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Working Paper

Political fragmentation and fiscal policy: Evidence from German municipalities

FiFo Discussion Paper, No. 17-03

Provided in Cooperation with:

FiFo Institute for Public Economics, University of Cologne

Suggested Citation: Bernard, René (2017) : Political fragmentation and fiscal policy: Evidence from German municipalities, FiFo Discussion Paper, No. 17-03, Finanzwissenschaftliches Forschungsinstitut an der Universität zu Köln (FiFo Köln), Köln

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FiFo Discussion Papers
Finanzwissenschaftliche Diskussionsbeiträge

FiFo Discussion Paper No. 17-3

**Political Fragmentation and Fiscal Policy:
Evidence from German Municipalities**

René Bernard

2017

Finanzwissenschaftliches Forschungsinstitut an der Universität zu Köln

Political Fragmentation and Fiscal Policy: Evidence from German Municipalities*

René Bernard**

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* The author thanks Michael Thöne, Marius Vogel, Jonas Dederichs-Eckertz, Lukas Römer, Robert Schwager, the audience members of the brown bag seminar of the Prognos AG and the Research Centre of the German Central Bank for helpful comments. The author thanks the FiFo Institute for Public Economics for its financial support and Anna Rauch, Mustafa Yeter, Marc-Daniel Mössinger and the team of the ZEW for sharing their data.

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Zusammenfassung

Politische Fragmentierung und Fiskalpolitik. Ergebnisse für deutsche Kommunen

Die Regierungsfragmentierungs-Hypothese (GFH) konstatiert, dass Koalitionsregierungen auf Grund eines Common Pool Problems höhere Staatsausgaben aufweisen als Einparteienregierungen. Ich teste die GFH für die kommunalen Steuersätze, das kommunale Schuldenwachstum und die kommunalen Ausgaben unter Verwendung eines Panel-Datensatzes, der 604 Baden-Württembergische Kommunen für den Zeitraum von 1994-2014 umfasst. Da der Regierungstyp generell nicht zufällig ist, können Studien, die herkömmliche Regressionsmethoden verwenden, den kausalen Effekt nicht identifizieren. Ich verwende ein Regressions-Diskontinuitäts-Design, das die quasi-zufällige Variation knapper Wahlausgänge ausnutzt, und trage zur neuen quasi-experimentellen Literatur bei, indem ich die GFH im Kontext einer Bürgermeisterverfassung analysiere. Entgegen der theoretischen Vorhersage erhöhen Koalitionsregierungen weder die Steuersätze noch die Verschuldung. Koalitionsregierungen haben einen nicht-robusten, negativen Effekt auf die Bruttoausgaben, der im Wesentlichen von den Verwaltungsausgaben und dem Laufenden Sachaufwand getrieben ist.

Schlagerworte: Fragmentierung der Regierung, Common Pool Problem, Gesetzgebung und politische Entscheidungsfindung, Staatsausgaben, kommunale Finanzpolitik, kommunale Steuern, Kommunalwahlen, Gemeindedaten, Regressions-Diskontinuitäts-Design

JEL-Classification: C21, D72, D78, H11, H71, H72

Abstract

Political Fragmentation and Fiscal Policy: Evidence from German Municipalities

The government fragmentation hypothesis (GFH) states that coalition governments spend more than single-party governments due to an underlying common pool problem. Using a large panel data set on 604 local governments in the German state of Baden-Württemberg for the 1994-2014 period, I test the GFH for tax rates, the growth in government debt as well as expenditures and its sub-categories. Studies using standard regression methods fail to identify causal effect as the type of government is generally not random. I apply a RDD, that exploits quasi-random variation generated by close elections. I add external validity to the recent quasi-experimental literature by investigating the GFH for a mayor-council system. I find that contrary to the theoretical prediction, coalition governments do not increase taxes and government debt. There is a non-robust, negative effect on total expenditures, which is mainly driven by administrative expenditures and material expenditures.

Keywords: government fragmentation, common pool problems, legislative policy-making, government spending, local fiscal policy, local taxation, local elections, municipality data, regression discontinuity design.

Political Fragmentation and Fiscal Policy: Evidence from German Municipalities

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List of Abbreviations

- AIC** Akaike's information criterion
- ATE** average treatment effect
- CDU** Christian Democratic Union of Germany
- CPI** consumer price index
- CSU** Christian Social Union in Bavaria
- FDP** Free Democratic Party
- FE** fixed effects
- FWG** Free Voters Association
- GemO** municipal code of Baden-Württemberg
- HERF** Herfindahl-Hirschman concentration index
- ITT** intention-to-treat effect
- IV** instrumental variable
- NoP** number of parties in government
- NRW** North Rhine-Westphalia
- OLS** ordinary least squares
- RDD** regression discontinuity design
- RKD** regression kink design
- SPD** Social Democratic Party of Germany
- ToG** type of government
- TOT** treatment-on-the-treated effect
- WATE** weighted average treatment effect

1 Introduction

This paper investigates the impact of political fragmentation on fiscal policy using a large panel data set on German local government. Political fragmentation arises when several political agents are involved in decision making as, for example, in a coalition government. The government fragmentation hypothesis from the political economy literature states that political fragmentation leads to an increase in public debt, taxes and expenditures due to an underlying common pool problem.

Yet, the empirical evidence on the effect of government fragmentation is mixed. The findings differ depending on the data set and the operationalization of government fragmentation. A major short-coming of the literature is the lack of causal evidence. The basic problem of identification is that the type of government - that is to say, coalition or single-party government - is not random. As a consequence, most empirical studies do have to cope with problems of omitted variable bias and reverse causality. A recent strand of literature offers a new way to identify causality by applying quasi-experimental methods. Nevertheless, the results of this literature remain ambiguous. Thus, the question is unresolved as to whether the type of government is an important determinant of fiscal policy.

The aim of this paper is to analyze the government fragmentation hypothesis in more detail by investigating the effect of coalition governments not only on taxes, as well as total debt and expenditures, but also on various sub-categories of spending and debt. Thus, it helps to understand why the government fragmentation hypothesis does not always hold. For identification I apply a regression discontinuity design (RDD) to exploit quasi-random variation generated by close elections. More specifically, I make use of the fact that in a parliamentary system the type of government changes discontinuously from coalition to single-party government if the strongest party's proportion of seats exceeds 50 percent. This identification strategy is closely related to Garmann (2012) and helps to overcome problems of reverse causality inherent in previous analyses.

The paper contributes to the literature in several ways. Firstly, I use a large panel data set on 604 local governments in the German state of Baden-Württemberg for the 1994-2014 period. This permits me to analyze the effect of government fragmentation in an institutionally-homogeneous setting as all political units operate under a common legal and institutional framework and are subjected to the same electoral system. Secondly, the paper sheds light on the question whether the effect of government fragmentation depends on the political regime employed at the local level. A large literature, reviewed extensively by Persson and Tabellini (2004) and Voigt (2011), shows that presidential regimes fundamentally differ from parliamentary system in terms of fiscal outcomes. As one of the first the paper studies the government fragmentation hypothesis in the context of a mayor-council system. This is of relevance, because the reforms in German local constitutions since the mid-1990s implied a shift from council-manager systems towards mayor-council systems. Thirdly, the study gives insights to what extent the fragmentation effect found for German municipalities depends on socio-economic factors. Finally, the paper offers a careful sensitivity analysis to check for the robustness of the results.

The empirical analysis shows that coalition governments do *not* increase deficits and taxes by more than single-party governments. In contrast to the theoretical prediction, there is a small negative effect on total expenditures, which is statistically significant in some specifications. When disaggregating the total effect by sub-categories, I find that it is mainly driven by a negative effect of coalition governments on administrative expenditures and material expenditures. Material expenditures decrease significantly by 6.3 percent if a coalition government takes over.

However, these effects are not robust to different polynomial specifications. Personnel expenditures and investment expenditures are unaffected by the type of government. Both, the main analysis using the within variation, as well as a supplementary cross-sectional analysis yield this result. The findings are in line with previous quasi-experimental studies by Freier and Odendahl (2012) and Garmann (2014) that use German municipal data as well.

The remainder of the paper is organized as follows: Section 2 sets out the theoretical framework. Section 3 reviews the empirical literature. Section 4 describes the institutional background and the data set. Section 5 discusses the identification strategy. Section 6 reports the main results and checks for robustness. Section 7 contains concluding remarks.

2 Theoretical Considerations

The political economy literature on common pool problems argues that coalition governments spend more than single-party governments due to an underlying common pool problem. The basic idea goes back to a paper of Shepsle and Weingast (1981). Parties are modeled as self-interested, rational actors who represent different interest groups. To get reelected they try to target as many resources as possible towards their constituency. All spending proposals are financed out of the government budget, which represents a common pool. This implies that the political costs of spending proposals in the form of higher taxation or debt are equally shared amongst all coalition partners. As a result, each coalition partner fully reaps the benefits of his spending proposal in terms of electoral support, while he internalizes only a fraction of the associated political costs. This sets an incentive to over-utilize the public budget. In contrast, in a single-party government the ruling party fully internalizes the political costs of its spending proposals for all its constituencies. Thus, fiscal policy outcomes should systematically differ between multi-party and single-party governments

Yet, several theoretical arguments suggest that coalition governments spend less, not more. First, Pettersson-Lidbom (2012) hypothesizes that legislative fragmentation at the local level alleviates an agency problem between the municipal council and the public bureaucracy. In the spirit of Niskanen (1971), public officials try to maximize their budget to raise prestige. In contrast, time-constrained legislators are assumed to favor austerity.¹ However, they can only imperfectly control the public bureaucracy as this is time consuming. Legislative fragmentation implies an increase in the council size. A higher number of legislators in the council strengthens the monitoring capacity of the legislative branch towards the public administration. As a result, legislative fragmentation leads to lower public spending. The same logic might apply to government fragmentation, as two parties in government should be able to control better the public administration than a single-party in government (Garmann 2014).² Moreover, a higher number of parties in government might fill the knowledge gap on behalf of politicians by increasing man power.

Second, coalitions might enforce self-control within governments and hence restrict rent-seeking and excessive spending. This is based on the notion that politicians are more likely to monitor each other effectively if their party affiliation differs (Garmann 2014).

Third, Primo and Snyder (2008) theoretically show that the common pool problem is alleviated, if there is partial cost sharing, that is to say, only a part of the costs is financed out of a

¹Politicians are assumed to correspond to the wishes of their voters, which are fiscally conservative. This view is based on Peltzman (1992).

²In a parliamentary system governing parties might exert a more effective control over the bureaucracy as they have direct access to the administration, as opposed to parties in the opposition.

common pool since the rest can be targeted towards beneficiaries of projects by charging user fees.³ In contrast to the typically assumed full cost sharing, partial cost sharing implies that beneficiaries of projects have to bear a disproportionate share of the project costs. Therefore political costs of special interest projects of a party are re-internalized.

Fourth, coalition governments might spend less since they are less likely to implement projects of fixed size. Assuming each party has a favorite (infrastructure) project that is indivisible, the spending decision is a discrete choice. Assuming that every coalition partner is only willing to agree on the other one's project, if his own project is realized, coalition governments face a take it or leave it situation: Either implement both projects or none at all. Especially if total costs of all projects are high, it is likely that the latter option is chosen. In contrast, single-party governments do not have to deal with this coordination problem. Instead, they implement their most favored project just according to their preferences. Therefore, single-party governments might exhibit higher expenditures (Freier and Odendahl 2012).

3 Empirical Findings

The empirical evidence for the government fragmentation hypothesis is mixed. The results are sensitive to the selected sample used, the operationalization of government fragmentation, and the statistical method employed. Therefore we still lack conclusive evidence on the effect of government fragmentation.

A major shortcoming of cross-country studies is that they fail to account for different institutional backgrounds and political culture since it is difficult to capture all intervening effects by means of control variables. Thus these studies likely yield biased results. Both, cross-country and state-level studies, face the problem that the electoral system is endogenous as higher tiers of governments typically determine their own voting rules, which in turn might have an influence on the type of government.⁴ In contrast local government studies avoid these endogeneity problems as all political units operate under a common legal and institutional framework and are subjected to the same voting rules that are set by state law. An additional advantage of those studies is that the sample size is much larger.

Studies using standard regression methods fail to identify the causal effect as that the type of government is generally not random. Therefore a recent strand of literature applies quasi-experimental methods to identify causality. The idea is to exploit close elections as source of exogenous variation in the type of government.

Relying on within variation Artés Caselles and Jurado (2014) apply a RDD to show that coalition governments run higher budget deficits for the Spanish case. To determine close elections the authors use the seat margin, which is defined as the number of seats by which the incumbent party is above the minimum needed to form a majority government. There are two problems with this study: on the one hand, the data set might be too short for using fixed effects (FE), since it covers only two electoral terms. On the other hand, the study might suffer from an omitted variable bias, as the bursting of the housing bubble in 2007 - right before second legislative period from 2008 to 2011 - might have caused both, a surge in government debt and a fragmentation of the party system, which in turn might have increased the number of coalition governments.

³Primo and Snyder (2008) originally refer to geographical fragmentation. However, the same logic applies to governments fragmentation, as well.

⁴For instance, in proportional election systems parties already in parliament might decide to introduce seat hurdles or to apply a seat allocation function that is known to discriminate against smaller parties and thus to reduce the incidence of coalition governments by strengthening larger parties.

Using a large panel on Finnish municipalities Meriläinen (2013) finds that coalition governments have significantly higher total expenditures. Although sub-categories of spending follow a similar pattern, the effect is not robust. The effect on tax revenues and deficits is ambiguous. To detect close elections Meriläinen (2013) computes the minimal vote change required to change the distribution of seats.

Freier and Odendahl (2012) and Garmann (2012, 2014) focus on German municipalities. For a panel of 2051 Bavarian municipalities over the 1996-2008 period Freier and Odendahl (2012) show that coalition governments decrease the property tax rates and spend less. Furthermore, there is weak evidence that coalition governments decrease total debt and investment spending. To detect close election the authors run computer simulations, where they randomly perturb voting outcomes many times and check how often this changes majorities in the council. However, their choice of simulation parameters and the magnitude of perturbation is not theoretically founded.

Analyzing a panel of 396 municipalities in the German State of North Rhine-Westphalia (NRW) for the 1985-2004 period by using an RDD, Garmann (2012) finds that coalition governments have significantly higher personnel expenditures. The remaining spending categories yield inconclusive results. To detect close elections he uses the strongest party's proportion of seats. The data set covers a major institutional reform, which might affect the treatment probability. In 1999 the seat allocation function was changed from D'Hondt to Hare-Niemeyer. The former is known to discriminate against smaller parties and hence might decrease fragmentation. Furthermore, the council-manager system was replaced by a mayor-council system, which gives more power to the mayor. Garmann (2012) therefore uses time fixed effects. However, it is questionable whether this is approach sufficient to account for such a structural break.

In a follow-up paper Garmann (2014) restricts the data set to the 1985-1999 period and applies a regression kink design (RKD) that exploits the fact that there is a slope change in the treatment probability when the vote share of the strongest party exceeds 50 percent. In contrast to his previous paper, he finds that coalition governments generate lower tax revenues and exhibit lower total spending. The latter effect is mainly due to lower spending on public administration.

4 Institutional Framework and Data

4.1 German Municipalities

Municipalities are the lowest tier of government in Germany. They are economically quite important, as they account for one-third of total government spending and employ 40 percent of all state employees. The state of Baden-Württemberg, located in the south-west of Germany, has 1101 municipalities, which can be characterized as small and medium-sized compared to the rest of Germany.⁵ Municipalities are allowed to set freely three tax rates: a local tax on business profits (*Gewerbesteuer*), a tax on agricultural land (*Grundsteuer A*), and a tax on business and private land (*Grundsteuer B*).⁶ Municipalities are required to balance their budget each year. Contrary to the state and federal level, municipalities are allowed to go into debt only to finance investments or to restructure existing debt. Debt for consumptive purposes is ruled

⁵Only nine cities have more than 100,000 citizens, whereas 1,001 municipalities have not more than 20,000 citizens and account for 50 percent of the state population (Wehling 2010).

⁶Aside from local tax revenues the municipal budget consists of grants from upper tiers of government, parts of the income tax revenue and value-added tax revenue, as well as fees and financial contributions.

out per state law. Municipalities have to fulfill a range of duties, which can be divided into two groups according to the degree of discretion: firstly, mandated spending are tasks that the state or federal government delegates to municipalities, which have no discretion with regard to type and scope of execution. Secondly, autonomy spending tasks, which subdivide in limited autonomy spending (*pflichtige Selbstverwaltungsangelegenheiten*) and voluntary spending (*freiwillige Selbstverwaltungsangelegenheiten*). For both groups of spending, municipalities hold planning sovereignty and bear financial, organizational and staff responsibility (Grunow 2012). Municipalities decide freely whether and in what manner they carry out voluntary spending tasks. Since upper tiers of government neither influence decision-making, nor contribute to funding of these tasks, I expect the effect of the government fragmentation to be particularly strong for voluntary spending.

As form of government, all municipalities in Baden-Württemberg apply the mayor-council system. Accordingly, the municipal affairs are managed jointly by the municipal council and the mayor, who is elected directly. The municipal council is the main legislative body at the local level. It exercises municipal planning competence, has the right to demand decision-relevant information from the local administration, lays down the guidelines for the municipal administration and decides on all issues concerning the municipality, except for those which lie within the competence of the mayor. The council has the exclusive right to enact community statutes, to decide on the municipal budget, and to appoint personnel. Resolutions are taken by simple majority. In the event of a tie, the resolution is deemed to be rejected. Elections of municipal officials and representatives for other municipal institutions are carried out by an absolute majority of the votes.

The council is elected directly by the citizens of the municipality every five years using an open-list proportional election system.⁷ The number of seats in the council is determined by population size and ranges from 8 to 60 seats.⁸ The vote shares of parties are translated into mandates using the d'Hondt seat allocation function (highest average method). There is no three or five percent hurdle that parties have to pass to enter parliament, like at the federal level.⁹ Since the D'Hondt method discriminates against smaller parties, it sets an incentive for mergers of smaller parties and hence may counteract fragmentation (Lübbert 2009).

Five parties characterize Baden-Württemberg's political landscape at the local level: the market-liberal Free Democratic Party (FDP); the moderately left-leaning Alliance '90/The Greens with a focus on environmental issues; the center-left Social Democratic Party of Germany (SPD); the culturally conservative, moderately market-oriented Christian Democratic Union of Germany (CDU); and the Free Voters Association (FWG), a conservative party that operates only on the local level. Baden-Württemberg is a traditional stronghold of the CDU. However, in recent years the FWG gained in importance especially in smaller municipalities. The remaining parties, if any, win local elections in larger cities.

⁷It is allowed to select applicants from other party list (*Panaschieren*) and assign up to three votes to one applicant (*Kumulieren*).

⁸The population size is translated into seats using a step-wise function. There are 11 council size categories ranging from 8 (municipalities with up to 1,000 inhabitants) to 60 seats (municipalities with more than 400,000 inhabitants).

⁹Although, all parties are considered in the seat allocation process, the D'hondt method can cause a hidden threshold, that is to say, the seat allocation process generates a minimum vote share that is needed to get one seat.

4.2 Data

I constructed a new panel data set covering yearly information on electoral, financial and population variables for all municipalities in the German state of Baden-Württemberg.¹⁰ The data set covers 4 legislative terms: 1994-1998, 1999-2003, 2004-2008, and 2009-2013. I use the following outcome variables: growth rate of total credit market debt, growth rate of credit market debt of the core budget, growth rate of credit market debt of public undertakings, total expenditures, administrative expenditures, personnel expenditures, material expenditures, investment expenditures, multiplier property tax A, multiplier property tax B, and trade tax multiplier.

Personnel expenditures and material expenditures are sub-categories of administrative expenditures, which in turn is a subcategory of total expenditures, along with investment expenditures. The expenditure variables are taken from the annual financial statement statistic (*Jahresrechnungsstatistik*) of the Baden-Württemberg State Statistical Office.¹¹ Municipal tax variables are taken from the German Federal Statistical Office. The multipliers for the three municipal tax rates are expressed in percentage points. I use the percentage change in debt, rather than levels, in order to obtain a stationary time series.¹² The debt variables only cover the period from 2000 to 2009 due to structural breaks in the debt statistic of the Baden-Württemberg State Statistical Office.¹³ To make observations comparable across years and legal entities I express all debt and public expenditures variables in per capita terms in constant 2010 prices (EUR) using the federal consumer price index (CPI).

In the robustness section I use the following baseline covariates as control variables: population size, population density, the share of old and young, the number of employees per capita, the number of commuters per capita, the number of parties in the council, and the council size. Population variables are taken from the German Federal Statistical Office and the Baden-Württemberg State Statistical Office. The number of commuters and employees are taken from the German Federal Employment Agency. Most of these variables are standard in the political economy literature and are presumably correlated with both, the public finance variables and the type of government. Some of the controls need clarification. I use the number of commuter outflows as an additional socio-economic control variable to proxy for economic strength of a municipality. I expect municipalities with less commuter outflows per capita to be economically more attractive and thus to have more fiscal resources. The share of old and young people, respectively, is included since both variables are likely to affect the partisan composition of the council and fiscal policy outcomes at the same time. Besides that, the data set contains regional and election year dummies and categorical variables that are explained further in the robustness section.¹⁴ Table 12 in the Appendix provides summary statistics for all variables.

To correct for right-skewness in the distribution of variables and to ease interpretation, I apply a logarithmic transformation to the outcome and control variables for estimation.¹⁵ Thus for small values the treatment effect can be interpreted as expected percentage change in the outcome variable.

¹⁰Table 11 in the Appendix provides an overview of specific definitions of variables and data sources.

¹¹I owe gratitude to the Fifo for financial support in data procurement.

¹²A similar approach is taken by Jochimsen and Nuscheler (2011).

¹³As of the year 2010 a new accounting method was applied - e.g. cash advances are included only since 2010. Furthermore, the graphical analysis revealed a structural break in the time trend of the debt variables in the year 2000. To calculate annual growth rates I use information on debt levels provided by the Baden-Württemberg State Statistical Office. If the debt level changes from zero to a positive level I set the growth rate equal to one.

¹⁴See also table 11 in the Appendix .

¹⁵Most of the histograms of the outcome variables and baseline covariates reveal a right-skewness, as shown in figure 2 and 3 in the Appendix.

For estimation all right-hand side variables are lagged by one year.¹⁶ I consider the treatment status in year $t - 1$ to be decisive for the fiscal outcomes in year t due to the long decision and implementation lags inherent to the budget implementation process.

A potential weakness of the data set is that it does not provide information about the type of government in a specific municipality, that is to say, whether a coalition government is in charge. This problem is common to studies using local election data. Following Garmann (2014), I use a proxy for coalition governments: I instrument for the realized treatment status by generating a treatment dummy D_i , which equals one if the strongest party's proportion of seats is smaller than 50 percent. I am interested in the treatment-on-the-treated effect (TOT), that is to say, the causal effect of coalition government for those municipalities without an absolute majority. However, the treatment dummy measures the intention-to-treat effect (ITT) - the causal effect of being assigned to treatment (having no absolute majority). This poses a problem if there is imperfect compliance. For instance, it is not a priori given that all municipalities with the strongest party having no absolute majority, exhibit a coalition government. Imperfect compliance might arise out of two reasons: On the one hand, there is the possibility of never-takers, that is to say, political parties who avoid building a coalition and opt for a minority government if they fail to win an absolute majority.¹⁷ However, minority governments are forced to bargain with other parties, just like coalition partners, since adopting an annual budget statute always requires an absolute majority of the seats. Thus, the common pool problem should appear, as well. What is more, minority governments are rare and stand in conflict with German political culture which favors stable majorities - at least at the state level they tend to be the exception rather than the rule. On the other hand, there is the possibility of always-takers - that is political parties, who always prefer to coalesce, even if they have an absolute majority of the seats. In the absence of crisis, however, it is rational to build a minimal winning coalition, as shown theoretically by Baron and Diermeier (2001). Accordingly, a party should not coalesce if it holds the absolute majority of seats. Empirics confirms this for the German case: oversized coalitions never occurred at the state level in the last five decades. Thus the proportion of always-takers should be rather low.

The desired TOT is given by ratio of the ITT over the corresponding difference in compliance rate between treatment and control groups (Angrist and Pischke 2015):

$$TOT = \frac{ITT}{\text{Compliance rate treatment group} - \text{Compliance rate control group}}$$

The discussion above suggests that minority governments and oversized coalitions are rather unlikely. Thus, I expect the percentage of treated in the treatment group to be close to one and the percentage treated in the control group to be close to zero. Therefore, the estimated ITT should be close to the TOT. Furthermore the ITT serves as lower-bound for the TOT as long as the compliance rate in the treatment group is greater than the compliance rate in the control group (Garmann 2014). Thus, if any, I would underestimate the actual causal effect. To sum up, the treatment dummy seems to be a reasonable proxy for coalition governments.

I consider only those observations where the mayor either belongs to the largest party in the

¹⁶I regress the outcome variables of the current year on the treatment dummy of the previous year. In an RDD setting this implies to use a lagged assignment variable, as well. It also requires to use lagged baseline covariates, since controlling for covariates, in order to avoid potential omitted variable bias, is only meaningful if covariates are predetermined. To avoid a loss of observations due to the lagging of the treatment variable, I include the year 2014 for all outcome and control variables.

¹⁷For a graphical illustration of treatment types see table 13 in the Appendix .

council or has no party affiliation.¹⁸ This permits me to isolate the fragmentation effect and to abstract from partisan conflicts between the council and the mayor.¹⁹ I assume that both, independent and party affiliated mayors, vote in favor of a motion of the strongest party in case of a tie in the council.²⁰ As a consequence, treatment (coalition government) is assigned if the proportion of seats held by the largest party is smaller than 50 percent.

I use only those observations with more than 2 parties in the council, that is to say, I focus on proportional elections and abstract from two-party-systems, as treatment (coalition government) is only possible if there are at least three parties in the council. Furthermore, I drop the nine county free cities since they are not comparable to the remaining municipalities - county free cities have to take on additional financial tasks. This leaves me with a panel of 607 municipalities, that I observe over a period of around 14 years on average. In total the data set contains 8352 municipality-year observations. Due to the restriction of the sample the panel data set is not perfectly balanced.

Table 1 shows the number of coalition and single-party governments for each election period. The fraction of coalition governments is roughly constant across election periods. It hovers around 67 percent.

Table 1: Type of government by legislature period

Election Period	Type of Government		Fraction of coalition gov.	Total
	Single-party	Coalition		
1994 - 1998	98	223	69%	322
1999 - 2004	192	331	63%	523
2005 - 2008	123	252	67%	375
2009 - 2013	122	329	73%	451

Notes: This table reports the number of coalition governments and single-party governments for each legislative period. The number of observations varies over legislative periods since only those municipality-year observations are considered for which the mayor either belongs to the strongest party in the council or runs as independent.

Source: Own calculations based on data from the Baden-Württemberg State Statistical Office.

Table 2 reports the type of government by different categorical variables. The upper panel in table 2 shows election results split up by party identity. The CDU is the strongest party in 60 percent of elections, followed by the FWG, which wins 36 percent of elections. Both parties, the CDU and the FWG, win an absolute majority with approximately the same probability of 32 to 35 percent in case they are the strongest party. In the rare event that one of the remaining parties is the local topdog, a single-party government is hardly ever build. The second panel in table 2 shows the distribution of the type of government for different council size categories. The probability that a municipality is ruled by a coalition government increases with the size of

¹⁸Since there are no official statistics on the party affiliation of the mayor, I rely on a unique data set of Foremny et al. (2014). It provides consistent information on the party affiliation of the mayors for almost all municipalities for the 1994 to 2014 period. I constructed the data for the missing years up to 2014 using information supplied by www.staatsanzeiger.de and www.wikipedia.de.

¹⁹Freier and Odendahl (2012) follow a similar approach.

²⁰In the robustness section I test whether the coding of independent mayors has an impact on the results.

the municipal council: Coalition governments are installed in roughly half of the municipalities with no more than 15 seats, whereas this is the case in more than two-thirds of the municipalities with more than 23 seats.

Table 2: Type of government by different categorical variables

	Type of government						Total	
	Single-party			Coalition				
	No.	% (row)	% (col.)	No.	% (row)	% (col.)	No.	% (col.)
Strongest party								
CDU	1,597	32.4%	59.6%	3,337	67.6%	58.8%	4,934	59.1%
SPD	23	6.4%	0.9%	338	93.6%	6.0%	361	4.3%
FWG	1,060	35.2%	39.6%	1,952	64.8%	34.4%	3,012	36.1%
FDP	0	0.0%	0.0%	5	100.0%	0.1%	5	0.1%
Gruene	0	0.0%	0.0%	40	100.0%	0.7%	40	0.5%
Number of seats								
Not more than 10	100	48.1%	3.7%	108	51.9%	1.9%	208	2.5%
10 - 12	286	42.9%	10.7%	380	57.1%	6.7%	666	8.0%
13 - 14	601	48.0%	22.4%	650	52.0%	11.5%	1,251	15.0%
15 - 18	513	30.1%	19.1%	1,193	69.9%	21.0%	1,706	20.4%
19 - 22	596	29.3%	22.2%	1,441	70.7%	25.4%	2,037	24.4%
23 - 26	256	22.9%	9.6%	861	77.1%	15.2%	1,117	13.4%
27 - 32	245	35.4%	9.1%	447	64.6%	7.9%	692	8.3%
more than 32	83	12.3%	3.1%	592	87.7%	10.4%	675	8.1%
Number of parties								
Three	1,978	50.1%	73.8%	1,973	49.9%	34.8%	3,951	47.3%
Four	633	20.2%	23.6%	2,499	79.8%	44.1%	3,132	37.5%
Five	65	6.2%	2.4%	982	93.8%	17.3%	1,047	12.5%
Six	4	1.9%	0.1%	208	98.1%	3.7%	212	2.5%
Seven	0	0.0%	0.0%	10	100.0%	0.2%	10	0.1%
Region								
Württemberg	1,290	32.0%	48.1%	2,743	68.0%	48.4%	4,033	48.3%
Baden	1,390	32.2%	51.9%	2,929	67.8%	51.6%	4,319	51.7%
Total	2,680	32.1%	100.0%	5,672	67.9%	100.0%	8,352	100.0%

Notes: This table reports the number of coalition governments and single-party governments for different categorical variables: The upper panel shows the type of government split up by the identity of the strongest party; the second panel shows the results for different council size categories; the third panel shows the relation between the number of parties in the council and the type of government; the last panel shows the type of government split up by regions. Column 2 and 5 indicate row percentages. Column 3, 6, and 8 indicate column percentages.

Source: Own calculations based on data from the Baden-Württemberg State Statistical Office.

The data set has several advantageous characteristics: firstly, I do not only examine total expenditures, but also its sub-categories. This allows me to analyze the causal chain in greater depth than existing studies. Secondly, a common problem for studies using German local government data is the changeover to double-entry accounting methods. In Baden-Württemberg there is no such structural break.²¹ Finally, in many German states there was a major reform of

²¹Municipalities in Baden-Württemberg are forced to change from cameralistic to double-entry provisions not

the local election system in the 1990 - not so in Baden-Württemberg. As a result, the data set covers an exceptionally long time period which allows me to exploit within variation.

5 Identification Strategy

The challenge for identification is that the type of government is generally not random. Unobserved variables, like voter preferences, might determine both, fiscal policy and the type of government. This poses a problem of omitted variable bias. Furthermore, reverse causality arises if policy outcomes affect voter preferences and thus in turn the type of government. Hence ordinary least squares (OLS) estimations likely yield biased results as the type of government is likely to be endogenous.

To overcome this identification problem, I use a RDD to exploit exogenous variation generated by close elections. The RDD makes use of the fact that the treatment status changes discontinuously at a known threshold of a continuous assignment variable:

$$D_i = \begin{cases} 0 & \text{if } x_i \geq x_0 \\ 1 & \text{if } x_i < x_0 \end{cases}, \quad (1)$$

where the assignment variable, x_i , is the share of seats of the strongest party in the council and the threshold value, x_0 , lies at 50 percent of the seats.

A municipality belongs to the treatment group ($D_i = 1$) if the strongest party gains less than 50 percent of the seats ($x < x_0$).²² Hence, treatment status changes discontinuously if the strongest party's share of seats surpasses the 50 percent threshold. The treatment effect, ρ is estimated as the difference in fiscal outcomes between observations, who lie just above and just below the threshold. This is formally given as the difference in the conditional expectation of the outcome variable, Y_i , at the discontinuity, x_0 :

$$\lim_{x \downarrow x_0} \mathbb{E}[Y_i | x_i = x] - \lim_{x \uparrow x_0} \mathbb{E}[Y_i | x_i = x] = \rho \quad (2)$$

The RDD is based on the fundamental identifying assumption that in close elections it is essentially random, whether the strongest party barely wins an absolute majority, that is to say, whether the assignment variable, x_i , is just just above or just below the threshold, x_0 . This assumption seems quite reasonable: Although, parties exert influence on their electoral success, they can only *imprecisely* control whether they win 50 percent or 51 percent of the seats. It is exactly this *imprecise* control of election outcomes, which leads to quasi-random variation in treatment assignment closeley at the threshold. As a consequence, both, observation slightly below and above the 50 percent threshold, have similar characteristics except for the treatment. Thus the latter serves as valid counterfactual for the former. Hence the RDD gives an average treatment effect (ATE) at the threshold, x_0 (Lee and Lemieux 2010).

Since the assignment variable is rather discrete, I use a parametric approach to identify the treatment effect.²³ To estimate the treatment effect at the threshold the parametric approach relies on regressions. For extrapolation it exploits the whole range of observations, even those far away from the cut-off point, and assumes a specific functional relation between the outcome

before 2020.

²²I consider only those cases in which the mayor belongs to the strongest party. Hence 50 percent of the seats ensures an absolute majority.

²³Garmann (2012) also follows this approach, which is suggested by Lee and Lemieux (2010).

and assignment variable. The parametric approach is formalized by the following fixed effects regression equation:²⁴

$$Y_{it} = D_{it}\rho + f(\tilde{x}_{it}) + \mathbf{X}_i\delta + \alpha_i + \lambda_t + \varepsilon_{it}, \quad (3)$$

where t and i indicate time and individuals, respectively. Y_{it} is the outcome variable for observation i at time t . D_{it} is the treatment dummy, which equals 1 if observation i belongs to the treatment group. ρ indicates the treatment effect at the cut-off point. α_i is an individual dummy that captures the unit-specific error component. To correct for the selection bias stemming from selection on observables (Heckman and Robb 1985), a control function, $f(\cdot)$, is included, that traces the relationship between the assignment variable, \tilde{x}_{it} , and the outcome variable, Y_{it} . To ease interpretation the assignment variable, \tilde{x}_{it} , is centered at the cut-off point, x_0 .²⁵ Furthermore, a vector of covariates, \mathbf{X}_i , and year-fixed effects, λ_t , can be included in the regression equation to improve precision of estimates by reducing sampling variability. However, this is generally not necessary to obtain an unbiased, consistent estimator of the treatment effect, as treatment is assigned independently of baseline covariates due to quasi-random variation (Lee and Lemieux 2010). The error term, ε_{it} , is likely to be autocorrelated within a municipality, i . To account for this, I follow Bertrand, Duflo, and Mullainathan (2004) and cluster standard errors at the municipal level.

The main challenge of the parametric approach is to specify a correct functional form, $f(\cdot)$. If the functional form is misspecified, the estimated treatment effect is biased (Angrist and Pischke 2015). Therefore I test different models with control functions that are flexible on either side of the threshold and contain polynomials up to order four. In general it holds that an effect is more reliable if it is robust to various specifications. To select the best-fitting model I use Akaike's information criterion (AIC), which captures the bias-precision trade-off of using a more complex model: on the one hand, it penalizes for complexity, that is to say for an increase in the number of parameters used in the model. On the other hand, it rewards goodness of fit, which is likely to grow with an increasing number of parameters in the model.

There are two potential problems to identification. First, the number of observations in the control group (single-party governments) is smaller than in the treatment group (coalition governments), and municipalities with a smaller council size are over-represented in the control group, as shown in the second panel of table 2. Thus the average council size is smaller in the control group. Second, the degree of closeness to the 50 percent seat share threshold varies with the council size: The proportion of seats of the strongest party for a municipality with a relatively small council can never be as close to the 50 percent threshold, as for a municipality with a relatively large council, whereas the probability of being exactly at the threshold is independent of the council size. This selection effect vanishes if the council size exceeds 18 seats.²⁶

There are two solutions to overcome the selection problem. Firstly, for the main estimation I follow Pettersson-Lidbom (2012), and exploit the variation within municipalities by using municipality fixed effects.²⁷ The fixed effects approach ensures that treatment and control group are balanced. By design, observations below and above the threshold are similar with respect to time-invariant covariates independently of sample size. As a consequence the control

²⁴The following section is based on Jacob et al. (2012).

²⁵The normalized assignment variable is given as: $\tilde{x}_{it} \equiv x_{it} - x_0$. Normalization of the assignment variable at the cut-off point is a common procedure in the literature.

²⁶For a more detailed discussion see section A2 in the Appendix.

²⁷The same approach is taken by Garmann (2012) and Artés Caselles and Jurado (2014).

group serves as valid counterfactual for the treatment group since the comparison is made within units. Thus there is no selection problem. However, some municipalities changed their council size. Thus the degree of closeness does not remain the same for all municipality. Therefore, I control for the size of the municipal council. An additional advantage of including fixed effects is that it might reduce sampling variance and hence might be more efficient than a pure cross-sectional approach (Lee and Lemieux 2010). Secondly, as a robustness check, I use the cross-sectional variation and follow Eggers et al. (2015), who suggest to exclude those observations close to the threshold, where imbalance exists. Therefore, I restrict the sample to municipalities with a council larger than 18 seats. To account for the fact that the degree of closeness to the threshold systematically varies with the council size, I control for the latter in the cross-sectional regression.

6 Results

6.1 Graphical Representation of RDD

Figure 1 illustrates the basic results of the RDD. It presents the logarithm of the debt, expenditure and tax variables, respectively, as a function of the assignment variable, that is to say, of the normalized share of seats of the strongest party in the municipal council.²⁸ Each point in figure 1 is a sample average of the outcome variable within an interval of the assignment variable - a so called bin.²⁹ To enhance the informative value of the graphs I restrict the horizontal axis to a band of +/- 30 percent around the normalized majority threshold. Additionally, each graph depicts a global polynomial fit, which is estimated separately by a rectangular kernel at either side of the threshold.³⁰ The order of the polynomial fit is selected by the AIC. If the government fragmentation hypothesis is correct, that is coalition governments have a positive effect on taxes, expenditures and debt, respectively, the regression function should display a downward jump at the majority threshold.

Starting with the effect on the growth rates on government debt, there seems to be no discontinuity for the growth in total debt at the majority threshold. This suggests that the growth rate in total government debt remains more or less stable if a coalition replaces a single-party government. For both sub-categories of total debt, debt of the core budget and debt of public undertakings, the regression function jumps upwards once the majority threshold is crossed. This implies that coalition governments do not tend to accumulate more debt for the core budget and public undertakings, but rather less than single-party governments.

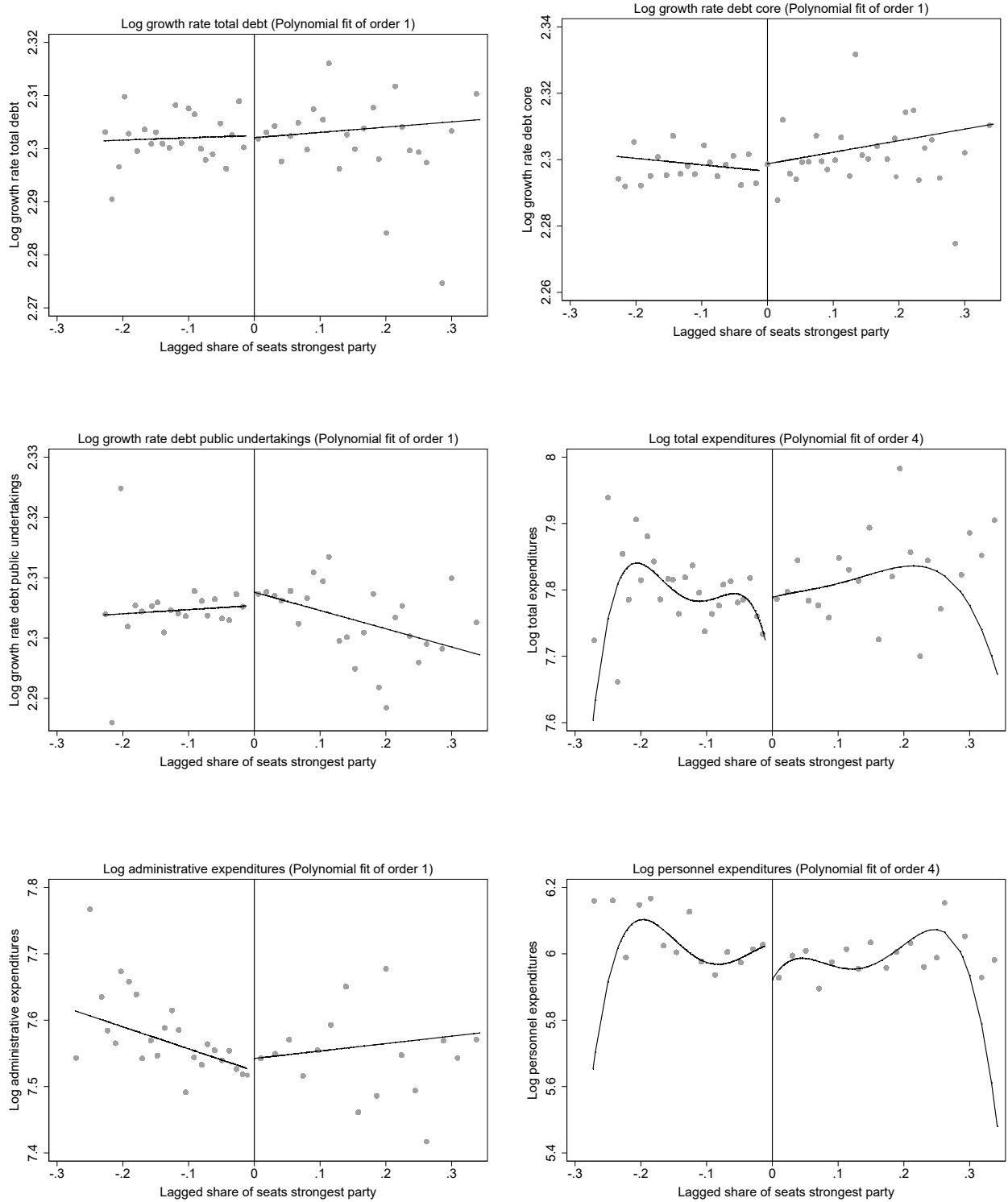
The expenditure variables present a mixed picture: The plots for total expenditures, administrative spending and material spending exhibit a positive discontinuity at the threshold. Thus it appears that more is spent if a party governs alone. This stands in contrast to the government fragmentation hypothesis. For investment expenditures there is no treatment effect visible. Personnel expenditures is the only spending variable which behaves in line with the government fragmentation hypothesis as it decreases discontinuously if the type of government changes from coalition to single-party government at the threshold.

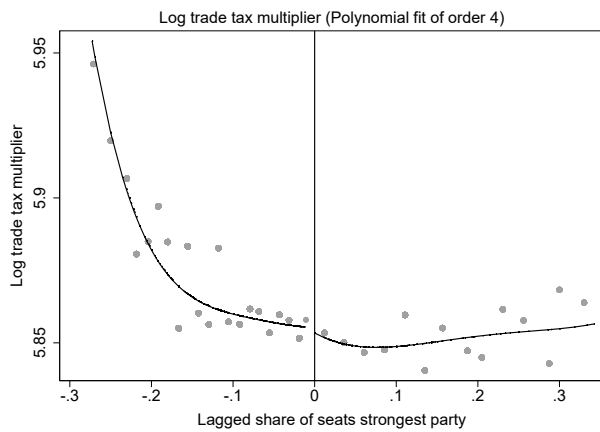
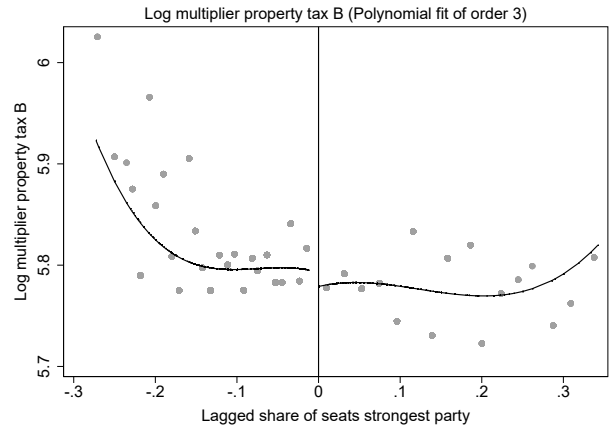
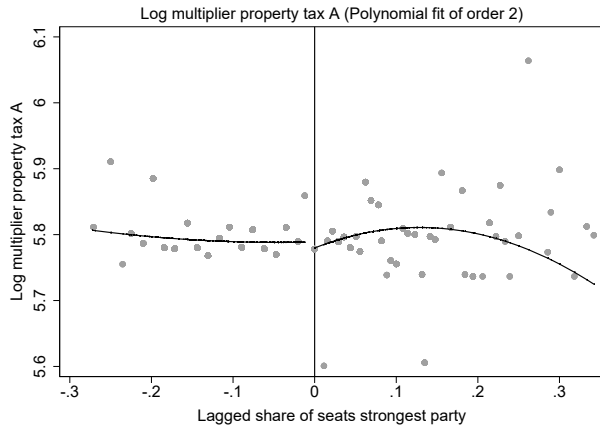
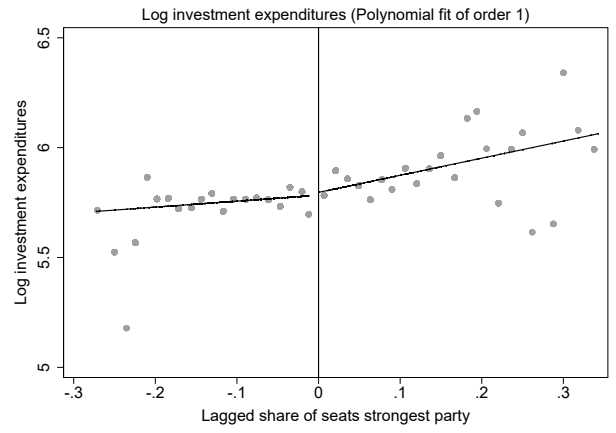
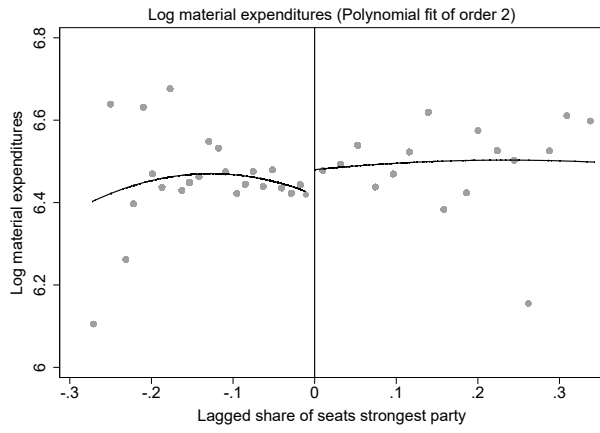
²⁸Note that the assignment variable is lagged by one year. To ease interpretation it is centered at the 50 percent threshold.

²⁹The optimal number of bins on either side of the cutoff is determined by the evenly-spaced method using spacings estimators, which is provided by Calonico, Cattaneo, and Titiunik (2014). Binning the observations eases interpretation and helps to reduce the influence of outliers.

³⁰Lee and Lemieux (2010) propose to use a rectangular kernel to approximate the population conditional expectation functions for control and treated units.

Figure 1: Graphical representation of RDD





For the multiplier of property tax A and the trade tax multiplier no discontinuity is apparent. The plot for the multiplier of property tax B reveals a downward jump at the absolute majority threshold. This suggests that governing parties tend to increase taxes on housing property if they are forced to coalesce, whereas they do not change the multipliers of the remaining local tax multipliers.

In general, the variation of the bins increases considerably if the share of seats grows beyond 60 percent. This might be a consequence of the smaller number of observations for single-party governments. For almost all outcome variables the slope of the regression function differs at either side of the threshold. To account for this it is necessary to include interaction terms in the regression equation. In general, using simple eyeball econometrics is not sufficient to assess the exact magnitude of the treatment effects. Thus, the following section discusses the estimation results of the RDD.

6.2 Estimation Results

Table 3 reports the OLS as well as RDD estimates from the global polynomial fixed effects regression with a control function estimated separately on either side of the threshold. It shows that, contrary to the theoretical prediction, government fragmentation has a negative effect on fiscal policy outcomes. Each cell reports the estimated treatment effect for the outcome variable given in the left-most column. The respective standard errors, given in parenthesis, are robust to heteroskedasticity and clustered at municipality level. All regressions include the logarithm of the council size.

Column (1) presents the OLS estimates from a simple regression of the respective outcome variable on the treatment dummy. The OLS estimates largely confirm the government fragmentation hypothesis. Both, total expenditures and its sub-categories, as well as tax multipliers, slightly increase if a coalition governments comes to power. However, the estimated effect is neither economically nor statistically significant. The only exception to this pattern are material expenditures, as well as the growth rate in total credit market debt and its sub-categories, as they reveal a small negative effect, which is not significant at the five percent level.

Column (2) to (5) show the results from the RDD estimations using a polynomial control function. The order of polynomials ranges from one in model (2) to four in model (5). The preferred estimate with the lowest AIC is marked in bold.

For the debt variables the RDD yields estimation results similar to OLS estimations, as shown in panel A. Coalition governments are found to increase debt by slightly less than single-party governments. The estimate with the best fit, selected by the AIC, implies that the growth rate of total credit market debt decreases by 0.3 percent if a party is forced to coalesce (model 2). Whereas the estimates with the best fit for both sub-categories of debt, the growth rate of credit market debt of the core budget and the growth rate in debt of public undertakings, suggest there is a negative effect of 0.4 percent (model 2). Although the estimated coefficients remain relatively stable across different polynomial specifications, none of them is statistically significant. The imprecision might be a consequence of the comparatively large variation of the debt variables as reported in the summary statistics in table 12 in the Appendix . Thus, if any, there seems to be a small negative effect of coalition governments on the growth rate of debt. This supports the findings of Freier and Odendahl (2012).

In contrast to OLS, the RDD estimation results for expenditures and tax multipliers run counter to the theoretical prediction: parties forced to build a coalition government are found to decrease spending and tax rates. This indicates that OLS yields biased results.

As shown in panel B, coalition governments affect three expenditure variables: Firstly, total

Table 3: OLS and RDD estimation results using fixed effects, full sample

	OLS	RDD			
	(1) -	(2) First Order	(3) Second Order	(4) Third Order	(5) Fourth Order
Panel A: Debt growth					
Log growth rate total debt	-0.003 (0.003)	-0.003 (0.004)	-0.004 (0.006)	-0.007 (0.008)	-0.001 (0.012)
Log growth rate debt core	-0.007 (0.004)*	-0.004 (0.005)	-0.006 (0.007)	-0.017 (0.011)	-0.012 (0.018)
Log growth rate debt public undertakings	-0.002 (0.003)	-0.004 (0.004)	-0.006 (0.005)	-0.008 (0.008)	-0.003 (0.012)
Number of clusters	544	544	544	544	544
Observation	3955	3955	3955	3955	3955
Panel B: Expenditures					
Log total expenditures	0.007 (0.010)	-0.015 (0.012)	-0.034 (0.018)*	-0.015 (0.029)	-0.058 (0.039)
Log administrative expenditures	0.006 (0.010)	-0.021 (0.013)*	-0.028 (0.018)	-0.017 (0.028)	-0.025 (0.038)
Log personnel expenditures	0.010 (0.009)	-0.009 (0.010)	0.009 (0.015)	-0.003 (0.024)	0.018 (0.036)
Log material expenditures	-0.021 (0.014)	-0.027 (0.018)	-0.063 (0.026)**	-0.038 (0.038)	-0.030 (0.053)
Log investment expenditures	0.004 (0.032)	-0.001 (0.039)	-0.044 (0.054)	-0.018 (0.078)	0.011 (0.105)
Number of clusters	604	604	604	604	604
Observation	8170	8170	8170	8170	8170
Panel C: Tax multipliers					
Log multiplier property tax A	0.006 (0.006)	-0.014 (0.008)*	-0.002 (0.012)	-0.014 (0.018)	-0.017 (0.026)
Log multiplier property tax B	0.009 (0.008)	-0.015 (0.010)	0.004 (0.015)	-0.024 (0.023)	-0.015 (0.035)
Log trade tax multiplier	0.001 (0.002)	-0.005 (0.003)	-0.004 (0.004)	-0.011 (0.006)*	-0.012 (0.009)
Number of clusters	604	604	604	604	604
Observation	8170	8170	8170	8170	8170

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each coefficients represents a separate regression using a treatment dummy for whether there was a coalition government ($D_i = 1$), which is defined as the strongest party holding less than 50 % of the seats in the council. The dependent variable is given in the left-most column. Standard errors robust to heteroskedasticity and clustered at municipality level are reported in parenthesis. The strongest party's share of seats of up to the fourth polynomial is included as control function separately for the two sides of the threshold (coefficients not reported). The order of polynomial used for the control function is given in the header. All regressions include municipality fixed effects and the logarithm of the council size. All right hand side variables are lagged by one year. The model with the lowest AIC among all RDD models for a respective outcome variable is marked in bold. Panel A contains fewer observations since data on the debt variables is available only for the 2000-2009 period.

Source: Own calculations based on data from the Baden-Württemberg State Statistical Office and the Federal Statistical Office of Germany.

expenditures decrease with government fragmentation. The estimate with the best fit (model 5) indicates that overall coalitions spend 5.8 percent less than single-party governments. Although this effect is economically relevant, the magnitude of the coefficient shrinks if instead polynomial specifications of lower order are used in the control function. Furthermore, only model (3) reaches the 10 percent significance level. This suggests that there is a non-robust negative relation between total expenditures and the presence of coalition governments. Garmann (2014) and Freier and Odendahl (2012) reach a similar conclusion.

Secondly, analyzing the sub-categories of total spending reveals that coalition governments exhibit lower administrative expenditures. The estimate with the best fit implies that coalition governments spend 2.1 percent less on public administration (model 2). This effect is significant at the 10 percent level. Although the estimated coefficient loses significance if higher order polynomials are used in the control function, the magnitude of the coefficient remains remarkably stable. This underscores the robustness of the finding. The direction of the effect is in line with Garmann (2014).

Thirdly, there is a consistent negative relation between coalition governments and material expenditures. According to the estimate with the best fit material spending falls by 6.3 percent (model 3). The effect is economically and statistically highly significant. Using alternative polynomial specifications, the coefficient is cut in half and no longer significant. However, it is still economically relevant. This confirms the results of Garmann (2014), who gets coefficients of similar magnitude, although less significant.

In line with Freier and Odendahl (2012), the remaining spending categories, personnel expenditures and investment expenditures, yield inconclusive results. The estimated coefficients change signs across different polynomial models and are far from being statistically significant. Especially the estimates for investment spending are very imprecise due to the larger standard errors. This reflects the high variability of the variable, as shown in the summary statistics in table 12 in the Appendix.

Turning to the tax multipliers, as reported in panel C, coalition governments, if any, have a small negative effect on local tax rates. The estimates suggest that municipalities, governed by more than one party, set tax multipliers for property tax A that are roughly 1 percent lower. The preferred estimate (model 3), however, yields an effect, which is close to zero. The coefficient is never significant at the 5 percent level. The picture is similar for the trade tax multiplier, as the coefficient fluctuates around -0.8 percent depending on the polynomial order applied. This small negative effect is only occasionally significant at the 10 percent level. There seems to be no relation between property tax B and the type of government, as the estimate hovers around zero and is far from being statistically significant. In sum, the estimates in table 3 contradict the government fragmentation hypothesis.

Table 4 shows that using the cross-sectional variation instead of the within variation as shown in table 3, yields remarkably similar results. For the cross-sectional RDD to be valid, I have to restrict the sample to observations with a council size greater than 18 seats, that is to say, to municipalities with at least 10.000 inhabitants.³¹ Just like in table 3, column 1 to 4 report the results from the RDD estimations for different polynomial specifications of the control function.

Especially the estimates for the expenditures variables are qualitatively the same. The point estimates, as well as standard errors, are generally inflated - possibly due to the reduced sample size. The negative impact of coalition governments on total spending is confirmed. The best fitting model from the RDD suggests that parties spend 12.4 percent less if they govern together with a coalition partner (model 4). The estimated effect, however, is sensitive to the order of the polynomial control function applied. The negative effect on administrative expenditures

³¹See also the discussion in section A.2 in the Appendix .

Table 4: RDD estimation results using cross sectional variation, restricted sample

	RDD			
	(1) First Order	(2) Second Order	(3) Third Order	(4) Fourth Order
Panel A: Debt Growth				
Log growth rate total debt	0.001 (0.002)	0.002 (0.004)	0.002 (0.006)	0.012 (0.008)
Log growth rate debt core	0.000 (0.004)	-0.002 (0.005)	-0.006 (0.008)	-0.004 (0.012)
Log growth rate debt public undertakings	-0.004 (0.003)	-0.005 (0.004)	-0.005 (0.005)	-0.001 (0.008)
Number of clusters	305	305	305	305
Observation	2079	2079	2079	2079
Panel B: Expenditures				
Log total expenditures	-0.019 (0.023)	-0.043 (0.031)	-0.033 (0.046)	-0.124 (0.064)*
Log administrative expenditures	-0.040 (0.021)*	-0.051 (0.030)*	-0.051 (0.046)	-0.117 (0.063)*
Log personnel expenditures	-0.019 (0.028)	-0.049 (0.034)	-0.038 (0.048)	-0.069 (0.067)
Log material expenditures	-0.032 (0.033)	-0.097 (0.043)**	-0.032 (0.065)	-0.063 (0.092)
Log investment expenditures	0.018 (0.052)	-0.067 (0.074)	-0.016 (0.103)	-0.091 (0.146)
Number of clusters	539	539	539	539
Observation	4619	4619	4619	4619
Panel C: Tax multipliers				
Log multiplier property tax A	-0.002 (0.019)	0.014 (0.025)	0.010 (0.037)	0.006 (0.055)
Log multiplier property tax B	-0.009 (0.016)	0.021 (0.022)	-0.018 (0.033)	-0.025 (0.047)
Log trade tax multiplier	-0.006 (0.005)	0.005 (0.007)	-0.006 (0.010)	-0.014 (0.014)
Number of clusters	539	539	539	539
Observation	4619	4619	4619	4619

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each coefficients represents a separate regression using a treatment dummy for whether there was a coalition government ($D_i = 1$), which is defined as the strongest party holding less than 50 % of the seats in the council. The dependent variable is given in the left-most column. Standard errors robust to heteroskedasticity and clustered at municipality level are reported in parenthesis. The strongest party's share of seats of up to the fourth polynomial is included as control function separately for the two sides of the threshold (coefficients not reported). The order of polynomial used for the control function is given in the header. All regressions include the logarithm of the council size. All right hand side variables are lagged by one year. The model with the lowest AIC among all RDD models for a respective outcome variable is marked in bold. The sample is restricted to municipalities with a council size of at least 19 seats. Panel A contains fewer observations since data on the debt variables is available only for the 2000-2009 period.

Source: Own calculations based on data from the Baden-Württemberg State Statistical Office and the Federal Statistical Office of Germany.

becomes more consistent for different types of control functions and gains in significance. The best point estimates implies that spending significantly decreases by 11.7 percent if a coalition governments is at the helm (model 4). This is roughly four times as much as the highest FE estimate suggests. The estimates for material expenditures are more volatile compared to the FE estimations. The negative effect remains highly significant for the second order polynomial specification. According to the best point estimate material expenditures is 3.2 percent lower for coalition governments (model 3). The estimates for the personnel and investment expenditures do not yield significant results, just like in the FE estimations. The cross-sectional approach does not confirm the small negative effects of coalition governments on tax multipliers and the growth rate of debt as the coefficients either shrink to zero or turn slightly positive. The only exception is the growth rate of credit market debt of public undertakings. The estimate with the best fit yields an insignificant negative effect of coalition governments of minus 0.4 percent (model 1).

The fact that standard errors are lower for the FE approach suggests that there is no indication for a misspecified functional form in the FE approach (Lee and Lemieux 2010). To sum up, the cross-sectional analysis also provides no evidence for the government fragmentation hypothesis. Rather there might be a disciplinary effect of coalition governments on fiscal policy, which is more pronounced for government expenditures than for tax multipliers and growth rates in debt.

6.3 Discussion

The analysis provides no evidence for the government fragmentation hypothesis. If any, coalition governments spend less, not more than single-party governments. There is some evidence that total spending, as well as administrative spending and material spending decrease if the strongest party in the council is forced to coalesce. Although these effects are economically significant, they are not statistically significant for all polynomial specifications.

How can these findings be explained? The literature offers several explanations why the government fragmentation hypothesis might not hold. Especially the principal-agent problem story of Pettersson-Lidbom (2012) and the partial cost sharing approach of Primo and Snyder (2008) seem to fit to the results. It is appropriate to take a closer look at the composition of total expenditures. The negative effect of government fragmentation is the strongest and most robust for material spending as the point estimates in table 17 indicate. Material spending accounts for 34 percent of administrative spending and for roughly 26 percent of overall total spending. As we do not observe consistent effects for the remaining spending sub-categories, material spending is likely to drive the overall results.

On the one hand, material spending contains comparatively small positions that mostly benefit public officials, like business trips, education and training of staff of administrative bodies, and office equipment, amongst others. Higher amounts of these items are likely to increase the prestige of the public administration. Therefore, public officials might exploit their informational advantage towards the council to expand spending on those items. As proposed by Pettersson-Lidbom (2012), a higher number of parties in government can alleviate this principal-agent-problem by enforcing monitoring of the public administration in accordance with the motto *two heads are better than one*. Therefore a change of majorities of the council might imply a shift of power from the public administration towards politicians in the struggle over public spending. In line with this argument we might expect a similar effect for personnel expenditures. However, this is not the case; possibly because personnel expenditures are larger in size and not as easily to change in the short run. In particular, staff reductions in the public

service are difficult to implement due to relatively strong dismissal protection.

On the other hand, material spending contains spending items where partial cost-sharing might apply. Amongst those are the maintenance of public swimming pools, leisure facilities, libraries, or theaters and museums. The beneficiaries of these spending items can be targeted at least in part via charging user fees. As a result, politicians re-internalize the fiscal consequences of spending decisions in those areas and hence have no incentive for overspending.³²

Taken together, the discussion reveals that the government fragmentation hypothesis might be too coarse as it neglects the role of the public administration and does not account for the peculiarities of individual spending categories. Depending on the type of fiscal policy other effects might superimpose the common pool problem.

The findings of the paper suggest that the effect of government fragmentation does neither depend on socio-economic factors nor on the political regime employed at the local level, at least for the German case. Garmann (2012, 2014) studies municipalities in the German state of NRW, which have to deal with inherited liabilities stemming from structural change. In contrast, municipalities in Baden-Württemberg are comparatively rich and face less pressure on the budget. This leaves more room for fiscal maneuver. Further the municipalities considered by Garmann (2014) employ a council-manager system, that could be perceived as a parliamentary regime. In contrast, municipalities in Baden-Württemberg use a mayor-council system, that gives more weight to the mayor and resembles a presidential regime. The fact that both studies draw similar conclusions suggests that neither the comparatively strong position of the mayor in Baden-Württemberg nor the differing economic circumstances have an intervening effect on the fragmentation channel.

Although the findings of the paper are in line with evidence from other quasi-experimental studies by Garmann (2014) and Freier and Odendahl (2012) on the German case, they stand in contrast to quasi-experimental studies which focus on Spanish and Finnish municipalities (Artés Caselles and Jurado (2014) and Meriläinen (2013)). This could be due to differing federal systems and political culture. In contrast to Spain, the German state is characterized by a close interweaving of all tiers of government - federal, state and local level. In such a system of multi-level governance both types of governments, coalition and single-party governments, are forced to cooperate with other tiers of governments. Furthermore, German political culture is rather consensus-oriented. As a consequence, parties are urged to take into account not only the interests of their own constituency, but also the interests of other social groups. This could have a dampening effect on the common pool problem. Further research, thus should investigate whether the type of federalism and political culture mitigate the common pool problem.

The external validity of the findings might be limited due to several reasons. First, the treatment effect is identified only locally for those observations with close majorities. Therefore the estimated effect of coalition governments on fiscal policy might not be transferable to majorities that are further away from the threshold. For instance, if a party wins with a slim majority it may face an incentive to please voters via spending increases in order to improve its starting position for the next election.

Second, to abstract from bargaining considerations between the mayor and the council I restricted the sample to those municipalities where the mayor either belongs to the strongest party in the council or runs as independent. Thus, we do not know whether coalition governments work different if the mayor belongs to an opposing party.

Third, the overall portion of left-wing governments - for both, coalition and single-party

³²Indivisibilities of spending, as proposed by Freier and Odendahl (2012), are less likely to be the driving force of the findings. Would this reasoning be correct, we would observe a strong negative effect of fragmentation on investment spending. However, this is not the case.

governments - is rather low in the sample.³³ This limits the external validity of the findings.

Finally, it is questionable whether the findings are applicable to higher tiers of government. On the one hand, debt, spending and taxes at the local level differ from higher tiers of government. In contrast to the federal level, debt for consumptive purposes is ruled out by law for municipalities. What is more, the common pool is larger at the federal level as public spending per capita is broader in size and scope. This offers more possibilities to target resources towards special interest groups - especially in the realm of transport- and infrastructure policy, as well as social and family policy. Furthermore, the degree of discretion in tax policy is much larger at the federal level. There are substantially more forms of taxes at the federal level that allow to reduce the tax burden on individual target groups at the expense of the general public. In addition, these taxes have a higher degree of fine-tuning - for instance it might be easier to target certain constituencies via exemptions from the value added tax or the design of the income tax schedule than by determining the level of the local property tax rate. Thus the common pool problem might be more severe at the federal level.

On the other hand, the political system at the local level differs from higher tiers of government. Interest groups are more heterogeneous at the federal level and lobbying pressure is higher as more is at stake. In contrast to the local level, parties representing geographically based interests come to the forefront at the federal level.³⁴ Thus at the federal level the government fragmentation effect might interfere with an effect stemming from geographical fragmentation.

Furthermore, media coverage is generally less comprehensive at the local level. Therefore the electorate is less informed. This in turn gives politicians and public officials the possibility for rent appropriation, as they might exploit their informational advantage to extract benefits for their own and their peers (Persson and Tabellini 2000).³⁵ The problem of rent extraction might be less severe for coalition governments as the mutual monitoring and self-control has a disciplinary effect on coalition partners and hence could act as countervailing force. In contrast, at higher tiers of government irrespective of the type of government rent appropriation is generally less likely to occur due to higher public scrutiny.

Lastly, contrary to the local level, politicians at higher levels of government act as professionals. In addition, the number of assistants and group staff on behalf of the parliament is much higher. This lessens the informational disadvantage of the parliament towards the public administration. As a consequence, the principal-agent problem might be less severe at higher tiers of government.

6.4 Robustness Checks

The identification strategy is based on the notion that close elections can be considered as quasi-random, as long as individuals are not able to *precisely* manipulate elections. Thus, the identifying assumption of the RDD is that individuals do exert at most *imprecise* control over the assignment variable. The assumption of no manipulation cannot be tested directly since we only observe one election per municipality per unit in time (Lee and Lemieux 2010).

In general, election rigging is hardly ever an issue in Western style democracies. Although,

³³ See also the discussion on partisan effects in section 6.4.

³⁴ For the German case, the Christian Social Union in Bavaria (CSU) and The Left could be viewed as such regional parties, which operate at the federal level.

³⁵ In a principal agent-model Persson and Tabellini (2000) show that the rent appropriation is higher if there is informational asymmetry. Adsera (2003) empirically support this claims by showing that rent appropriation declines if citizens are better informed about politicians actions.

Snyder (2005) and Grimmer et al. (2012) have shown that incumbents have a systematic advantage in U.S. House elections in recent legislative periods, Eggers et al. (2015) find no similar pattern for more than 40,000 close national and local elections in 10 Western democracies. Inter alia, the analysis provides no evidence for manipulation in favor of the incumbent party in elections of the German Bundestag for the 1953-2009 period, as well as in Bavarian mayoral elections for the 1994-2009 period.³⁶ From a theoretical perspective the type of manipulation found by Snyder (2005) and Grimmer et al. (2012) is less likely to occur in proportional election systems. In such a system there are no pre-determined seat thresholds in the vote distribution, as opposed to a majority voting system.³⁷ The number of votes required for a seat, the price of a seat in terms of votes, is determined jointly by the vote share of all parties. The lack of a pre-defined price for a seat makes it difficult for politicians to assess whether an election is going to be close (Folke 2014). Additionally, the votes cast is not translated directly into mandates, rather a quite complex seat allocation function is used to determine the number of seats for each party.³⁸ This suggests that precise manipulation of close elections is a hopeless venture in proportional election systems.

Another potential source of manipulation arises through the determination of the relevant cut-off value. In accordance with the state law, the council size and thus the relevant number of seats required to win an absolute majority is determined by the population size before election takes place. However, municipalities are allowed to choose the next lowest or next highest population size category as being relevant for the number of seats in the council.³⁹ It is at least conceivable that politicians lower the council size either by legislative means or by manipulating population statistics to prevent small parties from entering the parliament. However, it is doubtful, whether this instrument is sufficiently precise to determine the result of close elections. To sum up, there is no suggestive evidence for a *precise* manipulation of the assignment variable.

Lee and Lemieux (2010) provide a list of formal tests used in the literature to check whether the identifying assumption is fulfilled.⁴⁰

First, if there is manipulation, observations slightly below and above the threshold are likely to systematically differ with respect to observed baseline covariates. Using the sample restricted to observations with a council size of at least 19 seats, I examine in table 5 and 6 whether treatment and control group are balanced.⁴¹ Table 5 reports the means for the baseline covariates for the treatment and control group (column 1 and 2), the difference in means (column 3), and the p-value of a orthogonality-test with the null-hypothesis of equal means (column 4). Due to omitted variable bias the null hypothesis is rejected for almost all covariates. Therefore, OLS

³⁶Eggers et al. (2015) suspect that the incumbency advantage in recent close U.S. House election is a result from chance, that is to say, a consequence of the multiple testing problem.

³⁷In a majority voting system the threshold is always at 50 percent of the votes.

³⁸The seat allocation function itself is not known to distort close elections: during the election from 1994 to 2009 the D'Hondt seat allocation function without seat hurdle was applied. Although, it generally favors larger parties, especially if the distribution of votes across parties is highly unequal, this does not make it easier to win an additional seat if election are close.

³⁹All municipalities are allowed to choose the next lowest population size category as relevant for determining the number of seats in the council. Municipalities with suburb election system (*unechte Teilortswahl*) are even allowed to choose both, the next lowest and next highest population size category as being relevant for the number of seats in the council. Both options must be chosen before election takes place. For further information see www.landesrecht-bw.de.

⁴⁰Further robustness checks are presented in section A2 in the Appendix.

⁴¹Note that for the fixed effects approach treatment and control group are balanced by design since comparison is made within units.

yields biased results as treatment and control group are not balanced. If I consider only observations, that are no further away from the 50 percent threshold than 5 percent, the null hypothesis is no longer rejected, as shown in table 6. In addition, the number of observations is roughly the same in both treatment arms. Consequently, municipalities with the strongest party having a proportion of seats slightly above or below the majority threshold have similar characteristics except for the treatment status. Thus, the control group provides a valid counterfactual for the treatment group.

Table 5: Means of baseline covariates by type of government, restricted sample

	Single-party government (means)	Coalition government (means)	Difference in means	p-value from orthogonality test
Population	11424.79	18509.17	-7084.37	0.00
Council size	24.42	26.26	-1.84	0.00
Population density	257.41	488.85	-231.43	0.00
Employees	0.35	0.35	-0.00	0.39
Share of old	29.21	30.20	-0.99	0.00
Share of young	32.18	30.45	1.72	0.00
Commuters	0.24	0.24	-0.00	0.63
<i>N</i>	1180	3341		

Notes: The sample is restricted to municipalities with at least 19 seat and less than 33 seats in the municipal council. The right-most column reports the p-values from a t-test with the null hypothesis of equal means for the predetermined variables across treatment. The last row reports the number of observations for treatment and control group, respectively.

Table 6: Means of baseline covariates by type of government if the share of seats is within a band of +/- 5 % around the 50 % threshold, restricted sample

	$0.5 \leq s_{it} < 0.55$ (means)	$0.45 < s_{it} < 0.5$ (means)	Difference in means	p-value from orthogonality test
Population	13011.65	13341.14	-329.49	0.56
Council size	24.74	24.91	-0.17	0.61
Population density	305.11	296.72	8.39	0.60
Employees	0.35	0.35	0.00	0.53
Share of old	29.54	29.38	0.16	0.55
Share of young	31.65	31.51	0.14	0.56
Commuters	0.24	0.24	0.00	0.87
<i>N</i>	609	660	.	.

Notes: The sample is restricted to municipalities with at least 19 seat and less than 33 seats in the municipal council. The right-most column reports the p-values from a t-test with the null hypothesis of equal means for the predetermined variables across treatment. The last row reports the number of observations for treatment and control group, respectively.

Second, given the RDD is correctly specified, we should only observe a discontinuity in the fiscal policy variables, while all other predetermined control variables, which are likely to affect the outcome variable Y , should evolve smoothly with respect to the assignment variable. Intuitively the treatment should not affect baseline covariates that are determined prior to the realization of treatment. Thus there should not exist any discontinuity in pre-determined baseline covariates at the majority threshold. I formally test the null hypothesis of a zero ATE on baseline covariates for the FE model in table 7. I estimate equation ?? ?? by using each of the

observed baseline covariates as dependent variables. Panel A presents the results for a set of standard controls, namely the logarithm of population, population density, employees per capita and commuters per capita. Panel B reports the results for additional covariates, namely the share of young and share of old. It contains fewer observations since data is available only for the 2001-2014 period. None of the coefficients is significantly different from zero regardless of the polynomial order of the control function. Hence, the distribution of baseline covariates does not change discontinuously at the threshold for the FE approach. Using the baseline covariates as dependent variables in the cross-sectional approach yields similar results (see table 14 in the Appendix).⁴² The null hypothesis of a zero ATE on the baseline covariates is not rejected at the 5 percent significance level. The only exception to this rule are low order specifications of the population density and the share of young. The significance of these variables, however, could also be a reflection of the multiple testing problem. Therefore I conclude that the underlying assumption of local randomization is not violated.

Third, I test whether estimation results are sensitive to the inclusion of baseline covariates. If the estimated coefficients change, this would cast doubt on the assumption that there is no sorting of the assignment variable (Lee and Lemieux 2010). Furthermore, an increase in standard errors would put into question that the functional form is correctly specified.⁴³ Table 8 shows estimation results with the logarithm of the following variables as controls: population size, population density, employees, commuters, and council size.⁴⁴ Compared to the baseline specifications in table 19, the preferred estimates, selected by the AIC, and the respective standard errors do not increase. This indicates that the functional form is correctly specified. The estimates for the growth rate in debt and its sub-categories hardly change at all. The point estimates for material expenditures, as well as administrative and total spending also remain the same. The coefficients of the latter variable even gain in significance as a consequence of the reduced standard errors. For personnel and investment expenditures the results remain inconclusive. The effect on the tax multipliers becomes slightly smaller, but retains its sign. The cross-sectional regression presents a similar picture. Including baseline control variables into the cross-sectional regression, does not change considerably coefficients either, as shown in table 15 in the Appendix . In a nutshell, the fact that the estimates do not change considerably due to the inclusion of baseline covariates provides further evidence that there is no manipulation at the threshold.

Finally, I check whether the fiscal policy variables reveal a discontinuity at other values of the assignment variable, that is to say, at placebo thresholds. Intuitively, if there is a discontinuity in fiscal outcome variables, although there is no change in treatment status, this might suggest that the estimated treatment effect at the actual threshold is not causal.⁴⁵

Following Imbens and Lemieux (2007), I split the sample at the 50 percent cut-off into two sub-samples, each containing either exclusively coalition or single-party governments. I set the placebo threshold at the median of the assignment variable for each sub-samples. The median of

⁴²The number of observations in table 14 in the Appendix is lower as the sample is restricted to municipalities with more than 18 seats in the council.

⁴³If baseline covariates are correlated with the outcome variables, the inclusion of the former might improve the precision of estimates by reducing the sampling variability in the RDD estimates (Imbens and Lemieux 2007). Thus, if any, standard errors should decrease.

⁴⁴The remaining covariates, the share of young and the share of old, are not available for the whole sample period. The inclusion of these covariates would reduce the sample size and hence make the results no longer comparable with the baseline specifications in table 17. Thus they are not taken into account in table 8.

⁴⁵However, a significant effect at placebo thresholds does not necessarily imply that the estimated effect at the 50 percent threshold is not causal. At least theoretically it is possible that large absolute majorities, so called absolute super-majorities, act differently as the probability of getting re-elected is relatively high for them.

Table 7: Balance test for baseline covariates: RDD estimation results using fixed effects, full sample

	RDD			
	(1) First Order	(2) Second Order	(3) Third Order	(4) Fourth Order
Panel A: Standard controls				
Log (population)	-0.001 (0.004)	0.000 (0.005)	-0.010 (0.008)	0.005 (0.011)
Log (population density)	-0.002 (0.004)	-0.000 (0.005)	-0.010 (0.008)	0.005 (0.011)
Log (employees)	-0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	0.000 (0.001)
Log (commuters)	-0.000 (0.000)	-0.000 (0.000)	-0.001 (0.000)	0.000 (0.001)
Number of clusters	602	602	602	602
Observation	7714	7714	7714	7714
Panel B: Additional controls				
Log (share of old)	-0.019 (0.010)*	-0.004 (0.014)	-0.025 (0.020)	-0.026 (0.028)
Log (share of young)	0.005 (0.008)	0.001 (0.012)	0.012 (0.018)	0.010 (0.026)
Number of clusters	581	581	581	581
Observation	5803	5803	5803	5803

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each coefficients represents a separate regression using a treatment dummy for whether there was a coalition government ($D_i = 1$), which is defined as the strongest party holding less than 50 % of the seats in the council. In this table a set of baseline covariates is used as dependent variables. For each row the respective dependent variable is given in the left-most column. Standard errors robust to heteroskedasticity and clustered at municipality level are reported in parenthesis. The strongest party's share of seats of up to the fourth polynomial is included as control function separately for the two sides of the threshold (coefficients not reported). The order of polynomial used for the control function is given in the header. All regressions include municipality fixed effects and the logarithm of the council size. All right hand side variables are lagged by one year. Panel B contains fewer observations since data on the share of young and share of old is available only for the 2001 - 2014 period.

Source: Own calculations based on data from the Baden-Württemberg State Statistical Office and the Federal Statistical Office of Germany.

Table 8: Robustness checks: RDD estimation results controlling for baseline covariates, using fixed effects, full sample

	RDD			
	(1) First Order	(2) Second Order	(3) Third Order	(4) Fourth Order
Panel A: Debt Growth				
Log growth rate total debt	-0.003 (0.004)	-0.005 (0.006)	-0.007 (0.008)	-0.002 (0.011)
Log growth rate debt core	-0.004 (0.004)	-0.006 (0.007)	-0.017 (0.011)	-0.013 (0.018)
Log growth rate debt public undertakings	-0.004 (0.004)	-0.006 (0.005)	-0.008 (0.008)	-0.004 (0.012)
Number of clusters	544	544	544	544
Observation	3955	3955	3955	3955
Panel B: Expenditures				
Log total expenditures	-0.012 (0.012)	-0.034 (0.017)*	-0.010 (0.027)	-0.061 (0.037)*
Log administrative expenditures	-0.015 (0.011)	-0.028 (0.017)*	-0.003 (0.025)	-0.031 (0.034)
Log personnel expenditures	-0.008 (0.009)	0.007 (0.014)	-0.001 (0.024)	0.011 (0.036)
Log material expenditures	-0.026 (0.018)	-0.063 (0.026)**	-0.036 (0.038)	-0.031 (0.053)
Log investment expenditures	-0.018 (0.037)	-0.052 (0.053)	-0.063 (0.077)	0.013 (0.103)
Number of clusters	604	604	604	604
Observation	8169	8169	8169	8169
Panel C: Tax multipliers				
Log multiplier property tax A	-0.007 (0.006)	-0.001 (0.009)	0.001 (0.015)	-0.020 (0.020)
Log multiplier property tax B	-0.005 (0.007)	0.006 (0.010)	-0.002 (0.017)	-0.021 (0.024)
Log trade tax multiplier	-0.002 (0.002)	-0.003 (0.003)	-0.005 (0.005)	-0.014 (0.006)**
Number of clusters	604	604	604	604
Observation	8169	8169	8169	8169

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each coefficients represents a separate regression using a treatment dummy for whether there was a coalition government ($D_i = 1$), which is defined as the strongest party holding less than 50 % of the seats in the council. The dependent variable is given in the left-most column. Standard errors robust to heteroskedasticity and clustered at municipality level are reported in parenthesis. The strongest party's share of seats of up to the fourth polynomial is included as control function separately for the two sides of the threshold (coefficients not reported). The order of polynomial used for the control function is given in the header. All regressions include municipality fixed effects, and the logarithm of the following variables as controls: population size, population density, employees, commuters, council size. All right hand side variables are lagged by one year. The model with the lowest AIC among all RDD models for a respective outcome variable is marked in bold.

Source: Own calculations based on data from the Baden-Württemberg State Statistical Office and the Federal Statistical Office of Germany. Panel A contains fewer observations since data on the debt variables is available only for the 2000-2009 period.

Table 9: Robustness check: placebo-cutoff for coalition governments, using fixed effects

	RDD			
	(1) First Order	(2) Second Order	(3) Third Order	(4) Fourth Order
Panel A: Debt Growth				
Log growth rate total debt	0.006 (0.006)	0.006 (0.006)	0.007 (0.007)	0.008 (0.007)
Log growth rate debt core	0.005 (0.007)	0.004 (0.007)	0.002 (0.009)	0.002 (0.009)
Log growth rate debt public undertakings	0.005 (0.004)	0.005 (0.004)	0.005 (0.004)	0.005 (0.005)
Number of clusters	387	387	387	387
Observation	2584	2584	2584	2584
Panel B: Expenditures				
Log total expenditures	-0.014 (0.015)	-0.008 (0.017)	-0.002 (0.021)	-0.016 (0.024)
Log administrative expenditures	-0.009 (0.015)	-0.007 (0.017)	-0.003 (0.020)	-0.010 (0.023)
Log personnel expenditures	-0.003 (0.012)	-0.009 (0.013)	-0.017 (0.014)	-0.012 (0.016)
Log material expenditures	-0.030 (0.022)	-0.026 (0.023)	-0.012 (0.026)	-0.007 (0.030)
Log investment expenditures	-0.051 (0.045)	-0.042 (0.047)	-0.036 (0.056)	-0.015 (0.066)
Number of clusters	484	484	484	484
Observation	5545	5545	5545	5545
Panel C: Tax multipliers				
Log multiplier property tax A	0.007 (0.009)	0.005 (0.010)	-0.003 (0.011)	-0.008 (0.013)
Log multiplier property tax B	0.010 (0.012)	0.005 (0.013)	-0.010 (0.015)	-0.015 (0.017)
Log trade tax multiplier	-0.002 (0.004)	-0.002 (0.004)	-0.006 (0.004)	-0.008 (0.005)
Number of clusters	484	484	484	484
Observation	5545	5545	5545	5545

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent Variable is given in the left-most column. Each coefficients indicates the estimated treatment effect. Treatment is given if the strongest party holds less than 40 % of the seats in the council. Standard errors robust to heteroskedasticity and clustered at municipality level are reported in parenthesis. The strongest party's share of seats of up to the fourth polynomial is included as control function separately for the two sides of the threshold (coefficients not reported). The order of polynomial used for the control function is given in the header. All regressions include municipality fixed effects and the logarithm of the council size. All right hand side variables are lagged by one year. The sample is restricted to municipalities with a coalition government. Panel A contains fewer observations since data on the debt variables is available only for the 2000-2009 period.

Source: Own calculations based on data from the Baden-Württemberg State Statistical Office and the Federal Statistical Office of Germany.

Table 10: Robustness check: placebo-cutoff for single-party governments, using fixed effects

	RDD			
	(1) First Order	(2) Second Order	(3) Third Order	(4) Fourth Order
Panel A: Debt Growth				
Log growth rate total debt	0.002 (0.004)	0.001 (0.004)	-0.001 (0.005)	-0.000 (0.005)
Log growth rate debt core	0.000 (0.010)	-0.007 (0.014)	-0.014 (0.019)	-0.013 (0.019)
Log growth rate debt public undertakings	0.002 (0.005)	0.006 (0.006)	0.006 (0.007)	0.007 (0.007)
Number of clusters	239	239	239	239
Observation	1371	1371	1371	1371
Panel B: Expenditures				
Log total expenditures	-0.025 (0.017)	-0.031 (0.026)	-0.063 (0.031)**	-0.041 (0.033)
Log administrative expenditures	-0.016 (0.016)	-0.028 (0.022)	-0.036 (0.027)	-0.031 (0.028)
Log personnel expenditures	0.005 (0.016)	-0.040 (0.022)*	-0.058 (0.031)*	-0.057 (0.033)*
Log material expenditures	-0.016 (0.022)	-0.023 (0.031)	-0.032 (0.041)	-0.019 (0.042)
Log investment expenditures	-0.002 (0.067)	-0.047 (0.096)	-0.128 (0.110)	-0.065 (0.110)
Number of clusters	287	287	287	287
Observation	2625	2625	2625	2625
Panel C: Tax multipliers				
Log multiplier property tax A	-0.007 (0.009)	-0.006 (0.011)	-0.006 (0.012)	-0.006 (0.012)
Log multiplier property tax B	-0.009 (0.012)	-0.007 (0.016)	-0.003 (0.016)	0.001 (0.017)
Log trade tax multiplier	-0.000 (0.004)	-0.001 (0.006)	0.001 (0.007)	0.005 (0.007)
Number of clusters	287	287	287	287
Observation	2625	2625	2625	2625

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent Variable is given in the left-most column. Each coefficients indicates the estimated treatment effect. Treatment is given if the strongest party holds less than 53.3 % of the seats in the council. Standard errors robust to heteroskedasticity and clustered at municipality level are reported in parenthesis. The strongest party's share of seats of up to the fourth polynomial is included as control function separately for the two sides of the threshold (coefficients not reported). The order of polynomial used for the control function is given in the header. All regressions include municipality fixed effects and the logarithm of the council size. All right hand side variables are lagged by one year. The sample is restricted to municipalities with a single-party government. Panel A contains fewer observations since data on the debt variables is available only for the 2000-2009 period.

Source: Own calculations based on data from the Baden-Württemberg State Statistical Office and the Federal Statistical Office of Germany.

the proportion of seats of the strongest party equals 40 percent and 53.3 percent, respectively, for the right and left placebo-cutoff. I use the fixed effects approach to test whether there is a jump in the outcome variables at the placebo-cutoff for each sub-sample, as given in table 9 and table 10.⁴⁶ For the coalition sub-sample to the left of the majority threshold (table 9) the null hypothesis of no discontinuity at the placebo-cutoff is never rejected at the five percent level for all fiscal outcome variables. For the single-party sub-sample to the right of the majority threshold (table 10) the null hypothesis of no discontinuity at the placebo-cutoff is never rejected at the five percent level for almost all fiscal outcome variables. The coefficient for total expenditures is significant at the five percent level if a third order polynomial is used in the control function. However, for the remaining polynomial specifications the coefficient for total expenditures is far from being significant. In short, the lack of significant results from the placebo-cutoff implies that the estimation results from the baseline FE regression are valid.

7 Conclusion

This paper investigates the causal effect of the type of government on fiscal policy using panel data on municipalities for the German state of Baden-Württemberg. The political economy literature suggests that due to an underlying common pool problem coalition governments spend more, exhibit higher taxes, and run higher deficits than single-party governments. Yet, the literature review shows that there is only mixed evidence for this claim. This is likely to be a consequence of the problems to identification that most of the empirical studies face. In particular cross-country studies do not account for institutional and cultural heterogeneity. What is more, for causal inference the majority of the studies relies on standard regression methods, like OLS. Therefore, these studies likely face problems of omitted variable bias and reverse causality.

The paper addresses these problems as follows: First, using a data set on local governments, that are subjected to the same institutional and electoral rules defined by the state law, ensures that observations are relatively homogeneous. Second, for identification the paper relies on a RDD that exploits truly exogenous variation in the type of government generated by close elections. This overcomes the reverse causality problem and allows to draw clear causal conclusions. Third, a sensitivity analysis underscores that the findings are robust.

The main conclusion of the paper is that government fragmentation does not lead to sub-optimal fiscal policy outcomes. The analysis provides no evidence for the theoretical prediction that coalition government are subjected to a common pool problem. Neither, the growth in government debt, nor expenditures, nor tax rates increase when a governing party is forced to coalesce. Rather, there is weak evidence that coalition governments exhibit lower total spending, administrative expenditures and material expenditures.

Two theoretical consideration could explain the findings. First, coalition governments might be better able to solve a principal-agent problem between the public administration and the municipal council, as they have a higher monitoring capacity than single-party governments. Second, partial cost sharing might solve the common pool problem for some spending items, as decision-makers re-internalize the political costs of special interest projects if their constituency has to bear a more than proportionate share of the fiscal costs.

⁴⁶For estimation I use observations on one side of the majority only. This ensures that I do not estimate a regression function where I expected a discontinuity. Using the full sample instead of the sample restricted to either side of the threshold, would imply the assumption that the assignment variable is continuous at the majority threshold.

The results of this study are in line with the findings of recent quasi-experimental studies on German municipalities. These studies focus on council-manager system, whereas this study investigates a mayor-council system. This suggests that the political regime employed at the local level has no intervening effect, at least for the German case. However, the analysis contradicts the findings of quasi-experimental studies focusing on other countries with a different federal system and more conflict-oriented style of politics. Therefore, further empirical research should investigate whether the effect of government fragmentation depends on the type of federalism and political culture.

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Appendix

A1: Additional Tables and Graphs

Table 11: Definition and source of variables

Label	Period	Description	Source
Growth rate total debt	2000 - 2009	Growth rate of real credit market debt per capita, defined as sum of core debt and debt of public undertakings per capita (deflated by federal CPI, 2010 = 100). Public undertakings having separate legal personality are not included. Due to a structural break in the debt statistic years after 2009 are excluded from the data set (inter alia cash advances are excluded before the year 2010).	Baden-Wuerttemberg State Statistical Office
Growth rate debt core	2000 - 2009	Growth rate of real credit market debt per capita of the core budget (deflated by federal CPI, 2010 = 100). Due to a structural break in the debt statistic years after 2009 are excluded from the data set (inter alia cash advances are excluded before the year 2010).	Baden-Wuerttemberg State Statistical Office
Growth rate debt public undertakings	2000 - 2009	Growth rate of real credit market debt of public undertakings per capita (deflated by federal CPI, 2010 = 100). Public undertakings having separate legal personality are not included. Due to a structural break in the debt statistic years after 2009 are excluded from the data set (inter alia cash advances are excluded before the year 2010).	Baden-Wuerttemberg State Statistical Office

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Label	Period	Description	Source
Total expenditures	1994 - 2014	Real total expenditures (<i>Bruttoausgaben der Gemeinden</i>) per capita (deflated by federal CPI, 2010 = 100).	Baden-Wuerttemberg State Statistical Office
Administrative expenditures	1994 - 2014	Real total spending on public administration (<i>Verwaltungshaushalt insgesamt</i>) per capita (deflated by federal CPI, 2010 = 100).	Baden-Wuerttemberg State Statistical Office
Personnel expenditures	1994 - 2014	Real personnel expenditures (<i>Personalausgaben</i>) per capita (deflated by federal CPI, 2010 = 100).	Baden-Wuerttemberg State Statistical Office
Material expenditures	1994 - 2014	Real material expenditures (<i>laufender Sachaufwand</i>) per capita (deflated by federal CPI, 2010 = 100).	Baden-Wuerttemberg State Statistical Office
Investment expenditures	1994 - 2014	Real investment expenditures (<i>Ausgaben fuer Sachinvestitionen</i>) per capita (deflated by federal CPI, 2010 = 100).	Baden-Wuerttemberg State Statistical Office
Multiplier property tax A	1994 - 2014	Property tax multiplier (<i>Hebesatz Grundsteuer A</i>) which determines the effective property tax rate on real property used for agriculture and forestry in a municipality.	German Federal Statistical Office
Multiplier property tax B	1994 - 2014	Property tax multiplier (<i>Hebesatz Grundsteuer B</i>) which determines the effective property tax rate on constructible real property or real property with buildings in a municipality.	German Federal Statistical Office
Trade tax multiplier	1994 - 2014	Business tax multiplier (<i>Gewerbesteuerhebesatz</i>) which determines the effective business tax rate in a municipality.	German Federal Statistical Office

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Label	Period	Description	Source
Running Variable			
Share of seats of the strongest party	1994 - 2013	Seatshare of the strongest party centered at 50 percent (includes only observations with the mayor either belonging to the strongest party or having no party affiliation).	Own construction based on data from the Baden-Wuerttemberg State Statistical Office
Treatment dummy	1994 - 2013	Treatment dummy variable that equals 1 if strongest party holds less than 50 percent of the seats in the council (only observations with the mayor either belonging to the strongest party or having no party affiliation are included).	Own construction based on data from the Baden-Wuerttemberg State Statistical Office
Covariates			
Population	1994 - 2013	Population as of the 30th of June each year.	Baden-Wuerttemberg State Statistical Office
Share of old	2001 - 2013	Share of old constructed as the ratio of retirees (65 and older) to workers (18 to 64).	Own construction based on data from the German Federal Statistical Office
Share of young	2001 - 2013	Share of young constructed as the ratio of people under the age of 18 to workers (18 to 64).	Own construction based on data from the German Federal Statistical Office
Employees	1994 - 2013	Number of employees per capita at municipal level measured at place of residence.	German Federal Employment Agency
Commuters	1994 - 2013	Commuters outflows (<i>Auspendler</i>) per capita.	German Federal Employment Agency

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Label	Period	Description	Source
Population density	1994 - 2013	Population density.	Own construction based on data from the German Federal Statistical Office and the Baden-Wuerttemberg State Statistical Office
Council size	1994 - 2013	Number of seats in the council.	Own construction based on data from the Baden-Wuerttemberg State Statistical Office
Other variables			
Baden	1994 - 2013	Regional dummy variable for historical territories of Baden	Own construction based on the data from the German Statistical Office
Number of parties	1994 - 2013	Number of parties in the council	Own construction based on data from the Baden-Wuerttemberg State Statistical Office
Party affiliation of mayor	1994 - 2013	Categorical variable that indicates the party affiliation of the mayor.	Centre for European Economic Research (ZEW) and own collection for the years following 2010 based on www.staatsanzeiger.de and www.wikipedia.de (retrieved December 20, 2016)
Suburb election	1999 - 2009	Dummy variable that equals 1 if a compensation by balancing mandates takes places at suburb level (<i>unechte Teilortswahl</i>).	Baden-Wuerttemberg State Statistical Office

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Label	Period	Description	Source
Strongest party	1994 - 2013	Categorical variable that indicates the party which holds the most seats in the council.	Own construction based on data from the Baden-Wuerttemberg State Statistical Office

Table 12: Summary statistics

Variable		Mean	Std.dev.	Min.	Max.	N
Outcome variables						
Growth rate total debt	overall	0,003	0,373	-1	6,617	4135
	between		0,146	-0,435	1,23	565
	within		0,344	-2,1	5,901	7,319
Growth rate debt core	overall	-0,026	0,454	-1	7,976	4130
	between		0,173	-0,619	1,096	564
	within		0,42	-1,946	7,019	7,323
Growth rate debt public undertakings	overall	0,03	0,354	-1	7,742	4119
	between		0,132	-0,295	0,998	565
	within		0,33	-1,355	6,939	7,29
Total expenditures per cap.	overall	2483,135	646,73	1191,744	12900,37	8809
	between		490,69	1494,281	6192,91	607
	within		423,394	-658,148	11281,41	14,512
Administrative expenditures	overall	1937,581	462,09	873,037	8385,539	8809
	between		380,997	1204,082	4920,372	607
	within		267,438	-775,484	7139,429	14,512
Personnel expenditures	overall	400,884	98,304	47,489	1366,225	8809
	between		92,737	66,767	835,476	607
	within		37,162	224,631	1054,143	14,512
Material expenditures	overall	661,307	200,954	132,297	2815,527	8809
	between		180,54	273,51	1896,15	607
	within		103,408	-9,924	1716,856	14,512
Investment expenditures	overall	378,745	235,294	0	3114,45	8809
	between		133,584	91,735	1029,828	607
	within		197,972	-401,019	2910,032	14,512
Multiplier property tax A	overall	321,951	91,465	195	1800	8809
	between		92,163	200	1525	607
	within		24,659	-183,932	616,068	14,512
Multiplier property tax B	overall	319,022	47,61	190	800	8809
	between		40,456	200	505,714	607
	within		27,239	89,261	619,261	14,512
Trade tax multiplier	overall	340,121	16,709	280	410	8809
	between		14,861	290	400	607
	within		9,03	281,699	398,692	14,512
Treatment and Running variable						
Treatment dummy	overall	0,679	0,467	0	1	8352
	between		0,403	0	1	607

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Variable		Mean	Std.dev.	Min.	Max.	N
	within		0,257	-0,23	1,617	13,759
Seatshare strongest party	overall	-0,053	0,094	-0,273	0,342	8352
	between		0,089	-0,25	0,333	607
	within		0,04	-0,231	0,114	13,759
Control variables						
Population	overall	11410,21	12544,3	978	112618	8809
	between		12883,46	991	110956,8	607
	within		384,407	7511,262	15282,46	14,512
Council size	overall	20,798	7,539	8	61	8352
	between		7,511	8	52	607
	within		1,554	8,798	34,965	13,759
Population density	overall	410,058	357,262	36,305	2632,456	8809
	between		348,829	37,454	2493,634	607
	within		17,093	156,815	623,632	14,512
Employees	overall	0,352	0,034	0,179	0,457	8351
	between		0,031	0,197	0,424	607
	within		0,013	0,276	0,428	13,758
Share of old	overall	29,769	5,019	11,585	52,022	6400
	between		4,557	11,845	47,732	584
	within		2,467	16,88	43,156	10,959
Share of young	overall	30,776	3,51	16,829	48,647	6400
	between		2,971	19,332	40,734	584
	within		2,204	23,125	46,026	10,959
Commuters	overall	0,261	0,063	0,07	0,409	8352
	between		0,062	0,072	0,393	607
	within		0,016	0,184	0,334	13,759
Other variables						
Suburb election	overall	0,52	0,5	0	1	6744
	between		0,482	0	1	589
	within		0,146	-0,389	1,354	11,45
Baden	overall	0,517	0,5	0	1	8809
	between		0,5	0	1	607
	within		0	0,517	0,517	14,512
Number of parties in the council	overall	3,707	0,79	3	7	8352
	between		0,756	3	6	607
	within		0,288	2,04	5,373	13,759

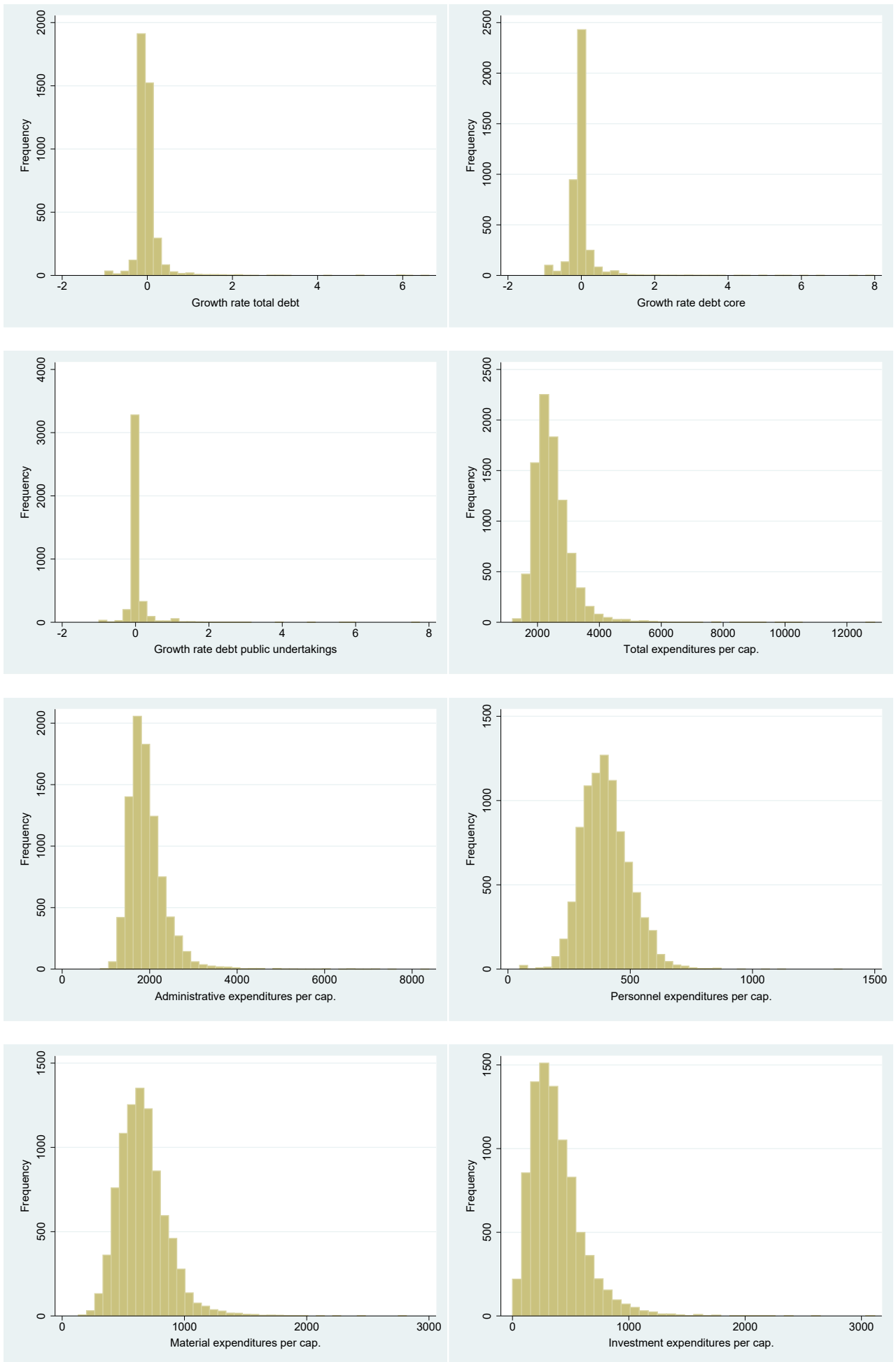
Table 13: Matrix of the four possible treatment types

		Proportion of seats $\geq 50\%$, $Z_i = 0$	
		Single-Party Gov. $D_i = 0$	Coalition Gov. $D_i = 1$
Proportion of seats $< 50\%$, $Z_i = 1$	Single-Party Gov. $D_i = 0$	Never-Takers (Minority Government)	Defiers
	Coalition Gov. $D_i = 1$	Compliers (Minimal Winning Coalition)	Always-takers (Oversized Coalition)

Notes: D_i is a treatment dummy equal to one if the type of government is a coalition. Z_i is dummy equal to one if individuals are assigned to the treatment group, that is to say that the largest party's proportion of the seats is smaller than 50 %.

Source: author's own compilation, design based on Angrist and Pischke (2015).

Figure 2: Histograms of dependent variables



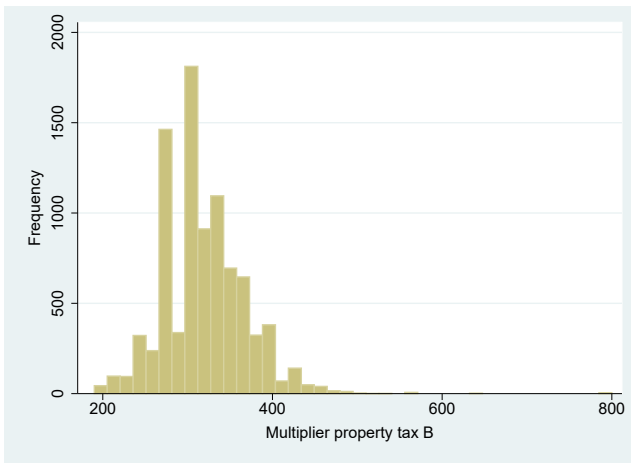
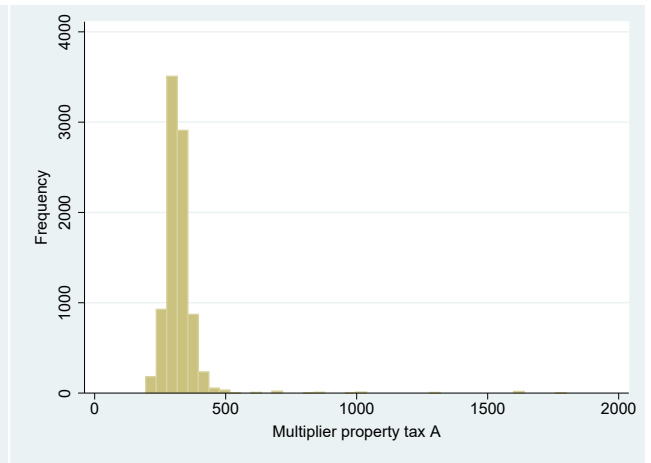
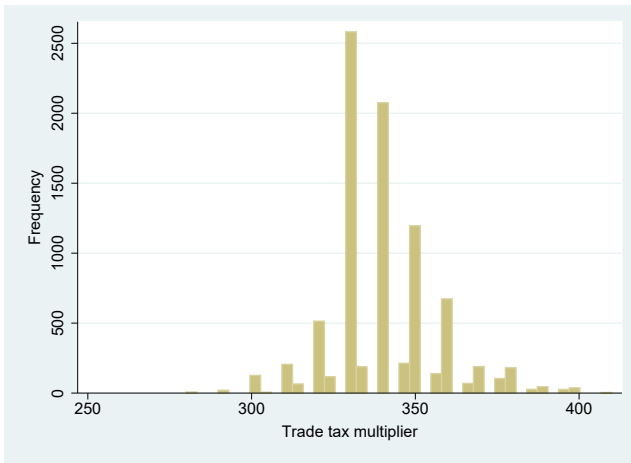


Figure 3: Histograms of baseline covariates

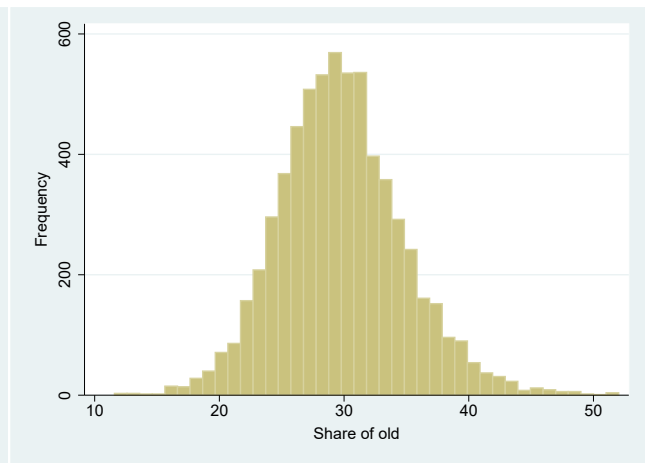
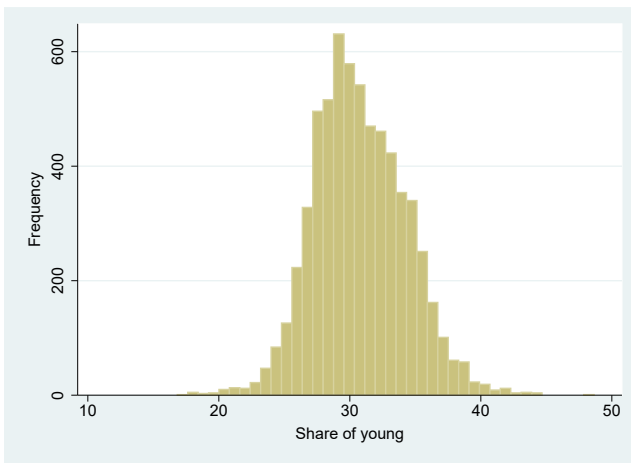
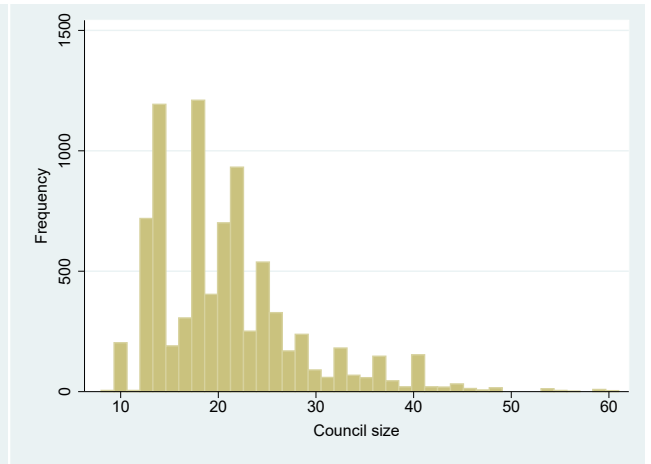
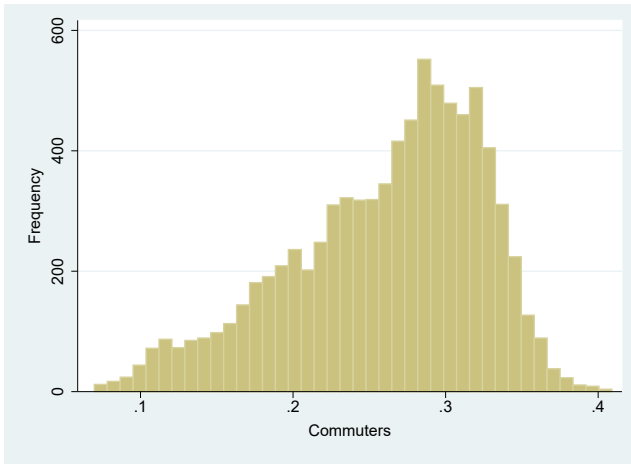
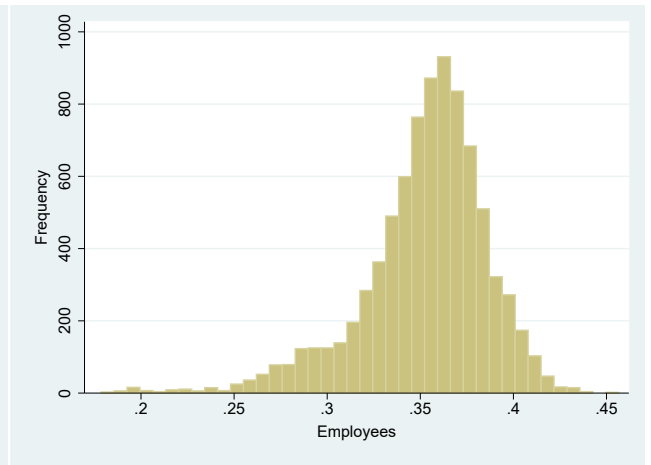
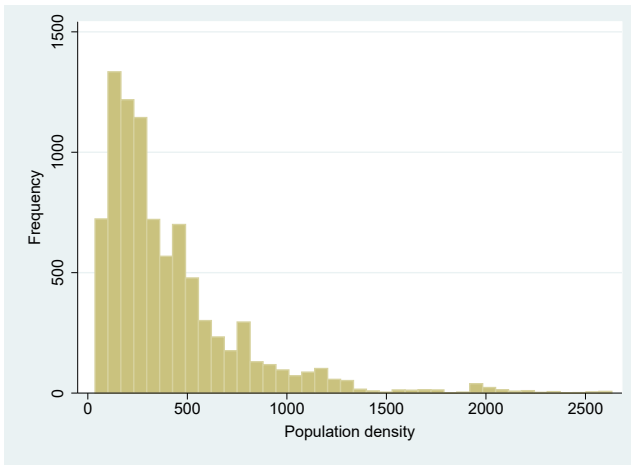
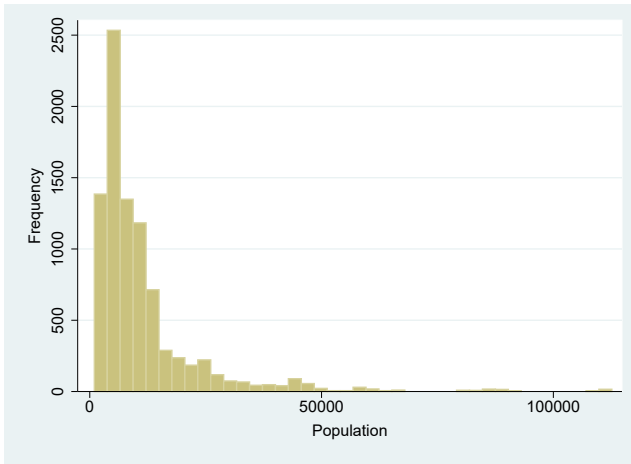


Table 14: Balance test for baseline covariates: RDD estimations results using cross sectional variation, restricted sample

	RDD			
	(1) First Order	(2) Second Order	(3) Third Order	(4) Fourth Order
Panel A: Standard controls				
Log (council size)	-0.019 (0.020)	0.004 (0.027)	0.014 (0.039)	0.018 (0.054)
Log (employees)	0.000 (0.000)	-0.001 (0.001)	-0.000 (0.001)	-0.001 (0.001)
Log (commuters)	0.000 (0.001)	-0.001 (0.001)	-0.001 (0.002)	-0.001 (0.002)
Log (population)	-0.097 (0.078)	0.038 (0.102)	0.127 (0.149)	0.134 (0.207)
Log (population density)	-0.203 (0.092)**	-0.276 (0.116)**	-0.221 (0.168)	-0.172 (0.230)
Number of clusters	539	539	539	539
Observation	4619	4619	4619	4619
Panel B: Additional controls				
Log (share of old)	-0.020 (0.016)	-0.035 (0.020)*	0.018 (0.028)	0.010 (0.042)
Log (share of young)	0.020 (0.010)**	0.028 (0.014)**	0.015 (0.021)	0.014 (0.030)
Number of clusters	524	524	524	524
Observation	3198	3198	3198	3198

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each coefficients represents a separate regression using a treatment dummy for whether there was a coalition government ($D_i = 1$), which is defined as the strongest party holding less than 50 % of the seats in the council. In this table a set of baseline covariates is used as dependent variables. For each row the respective dependent variable is given in the left-most column. Standard errors robust to heteroskedasticity and clustered at municipality level are reported in parenthesis. The strongest party's share of seats of up to the fourth polynomial is included as control function separately for the two sides of the threshold (coefficients not reported). The order of polynomial used for the control function is given in the header. All regressions include municipality fixed effects and the logarithm of the council size. All right hand side variables are lagged by one year. The sample is restricted to municipalities with more than 18 seats in the council. Panel B contains fewer observations since data on the share of young and share of old is available only for the 2001 - 2014 period.

Source: Own calculations based on data from the Baden-Württemberg State Statistical Office and the Federal Statistical Office of Germany.

Table 15: Robustness check: RDD estimation results controlling for baseline covariates using cross sectional variation, restricted sample

	RDD			
	(1) First Order	(2) Second Order	(3) Third Order	(4) Fourth Order
Panel A: Debt Growth				
Log growth rate total debt	0.001 (0.002)	0.002 (0.004)	0.002 (0.006)	0.011 (0.008)
Log growth rate debt core	-0.001 (0.004)	-0.003 (0.005)	-0.006 (0.008)	-0.003 (0.012)
Log growth rate debt public undertakings	-0.004 (0.003)	-0.005 (0.004)	-0.005 (0.005)	-0.000 (0.007)
Number of clusters	305	305	305	305
Observation	2079	2079	2079	2079
Panel B: Expenditures				
Log total expenditures	-0.019 (0.021)	-0.035 (0.028)	-0.021 (0.041)	-0.088 (0.057)
Log administrative expenditures	-0.036 (0.020)*	-0.039 (0.027)	-0.036 (0.041)	-0.075 (0.056)
Log personnel expenditures	-0.015 (0.026)	-0.044 (0.033)	-0.028 (0.046)	-0.027 (0.064)
Log material expenditures	-0.036 (0.033)	-0.097 (0.043)**	-0.030 (0.065)	-0.046 (0.091)
Log investment expenditures	-0.005 (0.048)	-0.079 (0.069)	-0.025 (0.098)	-0.066 (0.140)
Number of clusters	539	539	539	539
Observation	4618	4618	4618	4618
Panel C: Tax multipliers				
Log multiplier property tax A	-0.021 (0.019)	-0.014 (0.025)	-0.013 (0.035)	-0.010 (0.052)
Log multiplier property tax B	-0.014 (0.016)	0.008 (0.022)	-0.028 (0.032)	-0.029 (0.045)
Log trade tax multiplier	-0.006 (0.005)	0.005 (0.007)	-0.006 (0.010)	-0.011 (0.014)
Number of clusters	539	539	539	539
Observation	4618	4618	4618	4618

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each coefficients represents a separate regression using a treatment dummy for whether there was a coalition government ($D_i = 1$), which is defined as the strongest party holding less than 50 % of the seats in the council. The dependent variable is given in the left-most column. Standard errors robust to heteroskedasticity and clustered at municipality level are reported in parenthesis. The strongest party's share of seats of up to the fourth polynomial is included as control function separately for the two sides of the threshold (coefficients not reported). The order of polynomial used for the control function is given in the header. All regressions include the logarithm of the following variables as controls: population size, population density, employees, commuters, council size. All right hand side variables are lagged by one year. The model with the lowest AIC among all RDD models for a respective outcome variable is marked in bold. The sample is restricted to municipalities with a council size of at least 19 seats. Panel A contains fewer observations since data on the debt variables is available only for the 2000-2009 period.

Source: Own calculations based on data from the Baden-Württemberg State Statistical Office and the Federal Statistical Office of Germany.

Table 16: Subsample: RDD estimation results without outliers, using fixed effects

	RDD			
	(1) First Order	(2) Second Order	(3) Third Order	(4) Fourth Order
Panel A: Debt growth				
Log growth rate total debt	-0.002 (0.002)	-0.001 (0.003)	-0.006 (0.004)	-0.010 (0.006)
Log growth rate debt core	-0.002 (0.003)	-0.004 (0.004)	-0.010 (0.007)	-0.018 (0.010)*
Log growth rate debt public undertakings	-0.003 (0.002)	-0.003 (0.003)	-0.007 (0.005)	-0.004 (0.007)
Number of clusters	544	544	544	544
Observation	3875	3875	3875	3875
Panel B: Expenditures				
Log total expenditures	-0.014 (0.011)	-0.031 (0.017)*	-0.009 (0.026)	-0.035 (0.036)
Log administrative expenditures	-0.019 (0.012)	-0.026 (0.017)	-0.017 (0.025)	-0.019 (0.036)
Log personnel expenditures	-0.012 (0.010)	0.007 (0.015)	-0.001 (0.024)	0.023 (0.034)
Log material expenditures	-0.021 (0.017)	-0.059 (0.024)**	-0.021 (0.036)	-0.034 (0.050)
Log investment expenditures	-0.002 (0.036)	-0.056 (0.050)	-0.037 (0.073)	-0.018 (0.101)
Number of clusters	604	604	604	604
Observation	8001	8001	8001	8001
Panel C: Tax multipliers				
Log multiplier property tax A	-0.013 (0.008)*	-0.000 (0.011)	-0.013 (0.017)	-0.018 (0.024)
Log multiplier property tax B	-0.014 (0.010)	0.005 (0.014)	-0.021 (0.021)	-0.001 (0.031)
Log trade tax multiplier	-0.003 (0.003)	-0.002 (0.004)	-0.007 (0.006)	-0.013 (0.008)
Number of clusters	597	597	597	597
Observation	7941	7941	7941	7941

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each coefficients represents a separate regression using a treatment dummy for whether there was a coalition government ($D_i = 1$), which is defined as the strongest party holding less than 50 % of the seats in the council. The dependent variable is given in the left-most column. Standard errors robust to heteroskedasticity and clustered at municipality level are reported in parenthesis. The strongest party's share of seats of up to the fourth polynomial is included as control function separately for the two sides of the threshold (coefficients not reported). The order of polynomial used for the control function is given in the header. All regressions include municipality fixed effects and the logarithm of the council size. All right hand side variables are lagged by one year. Outliers were eliminated for all specifications by restricting the sample to the 1 - 99 percentiles of the outcome variable. The model with the lowest AIC among all RDD models for a respective outcome variable is marked in bold. Panel A contains fewer observations since data on the debt variables is available only for the 2000-2009 period.

Source: Own calculations based on data from the Baden-Württemberg State Statistical Office and the Federal Statistical Office of Germany.

Table 17: Robustness check: RDD estimation results with identical control function on either side of threshold, using fixed effects, full sample

	OLS	RDD			
	(1) -	(2) First Order	(3) Second Order	(4) Third Order	(5) Fourth Order
Panel A: Debt Growth					
Log growth rate total debt	-0.003 (0.003)	-0.002 (0.004)	-0.002 (0.004)	-0.002 (0.004)	-0.002 (0.005)
Log growth rate debt core	-0.007 (0.004)*	-0.002 (0.004)	-0.002 (0.004)	-0.000 (0.004)	-0.001 (0.005)
Log growth rate debt public undertakings	-0.002 (0.003)	-0.005 (0.004)	-0.005 (0.004)	-0.006 (0.004)	-0.005 (0.004)
Number of clusters	544	544	544	544	544
Observation	3955	3955	3955	3955	3955
Panel B: Expenditures					
Log total expenditures	0.007 (0.010)	-0.011 (0.012)	-0.011 (0.012)	-0.021 (0.013)	-0.024 (0.014)*
Log administrative expenditures	0.006 (0.010)	-0.017 (0.012)	-0.017 (0.012)	-0.019 (0.013)	-0.022 (0.014)
Log personnel expenditures	0.010 (0.009)	-0.005 (0.010)	-0.005 (0.010)	0.011 (0.012)	0.009 (0.012)
Log material expenditures	-0.021 (0.014)	-0.025 (0.016)	-0.025 (0.016)	-0.036 (0.018)**	-0.044 (0.019)**
Log investment expenditures	0.004 (0.032)	0.007 (0.040)	0.007 (0.040)	-0.001 (0.047)	-0.013 (0.045)
Number of clusters	604	604	604	604	604
Observation	8170	8170	8170	8170	8170
Panel C: Tax multipliers					
Log multiplier property tax A	0.006 (0.006)	-0.009 (0.007)	-0.009 (0.007)	-0.004 (0.008)	-0.003 (0.009)
Log multiplier property tax B	0.009 (0.008)	-0.009 (0.010)	-0.009 (0.010)	-0.002 (0.011)	0.001 (0.011)
Log trade tax multiplier	0.001 (0.002)	-0.004 (0.003)	-0.004 (0.003)	-0.003 (0.003)	-0.003 (0.003)
Number of clusters	604	604	604	604	604
Observation	8170	8170	8170	8170	8170

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each coefficients represents a separate regression using a treatment dummy for whether there was a coalition government ($D_i = 1$), which is defined as the strongest party holding less than 50 % of the seats in the council. The dependent variable is given in the left-most column. Standard errors robust to heteroskedasticity and clustered at municipality level are reported in parenthesis. The strongest party's share of seats of up to the fourth polynomial is included as control function (coefficients not reported). The order of polynomial used for the control function is given in the header. The control function is not allowed to be flexible at either side of the threshold. All regressions include municipality fixed effects and the logarithm of the council size. All right hand side variables are lagged by one year. The model with the lowest AIC among all RDD models for a respective outcome variable is marked in bold. Panel A contains fewer observations since data on the debt variables is available only for the 2000-2009 period.

Source: Own calculations based on data from the Baden-Württemberg State Statistical Office and the Federal Statistical Office of Germany.

Table 18: Robustness check: RDD estimation result controlling for the presence of suburb election system (*unechte Teilortswahl*), using fixed effects, full sample

Polynomial Order	First		Second		Third		Fourth	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Debt Growth								
Log growth rate total debt	-0.003 (0.004)	-0.003 (0.004)	-0.004 (0.006)	-0.004 (0.006)	-0.007 (0.008)	-0.007 (0.008)	-0.002 (0.012)	-0.001 (0.012)
Log growth rate debt core	-0.004 (0.005)	-0.004 (0.005)	-0.006 (0.007)	-0.006 (0.007)	-0.017 (0.011)	-0.017 (0.011)	-0.013 (0.018)	-0.012 (0.018)
Log growth rate debt public undertakings	-0.004 (0.004)	-0.004 (0.004)	-0.006 (0.005)	-0.006 (0.005)	-0.008 (0.008)	-0.008 (0.008)	-0.002 (0.012)	-0.003 (0.012)
Suburb election control	Yes	No	Yes	No	Yes	No	Yes	No
Number of clusters	544	544	544	544	544	544	544	544
Observation	3955	3955	3955	3955	3955	3955	3955	3955
Panel B: Expenditures								
Log total expenditures	-0.017 (0.013)	-0.015 (0.012)	-0.033 (0.019)*	-0.034 (0.018)*	-0.013 (0.031)	-0.015 (0.029)	-0.066 (0.043)	-0.058 (0.039)
Log administrative expenditures	-0.020 (0.012)	-0.021 (0.013)*	-0.023 (0.018)	-0.028 (0.018)	-0.014 (0.028)	-0.017 (0.028)	-0.037 (0.039)	-0.025 (0.038)
Log personnel expenditures	-0.011 (0.009)	-0.009 (0.010)	0.001 (0.013)	0.009 (0.015)	-0.015 (0.019)	-0.003 (0.024)	0.003 (0.027)	0.018 (0.036)
Log material expenditures	-0.031 (0.017)*	-0.027 (0.018)	-0.048 (0.025)*	-0.063 (0.026)**	-0.026 (0.036)	-0.038 (0.038)	-0.018 (0.051)	-0.030 (0.053)
Log investment expenditures	-0.027 (0.039)	-0.001 (0.039)	-0.114 (0.055)**	-0.044 (0.054)	-0.098 (0.083)	-0.018 (0.078)	-0.028 (0.114)	0.011 (0.105)
Suburb election control	Yes	No	Yes	No	Yes	No	Yes	No
Number of clusters	585	604	585	604	585	604	585	604
Observation	6463	8170	6463	8170	6463	8170	6463	8170
Panel C: Tax multipliers								
Log multiplier property tax A	-0.012 (0.008)	-0.014 (0.008)*	-0.001 (0.012)	-0.002 (0.012)	-0.006 (0.018)	-0.014 (0.018)	-0.006 (0.024)	-0.017 (0.026)
Log multiplier property tax B	-0.014 (0.009)	-0.015 (0.010)	0.011 (0.013)	0.004 (0.015)	-0.003 (0.020)	-0.024 (0.023)	0.004 (0.028)	-0.015 (0.035)
Log trade tax multiplier	-0.005 (0.003)	-0.005 (0.003)	-0.002 (0.004)	-0.004 (0.004)	-0.003 (0.006)	-0.011 (0.006)*	-0.005 (0.008)	-0.012 (0.009)
Suburb election control	Yes	No	Yes	No	Yes	No	Yes	No
Number of clusters	585	604	585	604	585	604	585	604
Observation	6463	8170	6463	8170	6463	8170	6463	8170

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each coefficients represents a separate regression using a treatment dummy for whether there was a coalition government ($D_i = 1$), which is defined as the strongest party holding less than 50 % of the seats in the council. The dependent variable is given in the left-most column. Standard errors robust to heteroskedasticity and clustered at municipality level are reported in parenthesis. The strongest party's share of seats of up to the fourth polynomial is included as control function separately for the two sides of the threshold (coefficients not reported). The order of polynomial used for the control function is given in the header. All regressions include municipality fixed effects and the logarithm of the council size. All right hand side variables are lagged by one year. Panel A contains fewer observations since data on the debt variables is available only for the 2000-2009 period. Models with odd number include a dummy variable equal to one if a municipality uses a suburb election system (*unechte Teilortswahl*). The inclusion of the dummy variable reduces the sample size since information on the dummy variable is available only from the year 1999 onwards.

Source: Own calculations based on data from the Baden-Württemberg State Statistical Office and the Federal Statistical Office of Germany.

A2: Additional Robustness Checks

In the following I check whether the results are robust to the inclusion of additional control variables and different sub-samples, respectively.

Partisan Effects

A major concern to identification is that the effect of fragmentation is confounded by a partisan effect. Table 2 shows that left wing parties hardly ever win an absolute majority. Only 1 percent of all single-party governments in the sample are led by the SPD, whereas the CDU and the FWG account for the majority of all single-party governments. Thus, a change from a coalition government to a single-party government could go in hand with an ideological right-shift of the council. If that were the case, the estimated effect would be mistaken for a fragmentation effect, although it is actually a partisan effect.

However, two arguments speak against this concern: On the one hand, right-wing governments are often expected to cut taxes and implement austerity measures. Hence, from a theoretical point of view a political swing to the right triggered by a change to an absolute majority in the council should lead to lower spending. In contrast, the findings suggests that absolute majorities spend more, not less. On the other hand, the overall presence of left-wing governments is rather low, as conservative parties, like the CDU and the FWG, respectively, are the dominant political forces in Baden-Württemberg. Table 2 reveals that the SPD wins only 4.3 percent of all elections in the sample. Thus the probability of an ideological shift of the government is generally rather low.

A way to empirically disentangle partisan effects and fragmentation effects is to include a categorical variable into regression equation 3 that indicates the partisan identity of the strongest force in the council. Table 19 shows that results do not differ considerably from the baseline specification in table 3. Both, magnitude and sign of coefficients, remain roughly the same after controlling for the partisanship of the strongest force in the council. Except for administrative spending and the multiplier of property tax A for the first order polynomial specifications all coefficients retain their significance. Therefore I conclude that the estimated effect is a pure result of the change in government fragmentation.

Mayors without Party Affiliation

In the main analysis I assumed that mayors without party affiliation act in a neutral way and vote for the strongest party in the council in case of a tie. This presumption seems to be justified as 90 percent of the mayors are administrative specialists without a classical party career and do not come from the municipality where they run for office. Furthermore, mayors are known to act generally in a non-partisan manner seeking the best interest for the municipality (Wehling 2012).

To check whether this assumption affects estimation results, I build sub-samples for municipalities with independent mayors and party affiliated mayors, as shown in table 20. Models 1, 3, 5, 7 contain only municipalities with independent mayors. Models 2, 4, 6, 8 contain only municipalities with the mayor belonging to the strongest party in the council. The bulk of the fiscal outcome variables estimated effects are rather similar for both types of mayors, party affiliated and independent ones. Furthermore, the point estimates especially for low order polynomials do not considerably differ from the main estimation, given in table 3. Splitting the sample inflates standard errors. As a result some of the coefficients lose its significance. The party

Table 19: Robustness checks: RDD estimation results controlling for ideology of government, using fixed effects, full sample

	RDD			
	(1) First Order	(2) Second Order	(3) Third Order	(4) Fourth Order
Panel A: Debt Growth				
Log growth rate total debt	-0.003 (0.004)	-0.004 (0.006)	-0.007 (0.008)	-0.002 (0.012)
Log growth rate debt core	-0.004 (0.005)	-0.006 (0.007)	-0.017 (0.011)	-0.012 (0.018)
Log growth rate debt public undertakings	-0.004 (0.004)	-0.006 (0.005)	-0.008 (0.008)	-0.003 (0.012)
Number of clusters	544	544	544	544
Observation	3955	3955	3955	3955
Panel B: Expenditures				
Log total expenditures	-0.011 (0.012)	-0.035 (0.018)*	-0.016 (0.029)	-0.061 (0.040)
Log administrative expenditures	-0.017 (0.013)	-0.030 (0.019)	-0.018 (0.029)	-0.027 (0.039)
Log personnel expenditures	-0.004 (0.010)	0.008 (0.016)	-0.004 (0.024)	0.015 (0.036)
Log material expenditures	-0.025 (0.018)	-0.064 (0.026)**	-0.038 (0.038)	-0.032 (0.053)
Log investment expenditures	0.005 (0.039)	-0.047 (0.055)	-0.020 (0.079)	0.006 (0.107)
Number of clusters	604	604	604	604
Observation	8170	8170	8170	8170
Panel C: Tax multipliers				
Log multiplier property tax A	-0.011 (0.008)	-0.003 (0.012)	-0.015 (0.018)	-0.018 (0.026)
Log multiplier property tax B	-0.010 (0.010)	0.002 (0.015)	-0.025 (0.023)	-0.018 (0.035)
Log trade tax multiplier	-0.004 (0.003)	-0.004 (0.004)	-0.011 (0.006)*	-0.013 (0.009)
Number of clusters	604	604	604	604
Observation	8170	8170	8170	8170

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each coefficients represents a separate regression using a treatment dummy for whether there was a coalition government ($D_i = 1$), which is defined as the strongest party holding less than 50 % of the seats in the council. The dependent variable is given in the left-most column. Standard errors robust to heteroskedasticity and clustered at municipality level are reported in parenthesis. The strongest party's share of seats of up to the fourth polynomial is included as control function separately for the two sides of the threshold (coefficients not reported). The order of polynomial used for the control function is given in the header. All regressions include municipality fixed effects, the logarithm of the council size and a categorical variable indicating the partisan identity of the strongest force in the council. All right hand side variables are lagged by one year. Panel A contains fewer observations since data on the debt variables is available only for the 2000-2009 period.

Source: Own calculations based on data from the Baden-Württemberg State Statistical Office and the Federal Statistical Office of Germany.

affiliation of the mayor affects the expenditure variables if a third or fourth order polynomial control function is used (column 5 to 7). For total expenditures, as well as administrative and material spending the coefficients turn positive for the sample with party affiliated mayors if a third order polynomial is used, whereas the coefficients of the independent mayor sample turn positive if a fourth order polynomial used. None of estimated coefficients with a positive sign is statistically significant from zero. This suggests that there is no systematic difference in the effect depending on whether the mayor runs as independent. Rather, it becomes clear that the negative effect of coalition governments on spending is non-robust to higher order polynomial specifications if the sample size is split in half. However, it is quite clear that there is no evidence for the government fragmentation hypothesis. Therefore, I conclude that it is reasonable to assume that there is no additional partisan conflict between the mayor and the strongest party in the council if the mayor runs as independent.

Table 20: Subsample: RDD estimation results by party affiliation of the mayor, using fixed effects

Polynomial Order	First		Second		Third		Fourth	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Debt Growth								
Log growth rate total debt	-0.002 (0.007)	-0.003 (0.006)	-0.006 (0.009)	0.001 (0.008)	-0.019 (0.016)	-0.000 (0.008)	-0.009 (0.028)	-0.007 (0.011)
Log growth rate debt core	-0.005 (0.007)	-0.002 (0.007)	-0.011 (0.010)	0.005 (0.013)	-0.025 (0.018)	-0.018 (0.013)	-0.025 (0.031)	-0.020 (0.023)
Log growth rate debt public undertakings	-0.003 (0.007)	-0.004 (0.004)	-0.007 (0.009)	-0.008 (0.007)	-0.005 (0.015)	-0.013 (0.011)	0.020 (0.025)	-0.019 (0.016)
Mayor	PL	Other	PL	Other	PL	Other	PL	Other
Number of clusters	327	277	327	277	327	277	327	277
Observation	2222	1733	2222	1733	2222	1733	2222	1733
Panel B: Expenditures								
Log total expenditures	-0.018 (0.017)	-0.007 (0.018)	-0.031 (0.025)	-0.036 (0.024)	0.008 (0.046)	-0.002 (0.029)	-0.105 (0.055)*	0.022 (0.040)
Log administrative expenditures	-0.019 (0.017)	-0.021 (0.019)	-0.021 (0.026)	-0.037 (0.024)	0.005 (0.044)	-0.006 (0.030)	-0.050 (0.058)	0.034 (0.044)
Log personnel expenditures	-0.013 (0.015)	-0.010 (0.014)	-0.004 (0.022)	0.021 (0.021)	-0.054 (0.032)*	0.045 (0.032)	-0.053 (0.046)	0.060 (0.046)
Log material expenditures	-0.016 (0.021)	-0.046 (0.028)	-0.066 (0.031)**	-0.073 (0.038)*	0.001 (0.046)	-0.066 (0.052)	-0.039 (0.062)	0.008 (0.072)
Log investment expenditures	-0.033 (0.057)	0.020 (0.059)	-0.066 (0.083)	-0.039 (0.076)	-0.053 (0.123)	0.028 (0.107)	-0.120 (0.170)	0.098 (0.145)
Mayor	PL	Other	PL	Other	PL	Other	PL	Other
Number of clusters	393	333	393	333	393	333	393	333
Observation	4562	3608	4562	3608	4562	3608	4562	3608
Panel C: Tax multipliers								
Log multiplier property tax A	-0.014 (0.010)	-0.005 (0.012)	0.012 (0.013)	0.007 (0.017)	-0.000 (0.021)	-0.000 (0.024)	0.015 (0.032)	-0.018 (0.032)
Log multiplier property tax B	-0.012 (0.013)	-0.008 (0.015)	0.028 (0.017)	0.001 (0.020)	-0.006 (0.028)	0.002 (0.027)	0.006 (0.042)	0.006 (0.041)
Log trade tax multiplier	-0.005 (0.004)	-0.001 (0.005)	0.004 (0.005)	-0.005 (0.006)	-0.004 (0.008)	-0.006 (0.008)	-0.008 (0.011)	-0.001 (0.011)
Mayor	PL	Other	PL	Other	PL	Other	PL	Other
Number of clusters	393	333	393	333	393	333	393	333
Observation	4562	3608	4562	3608	4562	3608	4562	3608

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each coefficients represents a separate regression using a treatment dummy for whether there was a coalition government ($D_i = 1$), which is defined as the strongest party holding less than 50 % of the seats in the council. The dependent variable is given in the left-most column. Standard errors robust to heteroskedasticity and clustered at municipality level are reported in parenthesis. All regressions include municipality fixed effects. The treatment and the assignment variable is lagged by one year. Models with odd number only contain municipalities with independent mayors. Models with even number only contain municipalities with the mayor belonging to the strongest party in the council.

Source: Own calculations based on data from the Baden-Württemberg State Statistical Office and the Federal Statistical Office of Germany.

Time Fixed Effects

The results might be driven by economic shocks in certain years. For instance, a recession might on the one hand require the adaption of fiscal policy, and on the other hand lead to a change of the majorities in the council. This would pose a problem of omitted variable bias. To check for this possibility I include year fixed effects into the regression equation 3. Table 21 shows that the overall picture remains qualitatively the same if I control for time fixed effects. The presence of coalition governments still lowers growth in debt, total expenditures, as well as administrative expenditures and material expenditures. Especially the coefficients of the debt variables and material expenditures remain remarkably stable in terms of size and significance. However, including fixed effects reduces the point estimates of total and administrative spending for the first and third order polynomial specification. The coefficients of the tax multipliers shrink to zero. In sum, accounting for time fixed effects confirms the findings of the baseline estimation in table 3. Coalition governments are not found to spend more. Rather there is weak evidence that they spend less than single-party governments.

Restricted Windows around the Threshold

In the baseline global polynomial approach in table 3. I use all observations, even those far away from the threshold. This might produce a bias in the estimated effects. Therefore, van der Klaauw (2008) suggests to examine the robustness of the parametric analysis by restricting the sample to a subset of observations more closely clustered around the threshold. Table 22 reports RDD estimation results for observations restricted to a band of +/- 20 %, +/- 15 %, and +/- 10 % around the 50 % threshold, using fixed effects. Narrowing the range of observations yields a remarkably similar picture. Especially the negative effect of coalition governments on the expenditures variables is essentially confirmed as almost all estimates retain their sign, as shown in panel B of table 22. For the 20 percent sample (model 1 to 4) the estimated coefficients for total spending are almost equal in terms of size and significance to the baseline estimation in table 3. The point estimates and standard errors generally increase if the band around the threshold is narrowed. For the sample restricted to observations no further away from the majority threshold than 10 percent the standard errors are partially more than twice as high (model 9 to 12). This indicates that the estimates for models 9 to 12 are generally rather imprecise. A similar pattern holds for administrative expenditures. However, some estimates for higher order polynomial specifications turn positive in particular if the bandwidth is restricted to at least 15 percent around the threshold. The estimated coefficients for material expenditures partially even gain in significance and remain in the same ballpark. However, the coefficients for the fourth order polynomial specification turn slightly positive if the bandwidth is no greater than 15 percent. The results for personnel expenditures and investment spending remain rather inconclusive.

Panel A reveals that narrowing the sample to a window size closely around the threshold has only a minor effect on the estimated effect for the growth in total debt and debt of the core budget, respectively, as the bulk of the coefficients remain negative and retain their magnitude. Although, the coefficients for the growth rate in debt of public undertakings turns significantly positive for some models, the effect is not robust, as there are also some coefficients that are significantly negative. This suggests that there is no clear relationship between government fragmentation and the growth rate in debt of public undertakings.

Panel C shows that the sign of coefficients turns positive for higher order polynomials if the bandwidth is narrowed. This implies that the negative effect of coalition governments on tax

Table 21: RDD estimation results controlling for year and municipality fixed effects, full sample

	RDD			
	(1) First Order	(2) Second Order	(3) Third Order	(4) Fourth Order
Panel A: Debt Growth				
Log growth rate total debt	-0.004 (0.004)	-0.005 (0.006)	-0.008 (0.008)	-0.001 (0.012)
Log growth rate debt core	-0.004 (0.005)	-0.006 (0.007)	-0.018 (0.011)	-0.012 (0.018)
Log growth rate debt public undertakings	-0.004 (0.004)	-0.006 (0.005)	-0.009 (0.008)	-0.003 (0.012)
Number of clusters	544	544	544	544
Observation	3955	3955	3955	3955
Panel B: Expenditures				
Log total expenditures	-0.003 (0.011)	-0.034 (0.017)**	-0.004 (0.028)	-0.058 (0.038)
Log administrative expenditures	-0.003 (0.010)	-0.027 (0.016)*	0.002 (0.025)	-0.025 (0.033)
Log personnel expenditures	0.003 (0.009)	0.003 (0.014)	0.003 (0.023)	0.009 (0.034)
Log material expenditures	-0.021 (0.018)	-0.063 (0.026)**	-0.031 (0.038)	-0.035 (0.053)
Log investment expenditures	-0.016 (0.035)	-0.063 (0.050)	-0.052 (0.074)	0.002 (0.098)
Number of clusters	604	604	604	604
Observation	8170	8170	8170	8170
Panel C: Tax multipliers				
Log multiplier property tax A	-0.003 (0.006)	-0.000 (0.009)	-0.000 (0.015)	-0.016 (0.020)
Log multiplier property tax B	0.002 (0.006)	0.007 (0.010)	-0.003 (0.017)	-0.014 (0.026)
Log trade tax multiplier	-0.000 (0.002)	-0.003 (0.003)	-0.006 (0.005)	-0.012 (0.007)*
Number of clusters	604	604	604	604
Observation	8170	8170	8170	8170

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each coefficients represents a separate regression using a treatment dummy for whether there was a coalition government ($D_i = 1$), which is defined as the strongest party holding less than 50 % of the seats in the council. The dependent variable is given in the left-most column. Standard errors robust to heteroskedasticity and clustered at municipality level are reported in parenthesis. The strongest party's share of seats of up to the fourth polynomial is included as control function separately for the two sides of the threshold (coefficients not reported). The order of polynomial used for the control function is given in the header. All regressions include year and municipality fixed effects and the logarithm of the council size. All right hand side variables are lagged by one year. The model with the lowest AIC among all RDD models for a respective outcome variable is marked in bold. Panel A contains fewer observations since data on the debt variables is available only for the 2000-2009 period.

Source: Own calculations based on data from the Baden-Württemberg State Statistical Office and the Federal Statistical Office of Germany.

multipliers is not robust.

Accounting for Outliers

In the following I check whether the results of the baseline regression in table 3 are sensitive to outliers. Table 16 in the Appendix presents the results from the fixed effects approach after outliers were eliminated for all specifications by restricting the sample to the 1st to 99th percentiles of the outcome variable. Compared to table 17 the signs of the effects do not change. However, eliminating outliers slightly makes the coefficients more stable across different polynomial specifications, though some lose their significance. Amongst others, the effect on public administration is no longer statistically significant. Furthermore, the effect on investment spending turns consistently negative and is more precisely estimated. However, it remains insignificant. Therefore I conclude that the rejection of the government fragmentation hypothesis is not driven by individual observations.

Identical Slope on either Side of the Threshold

Next, I test whether the results change if I assume that the slope of the control function is the same at both sides of the threshold. For estimation I drop the interaction terms from the regression equation 3. The results remain qualitatively the same, as shown in table 17 in the Appendix. The coefficients are somewhat deflated and tend to become more stable across different polynomial specifications. However, the general direction of the estimated effects is confirmed. Coalition governments seem to run smaller deficits, spend less, and set lower tax rates. This underscores the robustness of the findings.

Accounting for special Voting Procedure

Approximately half of all municipalities of Baden-Württemberg additionally apply a special voting procedure at the town district level. It is questionable whether the presence of this suburb election system (*unechte Teilortswahl*) poses a threat to identification. The suburb election system aims to ensure an appropriate representation of all urban districts in the council. The basic idea is to allocate an additional seat to an urban district, which is underrepresented in the council. To preserve the overall proportional representation the additional seat has to be evened out by balancing mandates (*Ausgleichsmandate*). Thus the total number of seats in the council can turn out higher than stipulated before the election.⁴⁷ Thus, the suburb election system could systematically increase the number of seats required to win an absolute majority. If this changes the probability of close election and if at the same time municipalities with suburb election system are structurally different, this would pose a selection problem.⁴⁸ In table 18 in the Appendix I test whether the presence of suburb election system affects outcomes for the FE approach. I include a dummy, that equals one if a municipality uses a suburb election system. The inclusion of the dummy variable reduces sample size and variation within units since I only have information on this variable from the year 1999 onwards. All estimated coefficients remain

⁴⁷Other possible effects are a lower voter turnout, a higher percentage of invalid votes due to the increased complexity of the ballot papers and a lower price of a seat in some suburb districts relative to the urban core district. Due to lack of space I refrain from laying out the exact procedure. For a thorough explanation see http://www.kommunalwahl-bw.de/wie_wird_gewaehlt_kommunalwahl.html.

⁴⁸For more information on this see <http://www.wahlrecht.de/kommunal/baden-wuerttemberg.htm>

Table 22: Subsample: RDD estimation results for observations restricted to a band of +/- 20 %, +/- 15 %, +/- 10 % around the 50 % threshold, using fixed effects

	+/- 20 %				+/- 15 %				+/- 10 %			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	First	Second	Third	Fourth	First	Second	Third	Fourth	First	Second	Third	Fourth
Panel A: Debt growth												
Log growth rate total debt	-0.003 (0.004)	-0.006 (0.006)	-0.005 (0.009)	-0.001 (0.013)	-0.004 (0.005)	-0.005 (0.007)	-0.006 (0.012)	0.027 (0.017)	-0.008 (0.005)	-0.008 (0.010)	-0.001 (0.016)	-0.003 (0.034)
Log growth rate debt core	-0.003 (0.004)	-0.009 (0.008)	-0.014 (0.012)	-0.022 (0.020)	-0.004 (0.005)	-0.009 (0.009)	-0.026 (0.017)	0.008 (0.027)	-0.007 (0.006)	-0.013 (0.014)	-0.028 (0.023)	0.005 (0.043)
Log growth rate debt public undertakings	-0.004 (0.004)	-0.006 (0.006)	-0.008 (0.010)	0.002 (0.013)	-0.006 (0.004)	-0.008 (0.007)	-0.003 (0.012)	0.036 (0.016)**	-0.011 (0.006)*	-0.006 (0.012)	0.032 (0.018)*	-0.024 (0.039)
Number of clusters	532	532	532	532	491	491	491	491	402	402	402	402
Observation	3828	3828	3828	3828	3398	3398	3398	3398	2582	2582	2582	2582
Panel B: Expenditures												
Log total expenditures	-0.015 (0.012)	-0.034 (0.018)*	-0.015 (0.029)	-0.058 (0.039)	-0.021 (0.013)	-0.028 (0.026)	-0.045 (0.035)	-0.064 (0.083)	-0.035 (0.020)*	-0.063 (0.029)**	-0.082 (0.071)	-0.195 (0.131)
Log administrative expenditures	-0.022 (0.013)*	-0.027 (0.020)	-0.018 (0.031)	0.017 (0.041)	-0.021 (0.014)	-0.028 (0.027)	0.011 (0.035)	0.030 (0.080)	-0.037 (0.021)*	-0.019 (0.029)	-0.010 (0.069)	0.021 (0.133)
Log personnel expenditures	-0.001 (0.011)	-0.000 (0.018)	0.008 (0.030)	0.048 (0.048)	-0.000 (0.013)	0.002 (0.023)	0.044 (0.042)	0.021 (0.066)	-0.001 (0.016)	0.016 (0.034)	0.014 (0.059)	-0.097 (0.118)
Log material expenditures	-0.035 (0.018)**	-0.055 (0.028)*	-0.037 (0.045)	0.004 (0.065)	-0.042 (0.019)**	-0.054 (0.034)	-0.010 (0.055)	0.047 (0.101)	-0.080 (0.025)**	-0.038 (0.045)	-0.022 (0.091)	-0.036 (0.175)
Log investment expenditures	-0.012 (0.040)	-0.039 (0.062)	-0.007 (0.097)	-0.058 (0.149)	-0.030 (0.045)	-0.000 (0.076)	-0.099 (0.120)	-0.067 (0.216)	-0.040 (0.055)	-0.158 (0.098)	0.003 (0.178)	-1.109 (0.409)***
Number of clusters	595	595	595	595	549	549	549	549	462	462	462	462
Observation	7834	7834	7834	7834	6827	6827	6827	6827	5053	5053	5053	5053
Panel C: Tax multipliers												
Log multiplier property tax A	-0.010 (0.008)	-0.011 (0.013)	-0.009 (0.022)	-0.005 (0.034)	-0.013 (0.009)	-0.006 (0.017)	0.003 (0.029)	-0.009 (0.043)	-0.010 (0.012)	-0.003 (0.026)	-0.019 (0.039)	-0.035 (0.077)
Log multiplier property tax B	-0.010 (0.011)	-0.006 (0.017)	-0.023 (0.029)	0.018 (0.047)	-0.011 (0.011)	-0.016 (0.022)	0.013 (0.041)	0.004 (0.061)	-0.007 (0.015)	-0.002 (0.035)	0.002 (0.056)	0.069 (0.097)
Log trade tax multiplier	-0.004 (0.003)	-0.007 (0.005)	-0.013 (0.007)*	0.005 (0.012)	-0.006 (0.003)*	-0.010 (0.006)*	-0.001 (0.010)	0.006 (0.018)	-0.007 (0.004)*	-0.013 (0.009)	0.002 (0.015)	0.015 (0.029)
Number of clusters	595	595	595	595	549	549	549	549	462	462	462	462
Observation	7834	7834	7834	7834	6827	6827	6827	6827	5053	5053	5053	5053

Notes: Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each coefficient represents a separate regression using a treatment dummy for whether there was a coalition government ($D_i = 1$), which is defined as the strongest party holding less than 50 % of the seats in the council. The dependent variable is given in the left-most column. Standard errors robust to heteroskedasticity and clustered at municipality level are reported in parenthesis. The strongest party's share of seats of up to the fourth polynomial is included as control function separately for the two sides of the threshold (coefficients not reported). The order of polynomial used for the control function is given in the header. All regressions include municipality fixed effects and the logarithm of the council size. All right hand side variables are lagged by one year. Models (1) to (4) use a sample restricted to observations with the strongest party holding at least 30 % and not more than 70 % of the seats in the council. Models (5) to (8) use a sample restricted to observations with the strongest party holding at least 35 % and not more than 65 % of the seats in the council. Models (9) to (12) use a sample restricted to observations with the strongest party holding at least 40 % and not more than 60 % of the seats in the council. Panel A contains fewer observations since data on the debt variables is available only for the 2000-2009 period.

Source: Own calculations based on data from the Baden-Württemberg State Statistical Office and the Federal Statistical Office of Germany.

in the same ballpark - only the effect on investment expenditures turns negative. Therefore, table 18 in the Appendix provides evidence that the presence of a suburb election system is not a concern for identification.

Balance Tests for Baseline Covariates

The identifying assumption of the RDD states that observations should exhibit similar pre-treatment characteristics at either side of the threshold. Using the full sample I examine in table 23 and table 24 whether treatment and control group are balanced close to the threshold. Each table reports the means for the baseline covariates for the treatment and control group (column 1 and 2), the difference in means (column 3), and the p-value of a orthogonality-test with the null-hypothesis of equal means (column 4). Table 23 considers all observations, while table 24 only considers observations, that are not further away from the 50 percent threshold than 5 percent. The null-hypothesis of equal means of the baseline covariates for the treatment and control group (column 4) is rejected not only in table 23, but also in table 24. The average size of the municipal council is significantly smaller for observations just above the threshold compared to those just below. Observations with a close absolute majority have a council, which is smaller by 5.6 seats on average, as shown in table 24. This implies that treatment and control group are locally not balanced with respect to pre-treatment characteristics.

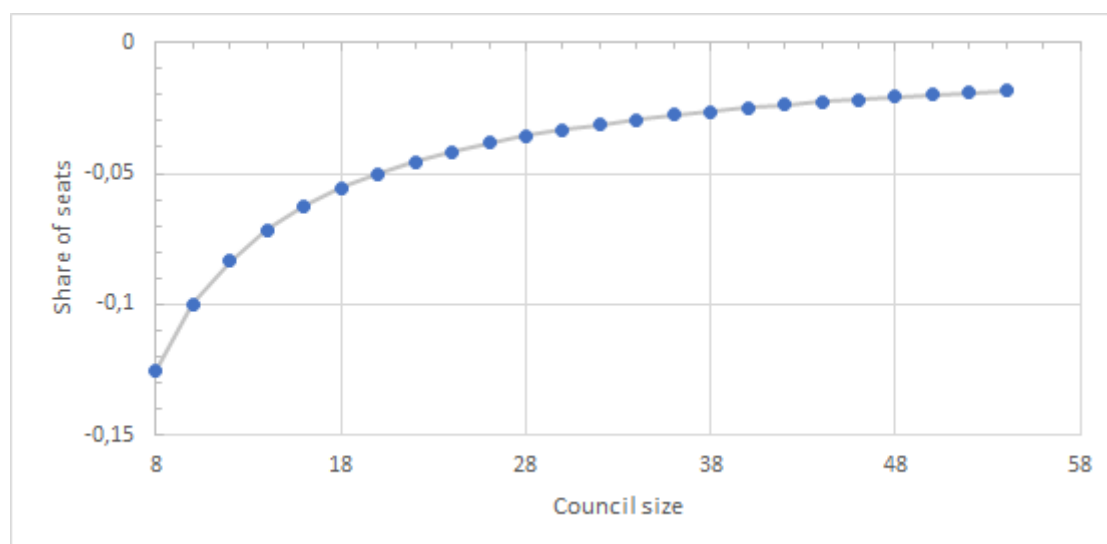
The underlying reason for this is a systematic selection problem in close elections: the strongest party in a municipality with a relatively small council can never be as close to the threshold as a municipality with a relatively large council. Figure 4 illustrates this with a calculation example. The horizontal axis reports a range of council sizes. The vertical axis reports the corresponding degree of closeness to the threshold, defined as the proportion of seats (normalized at the 50 percent threshold), which just misses an absolute majority by one seat. The minimal possible distance to the 50 percent majority threshold decreases with the council size. For example, take a municipality with a council size of 8 seats and another one with 18 council seats. Assume that the strongest party lacks one seat to the absolute majority in both municipalities. Then the strongest party in the small and the large municipality, holds 3 seats and 8 seats, respectively, while the proportions of seats equals 37.5 percent and 44.4 percent, respectively. Thus, the small municipality is more than 6 percentage points further away from the threshold. Note further that the function in figure 4 is approximately logarithmic, as the slope decreases with growing council size and converges to zero. This implies that the difference in the degree of closeness to the threshold between two council sizes diminishes with growing council size: the difference between a council of 8 seats and a council of 18 seats is much larger than the difference between a council of 18 seats and a council of 28 seats (6.9 vs. 1.9 percentage points). Now, if one considers only those election results as quasi-random which lie within a range of 5 percent around the 50 percent threshold, as done in table 24, all municipalities with a council size of at least 18 seats are not taken into account - in figure 4 these are all points below the horizontal line that intersects the y-axis at 5 percent. Hence municipalities with a small council are never selected into the treatment group (close coalitions), despite being just one seat away from the absolute majority.

What is more, directly at the majority threshold there is no systematic selection into the control group, as the probability of being exactly at the threshold is independent of the council size. Holding exactly half of the seats in a council consisting of 8 seats yields a proportion of seats equal to 50 percent, just as holding half of the seats in a council consisting of 28 seats. Therefore, municipalities with a small council size can be part of the control group (close absolute majorities), but never be part of the treatment group (close coalitions) in close

neighborhood of the majority threshold. As a consequence, the average size of the council is lower in the control group. Note that the selection into the treatment group is only a serious problem for very small councils as the minimal distance to the threshold becomes smaller and smaller with growing council size. This is reflected by the logarithmic function in figure 4.

There are three ways to circumvent this sample selection bias: Firstly, one could apply a fixed effects approach, where the treatment effect is estimated by using only the variation within municipalities, as done in the main section. By design, the degree of closeness does not differ within the unit of comparison as long as the council size for a municipality does not change over time. Furthermore, treatment and control group are automatically balanced in the fixed effects regression as both, observations below and above the threshold, are equal with respect to time-invariant covariates. Secondly, one can exclude the sub-sample, where imbalance exists, as suggested by Eggers et al. (2015). Such a so-called *donut RDD* is applied by Barreca et al. (2011) and Almond et al. (2011). In our case this implies to restrict the sample to municipalities with more than 18 seats, as for this sample there is no selection into treatment within a band of 5 percent around the threshold. This comes at the cost of a reduced number of observations and hence limited external validity. I use this approach as additional robustness check. Thirdly, instead of the proportion of seats I could use the seat margin as the assignment variable, which is defined as the number of seats by which the strongest party is below the minimum needed to form a majority government. In this case all municipalities with the strongest party lacking exactly one seat to the absolute majority are considered as equally close to the majority threshold regardless of the council size. This approach is taken by Artés Caselles and Jurado (2014). However, this approach comes at the cost of a rather discrete assignment variable, which lead to imprecise estimates. Therefore I do not use this approach.

Figure 4: Calculation example: degree of closeness as a function of municipal council size



Notes: The figure reports the degree of closeness to the threshold for a range of council sizes. The degree of closeness is defined as the proportion of seats of the strongest party (normalized at the 50 % threshold), that lacks just one seat to the absolute majority. The figure illustrates that the minimal possible distance to the 50 % majority threshold decreases with the council size.

Source: own construction.

Table 23: Means of baseline covariates by type of government, full sample

	Single-party government (means)	Coalition government (means)	Difference in means	p-value from orthogonality test
Population	7557.91	13183.29	-5625.38	0.00
Council size	18.79	21.75	-2.96	0.00
Population density	286.79	467.01	-180.22	0.00
Employees	0.35	0.35	0.00	0.17
Share of old	28.71	29.95	-1.24	0.00
Share of young	31.81	30.61	1.20	0.00
Commuters	0.26	0.26	0.01	0.00
<i>N</i>	2680	5672		

Notes: The right-most column reports the p-values from a t-test with the null hypothesis of equal means for the predetermined variables across treatment. The last row reports the number of observations for treatment and control group.

Table 24: Means of baseline covariates by type of government if the share of seats is within a band of +/- 5 % around the 50 % threshold, full sample

	$0.5 \leq s_{it} < 0.55$ (means)	$0.45 < s_{it} < 0.5$ (means)	Difference in means	p-value from orthogonality test
Population	7734.97	12365.48	-4630.51	0.00
Council size	18.30	23.86	-5.56	0.00
Population density	308.89	283.77	25.12	0.04
Employees	0.35	0.35	0.00	0.04
Share of old	28.92	29.34	-0.42	0.09
Share of young	31.64	31.44	0.21	0.26
Commuters	0.26	0.24	0.02	0.00
<i>N</i>	1539	741		

Notes: The right-most column reports the p-values from a t-test with the null hypothesis of equal means for the predetermined variables across treatment. The last row reports the number of observations for treatment and control group, respectively.

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