2011 had 90.681, 51.478 and 146.883 euros p.c. more debt in the post-reform period than the municipalities of the control group in the pre-reform period. With an average pre-reform-period debt stock of 619.238 Euro p.c. for the treatment group, again, the sizes of the effects are of economic relevance, too. Surprisingly, the coefficient $Change2010_i \times Post_p$ is the lowest (γ_{2010}). However, $Change2011_i \times Post_p$ has the highest coefficient (in the baseline regression), thus supporting Hypothesis 3 that the timing of the merger might have an impact on the degree of free-riding. This changes when adding controls.[36] In light of the results of Equation 4.3 and 4.4 (interaction terms with TP_{2009} were mainly insignificant), the results of Equation 4.5 do not justify the acceptance of Hypothesis 3. Hence, I report inconclusive results with respect to Hypothesis 3.

Table 4.6: Regression results for Equation 4.5, aggregated pre- and post-reform period

	Equation 4.5	
	Baseline	Add. controls
Change2008×Post	-13.115	277.614***
	(84.79)	(98.25)
Change2009×Post	90.681**	143.185**
	(41.53)	(70.47)
Change2010×Post	51.478*	79.653**
	(27.26)	(31.47)
Change2011×Post	146.883**	189.735**
	(65.17)	(79.16)
Observations	1716	1512
R^2	.0759741	.0757337
Municip. FE	yes	yes
Period dummy	yes	yes
Add. control	no	yes

[36]Keep in mind the data for 2010 is lacking, which concerns only municipalities which were merged or incorporated in 2011.

Notes: Fixed-effects regressions based on Equation 4.5 with standard errors in parentheses. Standard errors clustered on the municipality level. Significance levels are * < 0.10, ** < 0.05, *** < 0.01. Administrative data on municipalities of Saxony-Anhalt. Panel consists of only two time points: Observations of the pre-period (2000 to 2006) and post-period (2007 to 2009) are each aggregated to their unweighted means. All observations belonging to treatment group 1 are dropped. The unit of observation is a single municipality. Dependent variable is municipal-level debt (in euro p.c.). The independent variables of interest are the interaction terms $Change_{t,i} \times Post_p$ with $t = 2008, ..., 2011$ between the timing of the mergers or incorporation $Change_{t,i}$ with $t = 2008, ..., 2011$ and 2011 and the post-reform period dummy $Post_p$. All specifications include period fixed effects and municipality fixed effects. Add. controls include municipal-level controls for population, employees (p.c.), business and property tax revenue and county-level controls for income of private households and GDP (all in euro p.c.). All nominal values are deflated to 2000 prices. For unabridged version see Table 4.16 in Appendix 4.7.

4.5.3 Sensitivity analysis

Next, I present a number of robustness checks to validate my results.[37]

Serial correlation. I start my sensitivity analysis by explicitly addressing the issue of serial correlation (note that I employ clustered standard errors in all regressions).[38] For this I continue to use the aggregated dataset.[39] I thus run the specifications of Equation 4.3 and 4.4 using a fixed effects model with only two time points ($p = pre$ and $post$). My interaction terms of interest are $TG_i \times Post_p$ and $F_i \times TG_i \times Post_p$ and $TGj_i \times Post_p$ with $j = 1, ..., 3$ with respect to Equation 4.3 and 4.4. $Post_p$ consequently is the post-reform period dummy and is equal to 1 if $p = post$ and 0 otherwise. Table 4.15 and Table 4.16 (left two columns) in Appendix 4.7 present the results. Again, the interaction term $TG_i \times Post_p$ is statistically significant in Specification (I), but becomes insignificant in Specification (III), which combines the discrete and the continuous effect. $F_i \times TG_i \times Post_p$ is highly statistically and economically significant in Specifications (II) and (III). The interaction term $TG1_i \times Post_p$ is statistically insignificant, while $TG2_i \times Post_p$ and $TG3_i \times Post_p$ are statistically significant with a much higher coefficient for treatment group 3. The results are consistent with those using time series data. Consequently, a wrong rejection of the null hypothesis of no effect does not seem to be an issue.

[37] Since my main results do not support Hypothesis 3, I do not provide a robustness check on it.

[38] Bertrand et al. (2004) propose ignoring time series information as one possible solution to the serial correlation problem (for the case that all observations are treated at the same time).

[39] Observations belonging to treatment group 1 are included.

Self-selection. To address even the slightest concerns about self-selection into treatment I run Equation 4.3 and 4.4 again considering only those municipalities as treated for which the overall concept of regional policy left no doubt regarding their treatment and which could be clearly identified. These are municipalities with less than 1,000 inhabitants, municipalities which are part of an association of administration with a leading municipality and municipalities which belong to treatment group 3. For the baseline regression (add. controls) this reduces the number of observations from 3,595 (3,071) to 3,014 (2,493) and 9,176 (8,090) to 7,565 (6,482) in the short (2006 to 2009) and the extended panel (2000 to 2009), respectively. Table 4.17, 4.18 and 4.19 in Appendix 4.7 depict the results for Equation 4.3 and 4.4 for the short (2006 to 2009) and the extended (2000 to 2009) panel. In general the results of this robustness check confirm the previous results, even though there are some deviations with respect to size and significance.

Further robustness checks. As explained in Section 4.4.4, I identified three further potential threats to validity: a) lawsuit against initiating legislation, b) common pool with multiple mergers or incorporations and c) mergers or incorporations in pre-reform period. For the sake of brevity only an abridged version of the results for the period 2006 to 2009 (short) is provided in Table 4.20 in the Appendix. The column "Main results" depicts the results already provided in the previous section as a point of reference. Besides robustness checks a), b) and c) a combination of robustness checks a) and b) is presented, where all municipalities which were engaged in the initial lawsuit or were part of a common pool with multiple mergers are dropped. I find no systematic discrepancies. Hence, these robustness checks reveal a consistent picture supporting my main results.

Alternative control groups. Last, an alternative control group is proposed. The control group as employed in this paper so far (municipalities which were not concerned by the municipal territorial reform through merger, incorporation or absorption) is replaced by all those municipalities which were forced to merge or incorporate in the mandatory phase and filed a lawsuit against this at the constitutional court. This group of municipalities is an interesting alternative control group, since these municipalities were selected for treatment through the overall concept of regional policy.[40] Hence, they bear close resemblance to the treatment group. See Table 4.7 (bottom) in Appendix 4.7 for the number of observations by year in the alternative control group.

This group of municipalities is suitable as a control group since there is no reason to

[40]Note that this group is naturally subject to self-selection.

believe that they engaged in opportunistic behavior. The respective municipalities aimed to stay independent and fought for this at the constitutional court even after the reform had already been executed. Table 4.21, 4.22 and 4.23 in Appendix 4.7 present the results. The results of this last robustness check are twofold: The results of the estimates of the triple interaction terms $F_i \times TG_i \times Post_p$ and $TG3_i \times TP_t$ are relatively "close" to those in the main section. On the other hand, there are considerable deviations with respect to $TG_i \times TP_t$ and $TG2_i \times TP_t$. This casts doubt on the robustness of my results. Furthermore, I cannot confirm my conclusion suggested in Section 4.5.1 that the common trend with respect to the alternative control group is superior to that of the actual control group. The findings with respect to the interaction terms of interest of the pre-reform period are not more convincing than those using the actual control group.

4.6 Discussion and concluding remarks

Motivated by the mixed findings of the previous empirical literature and the recent developments in the literature on the "commons" this study explores the incentive patterns triggering free-riding behavior during the time window between the announcement of a municipal territorial reform and its actual execution. My research strategy draws on a difference-in-differences approach for the case of an interesting recent municipal territorial reform in the German federal state of Saxony-Anhalt to credibly identify the causal effects of interest. It uses a new and unique panel dataset with almost 10,000 observations from seven pre-reform years and three post-reform years.

In line with prior research I start with specifications in the spirit of Weingast et al. (1981). I confirm that being in a common pool triggers an incentive to free-ride (Hypothesis 1a). However, the size of the incentive is linked to the theoretical predictions based on the "law 1/n." It is greater, the smaller the municipality's own size relative to the size of its common pool (Hypothesis 1b). My results consequently support the findings of Hinnerich (2009) and Saarimaa and Tukiainen (2015). In the light of the recent developments in the literature on the "commons" this paper goes one step further and takes a broader perspective on the underlying incentive mechanisms. It emphasizes that there might as well be factors which reduce or prevent free-riding. Thanks to the specific features of the reform I can provide evidence that municipalities which remain accountable after the execution of the reform do not engage in free-riding. These are municipalities

which absorb other municipalities and municipalities which merge, but keep their name (Hypothesis 2a). Furthermore, I study if mergers or incorporations which are more in line with municipal preferences trigger less incentive to free-ride. My findings indeed confirm that: Municipalities which merge or are incorporate during the non-mandatory phase behave less opportunistically than those in the mandatory phase (Hypothesis 2b). These results are in line with Saarimaa and Tukiainen (2015), who only consider voluntary mergers and show that they are also affected by free-riding. Last, I study if the size of the effect is greater, the later the merger or incorporation takes places (Hypothesis 3). Contrary to Hinnerich (2009) my results are inconclusive, I cannot confirm Hypothesis 3.

Along with the previous findings, my results support the notion that governments should consider the opportunistic behavior when announcing a municipal territorial reform. In particular, in light of the economic relevance of the effects. Governments should take into account that the incentives structure differs depending on relative common pool size and further characteristics (voluntary vs. mandatory mergers and accountability). The results have important policy implications: First, to prevent (or at least mitigate) opportunistic behavior, governments could constrain municipal autonomy after a municipal territorial reform is made public. However, at least in Germany the implementation of spending restrictions might prove to be difficult due to the guarantee of municipal autonomy granted by the Basic Law. Furthermore, the findings of Blom-Hansen (2010) insinuate how difficult the design of effective spending restrictions is. Second, governments should be aware of the fact that free-riding also occurs in the case of voluntary mergers, but to a lesser extent. This is a particularly important point for Germany. Aside from extensive state-wide municipal territorial reforms, there are constantly voluntary mergers or incorporations of individual municipalities. Governments could encourage self-binding inter-municipal contracts to reduce free-riding. Third, they could mitigate the incentives to free-ride by giving the municipalities more leeway to merge or incorporate according to their preference. Last, they could reduce the number of free-riding municipalities by fostering incorporations and mergers where one municipality keeps its name. In the light of the controversy regarding the long-term benefits of municipal territorial reforms (see Footnote 7), my findings cast a further cloud over their success.

This research can be considered as a first milestone when it comes to understanding the side-effects of municipal territorial reforms in Germany. A minor caveat is that it concentrates solely on debt for investment purposes. By disregarding liquidity credits, liquid assets and asset liquidation as further channels for opportunistic behavior I might

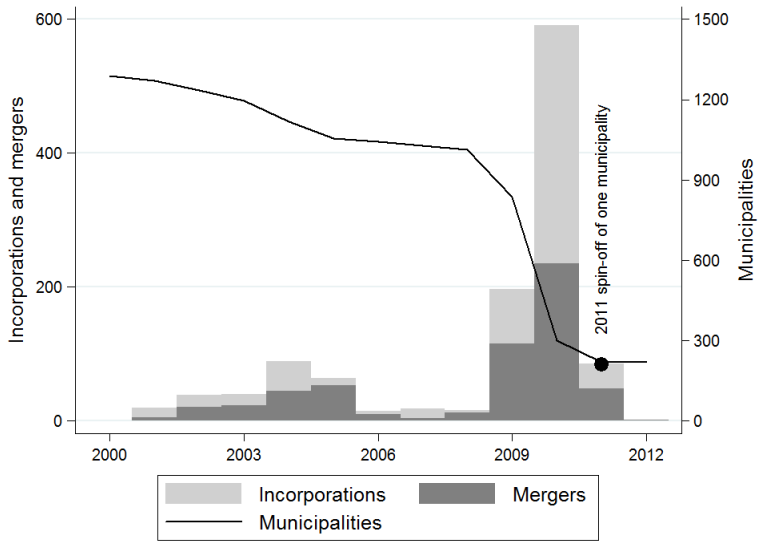
have underestimated the actual size of the free-riding effect. However, considering these aspects is challenging on various grounds (mainly data availability issues) and goes far beyond the scope of this research. Nevertheless, I have reason to believe that debt for investment purposes is the main channel. The municipalities' potential to reduce liquid assets is very limited, since German municipalities suffer from a funding shortfall. Due to the restricted time span between the announcement and execution of the reform it is very unlikely that asset liquidations are a serious issue. Liquidity credits might be a more interesting channel, but German local government law officially restricts their use to balancing short-term liquidity shortcomings. In practice, however, nowadays they are used in some federal states to finance consumption (and other) expenditures and might qualify for free-riding.[41] In those states, which strictly enforce credit rationing of liquidity credits, the institutional setting rules out free-riding on debt for consumption purposes and limits it to investment purposes. This reduces the problems related to opportunistic behavior.

To unleash a controversy at the end: To investigate the spending patterns coinciding with opportunistic behavior should be of utmost interest to the applied literature. To show that the municipalities behave opportunistically does not yet establish that they overspend and finance only inefficient public projects. In view of the German debate on underinvestment in public infrastructure, it might as well be that municipalities use the reform as a chance to finance efficient, but so far unrealized public projects which turn out to be welfare-enhancing in the long run.

[41] It would go beyond the scope of this research to study if liquidity credits were a further channel for opportunistic behavior, since this demands a careful analysis of the evolution of institutional arrangements and regulatory authorities' practice. Chapter 5 provides an introduction to the differing institutional arrangements in Germany and a first fundamental institutional analysis.

4.7 Appendix

Figure 4.7: Evolution of municipal territorial reform, 2000-2012



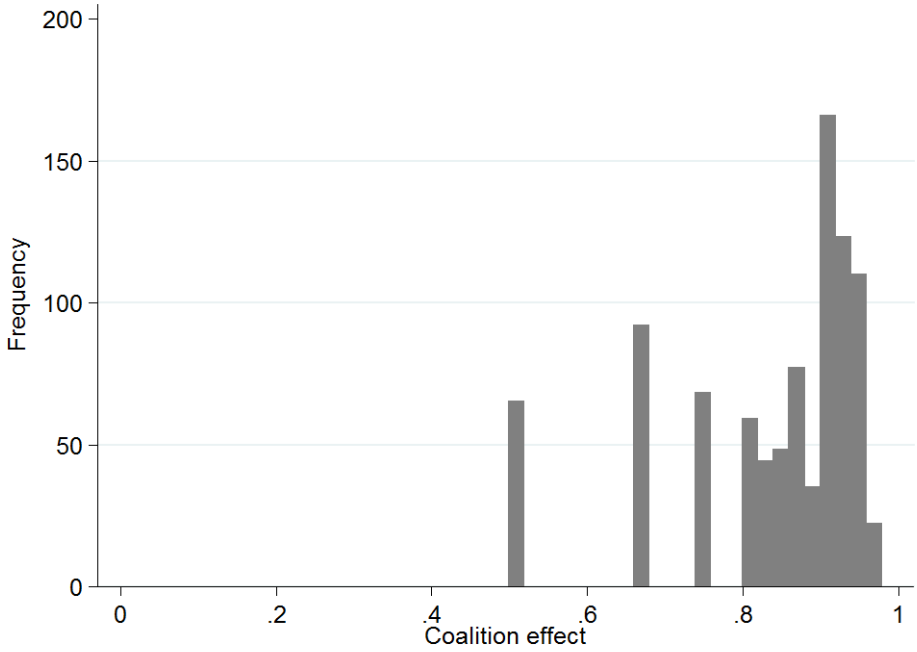
Notes: Light and dark gray bars depict number of incorporations and mergers between 2000 and 2012 (y-axis left). Black line depicts total number of municipalities between 2000 and 2012 (y-axis right).

Table 4.7: Number of municipalities by group and year, extended, 2000-2009

	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	Total
Control group	46	49	52	52	57	59	59	59	59	59	551
Treatment group	865	865	870	875	887	904	909	909	884	657	8,625
Total	911	914	922	927	944	963	968	968	943	716	9,176
	Treatment group										
Treatment group 1	79	79	82	86	93	107	110	110	102	60	908
Treatment group 2	687	687	689	689	693	696	698	698	683	498	6,718
Treatment group 3	99	99	99	100	101	101	101	101	99	99	999
Alternative control group	49	49	50	50	51	51	51	51	51	51	504

Notes: Number of municipalities of main sample by group. Middle part pinpoints the distribution by treatment groups. Bottom part provides information on the alternative control group.

Figure 4.8: Distribution of the coalition effect for treatment group, 2006



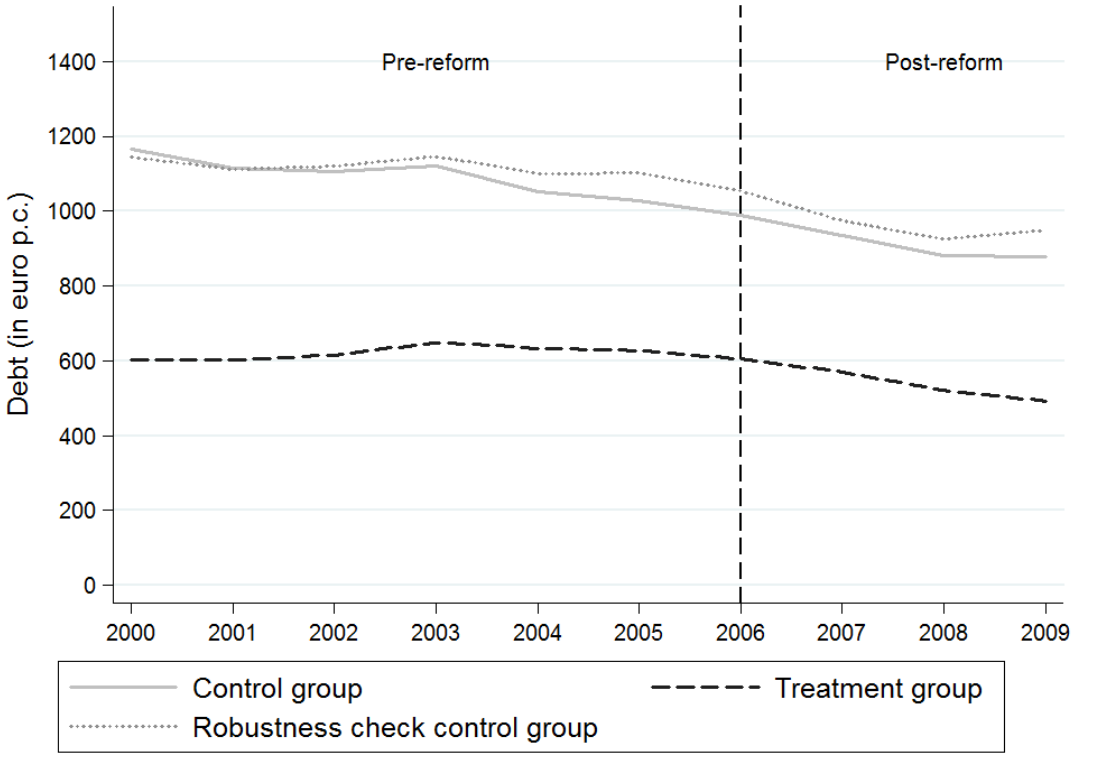
Notes: Distribution of the coalition effect measure C_i for all 909 municipalities of the treatment group in 2006.

Table 4.8: Descriptive statistics, 2000-2009

	Mean	Std. Dev.	Min.	Max.	Observations
Debt	619.852	889.642	0	19907.01	9176
TG	0.940	0.238	0	1	9176
$TG1$	0.099	0.299	0	1	9176
$TG2$	0.732	0.443	0	1	9176
$TG3$	0.109	0.311	0	1	9176
F	0.797	0.286	0	1	9176
C	0.788	0.234	0	0.967	9176
Population	1562.876	3987.382	47	46837	9176
Employees	0.172	0.23	0.009	5.003	8090
Business tax rev.	136.126	1734.37	-6977.196	81780.199	9176
Property tax rev.	56.608	34.982	-67.568	1788.82	9176
Priv. income	13231.93	385.952	12513.075	14324.152	9176
GDP	16389.194	1936.112	12977.342	22673.803	9176

Notes: Main sample for the period 2000 to 2009 with 9,176 municipalities. Debt (in euro p.c.), population, employees (p.c.), business and property tax revenue (both in euro p.c.) are municipal-level variables. Income of private households and GDP (both in euro p.c.) are county-level variables. F_i and C_i are the continuous free-riding and coalition size measures, respectively. TG_i is a dummy variable and equals 1 if the municipality is treated and 0 if not. $TG1_i$, $TG2_i$ and $TG3_i$ are equal to 1 if the municipality is part of the group of survivors, group of non-survivors (weak incentives) and the group of non-survivors (strong incentives), respectively, and 0 otherwise. All nominal values are deflated to 2000 prices.

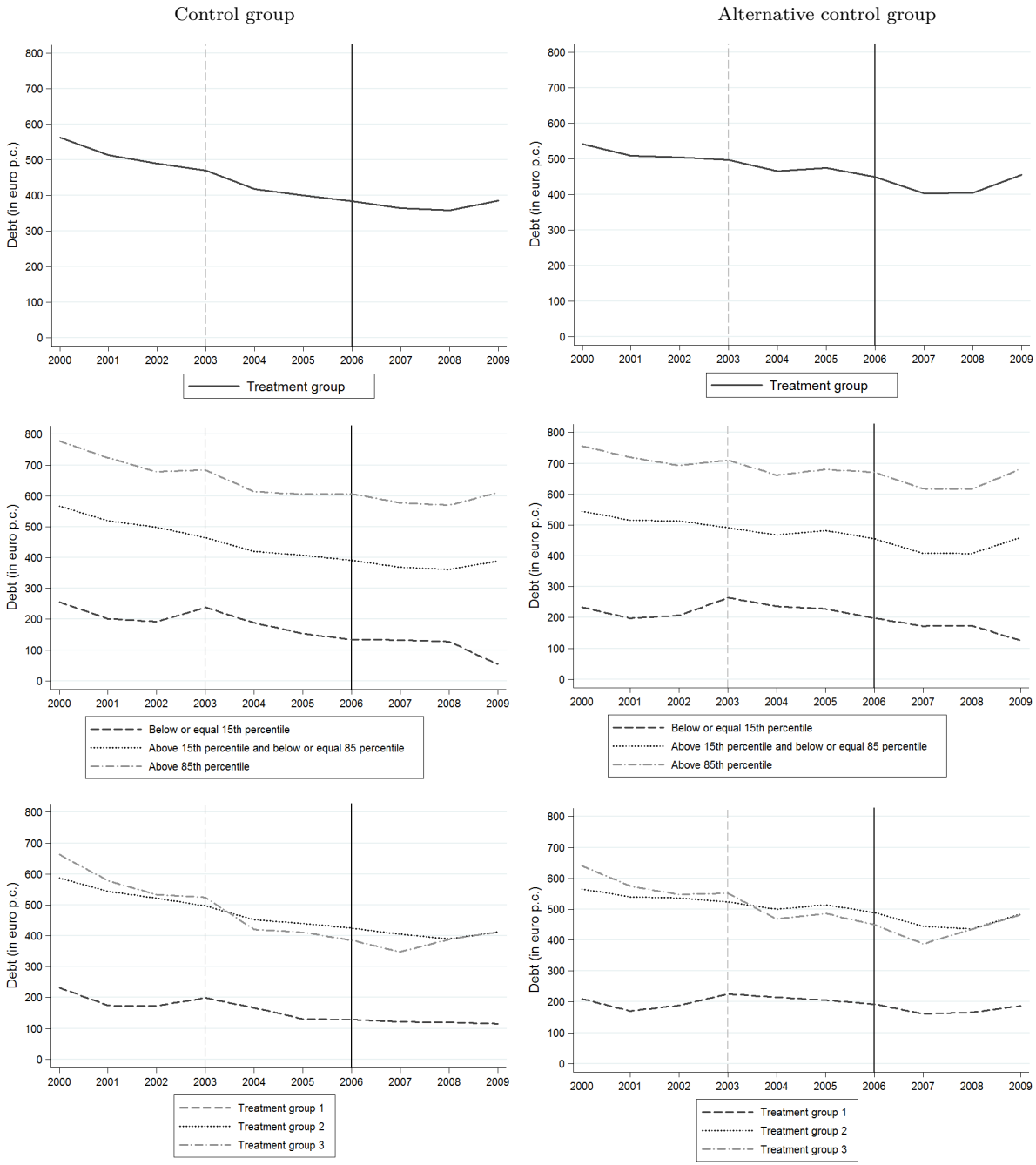
Figure 4.9: Development of average debt in euro p.c. (deflated to 2000 p.c.) by control group, alternative control, and treatment group, 2000-2009



,

Notes: Unweighted mean, deflated to 2000 prices.

Figure 4.10: Difference in average debt (in euro p.c.) between control (left)/alternative control group (right) and treatment group/differentiated treatment groups, 2000-2009



Notes: Unweighted mean, deflated to 2000 prices. Second row: Treatment group is divided into three groups depending on their value of F_i for illustrative purposes only (value of F_i below or equal to the 15th percentile, above the 15th percentile and below or equal to the 85th percentile and above the 85th percentile). Third row: Treatment groups 1, 2, and 3 refer to group of survivors, non-survivors (weak incentives), and the group of non-survivors (strong incentives), respectively.

Table 4.9: Unabridged version of Table 4.3.
Regression results for Equation 4.3, short, 2006-2009

	I		II		III	
	Baseline	Add. controls	Baseline	Add. controls	Baseline	Add. controls
TG×2007	20.156***	5.620			-11.063	-16.427
	(4.72)	(10.17)			(8.91)	(13.09)
TG×2008	39.697**	34.316**			-20.567	-25.685
	(15.44)	(15.76)			(23.20)	(23.14)
TG×2009	21.221	19.225			-50.813	-55.823
	(32.29)	(32.64)			(40.18)	(41.32)
F×TG×2007			31.270***	16.695*	37.340***	27.574**
			(5.42)	(9.87)	(8.92)	(12.72)
F×TG×2008			60.211***	58.663***	71.832***	77.079***
			(15.37)	(16.87)	(22.56)	(24.90)
F×TG×2009			50.236*	54.312*	84.799***	97.060***
			(26.82)	(30.04)	(31.00)	(36.90)
2007	-54.883***	-43.258***	-60.506***	-51.653***	-54.883***	-45.323***
	(4.14)	(9.65)	(4.49)	(8.28)	(4.15)	(9.85)
2008	-109.804***	-130.542***	-119.949***	-145.482***	-109.804***	-135.498***
	(13.75)	(18.52)	(11.51)	(17.95)	(13.75)	(18.57)
2009	-112.184***	-152.887***	-131.821***	-180.008***	-112.184***	-161.042***
	(30.50)	(45.15)	(21.73)	(37.76)	(30.52)	(45.27)
Population		0.003		-0.041		-0.058*
		(0.03)		(0.03)		(0.03)
Employees		-21.446		-11.582		-9.711
		(101.47)		(102.54)		(102.71)
Business tax rev.		-0.008		-0.008		-0.008
		(0.01)		(0.01)		(0.01)
Property tax rev.		0.440		0.464		0.472
		(0.71)		(0.74)		(0.75)
Priv. income		0.092		0.091*		0.087
		(0.06)		(0.06)		(0.06)
GDP		0.002		0.002		0.002
		(0.00)		(0.00)		(0.00)
Constant	622.746***	-622.824	622.643***	-510.423	622.594***	-418.448
	(3.79)	(766.16)	(3.79)	(746.12)	(3.78)	(769.13)
Observations	3595	3071	3595	3071	3595	3071
R^2	.0752869	.075696	.0774994	.0777302	.0780647	.0783733

Notes: Fixed-effects regressions based on Equation 4.3 with standard errors in parentheses. Standard errors clustered on the municipality level. Significance levels are * < 0.10, ** < 0.05, *** < 0.01. Administrative data on municipalities of Saxony-Anhalt. Years 2006 to 2009. The unit of observation is a single municipality. Dependent variable is municipal-level debt (in euro p.c.). The independent variables of interest are the interaction terms $TG_i \times TP_t$ between the treatment group TG_i and the treatment points TP_t with $t = 2007, ..., 2009$ and the triple interaction term $F_i \times TG_i \times TP_t$ between the municipality-specific continuous free-ride measure F_i, the treatment group TG_i, and the treatment points TP_t with $t = 2007, ..., 2009$. Base year is 2006. All specifications include year fixed effects and municipality fixed effects. Add. controls include municipal-level controls for population, employees (p.c.), business and property tax revenue and county-level controls for income of private households and GDP (all in euro p.c.). All nominal values are deflated to 2000 prices.

Table 4.10: Unabridged version of Table 4.5.
Regression results for Equation 4.3, extended, 2000-2009

	I		II		III	
	Baseline	Add. controls	Baseline	Add. controls	Baseline	Add. controls
TG×2000	-88.282	-87.110			16.222	33.534
	(53.67)	(55.01)			(86.44)	(90.64)
TG×2001	-58.496	-54.795			87.176	109.445
	(48.08)	(48.50)			(69.57)	(73.92)
TG×2002	-44.176	-44.524			65.928	78.222
	(44.43)	(46.06)			(63.39)	(65.72)
TG×2003	-24.541	-24.194			-26.367	-19.352
	(29.90)	(29.70)			(43.21)	(44.95)
TG×2004	-13.016	-26.706			-38.953	-25.186
	(26.49)	(28.66)			(47.01)	(46.80)
TG×2005	-11.629	-13.607			-11.019	-11.139
	(10.30)	(10.56)			(15.40)	(15.62)
TG×2007	20.156***	14.962**			-11.063	-9.917
	(4.72)	(7.08)			(8.91)	(10.19)
TG×2008	40.652***	35.393**			-20.832	-16.904
	(15.49)	(15.87)			(23.65)	(23.69)
TG×2009	18.257	14.754			-64.646	-62.017
	(33.05)	(34.31)			(44.27)	(45.10)
F×TG×2000			-110.515*	-119.147*	-120.771	-142.076
			(62.47)	(67.52)	(94.76)	(104.20)
F×TG×2001			-115.776**	-123.751**	-168.669**	-192.768**
			(55.87)	(59.89)	(80.53)	(89.07)
F×TG×2002			-87.071	-95.110	-127.622*	-145.153*
			(53.02)	(58.18)	(75.92)	(83.71)
F×TG×2003			-12.080	-13.136	2.832	-3.083
			(26.61)	(27.98)	(38.27)	(41.64)
F×TG×2004			8.805	-16.249	31.048	-2.797
			(22.23)	(27.95)	(39.32)	(44.55)
F×TG×2005			-6.566	-9.891	-0.595	-3.977
			(9.50)	(10.21)	(13.93)	(14.94)
F×TG×2007			31.270***	25.329**	37.340***	32.244**
			(5.42)	(9.97)	(8.92)	(13.92)
F×TG×2008			61.390***	53.769***	73.230***	65.384**
			(15.56)	(17.44)	(23.16)	(25.71)
F×TG×2009			50.016	45.398	96.276**	93.235**
			(30.85)	(35.30)	(40.17)	(46.01)
2000	110.603**	145.737***	117.480***	155.822***	110.603**	142.764***
	(47.07)	(54.67)	(42.32)	(58.45)	(47.09)	(54.37)
2001	79.722*	101.223**	119.042***	148.362***	79.722*	101.320**
	(41.73)	(47.20)	(34.08)	(49.05)	(41.75)	(47.52)
2002	77.749**	93.532**	106.989***	126.345***	77.749**	93.082**
	(37.89)	(40.88)	(31.04)	(40.49)	(37.91)	(40.99)
2003	91.906***	99.343***	78.922***	89.278***	91.906***	100.541***
	(28.16)	(30.90)	(21.51)	(24.72)	(28.17)	(31.44)
2004	53.919**	64.320**	34.933	53.434**	53.919**	67.225**
	(23.88)	(25.87)	(22.85)	(26.50)	(23.90)	(26.25)
2005	37.901***	52.976***	32.231***	47.161***	37.901***	53.098***
	(9.42)	(14.60)	(7.72)	(13.41)	(9.42)	(14.58)
2007	-54.883***	-61.350***	-60.506***	-66.629***	-54.883***	-63.205***
	(4.15)	(12.47)	(4.49)	(13.66)	(4.15)	(12.99)
2008	-109.804***	-131.498***	-120.019***	-136.513***	-109.804***	-129.150***
	(13.75)	(19.54)	(11.66)	(16.15)	(13.76)	(19.09)
2009	-112.184***	-137.373***	-135.035***	-153.037***	-112.184***	-132.211***
	(30.51)	(39.70)	(23.25)	(34.91)	(30.52)	(39.79)
Population		-0.016		-0.034**		-0.042**
		(0.01)		(0.02)		(0.02)
Employees		22.800		35.169		37.032
		(123.85)		(123.98)		(123.81)
Business tax rev.		-0.007***		-0.007***		-0.007***
		(0.00)		(0.00)		(0.00)
Property tax rev.		0.197		0.201		0.203
		(0.17)		(0.17)		(0.17)
Priv. income		0.077		0.059		0.055
		(0.05)		(0.05)		(0.05)

	I		II		III	
	Baseline	Add. controls	Baseline	Add. controls	Baseline	Add. controls
GDP		0.014		0.013		0.013
		(0.01)		(0.01)		(0.01)
Constant	615.685***	-583.288	615.472***	-308.062	615.399***	-243.154
	(7.94)	(823.28)	(7.95)	(778.28)	(7.96)	(768.35)
Observations	9176	8090	9176	8090	9176	8090
R^2	.0191573	.0206718	.0207753	.0223042	.0211508	.0227885

Notes: Fixed-effects regressions based on Equation 4.3 with standard errors in parentheses. Standard errors clustered on the municipality level. Significance levels are * < 0.10, ** < 0.05, *** < 0.01. Administrative data on municipalities of Saxony-Anhalt. Years 2000 to 2009. The unit of observation is a single municipality. Dependent variable is municipal-level debt (in euro p.c.). The independent variables of interest are the interaction terms $TG_i \times TP_t$ between the treatment group TG_i and the treatment points TP_t with $t = 2000$, ..., 2005 and $t = 2007$, ..., 2009 and the triple interaction term $F_i \times TG_i \times TP_t$ between the municipality-specific continuous free-ride measure F_i, the treatment group TG_i and the treatment points TP_t with $t = 2000$, ..., 2005 and $t = 2007$, ..., 2009. Pre-reform period 2000 to 2006 and post-reform period 2007 to 2009. Base year is 2006. All specifications include year fixed effects and municipality fixed effects. Add. controls include municipal-level controls for population, employees (p.c.), business and property tax revenue and county-level controls for income of private households and GDP (all in euro p.c.). All nominal values are deflated to 2000 prices.

Table 4.11: Unabridged version of Table 4.4.
Regression results for Equation 4.4

	Short (2006-2009)		Extended (2000-2009)	
	Baseline	Add. controls	Baseline	Add. controls
TG1×2000			-10.559	0.206
			(66.47)	(68.00)
TG1×2001			27.346	38.332
			(53.76)	(54.87)
TG1×2002			20.526	26.615
			(47.80)	(48.31)
TG1×2003			6.808	10.064
			(34.02)	(34.49)
TG1×2004			12.476	6.843
			(29.47)	(29.87)
TG1×2005			-2.619	-6.282
			(11.86)	(12.17)
TG1×2007	7.005	-6.020	7.005	3.350
	(7.67)	(11.76)	(7.67)	(8.66)
TG1×2008	4.581	-4.474	7.220	2.998
	(20.84)	(21.26)	(21.14)	(21.11)
TG1×2009	-33.925	-40.324	-46.164	-52.498
	(39.00)	(39.89)	(41.69)	(41.95)
TG2×2000			-78.676	-76.062
			(55.73)	(57.49)
TG2×2001			-54.773	-49.791
			(50.67)	(51.73)
TG2×2002			-41.626	-41.928
			(47.14)	(49.60)
TG2×2003			-16.999	-13.578
			(29.84)	(29.88)
TG2×2004			-14.221	-31.920
			(27.86)	(31.19)
TG2×2005			-10.895	-13.696
			(10.66)	(11.05)
TG2×2007	19.692***	6.334	19.692***	19.548***
	(4.87)	(10.66)	(4.87)	(7.13)
TG2×2008	42.335***	41.890**	43.861***	39.609**
	(16.28)	(16.86)	(16.31)	(17.10)
TG2×2009	26.106	32.467	20.829	20.493
	(33.01)	(34.20)	(34.19)	(35.98)
TG3×2000			-214.264**	-237.980**
			(94.29)	(103.33)
TG3×2001			-150.129**	-161.154**
			(70.53)	(75.19)
TG3×2002			-112.605*	-126.838*
			(67.10)	(73.64)
TG3×2003			-100.825*	-116.798*
			(60.66)	(66.87)
TG3×2004			-25.849	-37.413
			(28.51)	(30.54)
TG3×2005			-25.761	-29.193*
			(15.65)	(17.58)
TG3×2007	37.683***	18.297	37.683***	7.166
	(7.83)	(15.41)	(7.83)	(29.15)
TG3×2008	58.073***	57.224***	52.162***	55.765***
	(16.25)	(16.72)	(17.68)	(18.73)
TG3×2009	35.929	38.273	30.018	24.109
	(40.52)	(42.51)	(41.06)	(43.23)
2000			110.603**	148.385***
			(47.11)	(54.76)
2001			79.722*	105.635**
			(41.77)	(47.53)
2002			77.749**	96.108**
			(37.93)	(41.05)
2003			91.906***	103.379***
			(28.19)	(31.18)
2004			53.919**	69.290***
			(23.91)	(26.06)
2005			37.901***	55.257***

	Short (2006-2009)		Extended (2000-2009)	
	Baseline	Add. controls	Baseline	Add. controls
			(9.43)	(14.69)
2007	-54.883***	-45.396***	-54.883***	-65.308***
	(4.15)	(9.81)	(4.15)	(12.70)
2008	-109.804***	-136.191***	-109.804***	-130.332***
	(13.76)	(18.82)	(13.76)	(19.34)
2009	-112.184***	-161.825***	-112.184***	-131.067***
	(30.53)	(45.55)	(30.54)	(39.46)
Population		-0.055*		-0.039**
		(0.03)		(0.02)
Employees		-16.308		36.317
		(100.14)		(122.81)
Business tax rev.		-0.008		-0.008***
		(0.01)		(0.00)
Property tax rev.		0.453		0.195
		(0.74)		(0.17)
Priv. income		0.091		0.060
		(0.06)		(0.05)
GDP		0.002		0.016
		(0.00)		(0.01)
Constant	622.574***	-491.508	615.699***	-358.399
	(3.77)	(793.96)	(7.95)	(806.45)
Observations	3595	3071	9176	8090
R^2	.0782382	.0792284	.0226043	.024872

Notes: Fixed-effects regressions based on Equation 4.4 with standard errors in parentheses. Standard errors clustered on the municipality level. Significance levels are * < 0.10, ** < 0.05, *** < 0.01. Administrative data on municipalities of Saxony-Anhalt. Years 2006 to 2009 (left two columns) and 2000 to 2009 (right two columns). The unit of observation is a single municipality. Dependent variable is municipal-level debt (in euro p.c.). The independent variables of interest are the interaction terms $TGj_i \times TP_t$ between the treatment groups TGj_i with $j = 1$, 2 and 3 and the treatment points TP_t with $t = 2007$, ..., 2009 of the post-reform period. Pre-reform period 2000 to 2006 and post-reform period 2007 to 2009. Base year is 2006. All specifications include year fixed effects and municipality fixed effects. Add. controls include municipal-level controls for population, employees (p.c.), business and property tax revenue and county-level controls for income of private households and GDP (all in euro p.c.). All nominal values are deflated to 2000 prices.

Table 4.12: Testing for joint significance, extended, 2000-2009

Variable of interest		Baseline		Add. controls	
		Pre-reform	Post-reform	Pre-reform	Post-reform
Specification		Equation 4.3			
(I)	TG×TP	jointly insign.	jointly sign.***	jointly insign.	jointly sign.**
(II)	F×TG×TP	jointly insign.	jointly sign.***	jointly insign.	jointly sign.***
(III)	TG×TP	jointly insign.	jointly insign.	jointly insign.	jointly insign.
	F×TG×TP	jointly sign.*	jointly sign.***	jointly sign.**	jointly sign.**
		Equation 4.4			
	TG1×TP	jointly insign.	joinily insign.	jointly insign.	jointly insign.
	TG2×TP	jointly insign.	jointly sign.***	jointly insign.	jointly sign.***
	TG3×TP	jointly insign.	jointly sign.***	jointly insign.	jointly sign.**

Notes: Standard Wald test on composite linear hypotheses. Significance levels are * < 0.10, ** < 0.05, *** < 0.01.

Table 4.13: Regression results for Equation 4.3 with coalition, short, 2006-2009

	IV		V	
	Baseline	Add. controls	Baseline	Add. controls
TG×2007			-0.451	18.340
			(15.38)	(24.29)
TG×2008			-27.976	-17.528
			(36.78)	(39.02)
TG×2009			-63.180	-43.987
			(59.41)	(63.59)
F×TG×2007			43.429***	46.140**
			(13.29)	(18.00)
F×TG×2008			67.007**	82.471**
			(26.56)	(35.08)
F×TG×2009			75.472**	104.866**
			(37.85)	(52.56)
C×2007	24.382***	2.869	-18.784	-59.835*
	(6.20)	(11.33)	(25.00)	(33.47)
C×2008	56.603***	47.965**	13.705	-14.988
	(20.23)	(21.53)	(53.39)	(67.24)
C×2009	40.866	34.798	23.891	-21.602
	(34.92)	(35.74)	(79.82)	(97.25)
2007	-55.094***	-40.147***	-54.883***	-45.189***
	(4.96)	(9.11)	(4.15)	(9.94)
2008	-116.976***	-135.751***	-109.804***	-136.717***
	(13.39)	(19.84)	(13.76)	(18.27)
2009	-124.326***	-162.383***	-112.184***	-163.299***
	(26.72)	(41.55)	(30.53)	(45.24)
Population		-0.002		-0.061
		(0.03)		(0.04)
Employees		-16.870		-10.688
		(102.86)		(103.94)
Business tax rev.		-0.008		-0.008
		(0.01)		(0.01)
Property tax rev.		0.443		0.469
		(0.72)		(0.75)
Priv. income		0.092		0.091
		(0.06)		(0.06)
GDP		0.002		0.002
		(0.00)		(0.00)
Constant	622.677***	-595.923	622.596***	-469.055
	(3.79)	(757.09)	(3.80)	(779.66)
Observations	3595	3071	3595	3071
R^2	.0761569	.0765253	.0782065	.0786368

Notes: Fixed-effects regressions with standard errors in parentheses. Standard errors clustered on the municipality level. Significance levels are * < 0.10, ** < 0.05, *** < 0.01. Administrative data on municipalities of Saxony-Anhalt. Years 2006 to 2009. The unit of observation is a single municipality. Dependent variable is municipal-level debt (in euro p.c.). The independent variables of interest are the interaction terms $TGj_i \times TP_t$ between the treatment groups TGj_i with with $j = 1, 2$ and 3 and the treatment points TP_t with $t = 2007, ..., 2009$, the triple interaction term $F_i \times TG_i \times TP_t$ between the municipality-specific continuous free-ride measure F_i, the treatment group TG_i, and the treatment points TP_t with $t = 2007, ..., 2009$ and the triple interaction term $C_i \times TG_i \times TP_t$ between the municipality-specific continuous coalition measure C_i, the treatment group TG_i, and the treatment points TP_t with $t = 2007, ..., 2009$. Base year is 2006. All specifications include year fixed effects and municipality fixed effects. Add. controls include municipal-level controls for population, employees (p.c.), business and property tax revenue and county-level controls for income of private households and GDP (all in euro p.c.). All nominal values are deflated to 2000 prices.

Table 4.14: Regression results for Equation 4.3 with coalition, extended, 2000-2009

	IV		V	
	Baseline	Add. controls	Baseline	Add. controls
TG×2000			107.575	70.306
			(121.23)	(120.94)
TG×2001			195.929*	205.931*
			(105.30)	(111.69)
TG×2002			158.892	130.388
			(96.64)	(95.33)
TG×2003			-30.245	-33.613
			(58.93)	(63.20)
TG×2004			-92.331	-85.606
			(71.28)	(75.76)
TG×2005			-12.620	-27.223
			(23.59)	(26.00)
TG×2007			-0.451	25.956
			(15.39)	(28.19)
TG×2008			-31.945	-14.494
			(37.93)	(42.82)
TG×2009			-78.793	-51.591
			(67.78)	(72.10)
F×TG×2000			-49.597	-111.852
			(101.05)	(108.83)
F×TG×2001			-84.196	-117.387
			(77.29)	(88.15)
F×TG×2002			-55.151	-105.192
			(70.44)	(75.87)
F×TG×2003			0.821	-11.381
			(39.93)	(44.88)
F×TG×2004			-4.191	-41.441
			(39.21)	(45.57)
F×TG×2005			-1.578	-13.579
			(18.05)	(19.49)
F×TG×2007			43.429***	50.266***
			(13.30)	(19.12)
F×TG×2008			65.774**	64.766**
			(27.80)	(29.76)
F×TG×2009			82.596*	96.436**
			(47.74)	(48.93)
C×2000	-142.469**	-126.411*	-181.871	-75.040
	(70.52)	(72.86)	(154.58)	(148.45)
C×2001	-135.508**	-132.294**	-216.236*	-192.342
	(63.76)	(65.75)	(126.59)	(136.49)
C×2002	-105.388*	-94.787	-185.069	-103.317
	(59.76)	(62.57)	(113.61)	(103.40)
C×2003	-18.860	-17.355	6.627	24.987
	(31.99)	(32.58)	(65.18)	(73.03)
C×2004	15.121	-5.461	99.404	111.289
	(25.08)	(30.08)	(72.22)	(83.26)
C×2005	-9.581	-8.671	2.902	29.064
	(10.64)	(11.03)	(31.44)	(35.30)
C×2007	24.382***	12.263	-18.784	-60.709
	(6.20)	(11.70)	(25.01)	(44.71)
C×2008	58.916***	47.526**	20.739	-2.454
	(20.53)	(22.74)	(55.83)	(63.45)
C×2009	36.742	26.823	29.791	-16.158
	(37.95)	(42.13)	(93.17)	(96.47)
2000	140.353***	159.507***	110.603**	142.012***
	(46.11)	(60.09)	(47.11)	(54.22)
2001	132.049***	151.950***	79.722*	100.802**
	(40.35)	(52.80)	(41.77)	(47.43)
2002	119.528***	124.303***	77.749**	92.759**
	(36.60)	(43.68)	(37.93)	(40.95)
2003	83.758***	90.584***	91.906***	100.439***
	(24.77)	(27.30)	(28.19)	(31.43)
2004	29.809	43.676	53.919**	66.709**
	(24.75)	(27.97)	(23.91)	(26.18)
2005	34.520***	46.086***	37.901***	52.655***

	IV		V	
	Baseline	Add. controls	Baseline	Add. controls
	(8.93)	(14.63)	(9.43)	(14.43)
2007	-55.094***	-56.675***	-54.883***	-62.899***
	(4.96)	(12.88)	(4.15)	(12.91)
2008	-117.915***	-132.524***	-109.804***	-128.536***
	(13.57)	(17.49)	(13.76)	(18.96)
2009	-124.484***	-140.673***	-112.184***	-131.500***
	(27.70)	(38.07)	(30.54)	(39.85)
Population		-0.016		-0.038**
		(0.01)		(0.02)
Employees		30.001		39.147
		(123.79)		(123.89)
Business tax rev.		-0.007***		-0.007***
		(0.00)		(0.00)
Property tax rev.		0.197		0.200
		(0.17)		(0.17)
Priv. income		0.066		0.054
		(0.05)		(0.05)
GDP		0.013		0.013
		(0.01)		(0.01)
Constant	615.672***	-421.250	615.458***	-231.872
	(7.95)	(794.47)	(7.97)	(761.32)
Observations	9176	8090	9176	8090
R^2	.0205356	.0216659	.0219383	.0232575

Notes: Fixed-effects regressions with standard errors in parentheses. Standard errors clustered on the municipality level. Significance levels are $^{*} < 0.10$, $^{**} < 0.05$, $^{***} < 0.01$. Administrative data on municipalities of Saxony-Anhalt. Years 2000 to 2009. The unit of observation is a single municipality. Dependent variable is municipal-level debt (in euro p.c.). The independent variables of interest are the interaction terms $TGj_i \times TP_t$ between the treatment groups TGj_i with with $j = 1, 2$ and 3 and the treatment points TP_t with $t = 2007, ..., 2009$, the triple interaction term $F_i \times TG_i \times TP_t$ between the municipality-specific continuous free-ride measure F_i, the treatment group TG_i, and the treatment points TP_t with $t = 2007, ..., 2009$ and the triple interaction term $C_i \times TG_i \times TP_t$ between the municipality-specific continuous coalition measure C_i, the treatment group TG_i and the treatment points TP_t with $t = 2007, ..., 2009$. Pre-reform period 2000 to 2006 and post-reform period 2007 to 2009. Base year is 2006. All specifications include year fixed effects and municipality fixed effects. Add. controls include municipal-level controls for population, employees (p.c.), business and property tax revenue and county-level controls for income of private households and GDP (all in euro p.c.). All nominal values are deflated to 2000 prices.

Table 4.15: Regression results for Equation 4.3, aggregated pre- and post-reform period

	I		II		III	
	Baseline	Add. controls	Baseline	Add. controls	Baseline	Add. controls
TG×Post	58.508**	58.931**			-15.074	-55.968
	(26.92)	(28.32)			(38.23)	(46.56)
F×TG×Post			79.739***	111.412***	88.010**	148.941***
			(25.93)	(33.44)	(36.42)	(53.03)
Post	-150.199***	-131.598***	-157.861***	-152.146***	-150.199***	-120.764**
	(24.64)	(50.35)	(19.14)	(36.11)	(24.64)	(51.85)
Population		-0.005		-0.088**		-0.108***
		(0.03)		(0.03)		(0.04)
Employees		74.449		98.224		101.373
		(149.53)		(148.37)		(147.37)
Business tax rev.		-0.023**		-0.023**		-0.023**
		(0.01)		(0.01)		(0.01)
Property tax rev.		3.034*		3.137*		3.171*
		(1.69)		(1.75)		(1.78)
Priv. income		-0.081		-0.124		-0.148
		(0.10)		(0.10)		(0.11)
GDP		-0.010		-0.012		-0.012
		(0.01)		(0.01)		(0.01)
Constant	660.087***	1751.136	660.087***	2525.310*	660.087***	2866.535*
	(5.15)	(1299.08)	(5.14)	(1391.33)	(5.14)	(1547.79)
Observations	1936	1732	1936	1732	1936	1732
R^2	.0830176	.0887327	.0862291	.0942756	.0862921	.0950511

Notes: Fixed-effects regressions based on Equation 4.3 with standard errors in parentheses. Standard errors clustered on the municipality level. Significance levels are * < 0.10, ** < 0.05, *** < 0.01. Administrative data on municipalities of Saxony-Anhalt. Panel consists of only two time points: Observations of the pre-period (2000 to 2006) and post-period (2007 to 2009) are each aggregated to their unweighted means. The unit of observation is a single municipality. Dependent variable municipal-level debt (in euro p.c.). The independent variables of interest are the interaction terms $TG_i \times Post_p$ between the treatment group TG_i and the period dummy $Post_p$. Pre-reform (2000 to 2006) and post-reform (2007 to 2009) observations are aggregated to one observation each. All specifications include period fixed effects and municipality fixed effects. Add. controls include municipal-level controls for population, employees (p.c.), business and property tax revenue and county-level controls for income of private households and GDP (all in euro p.c.). All nominal values are deflated to 2000 prices.

Table 4.16: Regression results for Equation 4.4 and 4.5, aggregated pre- and post- reform period

	Equation 4.4		Equation 4.5	
	Baseline	Add. controls	Baseline	Add. controls
TG1×Post	1.629	-25.213		
	(31.83)	(33.48)		
TG2×Post	57.676**	66.546**		
	(27.71)	(28.87)		
TG3×Post	126.205***	150.721***		
	(44.02)	(50.71)		
Change2008×Post			-13.115	277.614***
			(84.79)	(98.25)
Change2009×Post			90.681**	143.185**
			(41.53)	(70.47)
Change2010×Post			51.478*	79.653**
			(27.26)	(31.47)
Change2011×Post			146.883**	189.735**
			(65.17)	(79.16)
Post	-150.199***	-108.930**	-150.199***	-220.628***
	(24.65)	(49.06)	(24.66)	(81.29)
Population		-0.099***		-0.191***
		(0.04)		(0.07)
Employees		111.159		121.021
		(153.54)		(154.50)
Business tax rev.		-0.023**		-0.016
		(0.01)		(0.01)
Property tax rev.		3.139*		1.837
		(1.75)		(2.29)
Priv. income		-0.169*		0.041
		(0.10)		(0.16)
GDP		-0.012		-0.014
		(0.01)		(0.02)
Constant	660.087***	3129.791**	626.434***	440.612
	(5.13)	(1392.44)	(5.63)	(2117.80)
Observations	1936	1732	1716	1512
R^2	.0905968	.1008895	.0759741	.0757337

Notes: Fixed-effects regressions based on Equation 4.4 (left two columns) and Equation 4.5 (right two columns) with standard errors in parentheses. Standard errors clustered on the municipality level. Significance levels are * < 0.10, ** < 0.05, *** < 0.01. Administrative data on municipalities of Saxony-Anhalt. Panel consists of only two time points: Observations of the pre-period (2000 to 2006) and post-period (2007 to 2009) are each aggregated to their unweighted means. The unit of observation is a single municipality. Dependent variable is municipal-level debt (in euro p.c). The independent variables of interest for Equation 4.4 are the interaction terms $TGj_i \times Post_p$ between the treatment groups TGj_i with with $j = 1$, 2 and 3 and period dummy $Post_p$. Equation 4.5 is only applied to the aggregated data. All observations belonging to treatment group 1 are dropped. The independent variables of interest are the interaction terms $Change_{t,i} \times Post_p$ with $t = 2008$, ..., 2011 between the timing of the mergers or incorporation $Change_{t,i}$ with $t = 2008$, ..., 2011 and 2011 and the period dummy $Post_p$. All specifications include period fixed effects and municipality fixed effects. Add. controls include municipal-level controls for population, employees (p.c.), business and property tax revenue and county-level controls for income of private households and GDP (all in euro p.c.). All nominal values are deflated to 2000 prices.

Table 4.17: Robustness check (self-selection).
Regression results for Equation 4.3, short, 2006-2009

	I		II		III	
	Baseline	Add. controls	Baseline	Add. controls	Baseline	Add. controls
TG×2007	24.493***	10.884			-2.810	-15.285
	(4.81)	(10.31)			(11.86)	(15.70)
TG×2008	40.343***	35.563**			-15.793	-27.744
	(14.54)	(14.70)			(25.91)	(28.13)
TG×2009	18.962	19.381			-38.864	-50.115
	(31.57)	(31.59)			(47.30)	(48.55)
F×TG×2007			29.193***	19.349*	31.208**	31.510**
			(5.66)	(10.44)	(12.27)	(15.95)
F×TG×2008			52.374***	53.874***	63.926***	77.020***
			(13.59)	(15.58)	(24.15)	(29.49)
F×TG×2009			32.696	41.074	65.232	85.787*
			(28.61)	(30.80)	(39.62)	(45.40)
2007	-54.883***	-43.915***	-55.850***	-49.212***	-54.883***	-45.143***
	(4.15)	(9.85)	(4.69)	(9.20)	(4.15)	(10.13)
2008	-109.804***	-122.341***	-115.019***	-133.724***	-109.804***	-126.380***
	(13.75)	(18.91)	(11.73)	(18.98)	(13.76)	(19.10)
2009	-112.184***	-140.212***	-121.075***	-157.443***	-112.184***	-147.078***
	(30.51)	(46.26)	(25.27)	(41.36)	(30.53)	(46.78)
Population		-0.035		-0.068*		-0.082**
		(0.03)		(0.04)		(0.04)
Employees		-99.554		-95.587		-94.841
		(85.02)		(84.48)		(84.62)
Business tax rev.		-0.024		-0.023		-0.023
		(0.02)		(0.02)		(0.02)
Property tax rev.		-0.428		-0.440		-0.446
		(0.41)		(0.40)		(0.41)
Priv. income		0.052		0.053		0.050
		(0.06)		(0.05)		(0.06)
GDP		0.000		-0.000		-0.001
		(0.00)		(0.00)		(0.00)
Constant	575.305***	20.885	575.174***	76.287	575.148***	144.036
	(2.95)	(732.80)	(2.95)	(713.47)	(2.94)	(739.04)
Observations	3014	2493	3014	2493	3014	2493
R^2	.1572495	.1770936	.159225	.1796468	.1597607	.1805121

Notes: Fixed-effects regressions based on Equation 4.3 with standard errors in parentheses. Standard errors clustered on the municipality level. Significance levels are * < 0.10, ** < 0.05, *** < 0.01. Administrative data on municipalities of Saxony-Anhalt. Years 2006 to 2009. Robustness check (self-selection): Treatment group is restricted to municipalities with less than 1,000 inhabitants, municipalities which are part of an association of administration with a leading municipality and municipalities belonging to treatment group 3. The unit of observation is a single municipality. Dependent variable is municipal-level debt (in euro p.c.). The independent variables of interest are the interaction terms $TG_i \times TP_t$ between the treatment group TG_i and the treatment points TP_t with $t = 2007, ..., 2009$ and the triple interaction term $F_i \times TG_i \times TP_t$ between the municipality-specific continuous free-ride measure F_i, the treatment group TG_i, and the treatment points TP_t with $t = 2007, ..., 2009$. Base year is 2006. All specifications include year fixed effects and municipality fixed effects. Add. controls include municipal-level controls for population, employees (p.c.), business and property tax revenue and county-level controls for income of private households and GDP (all in euro p.c.). All nominal values are deflated to 2000 prices.

Table 4.18: Robustness check (self-selection).
Regression results for Equation 4.3, extended, 2000-2009

	I		II		III	
	Baseline	Add. controls	Baseline	Add. controls	Baseline	Add. controls
TG×2000	-103.051*	-104.235*			47.482	55.604
	(56.10)	(58.68)			(98.89)	(103.29)
TG×2001	-65.020	-62.791			123.300	137.244
	(50.41)	(51.63)			(85.51)	(91.04)
TG×2002	-46.937	-47.964			110.900	113.489
	(46.86)	(50.15)			(79.04)	(82.24)
TG×2003	-26.772	-26.669			-13.307	-15.458
	(30.56)	(30.46)			(48.22)	(51.03)
TG×2004	-18.048	-37.012			-75.980	-62.912
	(27.63)	(31.68)			(72.33)	(71.05)
TG×2005	-12.779	-16.097			-3.359	-1.686
	(10.62)	(11.23)			(14.39)	(14.78)
TG×2007	24.466***	22.162***			-2.845	-7.258
	(4.82)	(8.09)			(11.87)	(15.42)
TG×2008	38.695***	31.224**			-26.244	-18.351
	(14.86)	(15.09)			(26.79)	(29.29)
TG×2009	12.754	6.594			-42.464	-32.964
	(32.70)	(34.93)			(51.68)	(53.83)
F×TG×2000			-131.229**	-136.002*	-168.026	-181.627
			(64.17)	(70.88)	(113.15)	(125.82)
F×TG×2001			-114.864*	-116.949*	-210.478**	-226.326*
			(60.18)	(65.06)	(104.25)	(116.02)
F×TG×2002			-89.805	-92.173	-176.445*	-183.690*
			(57.15)	(64.32)	(99.02)	(110.74)
F×TG×2003			-24.508	-21.727	-14.743	-10.809
			(27.63)	(28.98)	(43.38)	(48.37)
F×TG×2004			8.378	-18.191	65.735	29.068
			(23.10)	(30.34)	(64.31)	(68.09)
F×TG×2005			-12.836	-17.440*	-10.593	-17.124
			(8.84)	(10.40)	(11.29)	(13.72)
F×TG×2007			29.175***	28.794***	31.217**	35.844*
			(5.67)	(10.85)	(12.28)	(19.70)
F×TG×2008			54.484***	44.053***	73.853***	59.381*
			(13.79)	(16.23)	(24.83)	(30.50)
F×TG×2009			24.107	15.895	61.399	47.109
			(32.00)	(37.36)	(48.92)	(57.62)
2000	110.603**	153.083***	124.179***	165.009***	110.603**	150.547**
	(47.08)	(58.80)	(42.08)	(63.33)	(47.11)	(58.45)
2001	79.722*	104.030**	115.059***	141.727**	79.722*	104.877**
	(41.74)	(50.07)	(36.77)	(55.00)	(41.76)	(50.59)
2002	77.749**	95.286**	108.909***	125.257***	77.749**	95.272**
	(37.90)	(42.54)	(33.68)	(45.22)	(37.93)	(42.74)
2003	91.906***	98.178***	87.527***	93.659***	91.906***	100.389***
	(28.17)	(32.24)	(22.96)	(26.47)	(28.18)	(33.08)
2004	53.919**	65.230**	30.505	47.623	53.919**	68.419**
	(23.89)	(27.16)	(26.40)	(30.48)	(23.91)	(27.75)
2005	37.901***	55.616***	36.591***	54.292***	37.901***	55.679***
	(9.42)	(17.02)	(7.28)	(16.25)	(9.43)	(17.02)
2007	-54.883***	-61.517***	-55.860***	-64.192***	-54.883***	-63.822***
	(4.15)	(14.90)	(4.69)	(16.38)	(4.15)	(15.74)
2008	-109.804***	-137.031***	-118.312***	-139.414***	-109.804***	-133.472***

	I		II		III	
	Baseline	Add. controls	Baseline	Add. controls	Baseline	Add. controls
	(13.75)	(21.19)	(11.92)	(18.17)	(13.76)	(20.46)
2009	-112.184***	-146.453***	-120.217***	-145.827***	-112.184***	-138.498***
	(30.52)	(41.30)	(25.88)	(38.68)	(30.54)	(41.68)
Population		-0.023		-0.037		-0.046*
		(0.02)		(0.02)		(0.03)
Employees		-62.557		-58.108		-57.697
		(116.99)		(115.35)		(114.42)
Business tax rev.		-0.009***		-0.009***		-0.010***
		(0.00)		(0.00)		(0.00)
Property tax rev.		0.160		0.161		0.164
		(0.16)		(0.15)		(0.15)
Priv. income		0.102		0.084		0.077
		(0.06)		(0.06)		(0.06)
GDP		0.016		0.016		0.016
		(0.02)		(0.02)		(0.02)
Constant	574.071***	-969.742	573.928***	-703.351	573.886***	-605.055
	(9.15)	(963.95)	(9.16)	(911.13)	(9.17)	(880.45)
Observations	7565	6482	7565	6482	7565	6482
R^2	.0179386	.0204076	.019158	.0215078	.0196957	.0220831

Notes: Fixed-effects regressions based on Equation 4.3 with standard errors in parentheses. Standard errors clustered on the municipality level. Significance levels are * < 0.10, ** < 0.05, *** < 0.01. Administrative data on municipalities of Saxony-Anhalt. Years 2000 to 2009. Robustness check (self-selection): Treatment group is restricted to municipalities with less than 1,000 inhabitants, municipalities which are part of an association of administration with a leading municipality and municipalities belonging to treatment group 3. The unit of observation is a single municipality. Dependent variable is municipal-level debt (in euro p.c.). The independent variables of interest are the interaction terms $TG_i \times TP_t$ between the treatment group TG_i and the treatment points TP_t with $t = 2000, ..., 2005$ and $t = 2007, ..., 2009$ and the triple interaction term $F_i \times TG_i \times TP_t$ between the municipality-specific continuous free-ride measure F_i, the treatment group TG_i, and the treatment points TP_t with $t = 2000, ..., 2005$ and $t = 2007, ..., 2009$. Base year is 2006. Pre-reform period 2000 to 2006, and post-reform period 2007 to 2009. Base year is 2006. All specifications include year fixed effects and municipality fixed effects. Add. controls include municipal-level controls for population, employees (p.c.), business and property tax revenue and county-level controls for income of private households and GDP (all in euro p.c.). All nominal values are deflated to 2000 prices.

Table 4.19: Robustness check (self-selection).
Regression results for Equation 4.4

	Short (2006-2009)		Extended (2000-2009)	
	Baseline	Add. controls	Baseline	Add. controls
TG1×2000			8.150	25.280
			(59.74)	(60.95)
TG1×2001			31.749	43.497
			(49.35)	(49.90)
TG1×2002			46.173	57.902
			(43.96)	(43.66)
TG1×2003			13.652	18.598
			(33.03)	(33.43)
TG1×2004			10.218	6.273
			(26.52)	(27.28)
TG1×2005			1.682	1.621
			(11.38)	(12.10)
TG1×2007	9.485	-5.399	9.485	8.372
	(11.39)	(13.74)	(11.39)	(12.98)
TG1×2008	14.079	-2.277	14.651	11.846
	(26.58)	(28.08)	(27.01)	(27.87)
TG1×2009	-25.797	-36.544	-33.113	-40.065
	(47.22)	(46.96)	(49.74)	(50.09)
TG2×2000			-89.094	-88.103
			(58.42)	(61.72)
TG2×2001			-54.935	-50.445
			(53.36)	(55.57)
TG2×2002			-40.216	-40.947
			(49.89)	(54.52)
TG2×2003			-15.939	-12.176
			(30.29)	(30.21)
TG2×2004			-18.228	-41.458
			(29.32)	(34.82)
TG2×2005			-11.523	-15.903
			(11.03)	(11.82)
TG2×2007	23.447***	11.671	23.375***	26.494***
	(4.94)	(11.01)	(4.95)	(8.05)
TG2×2008	39.301***	38.453**	37.945**	30.157*
	(14.79)	(15.21)	(15.23)	(15.95)
TG2×2009	18.312	24.752	8.142	2.927
	(31.60)	(32.53)	(33.31)	(36.15)
TG3×2000			-214.264**	-237.013**
			(94.34)	(103.36)
TG3×2001			-150.129**	-159.723**
			(70.57)	(75.05)
TG3×2002			-112.605*	-125.023*
			(67.14)	(73.61)
TG3×2003			-100.825*	-117.216*
			(60.69)	(66.84)
TG3×2004			-25.849	-36.768
			(28.53)	(31.07)
TG3×2005			-25.761	-28.451
			(15.66)	(17.91)
TG3×2007	37.683***	20.400	37.683***	7.355
	(7.83)	(15.30)	(7.84)	(28.95)
TG3×2008	58.073***	57.647***	52.162***	55.374***
	(16.26)	(16.92)	(17.69)	(19.05)

	Short (2006-2009)		Extended (2000-2009)	
	Baseline	Add. controls	Baseline	Add. controls
TG3×2009	35.929	40.926	30.018	21.757
	(40.53)	(42.14)	(41.08)	(43.40)
2000			110.603**	157.233***
			(47.14)	(59.01)
2001			79.722*	109.869**
			(41.79)	(50.47)
2002			77.749**	98.798**
			(37.95)	(42.75)
2003			91.906***	103.652***
			(28.20)	(32.53)
2004			53.919**	70.559**
			(23.92)	(27.38)
2005			37.901***	58.110***
			(9.43)	(17.16)
2007	-54.883***	-45.354***	-54.883***	-66.117***
	(4.15)	(10.11)	(4.15)	(15.21)
2008	-109.804***	-125.724***	-109.804***	-134.819***
	(13.77)	(19.45)	(13.77)	(21.03)
2009	-112.184***	-145.397***	-112.184***	-137.258***
	(30.54)	(47.23)	(30.55)	(41.18)
Population		-0.075**		-0.040*
		(0.04)		(0.02)
Employees		-99.070		-61.574
		(83.84)		(112.77)
Business tax rev.		-0.023		-0.011***
		(0.02)		(0.00)
Property tax rev.		-0.448		0.159
		(0.41)		(0.16)
Priv. income		0.050		0.083
		(0.06)		(0.06)
GDP		-0.000		0.019
		(0.00)		(0.02)
Constant	575.173***	122.631	574.178***	-755.821
	(2.93)	(766.22)	(9.17)	(950.50)
Observations	3014	2493	7565	6482
R^2	.1594569	.1805495	.0209003	.024187

Notes: Fixed-effects regressions based on Equation 4.4 with standard errors in parentheses. Standard errors clustered on the municipality level. Significance levels are * < 0.10, ** < 0.05, *** < 0.01. Administrative data on municipalities of Saxony-Anhalt. Years 2006 to 2009 (left two columns) and 2000 to 2009 (right two columns). Robustness check (self-selection): Treatment group is restricted to municipalities with less than 1,000 inhabitants, municipalities which are part of an association of administration with a leading municipality and municipalities belonging to treatment group 3. The unit of observation is a single municipality. Dependent variable is municipal-level debt (in euro p.c.). The independent variables of interest are the interaction terms $TGj_i \times TP_t$ between the treatment groups TGj_i with $j = 1, 2$ and 3, and the treatment points TP_t with $t = 2007, ..., 2009$ of the post-reform period. Pre-reform period 2000 to 2006 and post-reform period 2007 to 2009. Base year is 2006. All specifications include year fixed effects and municipality fixed effects. Add. controls include municipal-level controls for population, employees (p.c.), business and property tax revenue and county-level controls for income of private households and GDP (all in euro p.c.). All nominal values are deflated to 2000 prices.

Table 4.20: Further robustness checks, short, 2006-2009

	Main results		a and b		a		b		c	
	Baseline	Add. controls	Baseline	Add. controls	Baseline	Add. controls	Baseline	Add. controls	Baseline	Add. controls
	Equation 4.3				I					
TG×2007	20.156***	5.620	17.015***	-0.617	19.638***	3.390	18.674***	3.531	18.674***	2.800
	(4.72)	(10.17)	(5.20)	(13.24)	(4.95)	(12.13)	(4.97)	(10.65)	(4.97)	(12.34)
TG×2008	39.697**	34.316**	45.017**	31.098	45.586***	34.514*	39.759**	34.385**	39.759**	42.995**
	(15.44)	(15.76)	(17.93)	(21.65)	(16.67)	(18.97)	(16.43)	(16.76)	(16.43)	(18.74)
TG×2009	21.221	19.225	18.179	21.185	20.613	21.676	19.907	18.953	19.907	26.930
	(32.29)	(32.64)	(38.62)	(35.53)	(37.58)	(35.13)	(33.14)	(33.60)	(33.14)	(38.12)
Observations	3595	3071	2333	2012	2993	2541	2809	2440	2809	1972
R^2	.0752869	.075696	.0607058	.0674536	.0698029	.0747954	.0644647	.0652725	.0644647	.0586299
	Equation 4.3				II					
F×TG×2007	31.270***	16.695*	28.005***	4.580	30.002***	12.231	31.088***	12.674	31.088***	5.931
	(5.42)	(9.87)	(6.48)	(14.40)	(5.81)	(12.08)	(6.11)	(11.45)	(6.11)	(13.97)
F×TG×2008	60.211***	58.663***	62.915***	53.402**	65.006***	61.059***	59.047***	55.985***	59.047***	53.607**
	(15.37)	(16.87)	(21.75)	(25.95)	(17.52)	(20.50)	(18.59)	(20.33)	(18.59)	(25.22)
F×TG×2009	50.236*	54.312*	48.767	55.699	53.443*	60.766*	47.528	50.889	47.528	50.408
	(26.82)	(30.04)	(35.85)	(38.31)	(31.89)	(34.38)	(29.87)	(33.51)	(29.87)	(40.39)
Observations	3595	3071	2333	2012	2993	2541	2809	2440	2809	1972
R^2	.0774994	.0777302	.0623458	.0691899	.0720328	.0770337	.066222	.06693	.066222	.0596455
	Equation 4.3				III					
TG×2007	-11.063	-16.427	-10.844	-9.431	-8.904	-13.348	-13.916	-14.392	-13.916	-3.191
	(8.91)	(13.09)	(10.85)	(15.95)	(9.39)	(14.28)	(10.20)	(14.49)	(10.20)	(18.71)
TG×2008	-20.567	-25.685	-11.217	-19.473	-14.713	-26.243	-17.846	-19.479	-17.846	5.039
	(23.20)	(23.14)	(27.67)	(28.36)	(24.78)	(26.16)	(25.51)	(24.20)	(25.51)	(27.73)
TG×2009	-50.813	-55.823	-53.616	-50.695	-56.752	-58.332	-48.552	-50.085	-48.552	-32.616
	(40.18)	(41.32)	(47.77)	(46.59)	(45.74)	(44.63)	(41.78)	(42.94)	(41.78)	(49.25)
F×TG×2007	37.340***	27.574**	34.600***	11.541	34.693***	21.185	39.768***	22.951	39.768***	8.879
	(8.92)	(12.72)	(11.47)	(17.88)	(9.41)	(14.41)	(10.83)	(15.59)	(10.83)	(20.90)
F×TG×2008	71.832***	77.079***	69.772**	68.512*	73.008***	79.724***	70.236**	71.041**	70.236**	51.314
	(22.56)	(24.90)	(32.21)	(35.17)	(25.18)	(28.56)	(28.08)	(29.73)	(28.08)	(37.70)
F×TG×2009	84.799***	97.060***	87.521**	96.704*	92.383**	105.765**	82.258**	90.856**	82.258**	78.408
	(31.00)	(36.90)	(43.39)	(51.76)	(36.38)	(44.07)	(36.37)	(42.61)	(36.37)	(52.54)
Observations	3595	3071	2333	2012	2993	2541	2809	2440	2809	1972
R^2	.0780647	.0783733	.0629161	.0696802	.0726905	.0776874	.0667101	.0674378	.0667101	.0599267
	Equation 4.4									
TG1×2007	7.005	-6.020	4.094	-8.542	7.450	-5.525	4.354	-8.247	4.354	-1.658
	(7.67)	(11.76)	(9.54)	(14.07)	(8.04)	(12.64)	(8.98)	(12.98)	(8.98)	(15.71)
TG1×2008	4.581	-4.474	12.343	-5.177	9.668	-6.074	7.737	-1.161	7.737	18.333
	(20.84)	(21.26)	(23.24)	(25.57)	(22.01)	(23.92)	(21.82)	(21.73)	(21.82)	(22.90)
TG1×2009	-33.925	-40.324	-42.042	-47.336	-37.405	-42.935	-37.017	-42.576	-37.017	-30.672
	(39.00)	(39.89)	(46.38)	(45.46)	(44.16)	(43.14)	(40.88)	(41.99)	(40.88)	(45.80)
TG2×2007	19.692***	6.334	18.004***	1.281	19.493***	4.214	19.250***	5.222	19.250***	3.693
	(4.87)	(10.66)	(5.44)	(14.18)	(5.06)	(12.87)	(5.24)	(11.21)	(5.24)	(12.79)
TG2×2008	42.335***	41.890**	49.418**	41.531*	49.148***	44.123**	43.829**	43.499**	43.829**	48.867**
	(16.28)	(16.86)	(19.45)	(23.81)	(17.62)	(20.20)	(17.66)	(18.62)	(17.66)	(21.29)
TG2×2009	26.106	32.467	28.108	42.684	27.473	39.874	28.372	35.582	28.372	42.698
	(33.01)	(34.20)	(39.64)	(37.92)	(38.36)	(36.80)	(34.00)	(35.59)	(34.00)	(40.47)
TG3×2007	37.683***	18.297	30.486***	1.422	41.482***	13.911	32.912***	12.835	32.912***	15.910

Table 4.20: Further robustness checks, short, 2006-2009

	Main results		a and b		a		b		c	
	Baseline	Add. controls	Baseline	Add. controls	Baseline	Add. controls	Baseline	Add. controls	Baseline	Add. controls
	(7.83)	(15.41)	(6.05)	(27.08)	(10.41)	(21.60)	(6.99)	(19.62)	(6.99)	(24.15)
TG3×2008	58.073***	57.224***	58.331***	45.057**	68.789***	58.826***	47.815***	46.700**	47.815***	65.069***
	(16.25)	(16.72)	(18.15)	(22.19)	(17.39)	(20.27)	(18.05)	(18.22)	(18.05)	(18.90)
TG3×2009	35.929	38.273	11.952	14.658	34.704	33.800	16.341	20.759	16.341	17.585
	(40.52)	(42.51)	(68.78)	(70.32)	(55.10)	(56.80)	(50.84)	(52.76)	(50.84)	(65.96)
Observations	3595	3071	2333	2012	2993	2541	2809	2440	2809	1972
R^2	.0782382	.0792284	.0635715	.0716101	.0729887	.078974	.0671558	.0685937	.0671558	.0616701

Notes: Fixed-effects regressions based on Equation 4.3 and 4.4 with standard errors in parentheses. Standard errors clustered on the municipality level. Significance levels are $^{*} < 0.10$, $^{**} < 0.05$, $^{***} < 0.01$. Administrative data on municipalities of Saxony-Anhalt. Years 2006 to 2009. Robustness check (a and b): All municipalities which are engaged in the initital lawsuit or are part of a common pool with multiple mergers are dopped. Robustness check a: All municipalities which are engaged in the initital lawsuit are dopped. Robustness check b: All municipalities which are part of a common pool with multiple mergers are dopped. Robustness check c: All municipalities affected by mergers or incorporation in pre-reform period are dropped. The unit of observation is a single municipality. Dependent variable is municipal-level debt (in euro p.c.). The independent variables of interest for Equation 4.3 are the interaction terms $TG_i \times TP_t$ between the treatment group TG_i and the treatment points TP_t with $t = 2007, ..., 2009$ and the triple interaction term $F_i \times TG_i \times TP_t$ between the municipality-specific continuous free-ride measure F_i, the treatment group TG_i, and the treatment points TP_t with $t = 2007, ..., 2009$. The independent variables of interest for Equation 4.4 are the interaction terms $TGj_i \times TP_t$ between the treatment groups TGj_i with $j = 1, 2$ and 3 and the treatment points TP_t with $t = 2007, ..., 2009$. Base year is 2006. All specifications include year fixed effects and municipality fixed effects. Add. controls include municipal-level controls for population, employees (p.c.), business and property tax revenue and county-level controls for income of private households and GDP (all in euro p.c.). All nominal values are deflated to 2000 prices. Unabridged version is not presented.

Table 4.21: Robustness check (alternative control group).
Regression results for Equation 4.3, short, 2006-2009

	I		II		III	
	Baseline	Add. controls	Baseline	Add. controls	Baseline	Add. controls
TG×2007	44.939*	40.293			48.120*	41.505
	(27.09)	(29.89)			(27.13)	(30.07)
TG×2008	57.803	52.772			63.637	58.419
	(38.58)	(40.48)			(38.74)	(40.48)
TG×2009	14.301	4.236			20.422	10.865
	(61.02)	(64.33)			(61.05)	(63.95)
F×TG×2007			33.918***	22.562	38.104***	27.051**
			(9.06)	(13.73)	(8.98)	(13.66)
F×TG×2008			66.783***	69.091***	72.412***	75.235***
			(22.32)	(26.51)	(22.49)	(26.19)
F×TG×2009			86.189***	98.111**	87.570***	99.005**
			(31.32)	(40.73)	(30.91)	(39.10)
2007	-79.666***	-75.708**	-65.622***	-59.158***	-114.704***	-101.337***
	(26.99)	(31.02)	(7.89)	(11.78)	(28.74)	(34.17)
2008	-127.909***	-159.510***	-129.549***	-167.438***	-194.494***	-228.648***
	(37.94)	(38.54)	(18.57)	(25.97)	(43.72)	(45.03)
2009	-105.265*	-157.364***	-165.028***	-234.616***	-185.788***	-248.296***
	(60.09)	(60.68)	(26.16)	(41.08)	(65.64)	(65.65)
Population		0.018		-0.059		-0.060
		(0.03)		(0.04)		(0.04)
Employees		-105.427		-103.008		-97.704
		(121.58)		(126.64)		(121.91)
Business tax rev.		-0.012		-0.012		-0.012
		(0.01)		(0.01)		(0.01)
Property tax rev.		0.493		0.539		0.523
		(0.69)		(0.72)		(0.73)
Priv. income		0.140**		0.127**		0.131**
		(0.06)		(0.06)		(0.06)
GDP		0.003		0.003		0.003
		(0.00)		(0.00)		(0.00)
Constant	623.150***	-1285.582	622.865***	-977.212	622.995***	-1026.736
	(4.11)	(785.03)	(4.08)	(760.96)	(4.10)	(788.19)
Observations	3563	3019	3563	3019	3563	3019
R^2	.0699309	.0708256	.0707353	.0714247	.0726539	.0731018

Notes: Fixed-effects regressions based on Equation 4.3 with standard errors in parentheses. Standard errors clustered on the municipality level. Significance levels are * < 0.10, ** < 0.05, *** < 0.01. Administrative data on municipalities of Saxony-Anhalt. Years 2006 to 2009. Control group is replaced by all those municipalities which were forced to merge or incorporate in the mandatory phase and filed a lawsuit against this at the constitutional court (alternative control group). The unit of observation is a single municipality. Dependent variable is municipal-level debt (in euro p.c.). The independent variables of interest are the interaction terms $TG_i \times TP_t$ between the treatment group TG_i and the treatment points TP_t with $t = 2007, ..., 2009$ and the triple interaction term $F_i \times TG_i \times TP_t$ between the municipality-specific continuous free-ride measure F_i, the treatment group TG_i, and the treatment points TP_t with $t = 2007, ..., 2009$. Base year is 2006. All specifications include year fixed effects and municipality fixed effects. Add. controls include municipal-level controls for population, employees (p.c.), business and property tax revenue and county-level controls for income of private households and GDP (all in euro p.c.). All nominal values are deflated to 2000 prices.

Table 4.22: Robustness check (alternative control group).
Regression results for Equation 4.3, extended, 2000-2009

	I		II		III	
	Baseline	Add. controls	Baseline	Add. controls	Baseline	Add. controls
TG×2000	-83.807	-165.145*			-91.405	-175.758*
	(102.24)	(95.10)			(102.90)	(96.74)
TG×2001	-52.209	-125.788			-62.748	-140.208
	(94.42)	(83.55)			(95.16)	(85.46)
TG×2002	-38.289	-96.659*			-46.210	-107.913*
	(66.59)	(55.06)			(67.77)	(57.92)
TG×2003	-30.500	-55.047			-29.966	-56.498
	(42.82)	(34.48)			(42.90)	(34.74)
TG×2004	-5.471	-35.308			-3.038	-37.410
	(32.92)	(25.22)			(32.46)	(25.29)
TG×2005	-21.775	-21.729			-21.716	-22.595
	(14.20)	(16.05)			(14.26)	(16.17)
TG×2007	44.939*	59.452*			48.120*	63.748*
	(27.09)	(34.69)			(27.14)	(34.90)
TG×2008	58.757	48.610			64.661*	54.140
	(38.61)	(39.82)			(38.76)	(39.94)
TG×2009	11.337	-0.619			17.185	5.766
	(61.43)	(64.08)			(61.62)	(64.25)
F×TG×2000			-119.430	-133.258	-127.708	-152.973
			(93.22)	(103.93)	(94.25)	(105.98)
F×TG×2001			-167.905**	-183.363**	-173.600**	-199.965**
			(78.88)	(88.27)	(80.12)	(90.53)
F×TG×2002			-127.582*	-136.075*	-131.726*	-149.009*
			(73.85)	(82.08)	(75.45)	(84.56)
F×TG×2003			3.154	-0.762	0.458	-8.207
			(38.02)	(43.78)	(38.08)	(43.90)
F×TG×2004			28.680	-1.178	28.385	-6.415
			(39.54)	(45.22)	(39.06)	(45.35)
F×TG×2005			1.227	-1.852	-0.679	-4.476
			(13.76)	(15.46)	(13.84)	(15.73)
F×TG×2007			33.918***	25.778*	38.104***	34.926**
			(9.06)	(14.52)	(8.98)	(14.28)
F×TG×2008			68.057***	63.641**	73.875***	70.183***
			(22.91)	(27.34)	(23.08)	(27.17)
F×TG×2009			98.053**	99.961**	100.415**	102.761**
			(40.00)	(46.61)	(40.02)	(46.56)
2000	106.128	223.687**	130.541*	180.490*	224.199*	362.430**
	(98.93)	(96.77)	(71.91)	(93.29)	(129.65)	(149.07)
2001	73.435	174.071**	169.582***	212.066***	233.891**	360.045***
	(91.34)	(84.98)	(55.12)	(76.05)	(115.30)	(130.88)
2002	71.863	146.192***	146.141***	170.840***	193.414**	283.858***
	(62.42)	(52.35)	(50.15)	(63.63)	(92.03)	(102.59)
2003	97.866**	134.333***	66.876**	84.855**	97.543*	146.948***
	(41.62)	(37.04)	(32.62)	(38.43)	(53.38)	(53.36)
2004	46.374	75.771***	17.156	44.534	20.273	85.841*
	(30.86)	(23.03)	(40.39)	(44.77)	(46.32)	(50.16)
2005	48.047***	60.677***	26.510**	41.214**	48.671***	64.717***
	(13.57)	(18.51)	(12.10)	(17.14)	(18.62)	(23.84)
2007	-79.666***	-107.325***	-65.622***	-72.488***	-114.704***	-141.002***
	(27.00)	(38.19)	(7.89)	(17.03)	(28.74)	(41.56)
2008	-127.909***	-138.049***	-129.821***	-145.030***	-195.839***	-198.345***

	I		II		III	
	Baseline	Add. controls	Baseline	Add. controls	Baseline	Add. controls
	(37.94)	(38.83)	(19.12)	(23.15)	(44.00)	(42.95)
2009	-105.265*	-109.444*	-179.880***	-196.312***	-197.600***	-196.564***
	(60.10)	(62.86)	(31.96)	(42.81)	(69.97)	(74.41)
Population		-0.015		-0.058**		-0.060**
		(0.02)		(0.02)		(0.02)
Employees		-100.521		-94.129		-87.457
		(194.29)		(202.42)		(194.35)
Business tax rev.		-0.008***		-0.009***		-0.009***
		(0.00)		(0.00)		(0.00)
Property tax rev.		0.122		0.118		0.126
		(0.19)		(0.20)		(0.19)
Priv. income		0.053		0.045		0.031
		(0.06)		(0.05)		(0.06)
GDP		0.015		0.013		0.014
		(0.01)		(0.01)		(0.01)
Constant	616.020***	-258.656	615.754***	-66.432	615.728***	111.369
	(8.09)	(853.01)	(8.11)	(798.55)	(8.11)	(801.28)
Observations	9129	7996	9129	7996	9129	7996
R^2	.018595	.0222896	.0198246	.0223046	.0206578	.0244516

Notes: Fixed-effects regressions based on Equation 4.3 with standard errors in parentheses. Standard errors clustered on the municipality level. Significance levels are * < 0.10, ** < 0.05, *** < 0.01. Administrative data on municipalities of Saxony-Anhalt. Years 2000 to 2009. Control group is replaced by all those municipalities which are forced to merge or incorporate in the mandatory phase and file a lawsuit against this at the constitutional court (alternative control group). The unit of observation is a single municipality. Dependent variable is municipal-level debt (in euro p.c.). The independent variables of interest are the interaction terms $TG_i \times TP_t$ between the treatment group TG_i and the treatment points TP_t with $t = 2000, ..., 2005$ and $t = 2007, ..., 2009$ and the triple interaction term $F_i \times TG_i \times TP_t$ between the municipality-specific continuous free-ride measure F_i, the treatment group TG_i, and the treatment points TP_t with $t = 2000, ..., 2005$ and $t = 2007, ..., 2009$. Base year is 2006. Pre-reform period 2000 to 2006 and post-reform period 2007 to 2009. Base year is 2006. All specifications include year fixed effects and municipality fixed effects. Add. controls include municipal-level controls for population, employees (p.c.), business and property tax revenue and county-level controls for income of private households and GDP (all in euro p.c.). All nominal values are deflated to 2000 prices.

Table 4.23: Robustness check (alternative control group).
Regression results for Equation 4.4

	Short (2006-2009)		Extended (2000-2009)	
	Baseline	Add. controls	Baseline	Add. controls
TG1×2000			-6.085	-71.248
			(109.57)	(101.34)
TG1×2001			33.632	-28.843
			(97.50)	(85.82)
TG1×2002			26.413	-23.581
			(68.93)	(56.92)
TG1×2003			0.849	-19.830
			(45.81)	(38.80)
TG1×2004			20.021	0.179
			(35.37)	(26.60)
TG1×2005			-12.765	-13.324
			(15.37)	(17.29)
TG1×2007	31.788	27.842	31.788	47.315
	(27.78)	(31.07)	(27.79)	(35.46)
TG1×2008	22.687	10.532	25.325	11.724
	(41.07)	(44.76)	(41.23)	(43.35)
TG1×2009	-40.845	-60.740	-53.083	-72.819
	(64.85)	(71.43)	(66.52)	(69.29)
TG2×2000			-74.201	-155.291
			(103.41)	(96.87)
TG2×2001			-48.486	-123.025
			(95.83)	(85.80)
TG2×2002			-35.740	-95.287
			(68.47)	(58.31)
TG2×2003			-22.958	-46.375
			(42.80)	(34.90)
TG2×2004			-6.676	-40.701
			(34.05)	(28.36)
TG2×2005			-21.041	-21.468
			(14.47)	(16.74)
TG2×2007	44.475	39.220	44.475	63.868*
	(27.14)	(29.96)	(27.15)	(34.57)
TG2×2008	60.440	55.848	61.966	50.350
	(38.95)	(40.74)	(38.98)	(40.31)
TG2×2009	19.187	10.746	13.909	3.475
	(61.44)	(64.59)	(62.09)	(65.04)
TG3×2000			-209.789	-316.454**
			(128.37)	(129.55)
TG3×2001			-143.843	-232.831**
			(107.66)	(100.88)
TG3×2002			-106.718	-179.550**
			(83.48)	(79.49)
TG3×2003			-106.784	-148.636**
			(67.98)	(67.89)
TG3×2004			-18.304	-43.827*
			(34.58)	(25.88)
TG3×2005			-35.907*	-35.574*
			(18.45)	(20.63)
TG3×2007	62.466**	51.214	62.466**	52.336
	(27.82)	(32.55)	(27.83)	(44.69)
TG3×2008	76.178*	71.000*	70.267*	67.227
	(38.94)	(40.66)	(39.57)	(41.06)

	Short (2006-2009)		Extended (2000-2009)	
	Baseline	Add. controls	Baseline	Add. controls
TG3×2009	29.009	15.264	23.098	6.104
	(65.77)	(69.62)	(66.12)	(68.83)
2000			106.128	227.871**
			(99.03)	(96.92)
2001			73.435	181.062**
			(91.43)	(85.19)
2002			71.863	150.420***
			(62.49)	(52.67)
2003			97.866**	140.699***
			(41.66)	(37.26)
2004			46.374	80.936***
			(30.89)	(23.21)
2005			48.047***	62.566***
			(13.58)	(18.72)
2007	-79.666***	-76.824**	-79.666***	-111.671***
	(27.02)	(31.09)	(27.03)	(38.25)
2008	-127.909***	-160.872***	-127.909***	-134.925***
	(37.97)	(38.73)	(37.98)	(38.81)
2009	-105.265*	-159.122***	-105.265*	-101.696
	(60.14)	(60.86)	(60.16)	(62.74)
Population		-0.054		-0.054**
		(0.04)		(0.02)
Employees		-103.973		-88.922
		(119.77)		(193.50)
Business tax rev.		-0.012		-0.009***
		(0.01)		(0.00)
Property tax rev.		0.499		0.118
		(0.72)		(0.20)
Priv. income		0.137**		0.035
		(0.06)		(0.06)
GDP		0.003		0.017
		(0.00)		(0.01)
Constant	622.977***	-1113.319	616.034***	-8.707
	(4.09)	(817.77)	(8.11)	(838.82)
Observations	3563	3019	9129	7996
R^2	.0726889	.0738118	.0219274	.026467

Notes: Fixed-effects regressions based on Equation refeq:-4.2 with standard errors in parentheses. Standard errors clustered on the municipality level. Significance levels are * < 0.10, ** < 0.05, *** < 0.01. Administrative data on municipalities of Saxony-Anhalt. Years 2006 to 2009 (left two columns) and 2000 to 2009 (right two columns). Control group is replaced by all those municipalities which are forced to merge or incorporate in the mandatory phase and file a lawsuit against this at the constitutional court (alternative control group). The unit of observation is a single municipality. Dependent variable is municipal-level debt (in euro p.c.). The independent variables of interest are the interaction terms $TGj_i \times TP_t$ between the treatment groups TGj_i with $j = 1$, 2 and 3 and the treatment points TP_t with $t = 2007$, ..., 2009 of the post-reform period. Pre-reform period 2000 to 2006 and post-reform period 2007 to 2009. Base year is 2006. All specifications include year fixed effects and municipality fixed effects. Add. controls include municipal-level controls for population, employees (p.c.), business and property tax revenue and county-level controls for income of private households and GDP (all in euro p.c.). All nominal values are deflated to 2000 prices.

Chapter 5

Tax smoothing and credit access

5.1 Introduction

In the context of the recent debt crises, political practitioners emphasize risks associated with high public debt levels, and try to design effective measures for limiting debt build-up. Public debate often portrays debt as something that is best avoided or at least strictly limited. Otherwise, countries might one day have to suffer the consequences in the form of debt crises, recessions, fiscal austerity and painful reforms to clean up the mess.[1]

On the other hand, economic theory suggests that public debt can be a useful tool to maximize social welfare. In his seminal contribution, Barro (1979) puts forward his theory of tax smoothing. From a welfare perspective, deadweight losses and hence tax rates should be smoothed over time in order to minimize distortionary costs from taxation. While the ability to run budget deficits and access debt is not a necessary condition for tax smoothing, it greatly improves chances of tax smoothing in practice. Credit rationing could thus inhibit tax smoothing. However, the proliferation of public debt and political efforts for restraining debt demonstrate that it is not used solely for this laudable purpose.[2] Rules on debt and budget deficits may therefore be warranted.

[1] In the aftermath of Reinhart and Rogoff's (2010) disputable contribution "Growth in a time of debt" a big controversy on the risk of high debt levels unleashed among economist and politicians. Advocates of fiscal austerity policy underpinned their call for an end to expansionary policy with Reinhart and Rogoff's (2010) research (they claim that there is a link between rising debt level and weak economic growth). However, the replication by Herndon et al. (2014) casts doubt on this research, nurturing the critics.

[2] Further purposes of debt established by economic theory are counter-cyclical fiscal policy or intergenerational equity.

The question about whether debt access should be limited is not only relevant at the national level. In federal states where subnational levels of governments enjoy tax autonomy, higher-level governments set the legal framework which determines budget and debt rules for lower levels. As this paper argues, governments may face a trade-off between enforcing budget discipline and easing tax smoothing when designing these rules. According to the standard recommendation of the fiscal federalism literature, lower levels of government should not be granted access to debt, at least not for financing current deficits (e.g., Oates 2005). However, ruling out debt finance could prevent subnational governments that enjoy tax autonomy over certain taxes from engaging in welfare-enhancing tax smoothing.

This paper investigates the empirical relationship between credit access and tax smoothing using the case of municipalities in Germany. Two distinguishing characteristics make German municipalities particularly suitable for the analysis: First, all municipalities in Germany enjoy autonomy over the business tax and the property tax, with the former being highly volatile. Their (in)ability to smooth these tax rates over time is thus a salient issue. Second, in the last two decades some German federal states have eased their municipalities' access to credits, while other states still strictly enforce credit rationing of municipalities for current expenditures. Hence, the case of German municipalities constitutes an ideal setting to analyze whether credit access is used by policy makers to smooth taxes. Given local tax autonomy and the variation in local credit rationing amongst German federal states, this paper asks: Are local tax rates less volatile in federal states where municipalities have easy access to credits?[3] If the answer is affirmative, there is some benefit to allowing public debt and the aforementioned trade-off between tax smoothing and budget discipline arises. If the answer is negative, the potential benefits of credit access do not materialize in practice and local credit rationing is advisable.

We contribute to the literature, first, by developing and using an innovative and intuitive approach to investigating the strength of tax smoothing behavior. In contrast to existing studies, we do not ask whether governments do or do not engage in tax smoothing. Instead, we analyze whether tax rates become smoother and thus closer to Barro's ideal if the institutional setting becomes more accommodating. We focus on smoothing taxes over the business cycle, factoring out the structural, long-term tax smoothing component. The sizable empirical and theoretical literature on tax smoothing following the pioneering work

[3]Note, in this chapter we use tax rate as synonym for tax multiplier.

of Barro (1979) and Bohn (1990) typically defines tax smoothing as constant tax rates in the case of perfect foresight. In case of imperfect foresight tax rates are changed whenever new information about the expected permanent expenditure path arises.[4] On this basis we define "smoother tax rates" as tax rates that are subject to a smaller number of changes within a given time period. This measure constitutes an intuitive and straightforward translation of theoretical tax smoothing definitions found in the literature into the context of local tax policy. Our approach also allows us to use tax rate data and investigate tax rate volatility directly.[5] This is in contrast to the indirect tax smoothing tests prevalent in existing literature which rely on the behavior of the budget balance or government expenditure over time.[6]

Second, we establish a link between tax smoothing and actual credit access that is to the best of our knowledge almost entirely missing from the literature to date. While credit access is typically not an issue at the national level, it becomes crucial once we move to subnational levels of government.[7] The empirical analysis yields an original index of local credit access based on the differing institutional, legal and administrative environment in each state. This index allows us to investigate the relationship between credit access and the strength of tax smoothing at the local level based on a dataset of over 10,000 municipalities in all German territorial states. In our empirical analysis we find no evidence in favor of a tax smoothing effect of credit access.

Third, we show that a rising number of German municipalities is making abusive

[4]See, e.g., Adler (2006), Bohn (1990), Ghosh (1995) and Strazicich (1997).

[5]We draw on the tax rate (tax multiplier) instead of the tax revenue, because German municipalities enjoy autonomy over it. The federal level defines the basic tax rate and the tax base on the other hand (see Section 5.2.1 for more information). Along with other local policies the municipality's tax rates choice might implicitly affect the tax base by attracting business and property. See Chapter 3 for more on this regard.

[6]Indirect tests of tax smoothing typically rely on a theoretical model which derives a clear relationship between tax rates and the budget surplus. They use the theoretical model to predict how the budget surplus should evolve if the government engages in tax smoothing. There is an array of different econometric tests (depending on the particular question, the available data, and model) to examine if the development of the budget surplus found in the actual data is in line with the predicted path of the model. Depending on the degree of divergence tax smoothing is confirmed or rejected. See, e.g., Adler (2006), Barro (1979, 1995), Bohn (1990), Olekalns (1997) and Strazicich (1997).

[7]Most existing studies focus on tax smoothing at the national level, where free credit access can be assumed. Notable exceptions are Strazicich (1997) and Reitschuler (2010). Strazicich (1997) investigates tax smoothing at the subnational level with data from states of the United States and Canadian provinces, pointing out that results differ by federal level. Reitschuler (2010) introduces the notion that fiscal rules such as those of the European Union, which limit debt access, may inhibit tax smoothing.

use of easier credit access and carries a high burden of short-term debt. So-called liquidity credits that are meant to fill temporary liquidity gaps are increasingly used to cover persistent budget deficits over a medium- to long-term horizon. Our results suggest that granting credit access is an (un)successful attempt of the respective federal states to compensate for a lack of sufficient revenue to cover rising spending needs and to limit upward pressure on tax rates. Credit access triggers unsustainable behavior, making calls for bailouts more likely. Our paper provides support for the notion that strict credit rationing of the local level may be the best institutional choice for higher-level governments even if there is substantial tax autonomy at the local level.

Section 5.2 starts by explaining the institutional setting. Section 5.2.1 portrays the rules governing local borrowing in the territorial states and provides information on local tax autonomy. Section 5.2.2 details the construction of an original index of local credit access and proposes a measure for tax rate volatility. In Section 5.3 we provide evidence against a contribution of credit access to tax smoothing; we present it in four steps. The first three steps in Section 5.3.1 are based on a descriptive analysis. This is complemented with an econometric approach in Step 4 in Section 5.3.2. Section 5.4 summarizes, discusses the findings and concludes.

5.2 Local credit access and tax autonomy

In the first part of this section we provide information on the institutional background: We outline the rules governing local borrowing in the territorial states and then turn to local tax autonomy. In the second part of this section we present our index of credit access and introduce our measure for tax rate volatility.

5.2.1 Institutional background

Local credit access. Federal states grant municipalities varying access to debt. The legal framework for local government finances is defined by local government laws (*Gemeindeordnungen*) and associated regulation in each state. In the following, we analyze legal provisions and their evolution in each of the 13 territorial states. The three city-states, in which a separate municipal level does not exist, are excluded.

When talking about local public debt in Germany, debt for consumption and investment purposes needs to be distinguished. As per state law in all states and contrary to the

state and federal level, municipalities may only go into debt to finance investment, investment assistance, or to restructure existing debt. Formally, debt for consumption purposes is ruled out. Accordingly, local government laws in all states stipulate that municipalities ought to balance their budget each year.

However, liquidity gaps that may arise throughout the budget year even when revenues do not fall short of expenditures make (short-term) credits for consumption purposes a necessity. Unlike financing credits, which are used to fund investment, such liquidity credits (*Liquiditätskredite* or *Kassenkredite*) are not part of local revenues in a legal sense. They are meant to ensure the timely settlement of liabilities and to fill short-term liquidity gaps until "real" revenues become available.[8]

In principle, municipalities cannot plan a budget deficit. This would seemingly constitute a violation of the balanced budget rules that are enshrined in all local government laws. At first sight, it thus seems that there is barely any scope for tax smoothing at the local level. In practice, however, (planned) budget deficits are commonplace.

First, the balanced budget rules of some states give municipalities some leeway by allowing the use of reserves to finance temporary deficits. Second, the regulatory authority in question may reject unlawful budgets, but it is not obliged to intervene. Regulatory authorities may tacitly approve deficits. Third, even if the budget is rejected, the deficit does not necessarily disappear. Instead, municipalities without a valid budget operate under the rules of provisional budget management, which allows them to carry out their most important tasks and, among other things, levy taxes at previous rates. Changes to tax rates are typically prohibited.

In addition, budgets containing a deficit can be made legal under certain circumstances. A number of states require municipalities that do not achieve budgetary equilibrium to submit a budget consolidation plan, which specifies how the municipality will return to budget balance in the coming years. In most cases, this plan requires approval by the regulatory authority. If approval is denied, the municipality once again finds itself under provisional budget management. Although they were intended as an instrument for enhancing fiscal sustainability, budget consolidation plans also provide a legal avenue for running deficits without the restrictions of provisional budget management. What was once devised as an arrangement for fiscal emergency situations has become widespread. Estimates claim that about a third of all municipalities in Germany was operating either

[8]Given that liquidity credits are not classified as revenues, they cannot be used to balance the budget.

under a budget consolidation plan or provisional budget management as of 2010 (Spars et al. 2010).

All local government laws contain a paragraph specifically on the purpose of and rules relating to liquidity credits. The wording of these paragraphs gives a first indication of the degree of local credit rationing in the respective state. Table 5.9 in Appendix 5.5 provides an overview of the current wording of these paragraphs in each of the federal states. All provisions emphasize that liquidity credits serve the timely settlement of expenses and the prevention of late payments. This purpose implies their temporary nature. Another common theme is the subsidiarity of liquidity credits: They may only be used if no other funds are available. Moreover, all local government laws require the municipality to define a ceiling up to which liquidity credits may be taken in a given budget year.[9] In all states except Brandenburg, this ceiling must be set in the budget by-law. The budget by-law typically has to be submitted to the competent regulatory authority, which checks its lawfulness.[10] In addition, some states specify that liquidity credit ceilings have to be approved by the regulatory authority if they exceed a threshold (Baden-Württemberg, Hesse, Lower Saxony, Mecklenburg-Vorpommern, Saxony, Saxony-Anhalt, Thuringia), which is defined relative to the municipalities' expenses or revenues. Bavaria goes one step further by ruling out credit ceilings above a threshold level.

A higher degree of local credit rationing can be assumed in states where approval clauses for liquidity credit ceilings (or fixed ceilings) exist. In these states, regulatory authorities are required by law to formally approve or reject problematic credit ceilings. A priori, this suggests stricter oversight. It also gives regulatory authorities an additional lever to limit municipal budget deficits, apart from rejecting budgets or, where applicable, budget consolidation plans.

State regulations regarding liquidity credit ceilings have evolved over time. In fact, all local government laws included an approval clause of credit ceilings at one time or another. Table 5.1 lists the most recent approval clauses that once were or still are in effect in state laws.[11] The time of abolishment of approval clauses varies greatly across

[9] This ceiling continues to hold under provisional budget management.

[10] In case of cities with county status and some big non-associated cities, the state interior ministry is usually the competent regulatory authority. In some states, lower public authorities fulfill this role. For municipalities belonging to a county, regulatory control is exercised by the county.

[11] Where current local government laws still contain approval clauses, the state listing corresponds to Table 5.9.

states. North Rhine-Westphalia and Rhineland-Palatinate were the first to abolish such clauses in 1994. Brandenburg enacted the most recent abolishment in 2008. Interestingly, there seems to be a trend reversal given that two states, Hesse and Saxony-Anhalt, have reintroduced approval clauses in recent years. This may have been in response to soaring levels of short-term local debt.

Table 5.1: Abolishment and introduction of approval clauses

Federal states	Approval clause abolished on	Wording of current or abolished approval clause
Baden-Württemberg	–	§ 89 (3) GemO The liquidity credit ceiling set in the budget by-law requires approval by the regulatory authority if it exceeds one fifth of the profit and loss budget's ordinary expenses.
Bavaria	01.09.1997	Art. 73 (2) GO The liquidity credit ceiling set in the budget by-law requires approval if 1. the ceiling for the core budget exceeds one sixth of the administrative budget's revenues, 2. the ceiling for owner-operated municipal enterprises exceeds one sixth of the profit plan's revenues.
Brandenburg	01.01.2008	§ 87 (2) GO The liquidity credit ceiling set in the budget by-law requires approval by the regulatory authority if it exceeds one sixth of the administrative budget's revenues.
Hesse	01.01.1999	§ 105 (2) HGO The ceiling set in the budget by-law requires approval by the regulatory authority if it exceeds one fifth of the administrative budget's revenues.
Lower Saxony	–	§ 122 (2) NKomVG The ceiling set in the budget by-law requires approval by the regulatory authority if it exceeds one sixth of the cash-flow budget's revenues for current administrative activities.
Mecklenburg-Vorpommern	–	§ 53 (3) KV M-V The liquidity credit ceiling set in the budget by-law requires approval by the regulatory authority if it exceeds ten percent of the cash-flow budget's current revenues for administrative activites.
North Rhine-Westphalia	17.10.1994	§ 74 (2) GO NRW The ceiling set in the budget by-law requires approval by the regulatory authority if it exceeds one sixth of the administrative budget's revenues.
Rhineland-Palatinate	12.06.1994	§ 105 (2) GemO The ceiling set in the budget by-law requires approval by the regulatory authority if it exceeds one sixth of the administrative budget's revenues.

Federal states	Approval clause abolished on	Wording of current or abolished approval clause
Saarland	01.01.2007	§ 94 (2) KSVG The ceiling set in the budget by-law requires approval by the regulatory authority if it exceeds one sixth of the administrative budget's revenues.
Saxony	–	§ 84 (3) SächsGemO The liquidity credit ceiling set in the budget by-law requires approval by the regulatory authority if it exceeds one fifth of the profit and loss budget's ordinary expenses.
Saxony-Anhalt	31.08.2003	§ 91 (2) GO LSA The liquidity credit ceiling set in the budget by-law requires approval by the regulatory authority if it exceeds one fifth of the administrative budget's revenues.
Schleswig-Holstein	31.03.2006	§ 87 (2) GO The ceiling set in the budget by-law requires approval by the regulatory authority.
Thuringia	–	§ 65 (2) ThürKO The ceiling set in the budget by-law requires approval if 1. the ceiling for the core budget exceeds one sixth of the administrative budget's revenues, 2. the ceiling for owner-operated municipal enterprises or municipal institutions exceeds one sixth of the profit plan's revenues.
	Approval clause reintroduced on	Wording of reintroduced approval clause
Hesse	24.12.2011	§ 105 (2) HGO The ceiling set in the budget by-law requires approval by the regulatory authority.
Saxony-Anhalt	01.07.2014	§ 110 (2) KVG LSA The liquidity credit ceiling set in the budget by-law requires approval by the regulatory authority if it exceeds one fifth of the cash budget's revenues for current administrative activities.

Notes: Local government laws. Translation by authors.

Yet, the effective degree of credit rationing does not only depend on the letter of the law, but also on its enforcement by regulatory authorities. For instance, approval clauses would not make a difference if all credit ceilings were approved indiscriminately. The administrative and political culture in each federal state may have an impact on how rules are interpreted when legal terms leave scope for discretion. For example, the requirement of a balanced budget and the stated purpose of liquidity credits imply that maturities should normally not exceed one fiscal year. However, local and state policy makers have started to argue that the law does not stipulate rules on permissible maturities for liquidity credits. On these grounds, credit periods of several years have been declared

acceptable. The political weight, personal beliefs and competence of individual decision-makers both in municipalities and regulatory authorities may also influence enforcement. In addition, the equipment and staffing of regulatory authorities may be pivotal both in their capacity to detect breaches in the rules and in their decision to intervene or to refrain from intervention when they do. Capacities may soon be exhausted when a large share of municipalities within the authority's jurisdiction is struggling with fiscal weakness and mounting liquidity credit stocks.

In order to grasp differences in law enforcement and interpretation across states, Table 5.10 in Appendix 5.5 provides information on relevant state circular decrees, ordinances and other official statements regarding the treatment of municipal liquidity credits and budget deficits. Through decrees and ordinances, state governments give instructions that are binding for regulatory authorities (Heinemann et al. 2009). Other official statements also shed light on the prevalent interpretation of legal rules.

A couple of findings emerge from Table 5.10. First, strictness of state enforcement and interpretation of their – very similar – legal rules relating to balanced budgets and liquidity credits differ markedly. While many states show a progressive trend towards a loosening of rules by lengthening acceptable maturities or allowing larger permanent stocks of liquidity credits, others maintain a stricter reading of the law. Moreover, the instructions given to regulatory authorities once again illustrate that a breach of the balanced budget rule often becomes acceptable once municipalities submit a budget consolidation plan. The circular decrees also highlight that there is a link between such budget consolidation plans and local tax policy, as regulatory authorities are urged to ensure that tax rates are sufficiently high in the municipalities in question. Provisional budget management, much like budget consolidation plans, has become widespread and has triggered increasingly lax responses by the authorities.

Thus, even if liquidity credits are not intended to be a tax smoothing tool, local governments may use them as such: Municipalities located in states where they can access liquidity credits to fill budget deficits may delay or even altogether avoid adjustments to their business and property tax rates. The question remains whether they make use of this theoretical possibility in practice.

Tax autonomy. Municipalities in Germany draw revenues from a variety of sources such as taxes, transfers from the state level, and duties and charges. Among the taxes,

two stand out: The business tax (*Gewerbesteuer*) and the property tax (*Grundsteuer*)[12] not only make up a significant share of local revenue, but in contrast to most other taxes their tax rates are set autonomously by municipalities. Every year, each of the more than 11,000 municipalities in Germany sets its own tax multipliers. In 2013, gross business tax revenue in Germany summed up to 43 billion (bn) euros while property tax revenue stood at nearly 12 bn euros. Net revenue from business and property taxes accounted for 15.8% and 5.8% of aggregate municipal income, respectively.

German municipalities do not enjoy full autonomy over business taxation. The federal government uniformly defines the tax base as well as a basic tax rate (*Steuermesszahl*)[13] for the entire country. The actual business tax rate, which will be levied on firm profits, is determined by multiplying the basic federal rate with a multiplier (*Hebesatz*) set by each municipality. Municipalities are free in their choice of a multiplier, as long as it does not fall below 200%.[14]

As in the case of business taxation, the property tax base and a basic tax rate are set at the federal level. The tax base is constituted by the so-called uniform values (*Einheitswerte*) of the property, which are meant to reflect the property's value as of a reference date (not its current market value) and which are fixed by federal tax authorities according to standardized procedures. Municipalities in turn fix the multiplier that produces the final tax rate in conjunction with the basic federal rate.[15] The latter is not uniform. It depends on the type of property to be taxed and its location (East versus West Germany).[16] For simplicity, we will refer to local business tax multipliers and local property tax multipliers as business tax rates and property tax rates in the remainder of this paper.

Business and property tax rates have to be set in the budget by-law at the beginning of each year. The budget by-law and hence applicable tax rates may be amended until the end of the budgetary year. However, tax rates changes within the budgetary year are rare in practice.

[12]When referring to the property tax, we mean the *Grundsteuer B*, which is one of two types of property taxes in Germany. The other, called *Grundsteuer A*, which is also levied by municipalities, only accounts for roughly 3% of total property tax revenue.

[13]The basic federal tax rate is currently set at 3.5%.

[14]This floor was introduced in 2004 in order to restrain detrimental tax competition. However, only a very small number of communities had multipliers below the threshold prior to the reform.

[15]There are no restrictions on admissible property tax multipliers.

[16]It currently varies between 2.6‰ and 10‰.

5.2.2 Measuring credit access and tax rate volatility

An index of credit access. In order to assess the impact of local credit access on the volatility of local tax rates, the information on institutional features discussed in Section 5.2.1 needs to be condensed into an index of credit rationing and credit access. The index presented in this paper reflects the qualitative and quantitative analysis of credit access in all states. The index takes into account observable institutional characteristics that have an influence on local credit access, as discussed in Section 5.2.1. We also aim to capture unobservable institutional characteristics relating to the enforcement of legal rules by regulatory authorities. To do so we consider quantitative information on local liquidity credit stocks. The resulting index will be the primary variable of interest in the descriptive analysis and the empirical model presented in Section 5.3.

More precisely, the qualitative part of the index accounts for institutional characteristics that were found to ease or limit credit access for municipalities. Three of the included characteristics relate to the existence and design of approval clauses for liquidity credit ceilings (Component 1). The fourth takes into account circular decrees or ordinances which expand local credit access by extending permissible maturities or allowing larger permanent stocks of liquidity credits, amongst others (Component 2). Yet, as discussed in Section 5.2.1, the effective degree of credit rationing does not only depend on the letter of the law, but also on the way in which regulatory authorities choose to act when it comes to approving credit ceilings or responding to rule breaches, for instance. Circular decrees can shed some light on the practice of regulatory authorities, but a lot remains unobservable. In part, such unobservable parameters may be captured by quantitative information on local liquidity credit stocks, which is thus used to complement the qualitative side of the index. By using data on local liquidity credit stocks, we assess the effective strength and credibility of the rules which also flow into the index (Component 3 and 4). Data on liquidity credits were obtained from North Rhine-Westphalia's statistical office. It is a panel dataset covering all German municipalities from 1998 to 2013.[17]

We decided to put equal weight on the qualitative and quantitative information (with a heavier weight on Components 1 and 3). We assessed our decisions with respect to index construction and our scoring system carefully. For instance, we talked with a number of experts working in the field of local government finance in Germany to get a more detailed

[17]The percentiles employed in the index construction are calculated using the full, unbalanced panel of all municipalities.

understanding on the nature of the evolution of local debt rules in Germany and to reflect our ideas. The credit access index starts from zero (no credit access/complete credit rationing). Index scores are assigned as follows.

We begin by considering the qualitative information gathered in Section 5.2.1. We translate it into index points running from zero to three. Component 1 concerns the wording of the approval clause:

- 0 points if the local government law contains an unconditional approval clause or an upper limit for liquidity credit ceilings (Component 1.1),
- + 1 point if the local government law contains a conditional approval clause for liquidity credit ceilings (Component 1.2),
- + 2 points if the local government law contains no approval clause for liquidity credit ceilings (Component 1.3).

Component 2 concerns circular decrees or ordinances:

- + up to 1 point if circular decrees are in force which expand credit access beyond what is suggested by the letter of the law, but 0 points if there is no relevant circular decree in place (Component 2).

Quantitative information on yearly liquidity credit stocks in all municipalities will be used as a proxy for severity of enforcement with corresponding index points from zero to three. The first three bullet points concern Component 3, the latter concerns Component 4:

- + 0 points if the aggregate stock of municipal liquidity credits per person is below the 15th percentile (14.8 euros per person) of all territorial states (Component 3.1),
- + 1 point if the aggregate stock of municipal liquidity credits per person is above the 15th percentile and below 85th percentile (544 euros per person) of all territorial states (Component 3.2),
- + 2 points if the aggregate stock of municipal liquidity credits per person is above the 85th percentile of all territorial states (Component 3.3),
- + 1 point if more than one third of municipalities in the respective federal state have a liquidity credit stock of more than 544 euros per person, but 0 points otherwise (Component 4).

Rules relating to the existence and approval of budget consolidation plans do not flow into the index as their impact on credit access is ambiguous. It would be desirable to also include a criterion relating to the percentage of municipalities with liquidity credit stocks of more than a certain percentage of their annual administrative revenues. However, we refrained from doing so due to data availability issues (see Section 5.3.2).

Table 5.2: Development of credit access index, 1998-2013

Federal states	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013
Baden-Württemberg	1	1	1	1	1	1	1	2	1	1	1	1	2	2	1	1
Bavaria	0	1	1	1	1	1	1	1	1	1	1	0	1	1	1	1
Brandenburg	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	2.5	3.5	3.5	3.5	3.5	3.5	3.5
Hesse	2	3	3	3	3	3	3	3	3	3	3	3	3	4	3	3
Lower Saxony	2	2	2	2	2	2	2	2	2	2	3	3	3	3	3	3
Mecklenburg-Vorpommern	1	1	2	2	2	2	2	2	2	2	2	2	2	2	2	2
North Rhine-Westphalia	3	3	3	3	3	3	3.5	4.5	5	5	5	5	6	6	6	6
Rhineland-Palatinate	3	3	3	3	3	3	3	3	3	4	5	5	5	5	5	5
Saarland	2	2	2	3	4	4	4	4	4	5	4	5	5	5	5	5
Saxony	2	2	1	1	2	2	2	2	2	2	1	1	1	1	1	1
Saxony-Anhalt	1	2	2	2	2	2	3	3	3	3	3	3	3	3	3	3
Schleswig-Holstein	0	0	0	1	1	1	1	1	2	4	4	4	4	4	4	4
Thuringia	1	1	2	2	2	2	2	2	2	2	2	2	2	2	2	2

Notes: Index based on institutional characteristics that ease or limit credit access for municipalities and on quantitative information on local liquidity credit stocks, developed by authors. See Table 5.14 in Appendix 5.5 for more detailed information on the assignment of index scores.

The index suffers from some caveats. In particular, some of the points can be assigned objectively, while others require the authors' individual assessment and interpretation. The latter is particularly true for the translation of circular decrees into index scores. Moreover, the thresholds involved in the construction of the quantitative part of the index are somewhat arbitrary, as is the weighting of the criteria. However, these caveats are

common when it comes to index constructions, see, e.g., Ciagala and Heinemann (2012).[18]

According to the above set of rules, the index of local credit access is calculated for each federal state for each year between 1998 and 2013. It runs from zero (no credit access/complete credit rationing) to six (complete credit access/no credit rationing). Table 5.2 lists the resulting index values. Table 5.14 in Appendix 5.5 details the points given for each characteristic in each year.

A measure of tax rate volatility. We propose the number of tax rate changes per municipality within a given time period as a measure of tax rate volatility. The number of tax rate changes gives equal importance to each tax rate adjustment regardless of its magnitude. A smaller number of changes would seem more in line with tax smoothing, ceteris paribus. Steady tax rates reduce the distortionary costs of taxation. Changes to tax rates not only raise distortionary costs but also induce further costs, e.g., bargaining costs between the administration and the public (inhabitants and firms). Over the business cycle, automatic stabilizers should be left to work without any intervention in tax rates.[19]

The number of tax rate changes will be used in the descriptive analysis in Section 5.3.1 (Step 2 and 3) and as the dependent variable in the empirical model presented in Section 5.3.2. We calculate the number of tax rate changes for the business and the property tax, respectively. In addition, the sum of changes for both taxes is considered to obtain a full picture of local tax policy. For this purpose, we built a panel dataset covering all German municipalities for the period 1998 to 2013. Data on local tax rates at the municipal level from 2009 to 2013 are made available online by the Regional Database Germany (*Regionaldatenbank Deutschland*). Tax rates between 2001 and 2008 were obtained from the Statistical Local (*Statistik Lokal*) publications. Data on all years before 2001 were requested directly from the respective statistical office of the federal state in question. Most analyses that follow were carried out for the balanced panel of 10,160 municipalities for which we have information on the business and property tax rates as well as on liquidity

[18]There are more advanced index construction procedures such as, for instance, indexes based on the principal component analysis (see, e.g., International Monetary Fund 2009). However, they are far beyond the scope of this research.

[19]Apart from this cyclical component, which is the focus of this paper, tax smoothing also has a strucutral, long term component. It demands that all available information on long-term future spending needs should be used to devise an optimal tax rate today that takes into account future developments, allowing for a steady tax rate and a smaller adjustment than if adjustment were delayed. Nevertheless, the number of changes necessary to deal with new information on spending needs should be kept to a minimum, too.

credits in all 16 years.[20]

5.3 Analyzing tax smoothing at the local level

It is plausible to assume that local governments that enjoy credit access find it easier to smooth taxes over time. They can afford to refrain from adjusting tax rates over the business cycle to ensure stable tax revenues. Allowing automatic stabilizers to work can easily cause deficits during economic downturns. The optimal response to adverse temporary economic shocks may also be to run a budget deficit rather than exacerbate economic costs through tax rate increases. Thus, we should see more stable tax rates in federal states where municipalities can make use of liquidity credits to finance current deficits. In other words, there should be a positive link between credit access and tax smoothing, as credit access contributes to smoother tax rates. In the following, we refute such a positive association for the case of German municipalities, based on four steps of empirical, i.e. descriptive and econometric, analysis.

5.3.1 Descriptive analysis

We start with our descriptive analysis by first taking a closer look at the actual development of liquidity credits (Step 1). Next, we study tax rate volatility (Step 2), before linking tax rate volatility and credit access (Step 3).

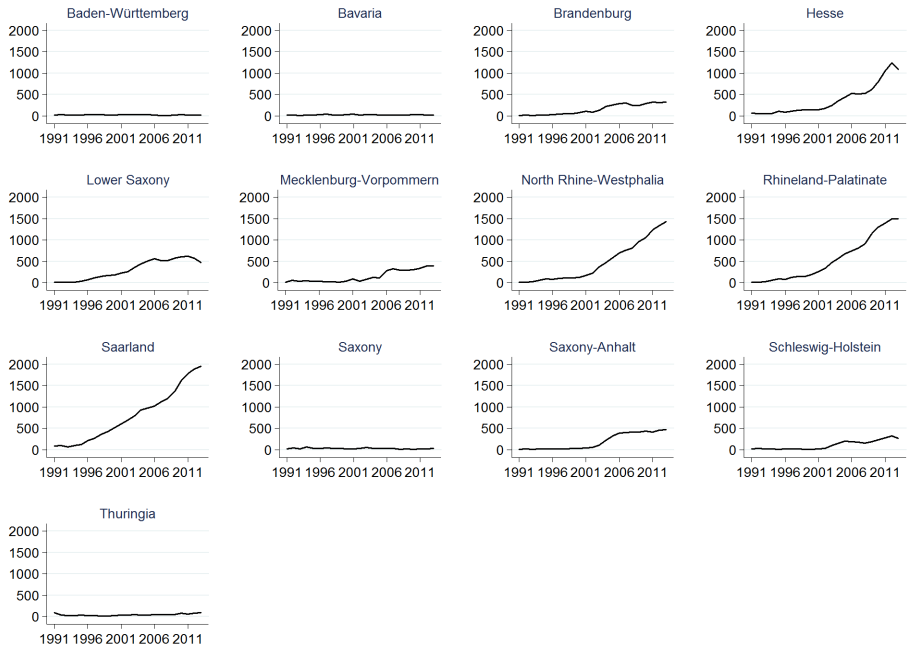
Step 1: Development of liquidity credits is not cyclical

If liquidity credits were used as a tax smoothing tool, the stock of liquidity credits should behave cyclically. However, this is not the case empirically. Figure 5.1 pinpoints that easier credit access has coincided with dramatic increases in local per capita short-term liquidity credits in some federal states (Hesse, North Rhine-Westphalia, Rhineland-Palatinate and Saarland) over the past decades. Levels of local per capita short-term liquidity credits

[20]The total number of municipalities in the balanced panel dataset is smaller than the actual total number of municipalities in some states where not all municipalities exist in all 16 years due to territorial reforms. This is particularly true for Brandenburg and Saxony-Anhalt. The balanced panel dataset is the data basis for Table 5.3, 5.4 and 5.5 as well as Table 5.11 in the Appendix. It is furthermore employed to calculate the average number of tax rate changes by state presented in Figure 5.3 and 5.4 and in the econometric analysis. For a deeper discussion on potential related issues and information on data see Section 5.3.2.

do not behave cyclically – at least at the aggregate level. Instead of showing alternating increases and reductions, they exhibit a steady rise with varying slopes over time. This suggests a misuse of liquidity credits to cover rising spending needs. It is a valid argument that this behavior does not rule out the simultaneous use of liquidity credits for tax smoothing. However, as the results of the next steps show, tax rates tend to be rather stable overall. In particular, the number of changes of municipalities in federal states affected by high local liquidity credit burdens are at the upper end of the scale. Thus, the evolution of liquidity credits suggests that credit access is not (mainly) used for tax smoothing purposes.

Figure 5.1: Evolution of short-term liquidity credits (in euro p.c.), 1991-2013



Notes: Own compilation with data retrieved from the Fachserie 14 Reihe 5 2013 by the Federal Statistical Office. Data cover the entire municipal level (municipalities, municipal associations and counties).

Step 2: Tax rate changes are overall rather rare

If local credit access facilitated tax smoothing at the local level, tax rate changes should be particularly rare in federal states where municipalities have ample access to credit. However, local tax rates are remarkably stable across Germany, regardless of local credit

access. Table 5.3 provides information on the proportion of municipalities with property and business tax rate changes within the reference period 1998 to 2013 by federal state.

Table 5.3: Proportion of municipalities with tax rate changes, 1998-2013

	Property tax				Business tax			
	No change	One change	Two to four changes	Five to eleven changes	No change	One change	Two to four changes	Five to eleven changes
Baden-Württemberg	10.3	34.7	51.1	3.9	21	54.1	24.4	.5
Bavaria	41.1	37.5	21.1	.3	48.4	33.6	17.7	.2
Brandenburg	20.7	44.8	31	3.4	53.4	31	15.5	0
Hesse	6.7	22.1	63.7	7.6	16.2	35.2	47.5	1.2
Lower Saxony	4	23.8	67.2	5.1	6.2	27.3	61.5	5
Mecklenburg-Vorpommern	22.3	41.1	35.9	.7	34.1	38	25.6	2.3
North Rhine-Westphalia	0	4.5	74.7	20.7	1	13.9	78.8	6.3
Rhineland-Palatinate	2.3	10.1	83.9	3.6	13.8	43.1	42.6	.4
Saarland	9.8	43.1	41.2	5.9	2	5.9	80.4	11.8
Saxony	4.8	19	63.8	12.4	19.3	30	46.9	3.8
Saxony-Anhalt	10.6	12.4	51.6	25.5	16.1	13	42.9	28
Schleswig-Holstein	11.8	24.8	53.5	10	14.1	35.8	47.4	2.7
Thuringia	8.9	57.3	33	.8	7.1	57.6	34.5	.8
Total	14.2	27.3	53.6	4.9	21.4	38.3	38.2	2.1

Notes: Proportion of municipalities with tax rate changes calculated for the balanced panel of 10,160 municipalities for the period 1998 to 2013.

Table 5.11 in Appendix 5.5 gives the equivalent information in terms of absolute numbers of municipalities.[21] From the tables, it is clear that tax rate changes are overall

[21] The total number of municipalities indicated in the Table 5.11 is smaller than the actual total number of municipalities in some states where not all municipalities exist in all 16 years due to territorial reforms. This is particularly true for Brandenburg and Saxony-Anhalt.

rather rare. In case of property taxes, the majority of municipalities changed their tax rate two to four times within our 16-year-horizon. Less than 5% enacted five or more changes, with a maximum of eleven changes. Roughly a quarter changed their tax rate only once, while 14.2% left their tax rate completely unchanged. Two to four is also the most frequent number of changes in most of the federal states. Only in Brandenburg, Mecklenburg-Vorpommern, Saarland and Thuringia, the majority of municipalities had only one tax rate change, but not by a big margin. Bavaria stands out in that a majority of its municipalities enacted no property tax rate change during the reference period.

Business tax rates show similar patterns, but with even more overall tax rate stability. Roughly a fifth of municipalities did not change their business tax rate at all from 1998 to 2013. One change or two to four changes were each carried out by about 38% of municipalities. Only 2% changed their business tax rate five to eleven times. It is also worth noting that changes in both property and business tax rates are most often in the direction of higher tax rates. 96% of property tax rate changes and 93% of business tax rate changes were increases. This might be a reflection of increased spending pressure at the local level in recent years. This suggests that German municipalities in general aim to smooth taxes (i.e. keep them stable) – cyclical tax policy would involve tax reductions as well as tax jumps.[22]

[22]During economic downturns German municipalities suffer from a shortfall of own tax revenue (business tax) while facing a rise in obligatory expenditures (social expenditures). The municipal fiscal equalization system works as an automatic stabilizer, but does not close the revenue gap completely. Municipalities face the choice of either crowding out other non-compulsory expenditures, raising their tax rates or taking on debt (if possible). Their leeway to engage in discretionary fiscal policy is very limited if it exists at all.

Table 5.4: Proportion of municipalities with property tax rate change from previous year to current year

	Baden-Württemberg	Bavaria	Brandenburg	Hesse	Lower Saxony	Mecklenburg-Vorpommern	North Rhine-Westphalia	Rhineland-Palatinate	Saarland	Saxony	Saxony-Anhalt	Schleswig-Holstein	Thuringia	Total
1999	6.9	4.2	8.5	11.4	12.4	1.4	9.6	4.5	5.9	24.5	9.2	7.9	2.6	6.6
2000	4.0	3.7	6.8	10.2	17.0	2.7	10.6	45.5	5.9	26.9	14.1	7.0	1.6	15.9
2001	6.5	3.7	3.4	3.1	10.9	2.4	14.9	23.6	2.0	22.8	8.6	27.8	4.0	12.7
2002	6.8	4.2	16.9	14.0	20.7	2.9	18.2	9.4	7.8	19.0	12.3	13.8	2.9	9.7
2003	15.2	12.7	10.2	9.0	22.6	2.4	77.5	6.6	19.6	22.1	19.0	11.1	3.2	13.8
2004	28.3	13.7	15.3	17.8	22.4	2.9	8.3	4.2	2.0	26.6	21.5	19.1	1.6	13.3
2005	34.4	8.5	11.9	17.1	19.8	8.9	15.9	16.0	13.7	19.7	20.2	10.2	5.9	15.1
2006	17.6	5.0	15.3	6.9	8.5	10.3	14.6	9.1	9.8	12.8	17.8	17.8	3.0	10.0
2007	6.1	3.5	3.4	10.2	8.7	11.5	8.1	2.0	5.9	11.7	14.1	6.7	3.2	5.5
2008	4.6	2.6	3.4	7.8	7.6	8.4	6.3	1.7	5.9	6.9	14.7	6.5	2.2	4.5
2009	2.8	2.6	6.8	5.9	10.0	5.0	4.8	1.5	3.9	9.0	33.1	10.0	2.2	4.9
2010	18.6	7.0	8.5	18.1	19.8	20.4	18.9	2.0	11.8	19.3	57.1	20.7	5.2	12.5
2011	20.5	7.7	13.6	22.1	11.4	19.2	49.0	76.5	17.6	13.1	38.0	30.8	60.1	35.0
2012	9.3	5.3	10.2	33.5	17.8	13.4	41.9	36.0	33.3	19.3	26.4	13.5	23.8	20.1
2013	5.1	3.9	11.9	50.4	22.6	20.2	40.7	14.6	33.3	13.4	27.0	19.1	15.6	15.8
Total	11.7	5.5	9.1	14.8	14.5	8.3	21.2	15.8	11.2	16.7	20.8	13.9	8.6	12.2

Notes: Proportion of municipalities with property tax rate change from previous year to current year calculated for the balanced panel of 10,160 municipalities for the period 1998 to 2013.

Tables 5.4 and 5.5 complement the above information by looking at the timing of tax rate changes. The tables present the share of municipalities in each federal state that changed their tax rate in a given year compared to the previous year's tax rate.

Table 5.5: Proportion of municipalities with business tax rate change from previous year to current year

	Baden-Württemberg	Bavaria	Brandenburg	Hesse	Lower Saxony	Mecklenburg-Vorpommern	North Rhine-Westphalia	Rhineland-Palatinate	Saarland	Saxony	Saxony-Anhalt	Schleswig-Holstein	Thuringia	Total
1999	3.5	2.9	6.8	6.4	8.3	3.1	7.3	3.7	2.0	19.7	9.8	4.1	4.1	4.8
2000	2.1	2.4	3.4	9.7	13.0	4.3	8.1	21.0	2.0	19.7	11.7	4.5	2.7	9.1
2001	5.0	4.9	5.1	2.9	13.0	3.1	11.6	23.6	98.0	16.6	8.6	27.8	4.9	13.2
2002	3.0	3.3	1.7	7.6	21.4	3.3	12.6	10.3	0.0	9.7	10.4	14.9	3.3	8.6
2003	4.0	6.5	3.4	4.5	20.4	2.9	64.6	7.1	2.0	12.1	12.9	9.6	3.0	9.9
2004	8.7	8.2	0.0	9.0	17.5	5.1	7.1	4.9	2.0	14.8	16.6	15.7	2.7	8.8
2005	24.1	5.4	5.1	9.0	15.3	6.7	12.1	14.5	82.4	7.2	16.0	7.6	5.2	11.6
2006	19.4	3.7	10.2	4.5	7.5	7.2	10.1	6.1	7.8	6.2	14.1	16.3	3.3	8.3
2007	5.3	2.6	3.4	5.9	6.2	8.9	6.1	2.1	11.8	8.6	14.1	5.4	2.7	4.4
2008	3.1	4.5	1.7	5.2	7.7	4.8	6.1	1.7	5.9	5.2	10.4	4.8	3.3	4.2
2009	2.3	4.2	3.4	6.4	11.3	3.4	4.5	1.1	5.9	6.6	23.9	8.1	2.4	4.7
2010	7.4	7.9	5.1	11.4	19.3	14.8	10.6	1.8	13.7	13.4	49.1	16.0	6.1	9.7
2011	12.9	8.3	10.2	16.4	13.0	16.6	38.6	20.7	13.7	13.1	42.9	16.2	60.6	19.7
2012	6.3	5.8	1.7	21.1	18.1	12.7	31.8	15.0	37.3	13.1	42.3	9.3	24.4	13.9
2013	4.4	4.5	10.2	38.5	20.2	17.8	30.6	10.0	19.6	11.0	35.6	12.5	15.1	12.8
Total	7.0	4.7	4.4	9.9	13.3	7.2	16.4	9.0	19.0	11.1	19.9	10.8	9.0	9.0

Notes: Proportion of municipalities with business tax rate change from previous year to current year calculated for the balanced panel of 10,160 municipalities for the period 1998 to 2013.

In Bavaria and Brandenburg, the share of municipalities with either property or business tax rate changes never exceeds 20%.[23] In Mecklenburg-Vorpommern and Saxony, this is true for business tax rates only. In all other states, there is at least one year and up to seven years of high tax rate volatility where more than 20% of all municipalities enacted

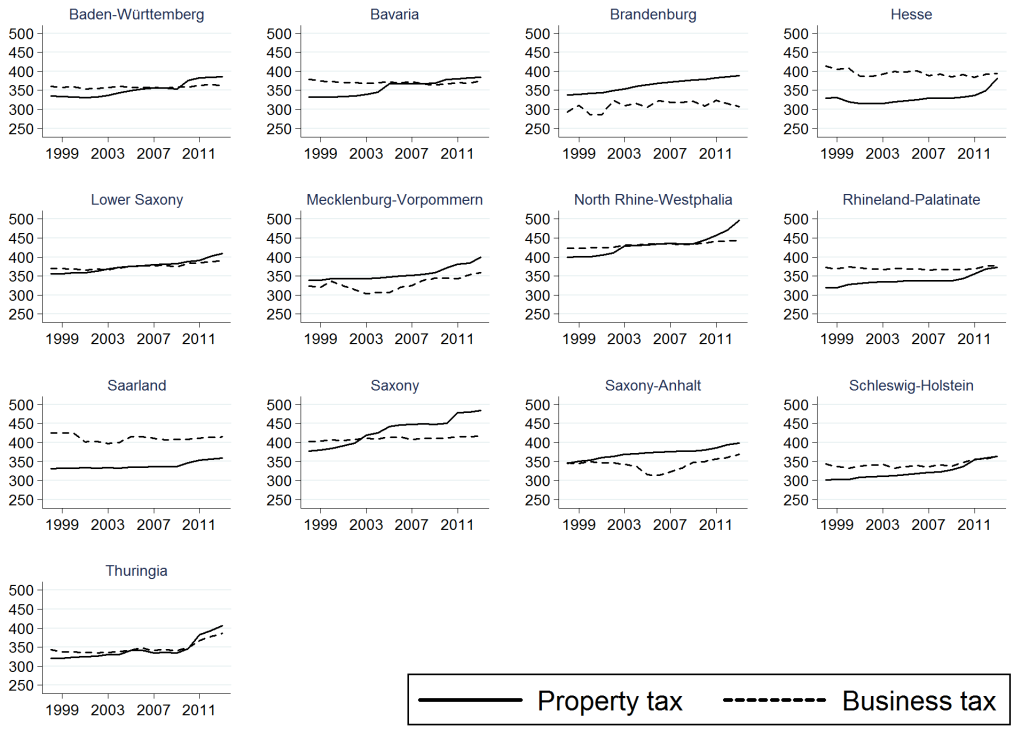
[23]Information on Brandenburg needs to be treated with caution given the high number of missing municipalities due to territorial reforms, see above.

tax rate changes.[24] Often, such years coincide for property and business tax rates, although business tax rates again show less overall volatility. However, years of high volatility do not necessarily coincide between states. This suggests that municipalities adjust their tax rates mostly in response to state-specific shocks. Such shocks could consist of changes in expenditure requirements when states devolve parts of their tasks to the local level or modify standards relating to the execution of local tasks. Changes in municipal fiscal equalization schemes are also possible triggers of tax rate changes. Changes in the degree of credit access granted by states could also constitute a shock to which local governments react. The tables also point to higher overall volatility of tax rates in recent years, starting in 2010, with many federal states experiencing high volatile years at the same time. The rise in tax rate volatility towards the end of our time horizon is a further indication that credit access does not seem to contribute to smoother tax rates, given that credit access has generally increased and not decreased over the last two decades.

Figure 5.2 shows the patterns of property and business tax rate levels that result from the observed tax rate adjustments. For each state, the figure displays the weighted average of tax rates of all municipalities in the respective state over the period 1998 to 2013. The small number of changes is reflected in slow-moving average tax rates in most states. An upward trend is discernible in most states concerning both taxes, which is in general more pronounced for the property tax. North Rhine-Westphalia and Saxony have seen particularly strong increases in their average property tax rate level. It is worth noting that North Rhine-Westphalia has seen dramatic increases of municipal liquidity credits over the same time period, while Saxony's liquidity credit stocks have remained low. Rising local spending pressures across Germany could explain the generalized upward trend in local tax rates. In sum, local tax rates are relatively stable across Germany, with little variation across states. They have followed a slow upward trend in recent decades, without any visible cyclicality. Whether the federal state in question has seen simultaneous increases in local credit access or built-ups in effective municipal liquidity credit stocks has no apparent effect on tax rate volatility.

[24]The share of municipalities with tax rate changes can go up to more than 50, 70 or even 90%.

Figure 5.2: Development of business and property tax rate (in %), 1998-2013

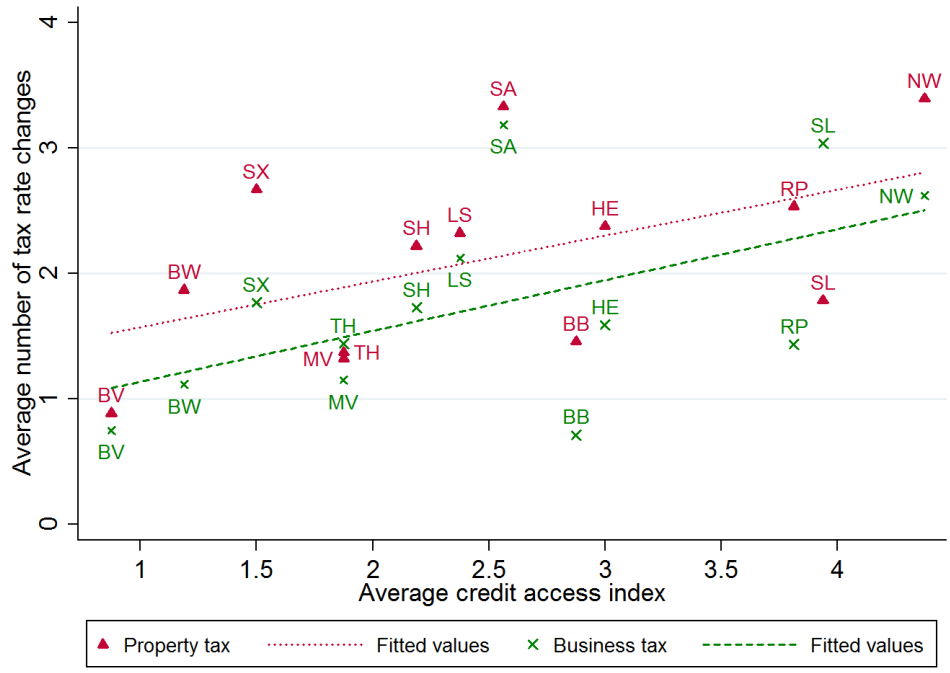


Notes: Own compilation. Data source for the evolution of property and business tax rate (in %) is the Regional Database Germany (Regionaldatenbank). Data represents the weighted average for all municipalties.

Step 3: Positive relationship between credit access and tax rate volatility

If local credit access contributed to tax smoothing, this positive association should be visible in a joint analysis of the data. In the third step of our line of argument, we approach such a possible relationship between credit access and tax rate volatility through a graphical analysis. Figure 5.3 plots the relationship between credit access and property and business tax rate changes. The y-axis represents the average number of tax rate changes in the municipalities in the state in question during the same period. The scattered triangles and crosses refer to the property and business tax, respectively. The x-axis shows the average credit access index for each federal state. This corresponds to the average of the index developed in Section 5.2.2 over the 16 years from 1998 to 2013.

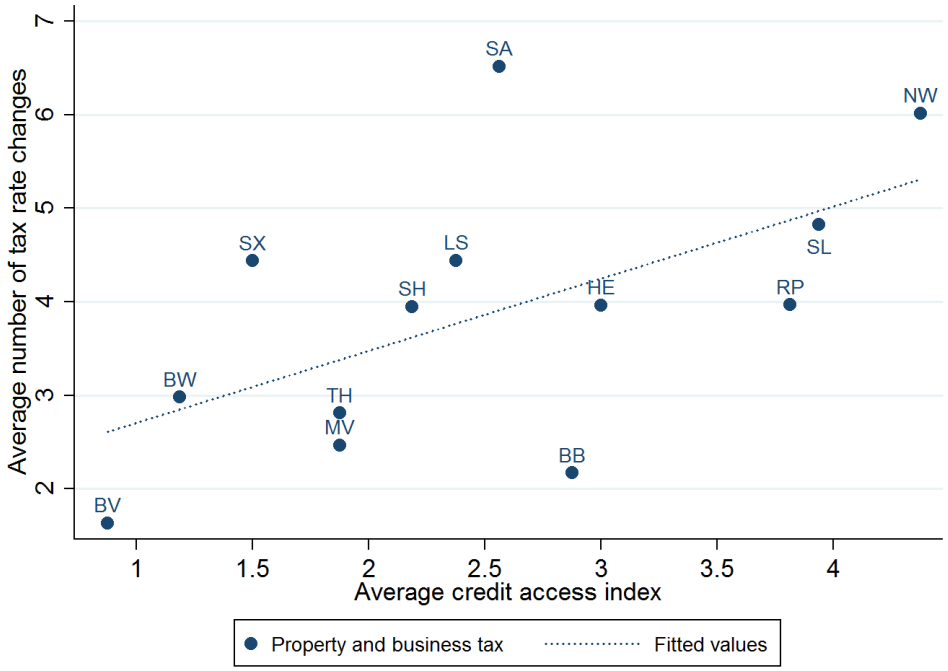
Figure 5.3: Relationship between average credit access and tax rate changes



Notes: Cross section aggregated at the state level derived from balanced panel of 10,160 municipalities for the period 1998 to 2013. Average number of tax rate changes (differentiated by business and property tax) in the municipalities for each federal state (y-axis). Average credit access index for each federal state (x-axis). Lines represent linear fit predicting the average number of tax rate changes from the average credit access index.

No clear relationship emerges from Figure 5.3. If anything, a positive association seems discernible, with Bavaria and North Rhine-Westphalia marking the two ends of the spectrum from low credit access and few tax rate changes to high credit access and many tax rate changes. The indication of a possible positive relationship is supported by the two lines representing the linear fit predicting the average number of (property and business) tax rates changes from the the average credit access index (the dotted and the dashed line refer to the property and business tax, respectively). Both lines ascend slightly and run almost in parallel. A positive link between credit access and tax rate changes would contradict the rationale of the tax smoothing hypothesis, whereby access to credit financing should allow municipalities to smooth their taxes and thus limit the number of tax rate changes.

Figure 5.4: Relationship between average credit access and tax rate changes



Notes: Cross section aggregated at the state level derived from balanced panel of 10,160 municipalities for the period 1998 to 2013. Average number of tax rate changes in the municipalities for each federal state (y-axis). Average credit access index for each federal state (x-axis). The dotted line represents the linear fit predicting the average number of tax rate changes from the average credit access index.

Figure 5.4 again investigates a possible relationship between credit access and the frequency of tax rate changes, this time considering the state average of the sum of changes in the business and property tax in the state's municipalities. Jointly considering both taxes allows for a more complete picture of the volatility of local tax rate policy. Empirically, a small number of property tax changes generally coincides with a small number of business tax changes and vice versa. As a result, the fitted line in Figure 5.4 has a positive and steeper slope than the fitted lines in Figure 5.3.

How can one explain that greater credit access does not seem to lead to less tax rate volatility, as evidenced by the preliminary graphical analysis? One potential explanation lies in the fact that we may have overlooked an important intervening variable: Local spending pressure. As alluded to in the preceding section, municipalities in Germany have in general witnessed an expansion of their spending responsibilities in recent years. In light of Germany's federal institutional design it stands to reason to evaluate this as a structural, mainly exogenous phenomenon from the local perspective. A further important

characteristic is that local spending pressure varies across federal states.[25] Consequently, the positive link between tax rate changes and the credit access index might be explained by spending pressure.

The results of this graphical analysis need to be interpreted with great caution. The plots show a supposed unconditional relationship, i.e. not controlling for potentially important covariates (e.g., spending pressure). Moreover, the information available in our dataset has been condensed to an exceptionally high level of aggregation to produce the above graphs. Behind the 13 data points depicted here for each state lie more than 10,000 individual municipalities over 16 years. Thus, the preliminary conclusions drawn from the graphical analysis should be verified by an econometric analysis at the municipal level that makes the most of the available data potential. This is what we do next (Step 4).

5.3.2 Econometric analysis

In this section, we first provide information on our data sources and descriptive statistics, before presenting our fourth step and last approach to our research question, an econometric analysis.

Data sources. The empirical analysis is based on a panel dataset with yearly administrative data on all German municipalities from 1998 to 2013. Data were retrieved from different sources, depending on the variable and the time horizon in question. Data on liquidity credits, which are part of the construction of the credit access index, was provided by North Rhine-Westphalia's statistical office. The indicator funding need growth was calculated by the authors and is only available between 1998 and 2006. Its components are obtained directly from the municipal financial statement statistic (*Jahresrechnungsstatistik der Gemeinden/Gemeindeverbände*), which is compiled by the Research Data Centres of the Federal Statistical Office and the statistical offices of the Länder (*Forschungsdatenzentren der Statistischen Ämter des Bundes und der Länder*) (Source: FDZ der Statistischen Ämter des Bundes und der Länder, Jahresrechnungsstatistik der Gemeinden/Gemeindeverbände, 1998-2006, own calculation). Data on employees is provided by the Federal Employment Agency (*Bundesagentur für Arbeit*). All other municipal-level

[25] Higher spending needs have arisen in particular in the area of social welfare, which is to a large degree a local task although standards are prescribed by federal law. The exact extent of municipal responsibilities depends on state law, as the states can choose to carry out (social and other) tasks themselves or to devolve them to the local level. The extent to which spending is decentralized and the extent to which potential spending increases are offset by increased state transfer allocations differ between federal states.

variables, including local business and property tax rates, were obtained from a combination of sources. Municipal-level data from 2009 to 2013 is available online through the Regional Database Germany (*Regionaldatenbank Deutschland*). Data for the years from 2001 to 2008 come from the Statistical Local (*Statistik Lokal*) publications. Data on all previous years were requested separately from each of the 13 state statistical offices.[26] Some control variables such as the gross domestic product (GDP) and income of private households are only available at the county level. All such variables are provided online by the Regional Database Germany for the full period of interest.

Our baseline sample covers all municipalities for which we have information on business and property tax rates as well as liquidity credits. This original dataset is an unbalanced panel containing 198,113 observations. The unbalanced nature of the panel is due above all to territorial reforms that took place in the New Länder and greatly reduced the number of municipalities. Hence, the number of observations in the original sample shrinks from 14,102 observations in 1998 to 11,112 observations in 2013. Table 5.12 in Appendix 5.5 reports for each state the share of municipalities for which we have one, two etc. years of observations. All Bavarian and North Rhine-Westphalian municipalities are represented in the dataset in all 16 years. For the remaining states, the share of municipalities that exist during the full time period varies from 9.1% in Brandenburg to 99.8% in Rhineland-Palatinate. In order to avoid biases caused by municipalities that only existed in a fraction of years, the remaining analysis is limited to the balanced panel of 10,160 municipalities with 16 years of observations.[27] The decision to employ the balanced panel might raise selection concerns.[28] However, employing the balanced panel dataset is superior to employing the unbalanced panel. We handle this issue by providing detailed information on the nature of the problem (see discussion for Table 5.12 and 5.13). We also present our results by state while carefully pointing out the related problems (see, e.g.,

[26]Not all statistical offices were able to provide information on municipal-level data for the requested time period on the basis of the territorial status of that time.

[27]To adjust for the territorial reforms by setting the current territorial status as, for example, done by Fuest et al. (2016) is not an option. This requires data harmonization by, e.g., demeaning and would lead to artifical variation in our variable of interest (tax rates). To maximize the number of municipalities in the balanced panel, we corrected the identifiers of municipalities that were affected by county-level territorial reforms. In such cases, the identity or size of the county a municipality belongs to is changed without changing the municipality itself.

[28]Whether a municipality is affected by a territorial reform mainly depends on population size besides geographical and administrative characteristics. See Chapter 4 for an introduction into municipal territorial reforms using the example of the German federal state Saxony-Anhalt.

Footnote 23). We furthermore discussed to employ weights, in particular, with respect to the econometric analysis, but decided against it. Instead we employ state dummies. Table 5.13 in Appendix 5.5 lists the number of municipalities contained in the balanced panel by state. While Saarland and Brandenburg are represented with less than 60 municipalities, Rhineland-Palatinate and Bavaria each contribute more than 2,000 observations to the sample. Such differences are partly due to the aforementioned territorial reforms, which cause incomplete samples for some states. However, they also arise because of differing institutional landscapes with major differences in city size and number. The two states with full samples are cases in point: The most populous federal state North Rhine-Westphalia is divided into only 396 municipalities while Bavaria, the state with the biggest surface area, is extremely fragmented with 2,056 municipalities.

Dependent variable and main independent variables of interest. Our research interest – the impact of credit access on tax rate volatility – demands some degree of data aggregation to derive meaningful volatility measures. It only makes sense to analyze the number of tax rate changes within a sufficiently long time frame. That is why we aggregate our dataset for our main analysis into a cross section without a time dimension. Our independent variable of interest is the credit access index, which is outlined in detail in Section 5.2.2. Following our train of thought on spending pressure (outlined in the previous section), another independent variable of importance is funding need growth (in %). We define local funding need as the difference between total municipal current outlays and non-autonomous municipal current revenue.[29]

Descriptive statistics. Table 5.6 shows summary statistics for the aggregated dataset derived from the balanced panel. The table first lists the three dependent variables tested in the regression analysis: The number of property tax (short: PT) changes, the number of business tax (short: BT) changes, and the sum of changes of both taxes, each calculated over the full 16 year-horizon. Next is our main independent variable of interest: the credit access index. The rest of the table shows the most important control variables: Funding need growth (in %), the dummy for municipal association status, population, number of employees at place of employment (per capita (p.c.)), income of

[29]Non-autonomous revenues are transfers from the state level in the form of shared taxes, conditional and unconditional transfers. Thus, local funding need measures local expenditures that need to be financed out of municipal own revenue, in particular business and property taxes. Funding need growth is defined as the percentage increase of funding need between 1998 and 2006. This is used as a proxy for funding need growth from 1998 to 2013, as necessary information on local revenue is not available beyond 2006. We excluded 121 observations that were outliers with respect to funding need growth.

private households, GDP (both in euro p.c.) and left majority at state parliamentary elections, respectively. Municipal association status is coded as 1 if the municipality belongs to a municipal association (*Amtsgemeinde, Samtgemeinde or Verbandsgemeinde*) and 0 otherwise. Municipal associations exist in seven out of the 13 territorial states. All independent variables correspond to the municipality-specific average of the underlying variable over the 16 years of the reference period.

Table 5.6: Summary statistics

	Mean	Std. Dev.	Min.	Max.	N
Number of changes PT rate	1.956	1.414	0	11	10160
Number of changes BT rate	1.441	1.215	0	11	10160
Number of changes	3.397	2.365	0	21	10160
Index	2.277	1.139	0.875	4.375	10160
Funding need growth	42.562	118.138	-984.519	999.782	10160
Mun. assoc. status	0.468	0.499	0	1	10160
Population	6895.983	28791.314	6.25	1287749.375	10160
Employees	0.187	0.175	0	7.697	10031
Priv. income	17308.891	1890.876	13055.429	28235.133	10160
GDP	21587.731	6345.324	6119.703	79894.067	10160
Left majority	0.307	0.361	0	1	10160

Notes: Cross section derived from balanced panel of 10,160 municipalities for the period 1998 to 2013. All variables except numbers of changes and funding need growth correspond to the mean over the 16-year horizon. Funding need growth (in %) is defined as the growth in funding need between 1998 and 2006. Number of changes, funding need growth (in %), municipal association status, population, employees (p.c.) are municipal level variables, GDP, private income (both in euro p.c.) and left majority at state elections are county level variables while the index of credit access is a state level variable.

Tables 5.15 and 5.16 in Appendix 5.5 provide summary statistics for the same variables by state. All variables except for the number of employees are available for all 10,160 municipalities.

Step 4: Positive relationship between credit access and tax rate volatility not rejected

Econometric model. To estimate the effect of credit access on tax rate volatility, we use the following model:

$$C_{i,t} = \alpha I_s + \beta \mathbf{X_i} + \delta \mathbf{Y_c} + \lambda_s + \varepsilon_i \tag{5.1}$$

where $C_{i,t}$ is the number of tax rate changes per municipality i between 1998 and 2013. The index t denotes the tax type (business, property or both).

Our independent variable of interest is local credit access, which is measured by the mean of the credit access index by federal state across time (I_S). We aggregate our dataset into a cross section in order to create the best possible volatility measures based on tax rates during the full time period. The obvious drawback is that we have to work in a cross section environment with significantly less observations.

We add a number of controls to account for further confounding factors: $\mathbf{X_i}$ and $\mathbf{Y_c}$ represent vectors of municipal-level controls (funding need growth (in %), population, gross outlays (in euro p.c.), employees at place of employment, employees at place of residence, in-commuters (all three p.c.), gross revenue excluding business and property taxes, revenue from shared taxes per capita (both in euro p.c.), and municipal association status) and county-level controls (GDP and income of private households (both in euro p.c.), left majority at state parliamentary elections), respectively. We calculated the mean and the standard deviation of all potential controls and tested them in the regressions. Furthermore, we include state dummies λ_s. The error term ε_i is clustered at county level.

Discussion of identification. The main concern with identification lies in the fact that our analysis is limited to a simple cross section due to the aggregation involved in investigating volatility over a sufficiently long time period. Aggregation over the full time horizon greatly reduces the number of observations from 162,560 to 10,160.[30] Since municipal fixed effects cannot be employed, it is essential to account for other covariates that are potentially related to the volatility of local tax rates, including time-constant information.

A priori, controlling for population and budgetary variables seems particularly important. The capacity of municipalities to adjust their tax rates to external requirements may depend on the size of their population. Fluctuations in population, captured by its

[30] As an alternative to full aggregation, one might consider splitting the reference period into sub-periods, thus maintaining a panel data structure. However, data availability limits our reference period to the 16 years between 1998 and 2013. 16 years is a sufficiently long time frame to derive volatility measures, but eight or even four years is not enough in practice. Given the low incidence of changes in tax rates and credit access, further limiting the time frame means there is not sufficient variation in the data to identify the effect of our key variable.

standard deviation over the covered time period, may also influence tax rate volatility. In addition, gross outlays per capita are expected to have a significant impact. Due to an array of tasks mandated by the federal level, municipalities in Germany have only limited autonomy of decision over their spending. Rising spending pressures might cause frequent (upward) adjustments in tax rates, which is why we consider funding need growth to be an important control variable. The concept of funding need growth captures the possibility that states may offset increased local spending responsibilities by allocating more transfers to municipalities. As defined here, funding need growth will only occur where spending rises without compensating transfers. Local revenue sources aside from business and property taxes may also be crucial in the decision to alter tax rates, for instance in hope of filling rising revenue gaps. Economic strength may also be an important intervening factor. Municipalities that are well-off economically may face less financial distress and may be in a position where raising tax rates is not necessary. Variables such as GDP and income of private households, as well as the number of employees and in-commuters, are meant to capture the economic position. Finally, past research found that the ideological identity of the government (left- or right-wing) may influence fiscal policy. This is proxied by an indicator variable for a left majority at the most recent state parliamentary elections.

The volatility of local tax rates may also differ systematically depending on the institutional characteristics of the municipalities. Some cities, particularly big ones, enjoy county status and have additional responsibilities compared with municipalities belonging to a county. Within the group of county-affiliated municipalities, one can further differentiate between municipalities grouped within a municipal association (*Amtsgemeinde*, *Samtgemeinde* or *Verbandsgemeinde*) and non-associated municipalities, with the latter facing more responsibilities than the former. All cities with county status are also non-associated. The regression controls for these administrative differences.

Results and discussion. Tables 5.7 and 5.8 show the regression results from the regression model in Equation 5.1. Columns I to III present the results from a regression of the number of property tax changes while Columns IV to VI show the results for the number of business tax changes (both in Table 5.7). To give a complete picture Columns VII to IX in Table 5.8 pinpoint the results for the sum of tax rates changes of both taxes (note the coefficients of Table 5.7 sum up to respective ones of Table 5.8, e.g., 0.47891 (I) + 0.47217 (IV) = 0.95108 (VII)).

Table 5.7: Regression results with number of tax changes as dependent variable

	Property tax			Business tax		
	I	II	III	IV	V	VI
Index	0.47891***	0.47810***	0.44103***	0.47217***	0.47108***	0.49288***
	(0.050)	(0.050)	(0.053)	(0.027)	(0.027)	(0.032)
Funding need growth		0.00024*	0.00020		0.00032**	0.00028**
		(0.000)	(0.000)		(0.000)	(0.000)
Mun. assoc. status			0.44813***			0.32980**
			(0.099)			(0.102)
Population			0.00000			-0.00000
			(0.000)			(0.000)
Employees			0.11709			0.25078*
			(0.117)			(0.120)
Priv. income			0.00002			-0.00002
			(0.000)			(0.000)
GDP			-0.00001			-0.00000
			(0.000)			(0.000)
Left majority			0.11729			-0.20938
			(0.176)			(0.158)
Constant	1.29872***	1.29070***	0.81940	0.55293***	0.54209***	0.57354
	(0.147)	(0.148)	(0.501)	(0.062)	(0.062)	(0.385)
Observations	10160	10160	10031	10160	10160	10031
Adjusted R^2	.254785	.2550867	.2638504	.1952969	.196146	.2045603
State dummies	yes	yes	yes	yes	yes	yes

Notes: Estimation based on Equation 5.1. Cross section derived from balanced panel of 10,160 municipalities for the period 1998 to 2013. Dependent variable: Number of property (*I* to *III*) and business (*IV* to *VI*) tax rate changes (municipal level). All independent variables (except for funding need growth) are averaged over time. Independent variable of interest: Index of credit access (state level). All specifications control for state dummies. Specification *II* and *V* additionally control for funding need growth (in %). Specification *III* and *VI* add municipal association status, population, employees (p.c.) (municipal level) and private income, GDP (both in euro p.c.) and left majority at state elections (county level). Standard errors in parentheses are clustered by county. Significance levels: * 0.10, ** 0.05, *** 0.01.

The first column for each tax (Regressions I, IV and VII) presents baseline results of a regression of tax rate changes on credit access without any control variables aside from state dummy variables. In each case, the credit access index is strongly significant with an estimated coefficient of about .48 and .47 for property and business tax, respectively, and .95 for both taxes. These results are in line with the graphical analysis presented in the previous section. Regressions II, V, and $VIII$ introduce the most important control variable: Funding need growth (in %).[31]

Credit access is still estimated to have a positive, significant impact on the number of tax rate changes, with the coefficients still ranging between .48 and .47 for the property and the business tax, respectively, and .95 for both taxes. Introducing funding need growth hence only has a marginal negative effect (and does not lead to a sign reversal). The coefficient for funding need growth is statistically significant and positive, but so close to zero that an economic significance is questionable. The last column (Regressions III, VI and IX) adds a dummy for the municipal association status, population, the number of employees at place of employment (p.c.), income of private households and GDP (both in euro p.c.) and a dummy for a left majority at the most recent state elections. Of these, only the municipal association status is significant. The introduction of the controls does not significantly alter the results. Compared to the previous regressions, the estimated coefficient of credit access dropped slightly to .44 for the property tax and to .93 for both taxes, but it slightly increased to .49 for the business tax.

These results seem to contradict the existence of countercyclical tax smoothing at the local level in Germany. Contrary to what one might expect, easy access to credit is not used by municipalities to limit the frequency of tax rate adjustments. Instead, credit access and tax rate volatility go hand in hand. One possible explanation lies in the institutional link between local debt and tax rates established by budget consolidation plans: Those municipalities that have ample credit access and have been making great use of it may well be facing severe financial difficulties. Where budget consolidation plans exist, they may be forced to raise their tax rates in order to ensure approval by the regulatory authority (see Section 5.2.1). We aimed to account for fiscal pressure that may lead to financial difficulties by controlling for funding need growth. Nevertheless, we cannot reject a positive link between credit access and tax rate volatility. This result

[31] Besides funding need growth we employed an array of other variables related to funding need (mean value of funding need over 1998-2006, funding need in 1998, average annual growth of funding need between 1998 and 2006). None of the specifications altered the coefficient of the credit access index significantly.

suggests that credit access may be associated with unsustainable fiscal behavior. Possibly, municipalities with credit access maintain unsustainable levels of spending, which need to be financed through higher taxes *and* liquidity credits.

Table 5.8: Regression results with number of tax changes as dependent variable

	VII	VIII	IX
Index	0.95108***	0.94918***	0.93391***
	(0.072)	(0.072)	(0.080)
Funding need growth		0.00056***	0.00048**
		(0.000)	(0.000)
Municipal association status			0.77793***
			(0.192)
Population			0.00000
			(0.000)
Employees			0.36788
			(0.228)
Priv. income			-0.00000
			(0.000)
GDP			-0.00001
			(0.000)
Left majority			-0.09209
			(0.286)
Constant	1.85165***	1.83278***	1.39295
	(0.197)	(0.198)	(0.814)
Observations	10160	10160	10031
Adjusted R^2	.2531876	.2538554	.2627839
State dummies	yes	yes	yes

Notes: Estimation based on Equation 5.1. Cross section derived from balanced panel of 10,160 municipalities for the period 1998 to 2013. Dependent variable: Number of tax rate changes (municipal level). All independent variables (except for funding need growth) are averaged over time. Independent variable of interest: Index of credit access (state level). All specifications control for state dummies. Specification *VIII* additionally controls for funding need growth (in %). Specification *IX* add municipal association status, population, employees (p.c.) (municipal level) and private income, GDP (both in euro p.c.) and left majority at state elections (county level). Standard errors in parentheses are clustered by county. Significance levels: * 0.10, ** 0.05, *** 0.01.

However, there are a number of caveats. First, as evidenced by a low $Adjusted\, R^2$ between .20 and .26, the regressions leave the bulk of tax rate volatility unexplained. This is despite a high number of observations.[32] Secondly, the applied method of ordinary least squares estimation in a cross section is particularly vulnerable to omitted variables that might bias estimated coefficients. Thirdly, credit access only varies at state level, making reliable identification of its effect more challenging. We are therefore left with interesting correlations that do not necessarily imply a causal effect. Nonetheless, the econometric analysis confirms the results of the descriptive analysis: Credit access does not seem to make a contribution towards smoother tax rates at the local level in Germany.

5.4 Discussion and concluding remarks

The theory of tax smoothing suggests that giving governments access to debt financing could be welfare enhancing, as allowing governments to smooth their taxes over time can reduce distortionary costs from taxation. While it is clear from a theoretical viewpoint that tax smoothing is beneficial, it is unknown whether governments with credit access do in fact engage in tax smoothing. Political decision-makers might not realize the potential benefits from tax smoothing and instead take advantage of debt to finance unsustainable levels of current expenditures. The possible relationship between credit access and tax smoothing is particularly important where lower level governments enjoy tax autonomy. The standard fiscal federalism literature objects to granting credit access to lower federal levels. At the same time, credit rationing might inhibit tax smoothing.

[32] Given the administrative differences between municipalities in Germany, excluding associated municipalities from the sample is a straightforward robustness check. The remaining municipalities are all non-associated (with or without county status), making the sample more homogeneous, but also smaller. We ran all regressions presented here using this subsample. The results confirm the existence of a positive link between credit access and tax rate volatility.

This paper uses the unique institutional setting of German fiscal federalism to study the behavior of municipalities in Germany and to test whether credit access is associated with lower tax rate volatility. Germany is a promising case to study given that municipalities enjoy autonomy over property and business tax rates and differ in the degree of credit access allowed by the respective federal state. To operationalize local credit access, the institutional environment and empirical level of local indebtedness in each of the 13 territorial states are examined in detail to derive an index of local credit access. We propose the number of tax rate changes within a 16 year-time frame as a measure of tax rate volatility. The descriptive and econometric modeling is based on a sample of more than 10,000 municipalities across Germany.

Employing a line of argument based on four steps of data analysis we provide evidence against a contribution of credit access to tax smoothing at the local level. We start by showing that the development of liquidity credit stocks has not been cyclical (Step 1). In fact, easier credit access coincides with dramatic increases in local per capita short-term liquidity credits in some federal states. In Step 2 we pinpoint that tax rate changes are rather rare in all federal states regardless of credit access. We find no cyclical behavior involving tax reductions and jumps. Instead, we find a rise in tax rate volatility towards the end of our time horizon, despite easing credit access. Next, we show graphically that there is a possible positive relationship between tax rate volatility and credit access. This contradicts the notion that credit access might induce less volatile tax rates (Step 3). We suggest spending pressure as a potential explanation. Last, we employ an econometric approach in which we account for spending pressure (Step 4). However, the empirical results also suggest a positive link, which would point to an improper use of local debt. Hence, we cannot reject the positive relationship between credit access and tax rate volatility.

It therefore appears that whether federal states allow their municipalities access to debt or not has no impact on the stability of their tax rate choices. Local tax rates are not less volatile in federal states which grant their municipalities ample access to debt. While local tax rates in Germany are generally rather stable over time, this still gives cause for concern. Further research will be required to validate this conclusion. If it is confirmed, important implications follow. If the major theoretical justification for public debt for consumption expenditures crumbles in practice, there is a case for more credit rationing, at least at the local level. Otherwise calls for bail-outs become more and more likely. In Germany, federal states with loose attitudes towards local debt should then follow the example of states that never expanded local credit access in the first place.

Making such a change would necessarily require revisiting local spending responsibilities and the adequacy of state transfers to the local level.

Beyond Germany, the findings of this paper suggest that higher-level governments should think twice before allowing subnational governments, and local governments in particular, access to credit to fund current expenditures. Chances are local debt will not be used in a welfare enhancing way.

5.5 Appendix

Table 5.9: Wording of current liquidity credit paragraph

Federal states	Current wording
Baden-Württemberg	§ 89 GemO (1) The municipality has to ensure timely settlements of expenses. (2) To ensure timely settlement of expenses, the municipality may take out liquidity credits up to the ceiling set in the budget by-law, provided no other funds are available. The authorization continues to hold until the budget by-law for the following year is passed. (3) The liquidity credit ceiling set in the budget by-law requires approval by the regulatory authority if it exceeds one fifth of the profit and loss budget's ordinary expenses.
Bavaria	Art. 73 GO (1) To ensure timely settlement of expenses, the municipality may take out liquidity credits up to the ceiling set in the budget by-law, provided no other funds are available. (2) The ceiling set in the budget by-law shall not exceed one fifth of the cash-flow budget's revenues for current administrative activities and one sixth of the administrative budget's revenues, respectively, for the core budget, and one sixth of the profit plan's revenues for owner-operated municipal enterprises.
Brandenburg	§ 76 BbgKVerf (1) The municipality has to ensure solvency through appropriate liquidity planning at all times. (2) For the timely settlement of expenses, the municipality may take out liquidity credits up to the ceiling set by the municipal council. The decision on the liquidity credit ceiling is to be reported to the regulatory authority immediately.
Hesse	§ 105 HGO (1) For the timely settlement of expenses, the municipality may take out liquidity credits up to the ceiling set in the budget by-law, provided no other funds are available. This authorization holds beyond the current budget year until the announcement of the new budget by-law. (2) The ceiling set in the budget by-law requires approval by the regulatory authority.
Lower Saxony	§ 122 NKomVG (1) For the timely settlement of expenses, the municipalities may take out liquidity credits up to the ceiling set in the budget by-law, provided no other funds are available. This authorization holds beyond the current budget year until the entry into force of the new budget by-law. Sentence 2 also holds for a new ceiling set in the new budget by-law before its entry into force, provided it does not exceed the amount specified in paragraph 2. (2) The ceiling set in the budget by-law requires approval by the regulatory authority if it exceeds one sixth of the cash-flow budget's revenues for current administrative activities.

Federal states	Current wording
Mecklenburg-Vorpommern	§ 53 KV M-V (1) The municipality has to ensure solvency at all times. (2) For the timely settlement of expenses, the municipality may take out liquidity credits up to the ceiling set in the budget by-law and approved according to paragraph 3, provided no other funds are available. This authorization holds beyond the current budget year until the public announcement of the new budget by-law. (3) The liquidity credit ceiling set in the budget by-law requires approval by the regulatory authority if it exceeds ten percent of the cash-flow budget's current revenues for administrative activites.
North Rhine-Westphalia	§ 89 GO NRW (1) The municipality has to ensure solvency through appropriate liquidity planning at all times. (2) For the timely settlement of expenses, the municipality may take out liquidity credits up to the ceiling set in the budget by-law, provided no other funds are available. This authorization holds beyond the current budget year until the promulgation of the new budget by-law.
Rhineland-Palatinate	§ 105 GemO (1) The municipality has to ensure solvency at all times. (2) For the timely settlement of expenses, the municipality may take out liquidity credits up to the ceiling set in the budget by-law, provided no other funds are available. This authorization holds beyond the current budget year until the public announcement of the new budget by-law. (3) § 49 does not apply to the take-up of liquidity credits.
Saarland	§ 94 KSVG (1) For the timely settlement of expenses, the municipality may take out liquidity credits up to the ceiling set in the budget by-law, provided no other funds are available. This authorization holds beyond the current budget year until the announcement of the new budget by-law. (2) If, given the budget reorganization plan, it is apparent that a balanced budget is not possible in the foreseeable future, the municipality may take out liquidity credits with maturities beyond the budget year, provided this is economically necessary.
Saxony	§ 84 SächsGemO (1) The municipality has to ensure timely settlements of expenses. (2) For the timely settlement of expenses, the municipality may take out liquidity credits up to the ceiling set in the budget by-law, provided no other funds are available. The authorization continues to hold until the budget by-law for the following year is passed. (3) The liquidity credit ceiling set in the budget by-law requires approval by the regulatory authority if it exceeds one fifth of the profit and loss budget's ordinary expenses.
Saxony-Anhalt	§ 110 KVG LSA (1) For the timely settlement of expenses, the municipality may take out credits up to the ceiling set in the budget by-law, provided no other funds are available. The authorization continues to hold until the budget by-law for the following year is passed. (2) The liquidity credit ceiling set in the budget by-law requires approval by the regulatory authority if it exceeds one fifth of the cash budget's revenues for current administrative activities.

Federal states	Current wording
Schleswig-Holstein	§ 87 GO For the timely settlement of expenses, the municipality may take out liquidity credits up to the ceiling set in the budget by-law, provided no other funds are available. This authorization holds beyond the current budget year until the announcement of the new budget by-law.
Thuringia	§ 65 ThürKO (1) For the timely settlement of expenses, the municipality may take out liquidity credits up to the ceiling set in the budget by-law, provided no other funds are available. This authorization holds beyond the current budget year until the promulgation of the new budget by-law. (2) The ceiling set in the budget by-law requires approval if 1. the ceiling for the core budget exceeds one sixth of the administrative budget's expenses, 2. the ceiling for owner-operated municipal enterprises or municipal institutions exceeds one sixth of the profit plan's revenues.

Notes: Local government laws. Translation by authors.

Table 5.10: Circular decrees and official government statements

Federal states	Date	Directive
Brandenburg	24.07.2013	If the budget by-law is passed in violation of the general balanced budget principle, this constitutes an unlawful act that is to be objected by the regulatory authority. The obligation for objection is not applicable if the budget by-law is accompanied by a budget consolidation plan. Budget consolidation plans require approval by the regulatory authority. A consolidation period beyond the financial planning horizon [five years] is usually not approvable. Exceptions are possible if the consolidation plan demonstrates exceptional willingness to consolidate. Municipalities requiring a budget consolidation plan should set property and business tax rates at least equal to the weighted average of tax rates in municipalities of their size range. A reduction of tax rates is not permissible until a structurally balanced budget is reached.
	22.06.2004	Liquidity credits beyond the credit ceiling will be condoned, though not formally approved, for municipalities under provisional budget management if the exceedance is due to irrefutable payment obligations. Budget consolidation plans should aim for the fastest possible restoration of budgetary equilibrium. In justified cases where the municipality demonstrates an exceptional willingness to consolidate, consolidation periods beyond the financial planning horizon may be acceptable.

Federal states	Date	Directive
	23.02.2000	If the budget by-law violates the principle of budgetary equilibrium, the budget is unlawful. In order to avoid objection to this unlawful act, a budget consolidation plan must be passed at the latest in the same session as the budget by-law. The budget consolidation plan must specify the time at which budget equilibrium of the administrative budget will be restored. In case more than one year is required, a maximum deficit must be specified for each year. In the exceptional case that the consolidation period will be longer than the financial planning horizon, financial planning must be carried forward until the point of budget equilibrium. A budget consolidation period beyond the financial planning horizon is usually not approvable. Criteria for an exceptional approval are, amongst others: Property tax rates are set at least equal to the average tax rates of municipalities in their size range.
Hesse	03.03.2014	There are special reporting requirements by regulatory authorities on muncipalities that will only achieve budget equilibrium in 2016, as well as those with liquidity credits beyond 200 euro p.c. The budget of a municipality with permanent deficits cannot be approved if the property tax rate is not at least 10% higher than the average in the respective size range. When raising business tax rates, possible consequences regarding jobs etc. are to be considered. The regulatory authority can therefore refrain from insisting on an adjustment to average tax rates. In case of permanent deficits, business tax rates below the standard tax rate of 310% are not acceptable. In case of rejection of budgets, regulatory authorities must inform municipalities that they are now operating under provisional budget management. They must monitor compliance effectively.
	06.05.2010	Municipalities with permanent deficits must have tax rates, particulary for property taxes, that are markedly above average rates in the respective size range.
	03.08.2005	In case of permanent deficits, tax rates for the property and business tax must be markedly above average rates in the respective size range.
Lower Saxony	21.07.2014	In case of permanent and irrefutable deficits, mid-term financing of the stock of liquidity credits may be justified. Maturities of up to four years may be agreed for this stock of liquidity credits. Municipalities without a deficit in the current budget year may also make use of this arrangement in case they have accumulated past deficits and a corresponding stock of liquidity credits. Municipalities making use of this exceptional arrangement must develop a concept for the medium-term reduction of liquidity credits.

Federal states	Date	Directive
	29.01.2008	In case of permanent deficits and approved liquidity credits, it is justifiable that municipalities take out the irrefutable stock of liquidity credits with a credit period of up to four years if this is more economical. This also applies to municipalities without a deficit in the current budget year if they have an irrefutable stock of liquidity credits due to past deficits.
	24.01.2003	Failure to achieve a balanced budget must be justified towards the regulatory authority, including by a budget consolidation plan. Such budgets are to be regarded as lawful even if they are in deficit.
Mecklenburg-Vorpommern	10.01.2007	Liquidity credit ceilings beyond the approval threshold should only be approved within tight limits and only after submission of a substantive liquidity preview. For municipalities in financial difficulties, further liquidity credits should only be approved within strict limits. Regular reports by the applicant on the prospective development of liquidity credits are required.
North Rhine-Westphalia	16.12.2014	The local government law does not specify maturities for liquidity credits. Municipalities have to agree on maturities of liquidity credits with creditors on their own authority. Municipalities may pass interest agreements for a part of its total stock of liquidity credits according to the following rules: For half of its total stock interest agreements may have maturities of up to ten years. For a further quarter of its total stock interest agreements may have maturities of up to five years. Interest agreements of more than five years require consultation with the competent regulatory authority.
	06.05.2011	The local government law does not specify maturities for liquidity credits. Municipalities have to agree on maturities of liquidity credits with creditors on their own authority. Municipalities may pass interest agreements for a part of its total stock of liquidity credits according to the following rules: For half of its total stock interest agreements may have maturities of up to ten years. For a further quarter of its total stock interest agreements may have maturities of up to five years. Interest agreements of more than five years require consultation with the competent regulatory authority.
	9.10.2006	The local government law does not specify maturities for liquidity credits. Municipalities have to agree on maturities of liquidity credits with creditors on their own authority. Interest agreement may not exceed five years. Interest agreements of more than three and up to five years require consultation with the competent regulatory authority.
	05.01.2006	The current rules for the approval of budget consolidation plans, according to which business and property tax rates should be markedly above average rates in the respective size range, were not intended to trigger an upward spiral of tax rates. In the future, tax rates equal to the average are sufficient. Tax rates may only be lowered when budget equilibrium has been achieved.

Federal states	Date	Directive
	30.08.2004	It is justifiable for municipalities to have a permanent stock of liquidity credits of up to 50% of average yearly liquidity credits.
	04.06.2003	Municipalities without an approved budget consolidation plan have to report the amount of liquidity credits to the regulatory authority quarterly. If the liquidity credits exceed a third of gross revenues in the administrative budget, they have to submit a liquidity plan to the regulatory authority detailing measures to reduce the liquidity credit stock.
Rhineland-Palatinate	26.09.2008	It has been considered acceptable for municipalities with permanent deficits to have a stock of liquidity credits, and to have liquidity credits with maturities of three to four years for the minimum stock. Now it is considered acceptable for regulatory authorities to allow maturities of five years if liquidity credits are needed to ensure solvency in the face of permanent deficits.
Saxony	14.12.2007	If the budget cannot be balanced in the foreseeable future, the rules of provisional budget management apply. Additionally, a budget consolidation plan must be passed specifying the feasible consolidation potentioal.
		The approval of credit ceilings beyond the approval threshold may be subject to conditions.
		Municipalities under provisional budget management may take out liquidity credits to fund permissible expenses. The municipality has to announce the take-out to the municipal council and the regulatory authority two weeks in advance for assessment.
		Liquidity credits must not exceed the ceiling set in the budget by-law at any time.
Schleswig-Holstein	31.03.2006	In case of permanent stocks of liquidity credits, it may be more economical to fund this stock with medium-term as opposed to short-term credits. It is therefore acceptable for municipalities with medium-term permanent deficits to take out liquidity credits with maturities up to the end of the financial programming period if this appears more economical.
Thuringia	09.07.2012	Municipalities operating under a budget consolidation plan must keep daily records of liquidity credits. Such municipalities must levy property and business taxes at at least the weighted average of tax rates in their size range.

Notes: Circular decrees and offical government statements. Selective summary and translation by authors.

Table 5.11: Number of municipalities with property tax rate changes, 1998-2013

	Property tax				Business tax			
	No change	One change	Two to four changes	Five to eleven changes	No change	One change	Two to four changes	Five to eleven changes
Baden-Württemberg	109	366	540	41	222	571	258	5
Bavaria	842	768	433	7	993	689	363	5
Brandenburg	12	26	18	2	31	18	9	0
Hesse	28	93	268	32	68	148	200	5
Lower Saxony	39	234	662	50	61	269	606	49
Mecklenburg-Vorpommern	128	236	206	4	196	218	147	13
North Rhine-Westphalia	0	18	296	82	4	55	312	25
Rhineland-Palatinate	53	230	1907	82	314	980	968	10
Saarland	5	22	21	3	1	3	41	6
Saxony	14	55	185	36	56	87	136	11
Saxony-Anhalt	17	20	83	41	26	21	69	45
Schleswig-Holstein	126	265	572	107	151	383	507	29
Thuringia	69	445	256	6	55	447	268	6
Total	1442	2778	5447	493	2178	3889	3884	209

Notes: Number of municipalities with property tax rate changes of business tax rate calculated for the balanced panel of 10,160 municipalities for the period 1998 to 2013. See Table 5.12 for details on panel structure of the unbalanced original panel.

Table 5.12: Panel structure by state

Federal states	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	N in all years
Baden-Württemberg	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.1	0.0	0.5	0.0	0.0	1.8	0.6	1.0	96.0	17600
Bavaria	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	100.0	32896
Brandenburg	2.1	0.2	13.2	9.7	23.2	1.3	0.7	0.6	2.7	5.6	7.6	2.9	1.1	3.5	16.5	9.1	10425
Hesse	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.1	0.0	0.1	0.0	0.0	0.2	0.0	0.4	99.1	6797
Lower Saxony	0.0	0.0	0.1	0.0	0.1	0.0	0.0	0.0	0.0	0.0	0.0	0.0	1.8	0.0	1.1	96.8	16304
Mecklenburg-Vorpommern	0.5	0.2	0.4	0.3	0.8	5.0	1.7	0.2	1.7	1.2	4.1	2.7	5.3	3.9	4.5	67.6	13795
North Rhine-Westphalia	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	100.0	6336
Rhineland-Palatinate	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.1	0.0	0.0	0.0	0.0	0.1	99.8	36887
Saarland	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	1.8	98.2	831
Saxony	2.9	0.2	0.4	0.2	0.8	0.6	0.3	0.4	0.9	1.7	0.8	1.3	3.7	5.0	24.0	56.8	8173
Saxony-Anhalt	0.0	0.0	0.4	2.0	1.7	3.5	3.3	0.8	1.4	1.2	13.8	53.5	0.2	0.0	0.2	17.9	14537
Schleswig-Holstein	0.0	0.0	0.0	0.0	0.2	0.1	0.0	0.0	0.1	0.5	0.4	0.0	0.0	0.0	0.8	97.9	17966
Thuringia	0.3	0.1	0.1	0.3	0.1	0.4	0.1	0.5	1.4	0.7	0.8	0.9	2.8	3.9	3.3	84.3	15566
Total	0.3	0.0	0.8	0.7	1.5	0.7	0.4	0.2	0.5	0.7	1.8	4.4	1.1	1.0	2.7	83.0	198113

Notes: Panel structure of unbalanced panel by federal state.

Table 5.13: Number of observations by state

Federal state	N
Baden-Württemberg	1056
Bavaria	2050
Brandenburg	58
Hesse	421
Lower Saxony	985
Mecklenburg-Vorpommern	574
North Rhine-Westphalia	396
Rhineland-Palatinate	2272
Saarland	51
Saxony	290
Saxony-Anhalt	161
Schleswig-Holstein	1070
Thuringia	776
Total	10160

Notes: Number of observation by state for the balanced panel.

Table 5.14: Components of credit access indicator by state and year, 1998-2013

Components	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013
	Baden-Württemberg															
1.1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
1.2	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
1.3	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
2	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
3.1	0	0	0	0	0	0	0	.	0	0	0	0	.	.	0	0
3.2	.	.	.	.	.	.	.	1	.	.	.	.	1	1	.	.
3.3	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
4	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
	Bavaria															
1.1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
1.2	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
1.3	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
2	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
3.1	0	.	.	.	.	.	.	.	.	.	.	0	.	.	.	.
3.2	.	1	1	1	1	1	1	1	1	1	1	.	1	1	1	1
3.3	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
4	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
	Brandenburg															
1.1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
1.2	1	1	1	1	1	1	1	1	1	1	.	.	.	.	.	.
1.3	.	.	.	.	.	.	.	.	.	.	2	2	2	2	2	2
2	.5	.5	.5	.5	.5	.5	.5	.5	.5	.5	.5	.5	.5	.5	.5	.5
3.1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
3.2	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
3.3	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
4	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
	Hesse															
1.1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	0	0
1.2	1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
1.3	.	2	2	2	2	2	2	2	2	2	2	2	2	2	.	.
2	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.

Components	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013
3.1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
3.2	1	1	1	1	1	1	1	1	1	1	1	1	1	.	.	.
3.3	.	.	.	.	.	.	.	.	.	.	.	.	.	2	2	2
4	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1	1
	Lower Saxony															
1.1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
1.2	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
1.3	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
2	.	.	.	.	.	.	.	.	.	.	1	1	1	1	1	1
3.1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
3.2	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
3.3	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
4	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
	Mecklenburg-Vorpommern															
1.1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
1.2	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
1.3	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
2	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
3.1	0	0	.	.	.	.	.	.	.	.	.	.	.	.	.	.
3.2	.	.	1	1	1	1	1	1	1	1	1	1	1	1	1	1
3.3	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
4	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
	North Rhine-Westphalia															
1.1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
1.2	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
1.3	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2
2	.	.	.	.	.	.	.5	.5	1	1	1	1	1	1	1	1
3.1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
3.2	1	1	1	1	1	1	1	.	.	.	.	.	.	.	.	.
3.3	.	.	.	.	.	.	.	2	2	2	2	2	2	2	2	2
4	.	.	.	.	.	.	.	.	.	.	.	.	1	1	1	1
	Rhineland-Palatinate															
1.1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
1.2	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.

Components	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013
1.3	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2	2
2	.	.	.	.	.	.	.	.	.	.	1	1	1	1	1	1
3.1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
3.2	1	1	1	1	1	1	1	1	1	.	.	.	.	.	.	.
3.3	.	.	.	.	.	.	.	.	.	2	2	2	2	2	2	2
4	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
	Saarland															
1.1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
1.2	1	1	1	1	1	1	1	1	1	.	.	.	.	.	.	.
1.3	.	.	.	.	.	.	.	.	.	2	2	2	2	2	2	2
2	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
3.1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
3.2	1	1	1	.	.	.	.	.	.	.	1	.	.	.	.	.
3.3	.	.	.	2	2	2	2	2	2	2	.	2	2	2	2	2
4	.	.	.	.	1	1	1	1	1	1	1	1	1	1	1	1
	Saxony															
1.1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
1.2	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
1.3	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
2	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
3.1	.	.	0	0	.	.	.	.	.	.	0	0	0	0	0	0
3.2	1	1	.	.	1	1	1	1	1	1	.	.	.	.	.	.
3.3	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
4	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
	Saxony-Anhalt															
1.1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
1.2	1	1	1	1	1	1	.	.	.	.	.	.	.	.	.	.
1.3	.	.	.	.	.	.	2	2	2	2	2	2	2	2	2	2
2	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
3.1	0	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
3.2	.	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
3.3	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
4	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.

Components	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013
	Schleswig-Holstein															
1.1	0	0	0	0	0	0	0	0	0	.	.	.	.	.	.	.
1.2	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
1.3	.	.	.	.	.	.	.	.	.	2	2	2	2	2	2	2
2	.	.	.	.	.	.	.	.	1	1	1	1	1	1	1	1
3.1	0	0	0	.	.	.	.	.	.	.	.	.	.	.	.	.
3.2	.	.	.	1	1	1	1	1	1	1	1	1	1	1	1	1
3.3	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
4	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
	Thuringia															
1.1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
1.2	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
1.3	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
2	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
3.1	0	0	.	.	.	.	.	.	.	.	.	.	.	.	.	.
3.2	.	.	1	1	1	1	1	1	1	1	1	1	1	1	1	1
3.3	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.
4	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.

Notes: Component 1.1 equals 0 if the local government law contains an unconditional approval clause or an upper limit for liquidity credit ceilings and missing (.) otherwise. Component 1.2 equals 1 point if the local government law contains a conditional approval clause for liquidity credit ceilings and missing (.) otherwise. Component 1.3 equals 2 if the local government law contains no approval clause for liquidity credit ceilings and missing (.) otherwise. Component 2 amounts to up to 1 point if circular decrees are in force which expand credit access beyond what is suggested by the letter of the law and missing (.) otherwise. Component 3.1 equals 0 if the aggregate stock of municipal liquidity credits per person is below the 15th percentile (14,8 euros per person) of all territorial states and missing (.) otherwise. Component 3.2 equals 1 if the aggregate stock of municipal liquidity credits per person is above the 15th percentile and below 85th percentile (544 euros per person) of all territorial states and missing (.) otherwise. Component 3.3 equals 2 if the aggregate stock of municipal liquidity credits per person is above the 85th percentile of all territorial states and missing (.) otherwise. Component 4 equals 1 point if more than one third of municipalities have a liquidity credit stock of more than 544 euros per person and missing (.) otherwise.

Table 5.15: Summary statistics by state

Federal state	Statistics	Number of changes			Mean of access index	Funding need growth
		PT	BT	BT and PT		
Baden-Württemberg	Mean	1.87	1.11	2.98	1.19	37.76
	Std. Dev	1.28	0.85	1.86	0.00	65.17
	Min	0.00	0.00	0.00	1.19	-154.83
	Max.	7.00	7.00	14.00	1.19	975.61
	N	1056	1056	1056	1056	1056
Bavaria	Mean	0.89	0.75	1.64	0.88	-2.82
	Std. Dev	0.96	0.90	1.64	0.00	58.45
	Min	0.00	0.00	0.00	0.88	-342.33
	Max.	6.00	7.00	9.00	0.88	725.78
	N	2050	2050	2050	2050	2050.
Brandenburg	Mean	1.47	0.72	2.19	2.88	30.52
	Std. Dev.	1.27	1.01	1.91	0.00	61.63
	Min.	0.00	0.00	0.00	2.88	-203.07
	Max.	5.00	4.00	8.00	2.88	238.79
	N	58	58	58	58	58
Hesse	Mean	2.38	1.59	3.96	3.00	46.86
	Std. Dev.	1.39	1.14	2.09	0.00	58.42
	Min.	0.00	0.00	0.00	3.00	-172.70
	Max.	7.00	7.00	12.00	3.00	374.00
	N	421	421	421	421	421
Lower	Mean	2.32	2.12	4.45	2.38	48.50
Saxony	Std. Dev.	1.30	1.26	2.31	0.00	87.57
	Min.	0.00	0.00	0.00	2.38	-376.93
	Max.	8.00	7.00	15.00	2.38	972.60
	N	985	985	985	985	985
Mecklenburg-	Mean	1.32	1.15	2.47	1.88	53.34
Vorpommern	Std. Dev.	1.08	1.20	2.01	0.00	131.11
	Min.	0.00	0.00	0.00	1.88	-642.64
	Max.	7.00	7.00	13.00	1.88	989.75
	N	574	574	574	574	574
North Rhine-	Mean	3.39	2.62	6.01	4.38	48.56
Westphalia	Std. Dev.	1.43	1.20	2.36	0.00	40.97
	Min.	1.00	0.00	1.00	4.38	-22.56
	Max.	9.00	8.00	17.00	4.38	365.52
	N	396	396	396	396	396
Rhineland-	Mean	2.54	1.44	3.98	3.81	58.89
Palatinate	Std. Dev.	1.07	0.94	1.69	0.00	126.53

Federal state	Statistics	Number of changes			Mean of access index	Funding need growth
		PT	BT	BT and PT		
	Min.	0.00	0.00	0.00	3.81	-891.83
	Max.	8.00	6.00	13.00	3.81	910.74
	N	2272	2272	2272	2272	2272
Saarland	Mean	1.78	3.04	4.82	3.94	92.00
	Std. Dev.	1.40	1.25	2.15	0.00	103.72
	Min.	0.00	0.00	1.00	3.94	-44.75
	Max.	7.00	6.00	10.00	3.94	431.03
	N	51	51	51	51	51
Saxony	Mean	2.67	1.77	4.44	1.50	42.71
	Std. Dev.	1.58	1.48	2.66	0.00	97.39
	Min.	0.00	0.00	0.00	1.50	-85.44
	Max.	11.00	8.00	18.00	1.50	940.92
	N	290	290	290	290	290
Saxony-	Mean	3.33	3.20	6.53	2.56	81.02
Anhalt	Std. Dev.	2.28	2.58	4.50	0.00	126.54
	Min.	0.00	0.00	0.00	2.56	-441.45
	Max.	10.00	11.00	21.00	2.56	833.36
	N	161	161	161	161	161
Schleswig-	Mean	2.23	1.73	3.96	2.19	84.59
Holstein	Std. Dev.	1.59	1.25	2.64	0.00	166.22
	Min.	0.00	0.00	0.00	2.19	-984.52
	Max.	7.00	6.00	12.00	2.19	999.78
	N	1070	1070	1070	1070	1070
Thuringia	Mean	1.37	1.43	2.80	1.88	31.94
	Std. Dev.	0.86	0.90	1.61	0.00	201.72
	Min.	0.00	0.00	0.00	1.88	-837.95
	Max.	6.00	6.00	12.00	1.88	974.03
	N	776	776	776	776	776
Total	Mean	1.96	1.44	3.40	2.28	42.56
	Std. Dev.	1.41	1.21	2.37	1.14	118.14
	Min.	0.00	0.00	0.00	0.88	-984.52
	Max.	11.00	11.00	21.00	4.38	999.78
	N	10160	10160	10160	10160	10160

Notes: Cross section derived from balanced panel of 10,160 municipalities for the period 1998 to 2013. Index of credit access corresponds to the mean over the 16-year horizon. Funding need growth (in %) is defined as the growth in funding need between 1998 and 2006. Number of changes and funding need growth are municipal level variables while the index of credit access is a state level variable.

Table 5.16: Summary statistics by state

Federal State	Statistics	Mun. assoc. status	Population	Employees	Private income	GDP	Left
Baden-	Mean	1.00	9941.35	0.24	19056.92	27137.46	0.01
Württemberg	Std. Dev.	0.00	26404.72	0.14	986.72	4070.35	0.07
	Min.	1.00	170.00	0.01	16761.07	20654.13	0.00
	Max.	1.00	594457.69	1.09	26845.67	56478.33	0.62
	N	1056	1056	1056	1056	1056	1056
Bavaria	Mean	1.00	6056.91	0.21	18251.39	24723.27	0.01
	Std. Dev.	0.00	32065.60	0.16	1817.73	8300.74	0.06
	Min.	1.00	232.19	0.01	15518.73	16508.47	0.00
	Max.	1.00	1.29e+06	1.84	28235.13	79894.07	0.31
N	2050	2050	2050	2050	2050	2050	
Brandenburg	Mean	0.66	11501.12	0.24	15216.02	18248.92	1.00
	Std. Dev.	0.48	12811.81	0.15	810.17	2770.97	0.00
	Min.	0.00	537.81	0.01	13775.47	14436.60	1.00
	Max.	1.00	65117.38	0.63	16822.00	28046.27	1.00
	N	58	58	58	58	58	58
Hesse	Mean	1.00	14326.22	0.22	18176.74	25212.67	0.23
	Std. Dev.	0.00	37715.47	0.14	1730.46	5724.32	0.29
	Min.	1.00	679.06	0.05	16015.93	19299.13	0.00
	Max.	1.00	660622.06	1.40	25293.80	74934.33	1.00
	N	421	421	421	421	421	421
Lower	Mean	0.28	7359.12	0.17	17011.27	20883.16	0.31
Saxony	Std. Dev.	0.45	15905.72	0.13	1203.93	4417.90	0.18
	Min.	0.00	314.19	0.01	14966.60	14517.80	0.00
	Max.	1.00	246477.81	0.91	20986.87	76327.27	1.00
	N	985	985	985	985	985	985
Mecklenburg-	Mean	0.06	2577.40	0.19	14014.18	15932.03	0.59
Vorpommern	Std. Dev.	0.24	10718.81	0.15	525.94	1596.47	0.36
	Min.	0.00	124.25	0.00	13055.43	14123.58	0.19
	Max.	1.00	201322.19	1.00	14893.60	28901.20	1.00
	N	574	574	568	574	574	574
North Rhine-	Mean	1.00	45292.18	0.26	18702.23	24064.24	0.34
Westphalia	Std. Dev.	0.00	87117.45	0.09	1593.17	4446.20	0.36
	Min.	1.00	4220.19	0.06	15109.40	16622.87	0.00
	Max.	1.00	987816.88	0.61	23023.87	64717.87	1.00
	N	396	396	396	396	396	396
Rhineland-	Mean	0.02	1767.11	0.13	17485.10	20209.04	0.74
Palatinate	Std. Dev.	0.14	7597.48	0.16	921.43	3447.34	0.33
	Min.	0.00	8.00	0.00	15634.53	12548.73	0.00

Federal State	Statistics	Mun. assoc. status	Population	Employees	Private income	GDP	Left
	Max.	1.00	192906.06	1.94	20931.13	57722.07	1.00
	N	2272	2272	2180	2272	2272	2272
Saarland	Mean	1.00	16901.06	0.24	17579.70	24651.89	0.12
	Std. Dev.	0.00	9953.28	0.15	2324.96	5017.17	0.19
	Min.	1.00	6407.94	0.04	15263.53	19034.07	0.00
	Max.	1.00	49003.63	0.67	22502.47	32668.40	0.47
	N	51	51	51	51	51	51
Saxony	Mean	0.50	10106.45	0.25	14796.39	6944.61	0.00
	Std. Dev.	0.50	43911.73	0.12	441.15	1219.15	0.00
	Min.	0.00	412.00	0.05	14070.50	6119.70	0.00
	Max.	1.00	504054.63	0.82	15741.33	22167.83	0.00
	N	290	290	290	290	290	290
Saxony-	Mean	0.44	7338.16	0.25	16130.09	20045.84	0.25
Anhalt	Std. Dev.	0.50	9589.40	0.15	518.85	2565.91	0.00
	Min.	0.00	569.19	0.03	15363.67	16259.33	0.25
	Max.	1.00	47484.38	0.91	17041.50	25971.17	0.25
	N	161	161	161	161	161	161
Schleswig-	Mean	0.07	2555.09	0.14	17514.71	21918.22	0.37
Holstein	Std. Dev.	0.26	11275.65	0.27	1213.23	3420.02	0.06
	Min.	0.00	6.25	0.00	15145.67	15213.33	0.36
	Max.	1.00	236321.94	7.70	21071.07	35782.73	1.00
	N	1070	1070	1060	1070	1070	1070
Thuringia	Mean	0.11	2658.63	0.20	14595.69	17020.85	0.03
	Std. Dev.	0.32	10355.50	0.21	481.05	1465.65	0.10
	Min.	0.00	42.38	0.00	13462.27	15074.00	0.00
	Max.	1.00	202767.94	2.18	16703.47	28488.20	0.67
	N	776	776	755	776	776	776
Total	Mean	0.47	6895.98	0.19	17308.89	21587.73	0.31
	Std. Dev.	0.50	28791.31	0.18	1890.88	6345.32	0.36
	Min.	0.00	6.25	0.00	13055.43	6119.70	0.00
	Max.	1.00	1.29e+06	7.70	28235.13	79894.07	1.00
	N	10160	10160	10031	10160	10160	10160

Notes: Cross section derived from balanced panel of 10,160 municipalities for the period 1998 to 2013. All variables correspond to the mean over the 16-year horizon. Municipal association status, population, employees (in p.c.) are municipal level variables, GDP, private income (both in euro p.c.) and left majority at state elections are county level variables.

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