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Political Fragmentation and Fiscal Policy: Evidence from German Municipalities*

René Bernard**

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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 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 nicht die Steuersätze. Koalitionsregierungen haben einen nicht-robusten, negativen Effekt auf die Bruttoausgaben, der im Wesentlichen von den Verwaltungsausgaben und dem Laufenden Sachaufwand getrieben ist.

Schlagworte: 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 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. 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

CPI consumer price index

FDP Free Democratic Party

FE fixed effects

FWG Free Voters Association

ITT intention-to-treat effect

NRW North Rhine-Westphalia

OLS ordinary least squares

RDD regression discontinuity design

RKD regression kink design

SPD Social Democratic Party

TOT treatment-on-the-treated 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 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 expenditures, but also on various sub-categories of spending. This 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 to previous analyses.

I contribute 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, I shed light on the question whether the effect of government fragmentation depends on the political regime employed at the local level. As one of the first I test the government fragmentation hypothesis in the context of a mayor-council system. Thirdly, I give insights to what extent the fragmentation effect found for German municipalities depends on socio-economic factors. Finally, I offer a careful sensitivity analysis to check for the robustness of the results.

The empirical analysis shows that coalition governments do not increase 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) on the German case.

The remainder of the paper is organized as follows: Section 2 discusses the theoretical framework. Section 3 reviews the empirical literature. Section 4 describes the institutional background and the data set. Section 5 sets out 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 they make 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 are expected to 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-constrainted 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. If only a part of the costs is financed out of a common pool, the remaining costs 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 bargaining 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).

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

[2] In 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.

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

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.

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 institutional framework and are subjected to the same voting rules 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.

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 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 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.

In sum, the evidence on the effect of government fragmentation is inconclusive.

[4] For 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.

4 Institutional Framework and Data

4.1 German Municipalities

Although 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. 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).[5] Municipalities are required to balance their budget each year. Municipalities have to fulfill a range of duties, which can be divided into two groups according to the degree of discretion: firstly, mandated spending tasks, which the state or federal government delegates to municipalities with 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).[6] 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 tasks.

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.[7] As form of government, all municipalities in Baden-Württemberg apply a mayor-council system. Accordingly, the municipal affairs are managed jointly by the municipal council and the directly elected mayor. The municipal council, as the main legislative body, decides on all issues concerning the municipality, except for those which lie within the competence of the mayor.[8] 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.[9] The number of seats in the council ranges from 8 to 60 seats population size.[10] 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.

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 (SPD); the culturally conservative, moderately market-oriented Christian Democratic Union (CDU); and the Free Voters Association (FWG), a conservative party that operates mainly 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,

[5] Aside 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.

[6] For both groups of spending, municipalities hold planning sovereignty and bear financial, organizational and staff responsibility (Grunow 2012).

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

[8] The municipal council exercises municipal planning competence, has the right to demand decision-relevant information from the local administration and lays down the guidelines for the municipal administration.

[9] The election rules allow to select applicants from other party list (Panaschieren) and assign up to three votes to one applicant (Kumulieren).

[10] The 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).

win local elections in larger cities.

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.[11] The data set covers 4 legislative terms: 1994-1998, 1999-2003, 2004-2008, and 2009-2013. I use the following outcome variables: 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. Municipal tax variables are taken from the German Federal Statistical Office. The multipliers for the three municipal tax rates are expressed in percentage points. To make observations comparable across years and legal entities I express all 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 outflows 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.[12] Table 12 in the Appendix provides summary statistics for all variables.

To correct for right-skewness in the distribution of variables, I apply a logarithmic transformation to the outcome and control variables.[13] Thus the treatment effect can be interpreted as expected percentage change in the outcome variable.

For estimation all right-hand side variables are lagged by one year. 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

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

[12] See also table 11 in the Appendix .

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

imperfect compliance. Imperfect compliance might arise out of two reasons: On the one hand, political parties might avoid building a coalition and opt for a minority government if they fail to win an absolute majority (never-takers).[14] 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, political parties might always prefer to coalesce, even if they have an absolute majority of the seats (always-takers). In the absence of crisis, however, it is rational to build a minimal winning coalition (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):

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

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.

I consider only those observations where the mayor either belongs to the largest party in the council or has no party affiliation.[15] This permits me to isolate the fragmentation effect and to abstract from partisan conflicts between the council and the mayor.[16] 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.[17] 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 since 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 in terms of 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 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 59 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

[14] For a graphical illustration of treatment types see table 13 in the Appendix . For a more detailed discussion of the potential problems of the proxy see Garmann (2014).

[15] Since 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`.

[16] Freier and Odendahl (2012) follow a similar approach.

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

Table 1: Type of government by legislature period

	Type of Government			
Election Period	Single-party	Coalition	Fraction of coalition gov.	Total
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: The 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.

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 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.

The data set has several advantageous characteristics: firstly, the data set does not only cover total expenditures, but also its sub-categories. This allows me to analyze the common pool problem in greater detail 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 for the respective time frame.[18] Finally, in many German states there was a major reform of 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.

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:

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

where the assignment variable, x_i, is the share of seats of the strongest party in the council. The

[18] Municipalities in Baden-Württemberg are forced to change from cameralistic to double-entry provisions not before 2020.

Table 2: Type of government by different categorical variables

	Type of government							
	Single-party			Coalition			Total	
	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: The 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.

threshold, x_0, is located 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 ($x < x_0$).[19]

The RDD gives an average treatment effect (ATE) at the threshold, x_0 (Lee and Lemieux 2010). 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 fiscal outcome variable, Y_i, at the threshold, x_0:

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

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. If the identifying assumption holds, both, observations slightly below and above the 50 percent threshold, should have similar characteristics except for the treatment. Thus the latter serve as valid counterfactual for the former.

I use a parametric approach to identify the treatment effect since the assignment variable is rather discrete.[20] To estimate the treatment effect at the threshold, the parametric approach relies on regressions. For extrapolation one exploits the whole range of observations, even those far away from the cut-off point, and assumes a specific functional relation between the outcome and assignment variable. The parametric apporach is formalized by the following fixed effects regression equation:

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

where Y_{it} is the fiscal outcome variable for observation i at time t. ρ indicates the treatment effect at the threshold. 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, $f(.)$, is included, that is flexible on either side of the threshold and traces the relationship between the normalized assignment variable, $\tilde{x}_{it}$, and the outcome variable, Y_{it}.[21] 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(Lee and Lemieux 2010). To account for potential autocorrelation of the error term, ε_{it}, 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, $f(.)$. If it is misspecified, the estimated treatment effect is biased (Angrist and Pischke 2015). Therefore I test different models with polynomial control functions 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 the 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, which rises with the parameters used in the model. On the other hand, it rewards the goodness of fit, which is likely to grow with an increasing number of parameters.

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 the average council size is smaller in the control group, as shown in the second panel of table 2. 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

[19] I consider only those cases in which the mayor belongs to the strongest party. For these cases holding 50 percent of the seats ensures an absolute majority.

[20] Lee and Lemieux (2010) suggest to follow this approach in case of a discrete assignment variable.

[21] To ease interpretation the assignment variable, $\tilde{x}_{it}$, is centered at the threshold, x_0. The normalized assignment variable is given as: $\tilde{x}_{it} \equiv x_{it} - x_0$.

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 becomes negligible if the council size exceeds 18 seats.[22] There a 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.[23] The comparison within units ensures that treatment and control group are balanced with respect to time-invariant covariates independently of sample size. However, some municipalities changed their council size during the sample period. Thus the degree of closeness does not remain the same for all municipalities. Therefore, I control for the size of the municipal council. An additional advantage of the fixed effects (FE) approach is that it might reduce sampling variance and hence might be more efficient than a cross-sectional approach (Lee and Lemieux 2010). Secondly, as a robustness check, I use the cross-sectional variation and exclude those observations close to the threshold, for which the imbalance exists.[24] 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 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.[25] Each point is a sample average of the outcome variable within an interval of the assignment variable.[26] 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. 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 expenditures and taxes, respectively, the regression function should display a downward jump at the majority threshold.

The expenditure variables present a mixed picture: 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.

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.

[22] For a more detailed discussion see section A2 in the Appendix.

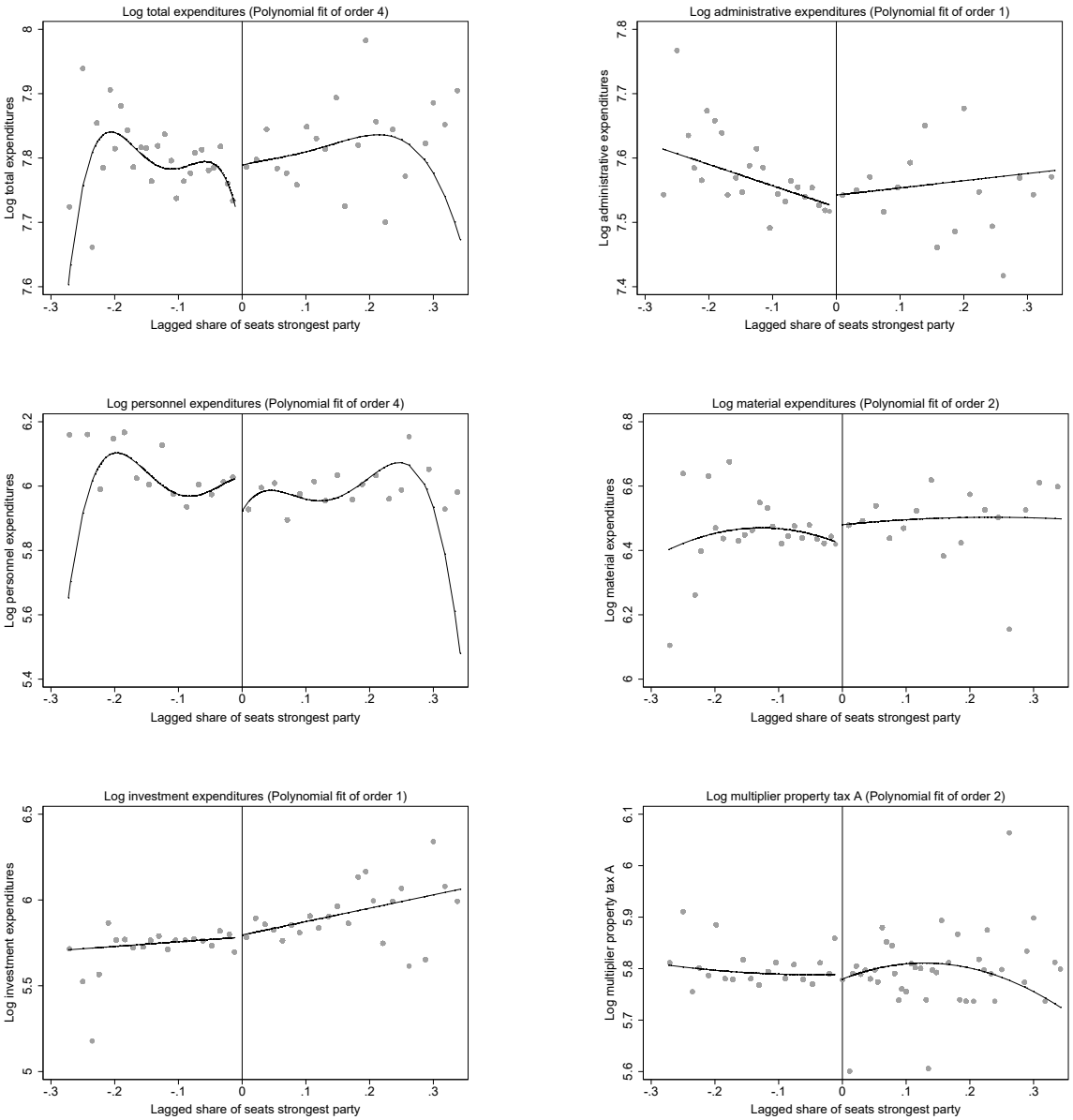
[23] The same approach is taken by Garmann (2012) and Artés Caselles and Jurado (2014).

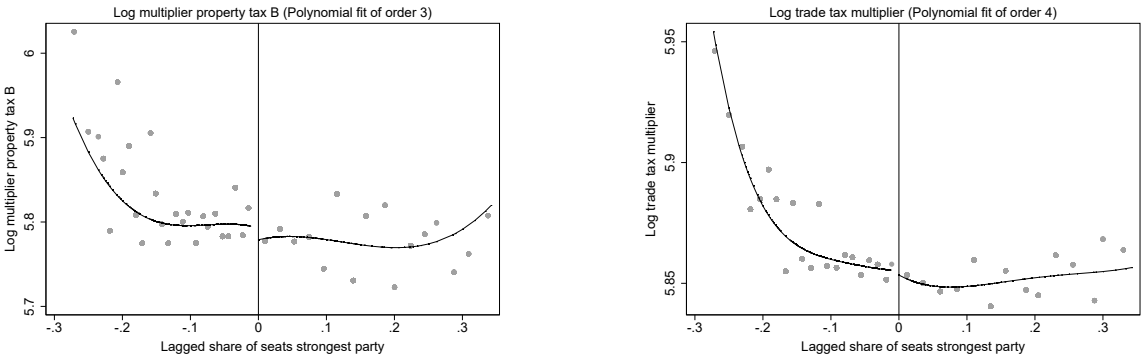
[24] This approach is suggested by Eggers et al. (2015).

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

[26] The optimal number of intervals 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.

Figure 1: Graphical representation of RDD





Notes: The figure illustrates presents the logarithm of the expenditure and tax variables, respectively, as a function of the lagged assignment variable - the normalized share of seats of the strongest party in the municipal council. Each point is a sample average of the outcome variable within an interval of the assignment variable. The optimal number of intervals on either side of the cutoff is determined by Calonico, Cattaneo, and Titiunik's 2014 evenly-spaced method using spacings estimators. Each sub-figure depicts a global polynomial fit, which is estimated separately by a rectangular kernel at either side of the threshold. The order of the polynomial fit is selected by the AIC. The horizontal axis is restricted to a band of +/- 30 percent around the normalized majority threshold. Source: Own Calculations based on data from the Baden-Württemberg State Statistical Office and the Federal Statistical Office of Germany.

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 municipal 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 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, which reveal a small negative, insignificant effect.

Column (2) to (5) show the results from the RDD. The order of the polynomial control function ranges from one in model (2) to four in model (5). The preferred estimate with the lowest AIC is marked in bold. 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 A, coalition governments affect three expenditure variables: firstly, total 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

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

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

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

this effect is economically relevant, the magnitude of the coefficient shrinks if instead polynomial specifications of lower order are used in the control function. 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) as well as 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. In line with Freier and Odendahl (2012), the remaining spending categories, personnel expenditures and investment expenditures, yield inconclusive results. Especially the estimates for investment spending are very imprecise due to the larger standard errors.

Turning to the tax multipliers, as reported in panel B, coalition governments, if any, have a small negative effect 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 and 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 fairly 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.[27] 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 becomes more consistent for different types of control functions and gains in significance compared to the FE estimations. The best point estimates implies that spending significantly decreases by 11.7 percent if a coalition government 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 as the coefficients either shrink to zero or turn positive. 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

[27] See 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: Expenditures				
Log total expenditures	-0.019	-0.043	-0.033	-0.124
	(0.023)	(0.031)	(0.046)	(0.064)*
Log administrative expenditures	-0.040	-0.051	-0.051	-0.117
	(0.021)*	(0.030)*	(0.046)	(0.063)*
Log personnel expenditures	-0.019	-0.049	-0.038	-0.069
	(0.028)	(0.034)	(0.048)	(0.067)
Log material expenditures	-0.032	-0.097	-0.032	-0.063
	(0.033)	(0.043)**	(0.065)	(0.092)
Log investment expenditures	0.018	-0.067	-0.016	-0.091
	(0.052)	(0.074)	(0.103)	(0.146)
Panel B: Tax multipliers				
Log multiplier property tax A	-0.002	0.014	0.010	0.006
	(0.019)	(0.025)	(0.037)	(0.055)
Log multiplier property tax B	-0.009	0.021	-0.018	-0.025
	(0.016)	(0.022)	(0.033)	(0.047)
Log trade tax multiplier	-0.006	0.005	-0.006	-0.014
	(0.005)	(0.007)	(0.010)	(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.

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

FE approach suggests that there is no indication for a misspecified functional form in the FE approach. To sum up, the cross-sectional analysis also provides no evidence for the government fragmentation hypothesis.

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? To answer this question, 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 - possibly because it contains many voluntary spending tasks. 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. The literature offers several explanations why the government fragmentation hypothesis might not hold.

On the one hand, principal-agent problem story of Pettersson-Lidbom (2012) seems to be convincing. 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. 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, the partial cost sharing approach of Primo and Snyder (2008) has some explanatory power since material spending contains several spending items were 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.

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.

Comparing the results with other studies suggests 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 indicates that neither the comparatively strong position of the mayor in in Baden-Württemberg nor the differing economic circumstances have an intervening effect on the fragmentation channel.

The external validity of the findings might be limited: firstly, 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. A party winning a slim majority may face an incentive to please voters via spending increases in order to improve its starting position for the next election.

Secondly, 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. However, coalition governments might work different if the mayor belongs to an opposing party.

Thirdly, the overall portion of left-wing governments - for both, coalition and single-party governments - is rather low in the sample.[28] 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, the common pool problem might be more severe at the federal level. Spending and taxes at the local level differ from higher tiers of government. 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.[29] On the other hand, the political system at the local level differs from higher tiers of government. Interests 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. 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 likely to be 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).[30] 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, it is assumed that individuals do exert at most imprecise control over the assignment variable. This

[28] See also the discussion on partisan effects in section 6.4.

[29] Federal 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.

[30] 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.

identifying assumption of no manipulation of the assignment variable cannot be tested directly since only one election per municipality per unit in time is observed (Lee and Lemieux 2010).

There seems to be no suggestive evidence for a precise manipulation of the assignment variable. On the one hand, precise manipulation of close elections is unlikely 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. 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 asses 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.

On the other hand, election rigging is hardly ever an issue in Western style democracies. Eggers et al. (2015) find no manipulation 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.

Furthermore, a manipulation of the relevant minimum cut-off value seems to be unlikely. 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. 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.[31] 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.

Lee and Lemieux (2010) provide a list of formal tests to check whether the identifying assumption is fulfilled.[32] 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 restricted sample that is used for the cross-sectional regressions, I test the null-hypothesis that treatment and control group are balanced. 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 (column 4). Due to omitted variable bias the null hypothesis is rejected for almost all covariates.[33] 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.

Second, given the RDD is correctly specified, predetermined baseline covariates, which are likely to affect the outcome variable Y, should evolve smoothly with respect to the assignment variable around the majority threshold. Intuitively the treatment should not affect baseline covariates that are determined prior to the realization of treatment. I formally test the null hypothesis of a zero ATE on baseline covariates for the FE model in table 7. I estimate equation 3 by using each of the observed baseline covariates as dependent variables. Panel A presents the

[31] All 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`.

[32] Further robustness checks are presented in section A2 in the Appendix.

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

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
N	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
N	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.

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.[34] 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. Furthermore, an increase in standard errors would put into question that the functional form is correctly specified (Lee and Lemieux 2010). Table 8 shows estimation results when controlling for the logarithm of population size, population density, employees, commuters, and council size.[35] Compared to the baseline specifications in table 3, 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 point estimates for material expenditures, as well as administrative and total spending remain the same and partially gain in 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. 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.[36] 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-sample. The median of 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.[37] For both sub-samples the null hypothesis of no discontinuity at the placebo-cutoff is not rejected at the five percent level for almost all fiscal outcome. In short, the lack of significant results from the placebo-cutoff implies that the estimation results from the baseline FE regression are valid.

[34] Using the baseline covariates as dependent variables in the cross-sectional approach yields similar results, as shown in table 14 in the Appendix. The null hypothesis of a zero ATE on the baseline covariates is not rejected at the 5 percent significance level, except for low order specifications of the population density and the share of young.

[35] The share of young and the share of old are not taken into account in table 8 since they are not available for the whole sample period.

[36] However, 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.

[37] For estimation I use observations on one side of the majority only. This ensures that I do not estimate a regression function where I expect 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.

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	0.000	-0.010	0.005
	(0.004)	(0.005)	(0.008)	(0.011)
Log (population density)	-0.002	-0.000	-0.010	0.005
	(0.004)	(0.005)	(0.008)	(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	-0.001	0.000
	(0.000)	(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	-0.004	-0.025	-0.026
	(0.010)*	(0.014)	(0.020)	(0.028)
Log (share of young)	0.005	0.001	0.012	0.010
	(0.008)	(0.012)	(0.018)	(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: 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: Expenditures				
Log total expenditures	-0.012	-0.034	-0.010	-0.061
	(0.012)	(0.017)*	(0.027)	(0.037)*
Log administrative expenditures	-0.015	-0.028	-0.003	-0.031
	(0.011)	(0.017)*	(0.025)	(0.034)
Log personnel expenditures	-0.008	0.007	-0.001	0.011
	(0.009)	(0.014)	(0.024)	(0.036)
Log material expenditures	-0.026	-0.063	-0.036	-0.031
	(0.018)	(0.026)**	(0.038)	(0.053)
Log investment expenditures	-0.018	-0.052	-0.063	0.013
	(0.037)	(0.053)	(0.077)	(0.103)
Panel B: Tax multipliers				
Log multiplier property tax A	-0.007	-0.001	0.001	-0.020
	(0.006)	(0.009)	(0.015)	(0.020)
Log multiplier property tax B	-0.005	0.006	-0.002	-0.021
	(0.007)	(0.010)	(0.017)	(0.024)
Log trade tax multiplier	-0.002	-0.003	-0.005	-0.014
	(0.002)	(0.003)	(0.005)	(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.

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

Table 9: Placebo-cutoff for coalition governments, using fixed effects

	RDD			
	(1) First Order	(2) Second Order	(3) Third Order	(4) Fourth Order
Panel A: Expenditures				
Log total expenditures	-0.014	-0.008	-0.002	-0.016
	(0.015)	(0.017)	(0.021)	(0.024)
Log administrative expenditures	-0.009	-0.007	-0.003	-0.010
	(0.015)	(0.017)	(0.020)	(0.023)
Log personnel expenditures	-0.003	-0.009	-0.017	-0.012
	(0.012)	(0.013)	(0.014)	(0.016)
Log material expenditures	-0.030	-0.026	-0.012	-0.007
	(0.022)	(0.023)	(0.026)	(0.030)
Log investment expenditures	-0.051	-0.042	-0.036	-0.015
	(0.045)	(0.047)	(0.056)	(0.066)
Panel B: Tax multipliers				
Log multiplier property tax A	0.007	0.005	-0.003	-0.008
	(0.009)	(0.010)	(0.011)	(0.013)
Log multiplier property tax B	0.010	0.005	-0.010	-0.015
	(0.012)	(0.013)	(0.015)	(0.017)
Log trade tax multiplier	-0.002	-0.002	-0.006	-0.008
	(0.004)	(0.004)	(0.004)	(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.

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

Table 10: Placebo-cutoff for single-party governments, using fixed effects

	RDD			
	(1) First Order	(2) Second Order	(3) Third Order	(4) Fourth Order
Panel A: Expenditures				
Log total expenditures	-0.025	-0.031	-0.063	-0.041
	(0.017)	(0.026)	(0.031)**	(0.033)
Log administrative expenditures	-0.016	-0.028	-0.036	-0.031
	(0.016)	(0.022)	(0.027)	(0.028)
Log personnel expenditures	0.005	-0.040	-0.058	-0.057
	(0.016)	(0.022)*	(0.031)*	(0.033)*
Log material expenditures	-0.016	-0.023	-0.032	-0.019
	(0.022)	(0.031)	(0.041)	(0.042)
Log investment expenditures	-0.002	-0.047	-0.128	-0.065
	(0.067)	(0.096)	(0.110)	(0.110)
Panel B: Tax multipliers				
Log multiplier property tax A	-0.007	-0.006	-0.006	-0.006
	(0.009)	(0.011)	(0.012)	(0.012)
Log multiplier property tax B	-0.009	-0.007	-0.003	0.001
	(0.012)	(0.016)	(0.016)	(0.017)
Log trade tax multiplier	-0.000	-0.001	0.001	0.005
	(0.004)	(0.006)	(0.007)	(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.

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

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 and exhibit higher taxes than single-party governments. Yet, the empirical evidence is mixed. 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 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.

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.

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Appendix

A1: Additional Tables and Graphs

Table 11: Definition and source of variables

Label	Period	Description	Source
Total expenditures	1994 - 2014	Real total expenditures (Bruttoausgaben der Gemeinden) per capita (deflated by federal CPI, 2010 = 100).	Baden-Wuerttemberg State Statistical Office
Administrative expenditures	1994 - 2014	Real total spending on public administration (Verwaltungshaushalt insgesamt) per capita (deflated by federal CPI, 2010 = 100).	Baden-Wuerttemberg State Statistical Office
Personnel expenditures	1994 - 2014	Real personnel expenditures (Personalausgaben) per capita (deflated by federal CPI, 2010 = 100).	Baden-Wuerttemberg State Statistical Office
Material expenditures	1994 - 2014	Real material expenditures (laufender Sachaufwand) per capita (deflated by federal CPI, 2010 = 100).	Baden-Wuerttemberg State Statistical Office
Investment expenditures	1994 - 2014	Real investment expenditures (Ausgaben fuer Sachinvestitionen) per capita (deflated by federal CPI, 2010 = 100).	Baden-Wuerttemberg State Statistical Office
Multiplier property tax A	1994 - 2014	Property tax multiplier (Hebesatz Grundsteuer A) 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 (Hebesatz Grundsteuer B) 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 (Gewerbesteuerhebesatz) 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 (Auspendler) per capita.	German Federal Employment Agency
Population density	1994 - 2013	Population density.	Own construction based on data from the German Federal Statistical Office and the Baden-Wuerttemberg State Statistical Office

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Label	Period	Description	Source
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 (unechte Teilortswahl).	Baden-Wuerttemberg State Statistical Office
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						
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
	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

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Variable		Mean	Std.dev.	Min.	Max.	N
	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

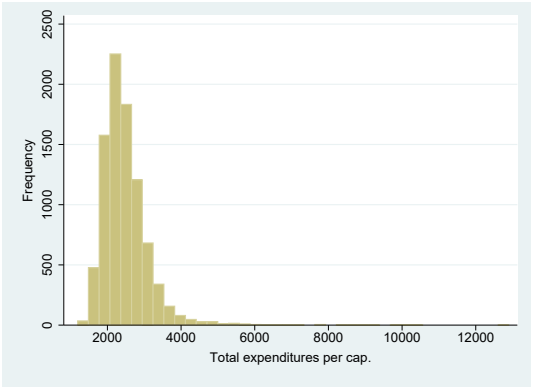
Table 13: Matrix of the four possible treatment types

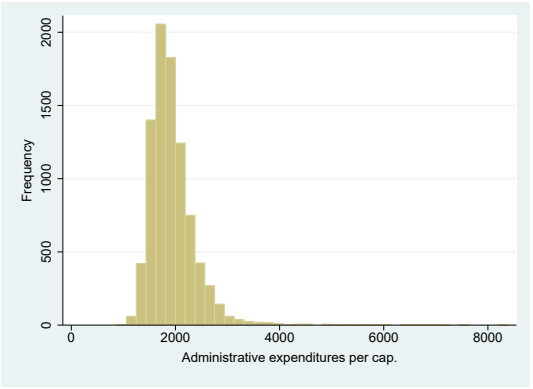
		Proportion of seats ≥ 50 %, $Z_i = 0$	
		Single-Party Gov. $D_i = 0$	Coalition Gov. $D_i = 1$
Proportion of seats < 50 %, $Z_i = 1$	Single-Party Gov. $D_i = 0$	Never-Takers (Minority Government)	Defiers
	Coalition Gov. $D_i = 1$	Compliers (Minimal Winning Coalition)	Always-takers (Oversized Coalition)

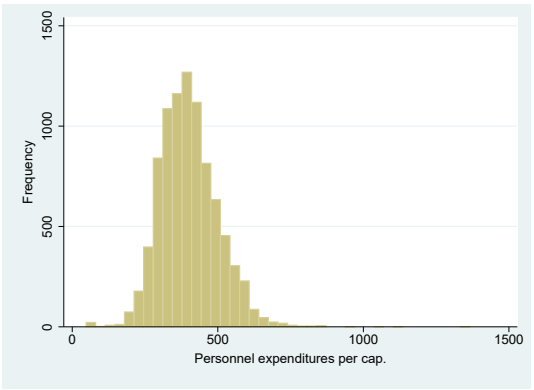
Notes: D_i is a treatment dummy equal to one if the tpye of government is a coalition. Z_i is dummy equal to one if individuals are assigned to the treatment group, that is to say that the largest party's proportion of the seats is smaller than 50 %.

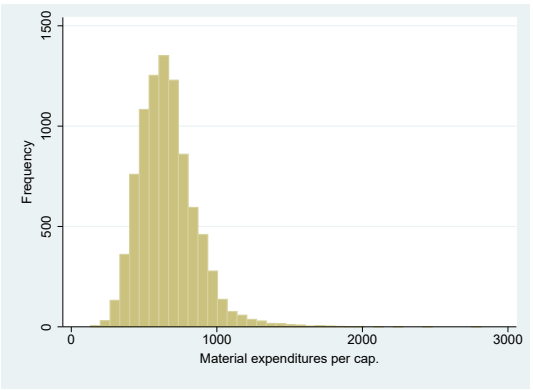
Source: author's own compilation, design based on Angrist and Pischke (2015).

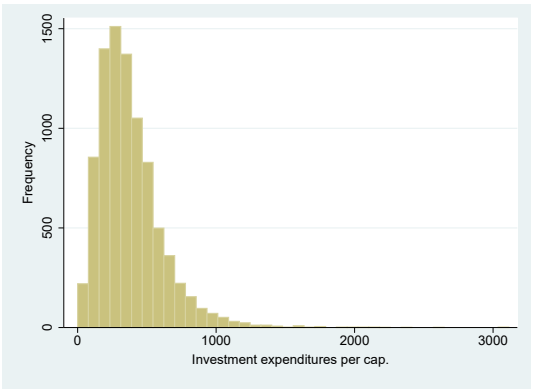
Figure 2: Histograms of dependent variables

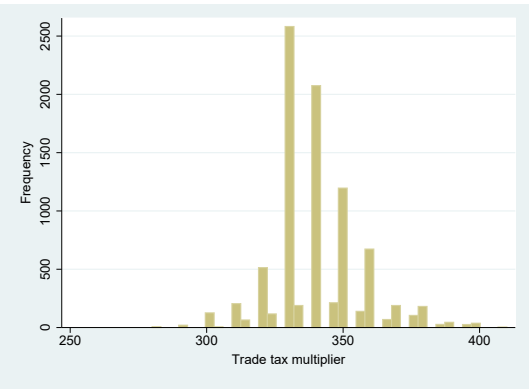


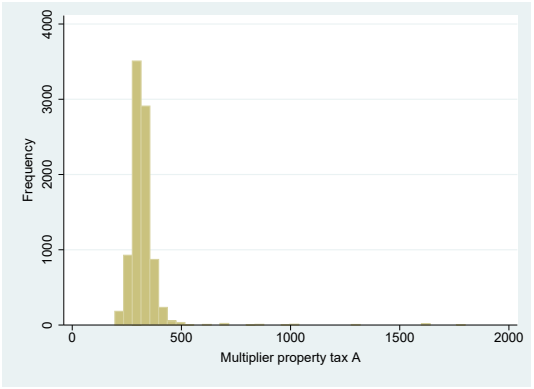












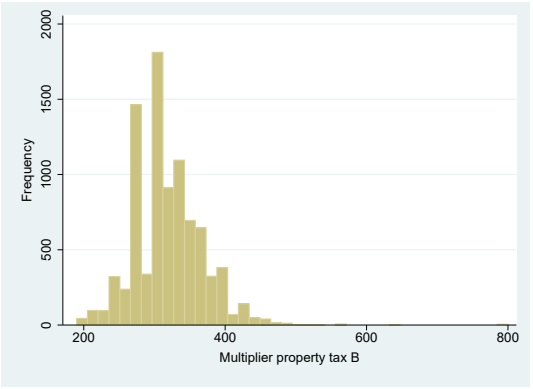
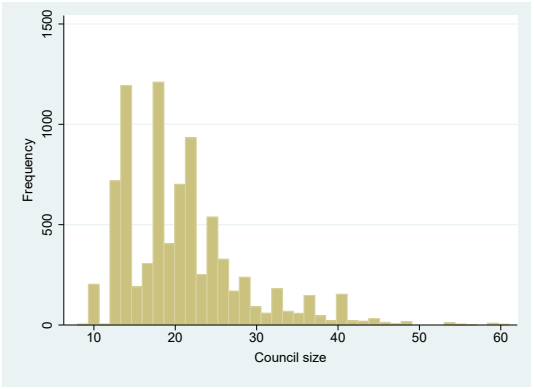
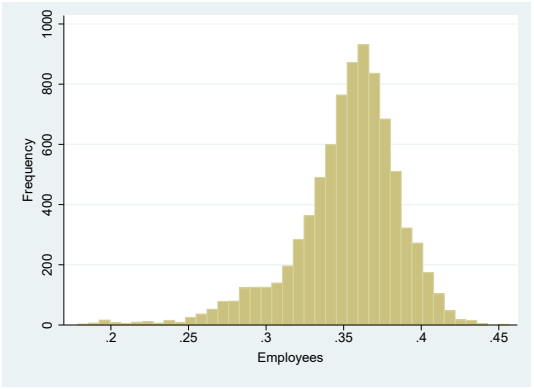
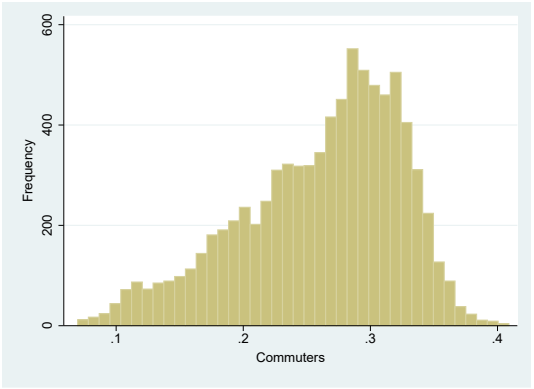
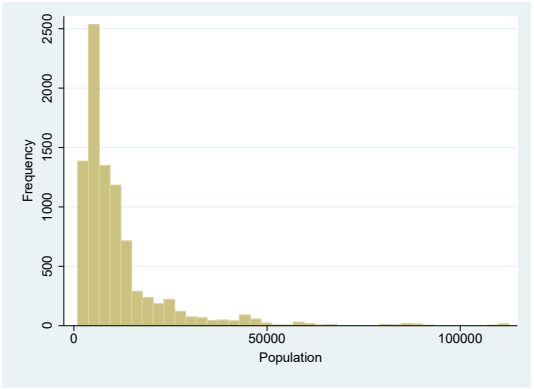


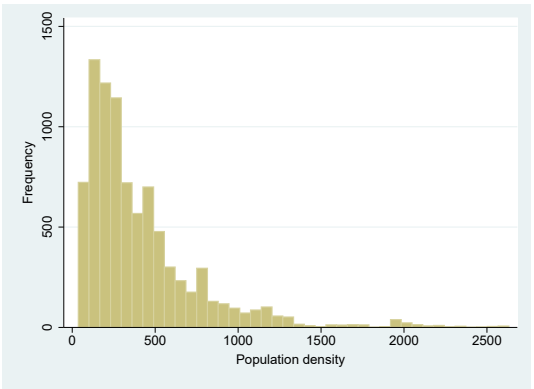
Figure 3: Histograms of baseline covariates

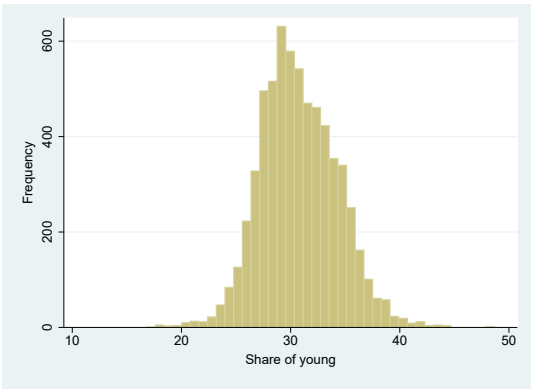












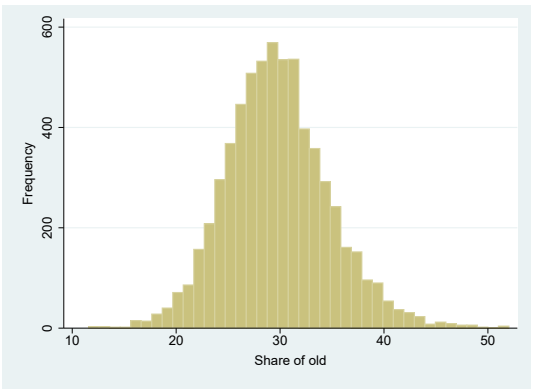


Table 14: Balance test for baseline covariates: RDD estimations results using cross sectional variation, restricted sample

	RDD			
	(1) First Order	(2) Second Order	(3) Third Order	(4) Fourth Order
Panel A: Standard controls				
Log (council size)	-0.019	0.004	0.014	0.018
	(0.020)	(0.027)	(0.039)	(0.054)
Log (employees)	0.000	-0.001	-0.000	-0.001
	(0.000)	(0.001)	(0.001)	(0.001)
Log (commuters)	0.000	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.002)	(0.002)
Log (population)	-0.097	0.038	0.127	0.134
	(0.078)	(0.102)	(0.149)	(0.207)
Log (population density)	-0.203	-0.276	-0.221	-0.172
	(0.092)**	(0.116)**	(0.168)	(0.230)
Number of clusters	539	539	539	539
Observation	4619	4619	4619	4619
Panel B: Additional controls				
Log (share of old)	-0.020	-0.035	0.018	0.010
	(0.016)	(0.020)*	(0.028)	(0.042)
Log (share of young)	0.020	0.028	0.015	0.014
	(0.010)**	(0.014)**	(0.021)	(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: 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: Expenditures				
Log total expenditures	-0.019	-0.035	-0.021	-0.088
	(0.021)	(0.028)	(0.041)	(0.057)
Log administrative expenditures	-0.036	-0.039	-0.036	-0.075
	(0.020)*	(0.027)	(0.041)	(0.056)
Log personnel expenditures	-0.015	-0.044	-0.028	-0.027
	(0.026)	(0.033)	(0.046)	(0.064)
Log material expenditures	-0.036	-0.097	-0.030	-0.046
	(0.033)	(0.043)**	(0.065)	(0.091)
Log investment expenditures	-0.005	-0.079	-0.025	-0.066
	(0.048)	(0.069)	(0.098)	(0.140)
Panel B: Tax multipliers				
Log multiplier property tax A	-0.021	-0.014	-0.013	-0.010
	(0.019)	(0.025)	(0.035)	(0.052)
Log multiplier property tax B	-0.014	0.008	-0.028	-0.029
	(0.016)	(0.022)	(0.032)	(0.045)
Log trade tax multiplier	-0.006	0.005	-0.006	-0.011
	(0.005)	(0.007)	(0.010)	(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.

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

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

	RDD			
	(1) First Order	(2) Second Order	(3) Third Order	(4) Fourth Order
Panel A: Expenditures				
Log total expenditures	-0.014	-0.031	-0.009	-0.035
	(0.011)	(0.017)*	(0.026)	(0.036)
Log administrative expenditures	-0.019	-0.026	-0.017	-0.019
	(0.012)	(0.017)	(0.025)	(0.036)
Log personnel expenditures	-0.012	0.007	-0.001	0.023
	(0.010)	(0.015)	(0.024)	(0.034)
Log material expenditures	-0.021	-0.059	-0.021	-0.034
	(0.017)	(0.024)**	(0.036)	(0.050)
Log investment expenditures	-0.002	-0.056	-0.037	-0.018
	(0.036)	(0.050)	(0.073)	(0.101)
Panel B: Tax multipliers				
Log multiplier property tax A	-0.013	-0.000	-0.013	-0.018
	(0.008)*	(0.011)	(0.017)	(0.024)
Log multiplier property tax B	-0.014	0.005	-0.021	-0.001
	(0.010)	(0.014)	(0.021)	(0.031)
Log trade tax multiplier	-0.003	-0.002	-0.007	-0.013
	(0.003)	(0.004)	(0.006)	(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.

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

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

	OLS	RDD			
	(1)	(2)	(3)	(4)	(5)
	-	First Order	Second Order	Third Order	Fourth Order
Panel A: Expenditures					
Log total expenditures	0.007	-0.011	-0.011	-0.021	-0.024
	(0.010)	(0.012)	(0.012)	(0.013)	(0.014)*
Log administrative expenditures	0.006	-0.017	-0.017	-0.019	-0.022
	(0.010)	(0.012)	(0.012)	(0.013)	(0.014)
Log personnel expenditures	0.010	-0.005	-0.005	0.011	0.009
	(0.009)	(0.010)	(0.010)	(0.012)	(0.012)
Log material expenditures	-0.021	-0.025	-0.025	-0.036	-0.044
	(0.014)	(0.016)	(0.016)	(0.018)**	(0.019)**
Log investment expenditures	0.004	0.007	0.007	-0.001	-0.013
	(0.032)	(0.040)	(0.040)	(0.047)	(0.045)
Panel B: Tax multipliers					
Log multiplier property tax A	0.006	-0.009	-0.009	-0.004	-0.003
	(0.006)	(0.007)	(0.007)	(0.008)	(0.009)
Log multiplier property tax B	0.009	-0.009	-0.009	-0.002	0.001
	(0.008)	(0.010)	(0.010)	(0.011)	(0.011)
Log trade tax multiplier	0.001	-0.004	-0.004	-0.003	-0.003
	(0.002)	(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.

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

Table 18: 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: Expenditures								
Log total expenditures	-0.017	-0.015	-0.033	-0.034	-0.013	-0.015	-0.066	-0.058
	(0.013)	(0.012)	(0.019)*	(0.018)*	(0.031)	(0.029)	(0.043)	(0.039)
Log administrative expenditures	-0.020	-0.021	-0.023	-0.028	-0.014	-0.017	-0.037	-0.025
	(0.012)	(0.013)*	(0.018)	(0.018)	(0.028)	(0.028)	(0.039)	(0.038)
Log personnel expenditures	-0.011	-0.009	0.001	0.009	-0.015	-0.003	0.003	0.018
	(0.009)	(0.010)	(0.013)	(0.015)	(0.019)	(0.024)	(0.027)	(0.036)
Log material expenditures	-0.031	-0.027	-0.048	-0.063	-0.026	-0.038	-0.018	-0.030
	(0.017)*	(0.018)	(0.025)*	(0.026)**	(0.036)	(0.038)	(0.051)	(0.053)
Log investment expenditures	-0.027	-0.001	-0.114	-0.044	-0.098	-0.018	-0.028	0.011
	(0.039)	(0.039)	(0.055)**	(0.054)	(0.083)	(0.078)	(0.114)	(0.105)
Panel B: Tax multipliers								
Log multiplier property tax A	-0.012	-0.014	-0.001	-0.002	-0.006	-0.014	-0.006	-0.017
	(0.008)	(0.008)*	(0.012)	(0.012)	(0.018)	(0.018)	(0.024)	(0.026)
Log multiplier property tax B	-0.014	-0.015	0.011	0.004	-0.003	-0.024	0.004	-0.015
	(0.009)	(0.010)	(0.013)	(0.015)	(0.020)	(0.023)	(0.028)	(0.035)
Log trade tax multiplier	-0.005	-0.005	-0.002	-0.004	-0.003	-0.011	-0.005	-0.012
	(0.003)	(0.003)	(0.004)	(0.004)	(0.006)	(0.006)*	(0.008)	(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. 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 might be 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.

Table 19: 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: Expenditures				
Log total expenditures	-0.011	-0.035	-0.016	-0.061
	(0.012)	(0.018)*	(0.029)	(0.040)
Log administrative expenditures	-0.017	-0.030	-0.018	-0.027
	(0.013)	(0.019)	(0.029)	(0.039)
Log personnel expenditures	-0.004	0.008	-0.004	0.015
	(0.010)	(0.016)	(0.024)	(0.036)
Log material expenditures	-0.025	-0.064	-0.038	-0.032
	(0.018)	(0.026)**	(0.038)	(0.053)
Log investment expenditures	0.005	-0.047	-0.020	0.006
	(0.039)	(0.055)	(0.079)	(0.107)
Panel B: Tax multipliers				
Log multiplier property tax A	-0.011	-0.003	-0.015	-0.018
	(0.008)	(0.012)	(0.018)	(0.026)
Log multiplier property tax B	-0.010	0.002	-0.025	-0.018
	(0.010)	(0.015)	(0.023)	(0.035)
Log trade tax multiplier	-0.004	-0.004	-0.011	-0.013
	(0.003)	(0.004)	(0.006)*	(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.

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

However, two arguments speak against this concern: On the one hand, right-wing gov-

ernments 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 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 originate from the municipality where they run for office. Furthermore, mayors are expected to act in a non-partisan manner seeking the best interest for the municipality (Wehling 2012).

To check whether mayors without party affiliation affect estimation results, I build subsamples 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 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 loose its significance. The party 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, 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: 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: Expenditures								
Log total expenditures	-0.018	-0.007	-0.031	-0.036	0.008	-0.002	-0.105	0.022
	(0.017)	(0.018)	(0.025)	(0.024)	(0.046)	(0.029)	(0.055)*	(0.040)
Log administrative expenditures	-0.019	-0.021	-0.021	-0.037	0.005	-0.006	-0.050	0.034
	(0.017)	(0.019)	(0.026)	(0.024)	(0.044)	(0.030)	(0.058)	(0.044)
Log personnel expenditures	-0.013	-0.010	-0.004	0.021	-0.054	0.045	-0.053	0.060
	(0.015)	(0.014)	(0.022)	(0.021)	(0.032)*	(0.032)	(0.046)	(0.046)
Log material expenditures	-0.016	-0.046	-0.066	-0.073	0.001	-0.066	-0.039	0.008
	(0.021)	(0.028)	(0.031)**	(0.038)*	(0.046)	(0.052)	(0.062)	(0.072)
Log investment expenditures	-0.033	0.020	-0.066	-0.039	-0.053	0.028	-0.120	0.098
	(0.057)	(0.059)	(0.083)	(0.076)	(0.123)	(0.107)	(0.170)	(0.145)
Panel B: Tax multipliers								
Log multiplier property tax A	-0.014	-0.005	0.012	0.007	-0.000	-0.000	0.015	-0.018
	(0.010)	(0.012)	(0.013)	(0.017)	(0.021)	(0.024)	(0.032)	(0.032)
Log multiplier property tax B	-0.012	-0.008	0.028	0.001	-0.006	0.002	0.006	0.006
	(0.013)	(0.015)	(0.017)	(0.020)	(0.028)	(0.027)	(0.042)	(0.041)
Log trade tax multiplier	-0.005	-0.001	0.004	-0.005	-0.004	-0.006	-0.008	-0.001
	(0.004)	(0.005)	(0.005)	(0.006)	(0.008)	(0.008)	(0.011)	(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 total expenditures, as well as administrative expenditures and material expenditures. Especially 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.

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: Expenditures				
Log total expenditures	-0.003	-0.034	-0.004	-0.058
	(0.011)	(0.017)**	(0.028)	(0.038)
Log administrative expenditures	-0.003	-0.027	0.002	-0.025
	(0.010)	(0.016)*	(0.025)	(0.033)
Log personnel expenditures	0.003	0.003	0.003	0.009
	(0.009)	(0.014)	(0.023)	(0.034)
Log material expenditures	-0.021	-0.063	-0.031	-0.035
	(0.018)	(0.026)**	(0.038)	(0.053)
Log investment expenditures	-0.016	-0.063	-0.052	0.002
	(0.035)	(0.050)	(0.074)	(0.098)
Panel B: Tax multipliers				
Log multiplier property tax A	-0.003	-0.000	-0.000	-0.016
	(0.006)	(0.009)	(0.015)	(0.020)
Log multiplier property tax B	0.002	0.007	-0.003	-0.014
	(0.006)	(0.010)	(0.017)	(0.026)
Log trade tax multiplier	-0.000	-0.003	-0.006	-0.012
	(0.002)	(0.003)	(0.005)	(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.

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

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 A 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 B 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 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 1^{st} to 99^{th} percentiles of the outcome variable. Compared to table 17 the signs of the effects do not change. However, eliminating outliers 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. However, it remains insignificant. Therefore I conclude that the rejection of the government fragmentation hypothesis is not driven by outliers.

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.

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

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

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

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.[38] Thus, the suburb election system could systematically increase the number of seats required to win a 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.[39] In table 18 in the Appendix I test whether the presence of suburb election system affects outcomes for the 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 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 seats. Assume that the strongest party lacks one seat to the absolute majority in both municipalities. Than 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,

[38] Other 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`.

[39] For more information on this see `http://www.wahlrecht.de/kommunal/baden-wuerttemberg.htm`

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.

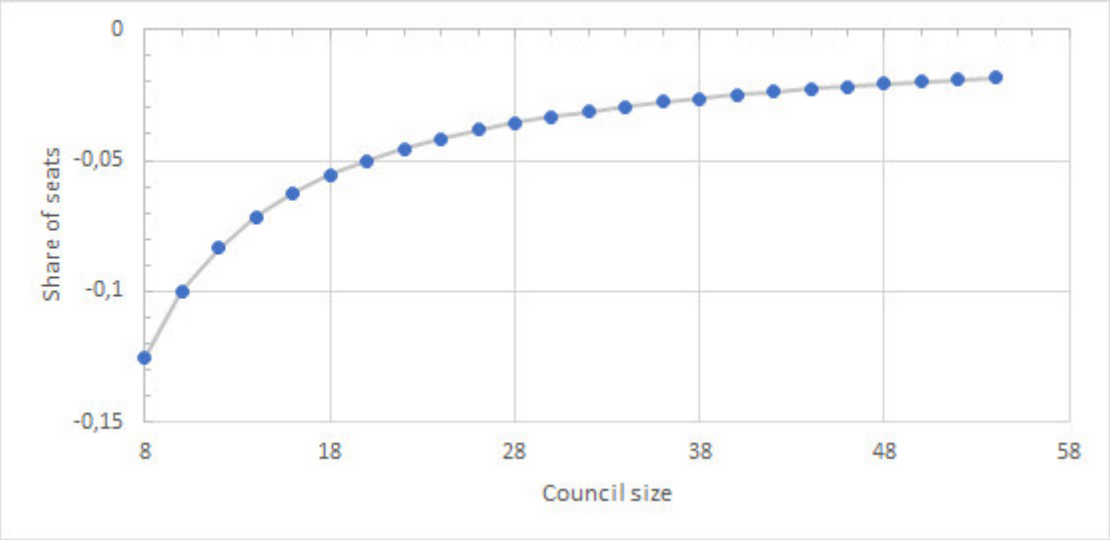
There are two ways to circumvent this sample selection bias: Firstly, one could apply a fixed effects approach, where the treatment effect is estimated by using only the variation within municipalities, as done in the main section. By design, the degree of closeness does not differ within the unit of 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.

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
N	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.

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

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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 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 nicht die Steuersätze. Koalitionsregierungen haben einen nicht-robusten, negativen Effekt auf die Bruttoausgaben, der im Wesentlichen von den Verwaltungsausgaben und dem Laufenden Sachaufwand getrieben ist.

Schlagworte: 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 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. 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

CPI consumer price index

FDP Free Democratic Party

FE fixed effects

FWG Free Voters Association

ITT intention-to-treat effect

NRW North Rhine-Westphalia

OLS ordinary least squares

RDD regression discontinuity design

RKD regression kink design

SPD Social Democratic Party

TOT treatment-on-the-treated 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 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 expenditures, but also on various sub-categories of spending. This 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 to previous analyses.

I contribute 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, I shed light on the question whether the effect of government fragmentation depends on the political regime employed at the local level. As one of the first I test the government fragmentation hypothesis in the context of a mayor-council system. Thirdly, I give insights to what extent the fragmentation effect found for German municipalities depends on socio-economic factors. Finally, I offer a careful sensitivity analysis to check for the robustness of the results.

The empirical analysis shows that coalition governments do not increase 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) on the German case.

The remainder of the paper is organized as follows: Section 2 discusses the theoretical framework. Section 3 reviews the empirical literature. Section 4 describes the institutional background and the data set. Section 5 sets out 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 they make 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 are expected to 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-constrainted 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. If only a part of the costs is financed out of a common pool, the remaining costs 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 bargaining 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).

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

[2] In 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.

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

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.

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 institutional framework and are subjected to the same voting rules 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.

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 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 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.

In sum, the evidence on the effect of government fragmentation is inconclusive.

[4] For 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.

4 Institutional Framework and Data

4.1 German Municipalities

Although 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. 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).[5] Municipalities are required to balance their budget each year. Municipalities have to fulfill a range of duties, which can be divided into two groups according to the degree of discretion: firstly, mandated spending tasks, which the state or federal government delegates to municipalities with 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).[6] 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 tasks.

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.[7] As form of government, all municipalities in Baden-Württemberg apply a mayor-council system. Accordingly, the municipal affairs are managed jointly by the municipal council and the directly elected mayor. The municipal council, as the main legislative body, decides on all issues concerning the municipality, except for those which lie within the competence of the mayor.[8] 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.[9] The number of seats in the council ranges from 8 to 60 seats population size.[10] 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.

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 (SPD); the culturally conservative, moderately market-oriented Christian Democratic Union (CDU); and the Free Voters Association (FWG), a conservative party that operates mainly 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,

[5] Aside 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.

[6] For both groups of spending, municipalities hold planning sovereignty and bear financial, organizational and staff responsibility (Grunow 2012).

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

[8] The municipal council exercises municipal planning competence, has the right to demand decision-relevant information from the local administration and lays down the guidelines for the municipal administration.

[9] The election rules allow to select applicants from other party list (Panaschieren) and assign up to three votes to one applicant (Kumulieren).

[10] The 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).

win local elections in larger cities.

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.[11] The data set covers 4 legislative terms: 1994-1998, 1999-2003, 2004-2008, and 2009-2013. I use the following outcome variables: 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. Municipal tax variables are taken from the German Federal Statistical Office. The multipliers for the three municipal tax rates are expressed in percentage points. To make observations comparable across years and legal entities I express all 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 outflows 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.[12] Table 12 in the Appendix provides summary statistics for all variables.

To correct for right-skewness in the distribution of variables, I apply a logarithmic transformation to the outcome and control variables.[13] Thus the treatment effect can be interpreted as expected percentage change in the outcome variable.

For estimation all right-hand side variables are lagged by one year. 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

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

[12] See also table 11 in the Appendix .

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

imperfect compliance. Imperfect compliance might arise out of two reasons: On the one hand, political parties might avoid building a coalition and opt for a minority government if they fail to win an absolute majority (never-takers).[14] 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, political parties might always prefer to coalesce, even if they have an absolute majority of the seats (always-takers). In the absence of crisis, however, it is rational to build a minimal winning coalition (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):

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

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.

I consider only those observations where the mayor either belongs to the largest party in the council or has no party affiliation.[15] This permits me to isolate the fragmentation effect and to abstract from partisan conflicts between the council and the mayor.[16] 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.[17] 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 since 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 in terms of 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 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 59 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

[14] For a graphical illustration of treatment types see table 13 in the Appendix . For a more detailed discussion of the potential problems of the proxy see Garmann (2014).

[15] Since 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`.

[16] Freier and Odendahl (2012) follow a similar approach.

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

Table 1: Type of government by legislature period

	Type of Government			
Election Period	Single-party	Coalition	Fraction of coalition gov.	Total
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: The 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.

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 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.

The data set has several advantageous characteristics: firstly, the data set does not only cover total expenditures, but also its sub-categories. This allows me to analyze the common pool problem in greater detail 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 for the respective time frame.[18] Finally, in many German states there was a major reform of 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.

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:

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

where the assignment variable, x_i, is the share of seats of the strongest party in the council. The

[18] Municipalities in Baden-Württemberg are forced to change from cameralistic to double-entry provisions not before 2020.

Table 2: Type of government by different categorical variables

	Type of government							
	Single-party			Coalition			Total	
	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: The 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.

threshold, x_0, is located 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 ($x < x_0$).[19]

The RDD gives an average treatment effect (ATE) at the threshold, x_0 (Lee and Lemieux 2010). 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 fiscal outcome variable, Y_i, at the threshold, x_0:

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

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. If the identifying assumption holds, both, observations slightly below and above the 50 percent threshold, should have similar characteristics except for the treatment. Thus the latter serve as valid counterfactual for the former.

I use a parametric approach to identify the treatment effect since the assignment variable is rather discrete.[20] To estimate the treatment effect at the threshold, the parametric approach relies on regressions. For extrapolation one exploits the whole range of observations, even those far away from the cut-off point, and assumes a specific functional relation between the outcome and assignment variable. The parametric apporach is formalized by the following fixed effects regression equation:

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

where Y_{it} is the fiscal outcome variable for observation i at time t. ρ indicates the treatment effect at the threshold. 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, $f(.)$, is included, that is flexible on either side of the threshold and traces the relationship between the normalized assignment variable, $\tilde{x}_{it}$, and the outcome variable, Y_{it}.[21] 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(Lee and Lemieux 2010). To account for potential autocorrelation of the error term, ε_{it}, 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, $f(.)$. If it is misspecified, the estimated treatment effect is biased (Angrist and Pischke 2015). Therefore I test different models with polynomial control functions 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 the 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, which rises with the parameters used in the model. On the other hand, it rewards the goodness of fit, which is likely to grow with an increasing number of parameters.

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 the average council size is smaller in the control group, as shown in the second panel of table 2. 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

[19] I consider only those cases in which the mayor belongs to the strongest party. For these cases holding 50 percent of the seats ensures an absolute majority.

[20] Lee and Lemieux (2010) suggest to follow this approach in case of a discrete assignment variable.

[21] To ease interpretation the assignment variable, $\tilde{x}_{it}$, is centered at the threshold, x_0. The normalized assignment variable is given as: $\tilde{x}_{it} \equiv x_{it} - x_0$.

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 becomes negligible if the council size exceeds 18 seats.[22] There a 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.[23] The comparison within units ensures that treatment and control group are balanced with respect to time-invariant covariates independently of sample size. However, some municipalities changed their council size during the sample period. Thus the degree of closeness does not remain the same for all municipalities. Therefore, I control for the size of the municipal council. An additional advantage of the fixed effects (FE) approach is that it might reduce sampling variance and hence might be more efficient than a cross-sectional approach (Lee and Lemieux 2010). Secondly, as a robustness check, I use the cross-sectional variation and exclude those observations close to the threshold, for which the imbalance exists.[24] 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 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.[25] Each point is a sample average of the outcome variable within an interval of the assignment variable.[26] 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. 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 expenditures and taxes, respectively, the regression function should display a downward jump at the majority threshold.

The expenditure variables present a mixed picture: 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.

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.

[22] For a more detailed discussion see section A2 in the Appendix.

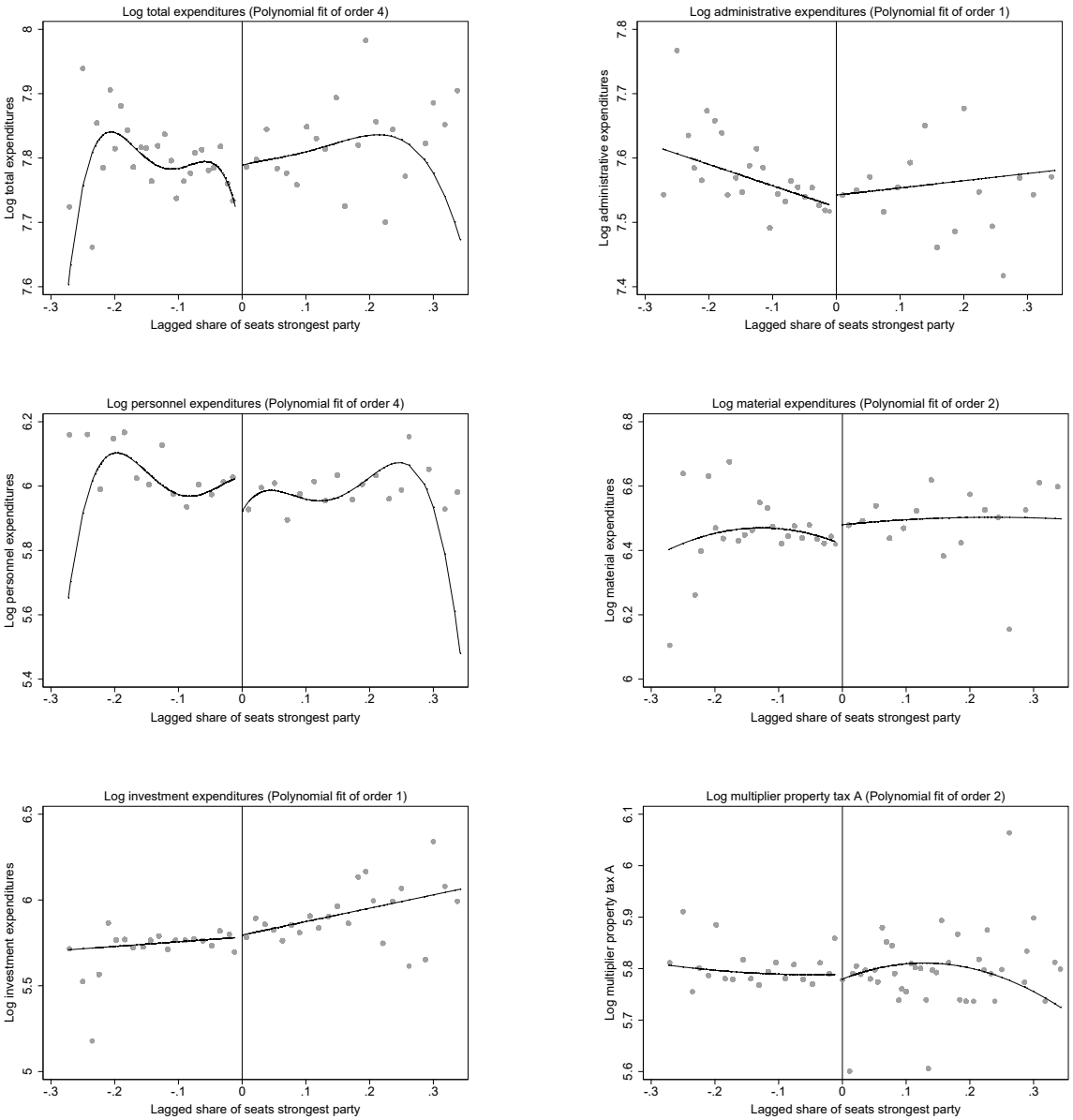
[23] The same approach is taken by Garmann (2012) and Artés Caselles and Jurado (2014).

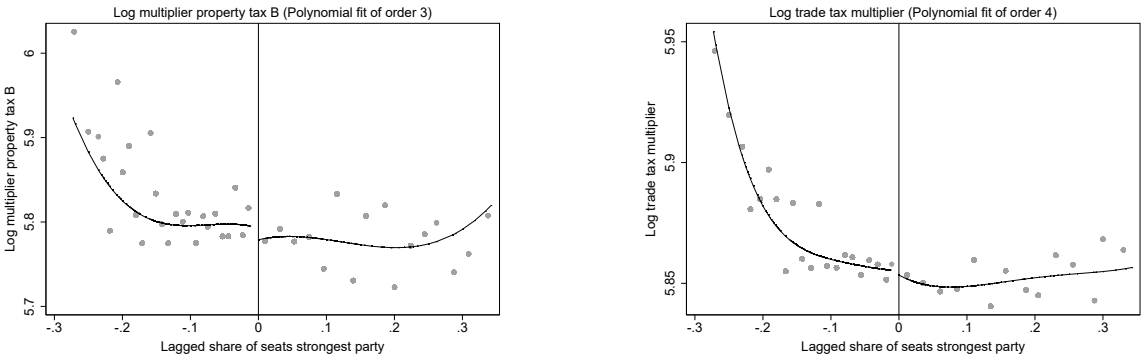
[24] This approach is suggested by Eggers et al. (2015).

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

[26] The optimal number of intervals 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.

Figure 1: Graphical representation of RDD





Notes: The figure illustrates presents the logarithm of the expenditure and tax variables, respectively, as a function of the lagged assignment variable - the normalized share of seats of the strongest party in the municipal council. Each point is a sample average of the outcome variable within an interval of the assignment variable. The optimal number of intervals on either side of the cutoff is determined by Calonico, Cattaneo, and Titiunik's 2014 evenly-spaced method using spacings estimators. Each sub-figure depicts a global polynomial fit, which is estimated separately by a rectangular kernel at either side of the threshold. The order of the polynomial fit is selected by the AIC. The horizontal axis is restricted to a band of +/- 30 percent around the normalized majority threshold. Source: Own Calculations based on data from the Baden-Württemberg State Statistical Office and the Federal Statistical Office of Germany.

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 municipal 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 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, which reveal a small negative, insignificant effect.

Column (2) to (5) show the results from the RDD. The order of the polynomial control function ranges from one in model (2) to four in model (5). The preferred estimate with the lowest AIC is marked in bold. 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 A, coalition governments affect three expenditure variables: firstly, total 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

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

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

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

this effect is economically relevant, the magnitude of the coefficient shrinks if instead polynomial specifications of lower order are used in the control function. 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) as well as 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. In line with Freier and Odendahl (2012), the remaining spending categories, personnel expenditures and investment expenditures, yield inconclusive results. Especially the estimates for investment spending are very imprecise due to the larger standard errors.

Turning to the tax multipliers, as reported in panel B, coalition governments, if any, have a small negative effect 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 and 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 fairly 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.[27] 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 becomes more consistent for different types of control functions and gains in significance compared to the FE estimations. The best point estimates implies that spending significantly decreases by 11.7 percent if a coalition government 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 as the coefficients either shrink to zero or turn positive. 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

[27] See 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: Expenditures				
Log total expenditures	-0.019	-0.043	-0.033	-0.124
	(0.023)	(0.031)	(0.046)	(0.064)*
Log administrative expenditures	-0.040	-0.051	-0.051	-0.117
	(0.021)*	(0.030)*	(0.046)	(0.063)*
Log personnel expenditures	-0.019	-0.049	-0.038	-0.069
	(0.028)	(0.034)	(0.048)	(0.067)
Log material expenditures	-0.032	-0.097	-0.032	-0.063
	(0.033)	(0.043)**	(0.065)	(0.092)
Log investment expenditures	0.018	-0.067	-0.016	-0.091
	(0.052)	(0.074)	(0.103)	(0.146)
Panel B: Tax multipliers				
Log multiplier property tax A	-0.002	0.014	0.010	0.006
	(0.019)	(0.025)	(0.037)	(0.055)
Log multiplier property tax B	-0.009	0.021	-0.018	-0.025
	(0.016)	(0.022)	(0.033)	(0.047)
Log trade tax multiplier	-0.006	0.005	-0.006	-0.014
	(0.005)	(0.007)	(0.010)	(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.

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

FE approach suggests that there is no indication for a misspecified functional form in the FE approach. To sum up, the cross-sectional analysis also provides no evidence for the government fragmentation hypothesis.

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? To answer this question, 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 - possibly because it contains many voluntary spending tasks. 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. The literature offers several explanations why the government fragmentation hypothesis might not hold.

On the one hand, principal-agent problem story of Pettersson-Lidbom (2012) seems to be convincing. 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. 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, the partial cost sharing approach of Primo and Snyder (2008) has some explanatory power since material spending contains several spending items were 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.

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.

Comparing the results with other studies suggests 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 indicates that neither the comparatively strong position of the mayor in in Baden-Württemberg nor the differing economic circumstances have an intervening effect on the fragmentation channel.

The external validity of the findings might be limited: firstly, 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. A party winning a slim majority may face an incentive to please voters via spending increases in order to improve its starting position for the next election.

Secondly, 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. However, coalition governments might work different if the mayor belongs to an opposing party.

Thirdly, the overall portion of left-wing governments - for both, coalition and single-party governments - is rather low in the sample.[28] 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, the common pool problem might be more severe at the federal level. Spending and taxes at the local level differ from higher tiers of government. 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.[29] On the other hand, the political system at the local level differs from higher tiers of government. Interests 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. 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 likely to be 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).[30] 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, it is assumed that individuals do exert at most imprecise control over the assignment variable. This

[28] See also the discussion on partisan effects in section 6.4.

[29] Federal 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.

[30] 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.

identifying assumption of no manipulation of the assignment variable cannot be tested directly since only one election per municipality per unit in time is observed (Lee and Lemieux 2010).

There seems to be no suggestive evidence for a precise manipulation of the assignment variable. On the one hand, precise manipulation of close elections is unlikely 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. 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 asses 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.

On the other hand, election rigging is hardly ever an issue in Western style democracies. Eggers et al. (2015) find no manipulation 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.

Furthermore, a manipulation of the relevant minimum cut-off value seems to be unlikely. 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. 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.[31] 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.

Lee and Lemieux (2010) provide a list of formal tests to check whether the identifying assumption is fulfilled.[32] 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 restricted sample that is used for the cross-sectional regressions, I test the null-hypothesis that treatment and control group are balanced. 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 (column 4). Due to omitted variable bias the null hypothesis is rejected for almost all covariates.[33] 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.

Second, given the RDD is correctly specified, predetermined baseline covariates, which are likely to affect the outcome variable Y, should evolve smoothly with respect to the assignment variable around the majority threshold. Intuitively the treatment should not affect baseline covariates that are determined prior to the realization of treatment. I formally test the null hypothesis of a zero ATE on baseline covariates for the FE model in table 7. I estimate equation 3 by using each of the observed baseline covariates as dependent variables. Panel A presents the

[31] All 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`.

[32] Further robustness checks are presented in section A2 in the Appendix.

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

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
N	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
N	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.

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.[34] 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. Furthermore, an increase in standard errors would put into question that the functional form is correctly specified (Lee and Lemieux 2010). Table 8 shows estimation results when controlling for the logarithm of population size, population density, employees, commuters, and council size.[35] Compared to the baseline specifications in table 3, 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 point estimates for material expenditures, as well as administrative and total spending remain the same and partially gain in 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. 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.[36] 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-sample. The median of 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.[37] For both sub-samples the null hypothesis of no discontinuity at the placebo-cutoff is not rejected at the five percent level for almost all fiscal outcome. In short, the lack of significant results from the placebo-cutoff implies that the estimation results from the baseline FE regression are valid.

[34] Using the baseline covariates as dependent variables in the cross-sectional approach yields similar results, as shown in table 14 in the Appendix. The null hypothesis of a zero ATE on the baseline covariates is not rejected at the 5 percent significance level, except for low order specifications of the population density and the share of young.

[35] The share of young and the share of old are not taken into account in table 8 since they are not available for the whole sample period.

[36] However, 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.

[37] For estimation I use observations on one side of the majority only. This ensures that I do not estimate a regression function where I expect 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.

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	0.000	-0.010	0.005
	(0.004)	(0.005)	(0.008)	(0.011)
Log (population density)	-0.002	-0.000	-0.010	0.005
	(0.004)	(0.005)	(0.008)	(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	-0.001	0.000
	(0.000)	(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	-0.004	-0.025	-0.026
	(0.010)*	(0.014)	(0.020)	(0.028)
Log (share of young)	0.005	0.001	0.012	0.010
	(0.008)	(0.012)	(0.018)	(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: 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: Expenditures				
Log total expenditures	-0.012	-0.034	-0.010	-0.061
	(0.012)	(0.017)*	(0.027)	(0.037)*
Log administrative expenditures	-0.015	-0.028	-0.003	-0.031
	(0.011)	(0.017)*	(0.025)	(0.034)
Log personnel expenditures	-0.008	0.007	-0.001	0.011
	(0.009)	(0.014)	(0.024)	(0.036)
Log material expenditures	-0.026	-0.063	-0.036	-0.031
	(0.018)	(0.026)**	(0.038)	(0.053)
Log investment expenditures	-0.018	-0.052	-0.063	0.013
	(0.037)	(0.053)	(0.077)	(0.103)
Panel B: Tax multipliers				
Log multiplier property tax A	-0.007	-0.001	0.001	-0.020
	(0.006)	(0.009)	(0.015)	(0.020)
Log multiplier property tax B	-0.005	0.006	-0.002	-0.021
	(0.007)	(0.010)	(0.017)	(0.024)
Log trade tax multiplier	-0.002	-0.003	-0.005	-0.014
	(0.002)	(0.003)	(0.005)	(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.

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

Table 9: Placebo-cutoff for coalition governments, using fixed effects

	RDD			
	(1) First Order	(2) Second Order	(3) Third Order	(4) Fourth Order
Panel A: Expenditures				
Log total expenditures	-0.014	-0.008	-0.002	-0.016
	(0.015)	(0.017)	(0.021)	(0.024)
Log administrative expenditures	-0.009	-0.007	-0.003	-0.010
	(0.015)	(0.017)	(0.020)	(0.023)
Log personnel expenditures	-0.003	-0.009	-0.017	-0.012
	(0.012)	(0.013)	(0.014)	(0.016)
Log material expenditures	-0.030	-0.026	-0.012	-0.007
	(0.022)	(0.023)	(0.026)	(0.030)
Log investment expenditures	-0.051	-0.042	-0.036	-0.015
	(0.045)	(0.047)	(0.056)	(0.066)
Panel B: Tax multipliers				
Log multiplier property tax A	0.007	0.005	-0.003	-0.008
	(0.009)	(0.010)	(0.011)	(0.013)
Log multiplier property tax B	0.010	0.005	-0.010	-0.015
	(0.012)	(0.013)	(0.015)	(0.017)
Log trade tax multiplier	-0.002	-0.002	-0.006	-0.008
	(0.004)	(0.004)	(0.004)	(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.

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

Table 10: Placebo-cutoff for single-party governments, using fixed effects

	RDD			
	(1) First Order	(2) Second Order	(3) Third Order	(4) Fourth Order
Panel A: Expenditures				
Log total expenditures	-0.025	-0.031	-0.063	-0.041
	(0.017)	(0.026)	(0.031)**	(0.033)
Log administrative expenditures	-0.016	-0.028	-0.036	-0.031
	(0.016)	(0.022)	(0.027)	(0.028)
Log personnel expenditures	0.005	-0.040	-0.058	-0.057
	(0.016)	(0.022)*	(0.031)*	(0.033)*
Log material expenditures	-0.016	-0.023	-0.032	-0.019
	(0.022)	(0.031)	(0.041)	(0.042)
Log investment expenditures	-0.002	-0.047	-0.128	-0.065
	(0.067)	(0.096)	(0.110)	(0.110)
Panel B: Tax multipliers				
Log multiplier property tax A	-0.007	-0.006	-0.006	-0.006
	(0.009)	(0.011)	(0.012)	(0.012)
Log multiplier property tax B	-0.009	-0.007	-0.003	0.001
	(0.012)	(0.016)	(0.016)	(0.017)
Log trade tax multiplier	-0.000	-0.001	0.001	0.005
	(0.004)	(0.006)	(0.007)	(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.

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

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 and exhibit higher taxes than single-party governments. Yet, the empirical evidence is mixed. 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 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.

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.

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Appendix

A1: Additional Tables and Graphs

Table 11: Definition and source of variables

Label	Period	Description	Source
Total expenditures	1994 - 2014	Real total expenditures (Bruttoausgaben der Gemeinden) per capita (deflated by federal CPI, 2010 = 100).	Baden-Wuerttemberg State Statistical Office
Administrative expenditures	1994 - 2014	Real total spending on public administration (Verwaltungshaushalt insgesamt) per capita (deflated by federal CPI, 2010 = 100).	Baden-Wuerttemberg State Statistical Office
Personnel expenditures	1994 - 2014	Real personnel expenditures (Personalausgaben) per capita (deflated by federal CPI, 2010 = 100).	Baden-Wuerttemberg State Statistical Office
Material expenditures	1994 - 2014	Real material expenditures (laufender Sachaufwand) per capita (deflated by federal CPI, 2010 = 100).	Baden-Wuerttemberg State Statistical Office
Investment expenditures	1994 - 2014	Real investment expenditures (Ausgaben fuer Sachinvestitionen) per capita (deflated by federal CPI, 2010 = 100).	Baden-Wuerttemberg State Statistical Office
Multiplier property tax A	1994 - 2014	Property tax multiplier (Hebesatz Grundsteuer A) 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 (Hebesatz Grundsteuer B) 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 (Gewerbesteuerhebesatz) 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 (Auspendler) per capita.	German Federal Employment Agency
Population density	1994 - 2013	Population density.	Own construction based on data from the German Federal Statistical Office and the Baden-Wuerttemberg State Statistical Office

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Label	Period	Description	Source
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 (unechte Teilortswahl).	Baden-Wuerttemberg State Statistical Office
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						
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
	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

continued ...

...continued

Variable		Mean	Std.dev.	Min.	Max.	N
	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

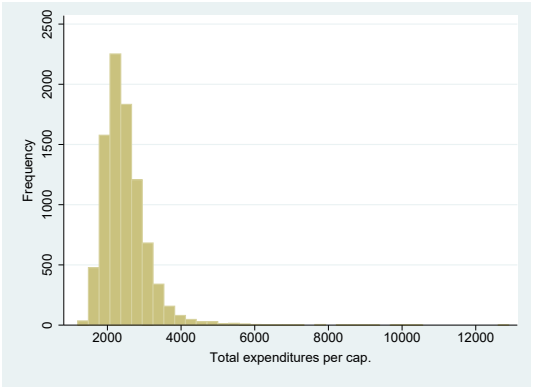
Table 13: Matrix of the four possible treatment types

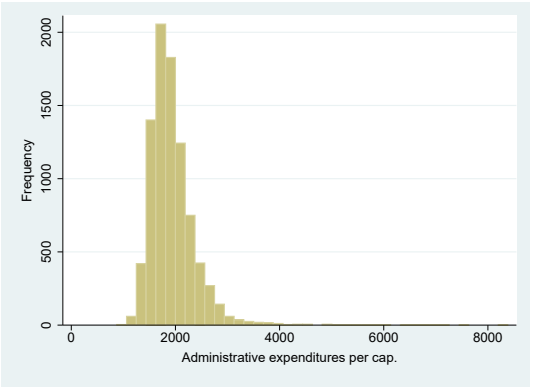
		Proportion of seats ≥ 50 %, $Z_i = 0$	
		Single-Party Gov. $D_i = 0$	Coalition Gov. $D_i = 1$
Proportion of seats < 50 %, $Z_i = 1$	Single-Party Gov. $D_i = 0$	Never-Takers (Minority Government)	Defiers
	Coalition Gov. $D_i = 1$	Compliers (Minimal Winning Coalition)	Always-takers (Oversized Coalition)

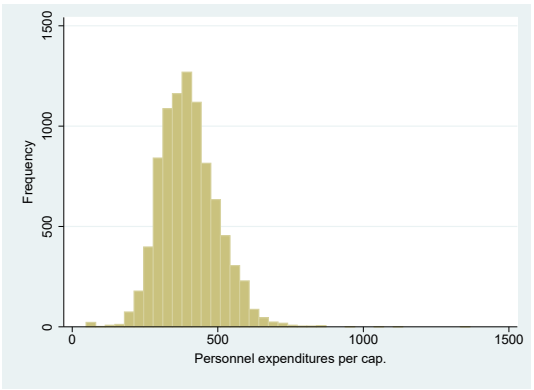
Notes: D_i is a treatment dummy equal to one if the tpye of government is a coalition. Z_i is dummy equal to one if individuals are assigned to the treatment group, that is to say that the largest party's proportion of the seats is smaller than 50 %.

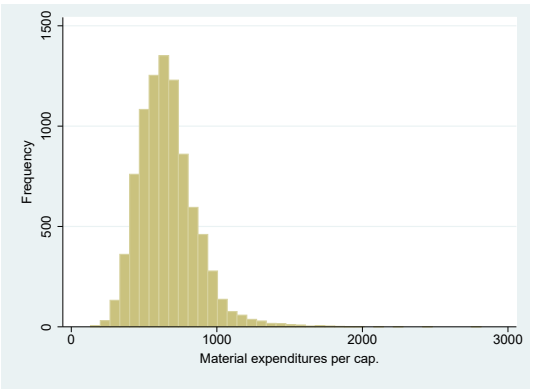
Source: author's own compilation, design based on Angrist and Pischke (2015).

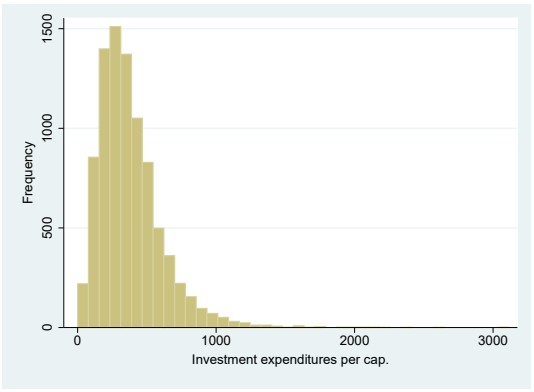
Figure 2: Histograms of dependent variables

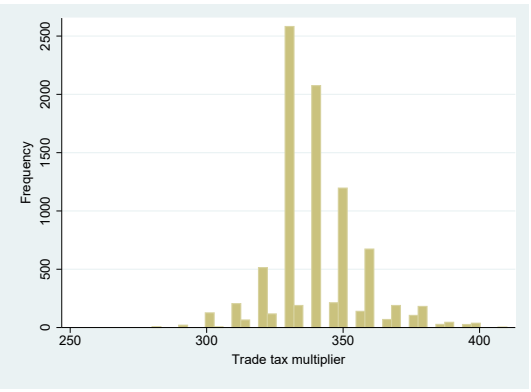


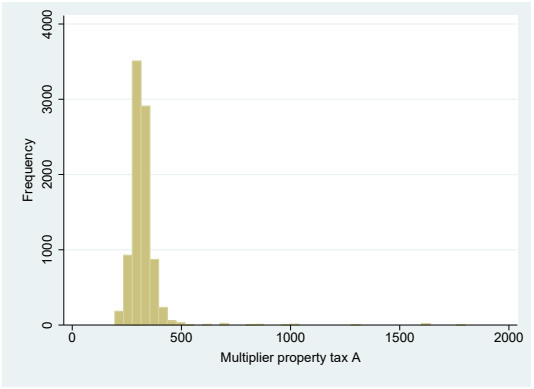












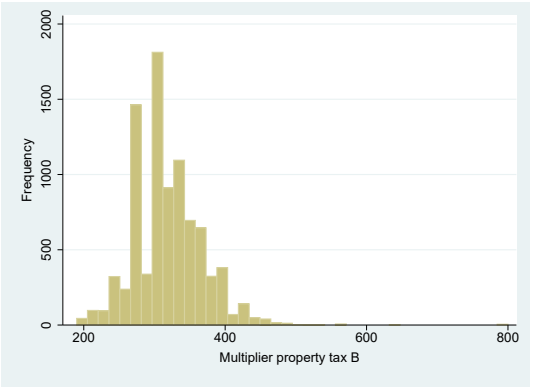
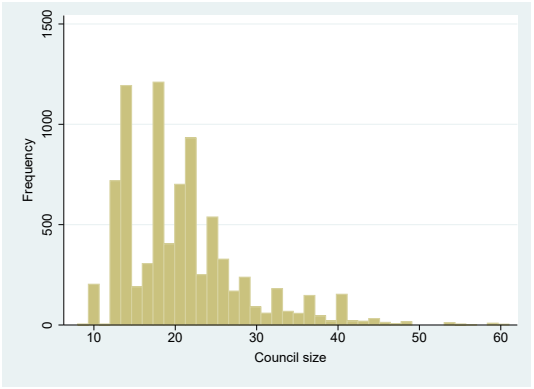
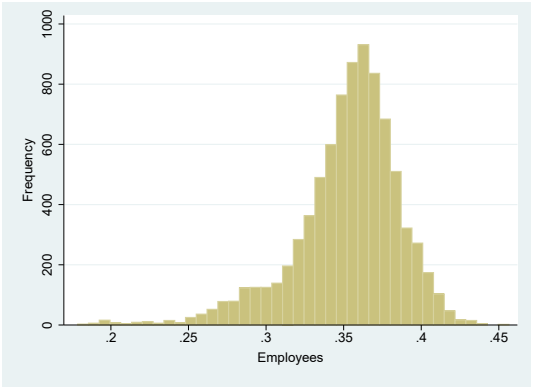
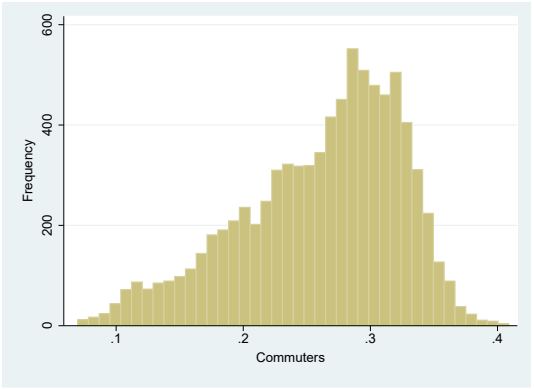
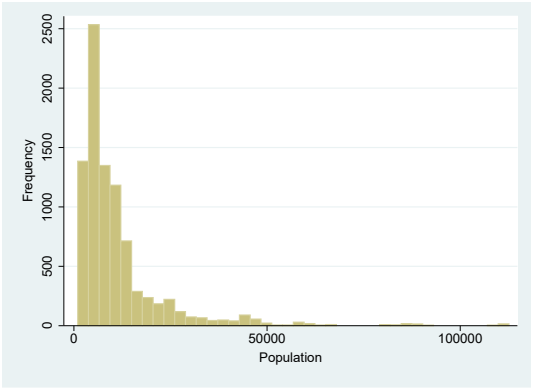


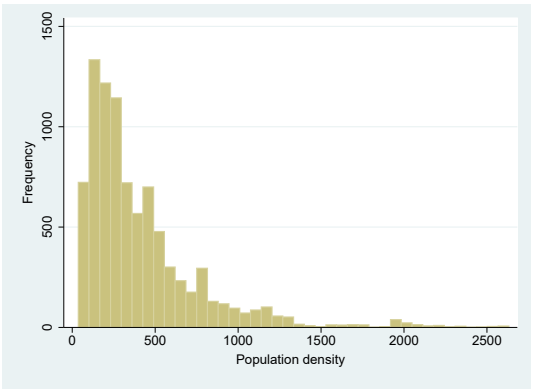
Figure 3: Histograms of baseline covariates

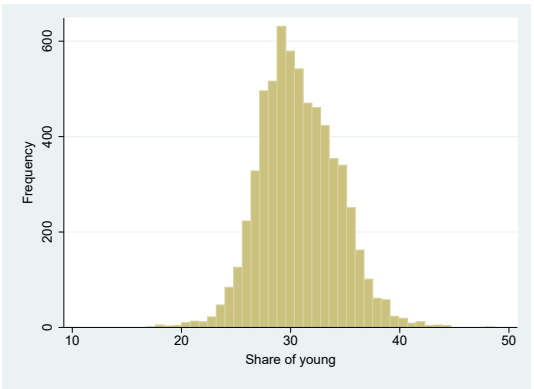












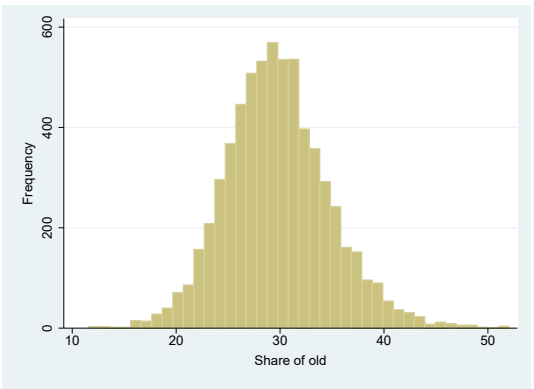


Table 14: Balance test for baseline covariates: RDD estimations results using cross sectional variation, restricted sample

	RDD			
	(1) First Order	(2) Second Order	(3) Third Order	(4) Fourth Order
Panel A: Standard controls				
Log (council size)	-0.019	0.004	0.014	0.018
	(0.020)	(0.027)	(0.039)	(0.054)
Log (employees)	0.000	-0.001	-0.000	-0.001
	(0.000)	(0.001)	(0.001)	(0.001)
Log (commuters)	0.000	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.002)	(0.002)
Log (population)	-0.097	0.038	0.127	0.134
	(0.078)	(0.102)	(0.149)	(0.207)
Log (population density)	-0.203	-0.276	-0.221	-0.172
	(0.092)**	(0.116)**	(0.168)	(0.230)
Number of clusters	539	539	539	539
Observation	4619	4619	4619	4619
Panel B: Additional controls				
Log (share of old)	-0.020	-0.035	0.018	0.010
	(0.016)	(0.020)*	(0.028)	(0.042)
Log (share of young)	0.020	0.028	0.015	0.014
	(0.010)**	(0.014)**	(0.021)	(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: 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: Expenditures				
Log total expenditures	-0.019	-0.035	-0.021	-0.088
	(0.021)	(0.028)	(0.041)	(0.057)
Log administrative expenditures	-0.036	-0.039	-0.036	-0.075
	(0.020)*	(0.027)	(0.041)	(0.056)
Log personnel expenditures	-0.015	-0.044	-0.028	-0.027
	(0.026)	(0.033)	(0.046)	(0.064)
Log material expenditures	-0.036	-0.097	-0.030	-0.046
	(0.033)	(0.043)**	(0.065)	(0.091)
Log investment expenditures	-0.005	-0.079	-0.025	-0.066
	(0.048)	(0.069)	(0.098)	(0.140)
Panel B: Tax multipliers				
Log multiplier property tax A	-0.021	-0.014	-0.013	-0.010
	(0.019)	(0.025)	(0.035)	(0.052)
Log multiplier property tax B	-0.014	0.008	-0.028	-0.029
	(0.016)	(0.022)	(0.032)	(0.045)
Log trade tax multiplier	-0.006	0.005	-0.006	-0.011
	(0.005)	(0.007)	(0.010)	(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.

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

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

	RDD			
	(1) First Order	(2) Second Order	(3) Third Order	(4) Fourth Order
Panel A: Expenditures				
Log total expenditures	-0.014	-0.031	-0.009	-0.035
	(0.011)	(0.017)*	(0.026)	(0.036)
Log administrative expenditures	-0.019	-0.026	-0.017	-0.019
	(0.012)	(0.017)	(0.025)	(0.036)
Log personnel expenditures	-0.012	0.007	-0.001	0.023
	(0.010)	(0.015)	(0.024)	(0.034)
Log material expenditures	-0.021	-0.059	-0.021	-0.034
	(0.017)	(0.024)**	(0.036)	(0.050)
Log investment expenditures	-0.002	-0.056	-0.037	-0.018
	(0.036)	(0.050)	(0.073)	(0.101)
Panel B: Tax multipliers				
Log multiplier property tax A	-0.013	-0.000	-0.013	-0.018
	(0.008)*	(0.011)	(0.017)	(0.024)
Log multiplier property tax B	-0.014	0.005	-0.021	-0.001
	(0.010)	(0.014)	(0.021)	(0.031)
Log trade tax multiplier	-0.003	-0.002	-0.007	-0.013
	(0.003)	(0.004)	(0.006)	(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.

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

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

	OLS	RDD			
	(1)	(2)	(3)	(4)	(5)
	-	First Order	Second Order	Third Order	Fourth Order
Panel A: Expenditures					
Log total expenditures	0.007	-0.011	-0.011	-0.021	-0.024
	(0.010)	(0.012)	(0.012)	(0.013)	(0.014)*
Log administrative expenditures	0.006	-0.017	-0.017	-0.019	-0.022
	(0.010)	(0.012)	(0.012)	(0.013)	(0.014)
Log personnel expenditures	0.010	-0.005	-0.005	0.011	0.009
	(0.009)	(0.010)	(0.010)	(0.012)	(0.012)
Log material expenditures	-0.021	-0.025	-0.025	-0.036	-0.044
	(0.014)	(0.016)	(0.016)	(0.018)**	(0.019)**
Log investment expenditures	0.004	0.007	0.007	-0.001	-0.013
	(0.032)	(0.040)	(0.040)	(0.047)	(0.045)
Panel B: Tax multipliers					
Log multiplier property tax A	0.006	-0.009	-0.009	-0.004	-0.003
	(0.006)	(0.007)	(0.007)	(0.008)	(0.009)
Log multiplier property tax B	0.009	-0.009	-0.009	-0.002	0.001
	(0.008)	(0.010)	(0.010)	(0.011)	(0.011)
Log trade tax multiplier	0.001	-0.004	-0.004	-0.003	-0.003
	(0.002)	(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.

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

Table 18: 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: Expenditures								
Log total expenditures	-0.017	-0.015	-0.033	-0.034	-0.013	-0.015	-0.066	-0.058
	(0.013)	(0.012)	(0.019)*	(0.018)*	(0.031)	(0.029)	(0.043)	(0.039)
Log administrative expenditures	-0.020	-0.021	-0.023	-0.028	-0.014	-0.017	-0.037	-0.025
	(0.012)	(0.013)*	(0.018)	(0.018)	(0.028)	(0.028)	(0.039)	(0.038)
Log personnel expenditures	-0.011	-0.009	0.001	0.009	-0.015	-0.003	0.003	0.018
	(0.009)	(0.010)	(0.013)	(0.015)	(0.019)	(0.024)	(0.027)	(0.036)
Log material expenditures	-0.031	-0.027	-0.048	-0.063	-0.026	-0.038	-0.018	-0.030
	(0.017)*	(0.018)	(0.025)*	(0.026)**	(0.036)	(0.038)	(0.051)	(0.053)
Log investment expenditures	-0.027	-0.001	-0.114	-0.044	-0.098	-0.018	-0.028	0.011
	(0.039)	(0.039)	(0.055)**	(0.054)	(0.083)	(0.078)	(0.114)	(0.105)
Panel B: Tax multipliers								
Log multiplier property tax A	-0.012	-0.014	-0.001	-0.002	-0.006	-0.014	-0.006	-0.017
	(0.008)	(0.008)*	(0.012)	(0.012)	(0.018)	(0.018)	(0.024)	(0.026)
Log multiplier property tax B	-0.014	-0.015	0.011	0.004	-0.003	-0.024	0.004	-0.015
	(0.009)	(0.010)	(0.013)	(0.015)	(0.020)	(0.023)	(0.028)	(0.035)
Log trade tax multiplier	-0.005	-0.005	-0.002	-0.004	-0.003	-0.011	-0.005	-0.012
	(0.003)	(0.003)	(0.004)	(0.004)	(0.006)	(0.006)*	(0.008)	(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. 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 might be 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.

Table 19: 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: Expenditures				
Log total expenditures	-0.011	-0.035	-0.016	-0.061
	(0.012)	(0.018)*	(0.029)	(0.040)
Log administrative expenditures	-0.017	-0.030	-0.018	-0.027
	(0.013)	(0.019)	(0.029)	(0.039)
Log personnel expenditures	-0.004	0.008	-0.004	0.015
	(0.010)	(0.016)	(0.024)	(0.036)
Log material expenditures	-0.025	-0.064	-0.038	-0.032
	(0.018)	(0.026)**	(0.038)	(0.053)
Log investment expenditures	0.005	-0.047	-0.020	0.006
	(0.039)	(0.055)	(0.079)	(0.107)
Panel B: Tax multipliers				
Log multiplier property tax A	-0.011	-0.003	-0.015	-0.018
	(0.008)	(0.012)	(0.018)	(0.026)
Log multiplier property tax B	-0.010	0.002	-0.025	-0.018
	(0.010)	(0.015)	(0.023)	(0.035)
Log trade tax multiplier	-0.004	-0.004	-0.011	-0.013
	(0.003)	(0.004)	(0.006)*	(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.

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

However, two arguments speak against this concern: On the one hand, right-wing gov-

ernments 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 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 originate from the municipality where they run for office. Furthermore, mayors are expected to act in a non-partisan manner seeking the best interest for the municipality (Wehling 2012).

To check whether mayors without party affiliation affect estimation results, I build subsamples 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 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 loose its significance. The party 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, 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: 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: Expenditures								
Log total expenditures	-0.018	-0.007	-0.031	-0.036	0.008	-0.002	-0.105	0.022
	(0.017)	(0.018)	(0.025)	(0.024)	(0.046)	(0.029)	(0.055)*	(0.040)
Log administrative expenditures	-0.019	-0.021	-0.021	-0.037	0.005	-0.006	-0.050	0.034
	(0.017)	(0.019)	(0.026)	(0.024)	(0.044)	(0.030)	(0.058)	(0.044)
Log personnel expenditures	-0.013	-0.010	-0.004	0.021	-0.054	0.045	-0.053	0.060
	(0.015)	(0.014)	(0.022)	(0.021)	(0.032)*	(0.032)	(0.046)	(0.046)
Log material expenditures	-0.016	-0.046	-0.066	-0.073	0.001	-0.066	-0.039	0.008
	(0.021)	(0.028)	(0.031)**	(0.038)*	(0.046)	(0.052)	(0.062)	(0.072)
Log investment expenditures	-0.033	0.020	-0.066	-0.039	-0.053	0.028	-0.120	0.098
	(0.057)	(0.059)	(0.083)	(0.076)	(0.123)	(0.107)	(0.170)	(0.145)
Panel B: Tax multipliers								
Log multiplier property tax A	-0.014	-0.005	0.012	0.007	-0.000	-0.000	0.015	-0.018
	(0.010)	(0.012)	(0.013)	(0.017)	(0.021)	(0.024)	(0.032)	(0.032)
Log multiplier property tax B	-0.012	-0.008	0.028	0.001	-0.006	0.002	0.006	0.006
	(0.013)	(0.015)	(0.017)	(0.020)	(0.028)	(0.027)	(0.042)	(0.041)
Log trade tax multiplier	-0.005	-0.001	0.004	-0.005	-0.004	-0.006	-0.008	-0.001
	(0.004)	(0.005)	(0.005)	(0.006)	(0.008)	(0.008)	(0.011)	(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 total expenditures, as well as administrative expenditures and material expenditures. Especially 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.

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: Expenditures				
Log total expenditures	-0.003	-0.034	-0.004	-0.058
	(0.011)	(0.017)**	(0.028)	(0.038)
Log administrative expenditures	-0.003	-0.027	0.002	-0.025
	(0.010)	(0.016)*	(0.025)	(0.033)
Log personnel expenditures	0.003	0.003	0.003	0.009
	(0.009)	(0.014)	(0.023)	(0.034)
Log material expenditures	-0.021	-0.063	-0.031	-0.035
	(0.018)	(0.026)**	(0.038)	(0.053)
Log investment expenditures	-0.016	-0.063	-0.052	0.002
	(0.035)	(0.050)	(0.074)	(0.098)
Panel B: Tax multipliers				
Log multiplier property tax A	-0.003	-0.000	-0.000	-0.016
	(0.006)	(0.009)	(0.015)	(0.020)
Log multiplier property tax B	0.002	0.007	-0.003	-0.014
	(0.006)	(0.010)	(0.017)	(0.026)
Log trade tax multiplier	-0.000	-0.003	-0.006	-0.012
	(0.002)	(0.003)	(0.005)	(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.

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

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 A 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 B 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 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 1^{st} to 99^{th} percentiles of the outcome variable. Compared to table 17 the signs of the effects do not change. However, eliminating outliers 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. However, it remains insignificant. Therefore I conclude that the rejection of the government fragmentation hypothesis is not driven by outliers.

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.

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

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

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

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.[38] Thus, the suburb election system could systematically increase the number of seats required to win a 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.[39] In table 18 in the Appendix I test whether the presence of suburb election system affects outcomes for the 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 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 seats. Assume that the strongest party lacks one seat to the absolute majority in both municipalities. Than 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,

[38] Other 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`.

[39] For more information on this see `http://www.wahlrecht.de/kommunal/baden-wuerttemberg.htm`

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.

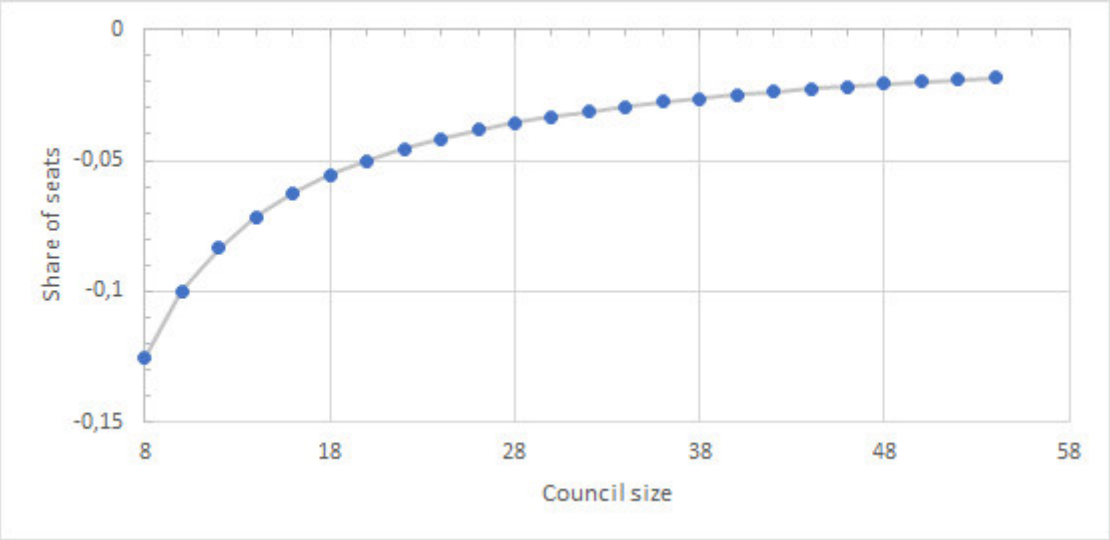
There are two ways to circumvent this sample selection bias: Firstly, one could apply a fixed effects approach, where the treatment effect is estimated by using only the variation within municipalities, as done in the main section. By design, the degree of closeness does not differ within the unit of 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.

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
N	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.

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 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
N	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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