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Mixed Evidence from a Regression Discontinuity Design

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Sebastian Garmann[1]

Do Coalitions Really Cause Larger Government Expenditures? – Mixed Evidence from a Regression Discontinuity Design

Abstract

This paper measures the causal effect of coalition vs. single-party governments on fiscal policies using a data set of 396 municipalities in the German state of North Rhine-Westphalia in the time period 1985-2004. Using a regression discontinuity design to take the endogeneity of the type of government into account, we exploit a discontinuity that comes through the change from a coalition to a single-party government at 50% of the seat share of the strongest party. Our results point to a significant effect of the type of government on personnel expenditures, while we do not find significant results for material spending and investment expenditures. These results differ substantially from simple OLS estimates.

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

Keywords: Legislative organization; regression discontinuity design; local fiscal policy; coalition governments; government spending; panel data

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1. Introduction

In the political economy literature, it is often claimed that coalition governments spend more than single-party governments and that therefore the type of government is one determinant of the magnitude of government spending. This claim gets its theoretical foundation by the contribution of Weingast et al.(1981): Their model assumes that the fiscal budget is a common pool for all political actors. These political actors seek re-election and therefore the costs and benefits of their constituencies are more important to them than those of other interest groups. They target resources towards their own interest groups while the costs of pork-barrel projects are spread over the whole population, assuming that taxation cannot be as easily targeted as these projects. This leads to the law of 1/n: Political actors seeking re-election internalise all the benefits associated with targeted spending towards their constituencies but only a fraction of the associated costs(see Schaltegger and Feld, 2009). As the fraction of costs that political decision-makers perceive goes down with the number of actors involved in the decision-making process we should expect that multi-party governments spend more than single-party governments. In a recent contribution, Primo and Snyder(2008) question the model of Weingast et al.(1981) and show that under certain conditions, there can even exist a negative relationship between the degree of government fragmentation and spending.

Although theoretically founded, empirical evidence for this claim is mixed: Early empirical studies(see e.g. Roubini and Sachs,1989, Edin and Ohlsson, 1991, De Haan and Sturm,1994) try to measure the effect of the type of government on spending or deficits using panel OLS regressions without taking the endogeneity of the type of government and a possible omitted variable bias(e.g. because of unobservable voter preferences) in-

to account.[1] The effects found in these studies seem to be dependent on variable specifications and the data sets used. More recent studies by Perotti and Kontopoulos(2002) focusing on OECD countries, Bawn and Rosenbluth(2006) focusing on European countries and Schaltegger and Feld(2009) focusing on Swiss cantons use panel data and include fixed effects to capture time-invariant omitted variables. In the research of Schaltegger and Feld(2009), the fixed effects approach is problematic because changes in coalition size do not frequently occur. Of course, using fixed-effects can only solve problems that are due to time-invariant variables. While Perotti and Kontopoulos(2002) as well as Schaltegger and Feld(2009) come to inconclusive results, Bawn and Rosenbluth(2006) find that spending significantly increases with the number of parties in government.

In another contribution, under certain assumptions Persson et al.(2007) theoretically derive that the influence of electoral rules, i.e. the distinction between a majoritarian and a proportional election system, on government spending only comes through the association of the electoral system with the number of parties in government. Empirically, this means that it might be possible to use the electoral rules as an instrument for the number of parties in government, given that according to the theoretical prediction, the electoral rules should be exogenous to government spending and correlated with the number of parties in government. In particular, they assume the following chain of causality:

Electoral system → Number of parties in government → Government spending

[1] Early empirical studies use deficits as fiscal outcome variable, while spending as outcome variable is more in line with the basic theoretical model: It assumes that the budget is always balanced such that revenues are simply adjusted to spending.

Using an Instrumental Variable(IV) approach and a cross-country data set, they find that the presence of a coalition government significantly increases government spending. But, as they point out, it is possible that the electoral rules are not valid instruments because political units might self-select into alternative electoral rules based on unobserved features correlated with government spending. Another problem with their approach might be the ordering of the causal chain: Colomer(2005) argues that it is the number of parties that determines the electoral system, therefore reversing the causal chain proposed by Persson et al.(2007). Thus, it is not clear whether existing research has revealed the causal effect of a coalition government on government spending due to problems of endogeneity and omitted variable bias.

In this paper, we estimate the causal effect of multi – party governments on fiscal policies using a regression-discontinuity design(RDD) and municipal data from North Rhine – Westphalia(NRW), Germany. We make use of the fact that the form of government changes discontinuously at 50% of the seat share of the strongest party in municipal parliaments: In municipalities in which the strongest party reaches less than 50% of the seat share, a coalition government has to be implemented, while in municipalities in which the strongest party reaches more than 50% of the seat share, there exists a single – party government.[2] We argue that in a close neighbourhood of 50%, the treatment is as good as randomly assigned such that those units slightly below the threshold provide a counterfactual outcome for those units slightly above the threshold as they are expected to have similar characteristics except for the treatment status, therefore avoiding endogeneity problems and an omitted variable bias(for standard references on the RDD see Imbens and Lemieux, 2008 or Lee and Lemieux, 2010).

[2] We define a coalition as any kind of government in which (at least) two parties have to bargain to decide on policy. This also includes minority governments.

With the exception of Schaltegger and Feld(2009), all of the mentioned studies use a cross-country sample. This can be criticized because the institutional framework in different countries is far too dispersed to be able to capture all potentially relevant factors with the help of one model(see Voigt, 2011). Therefore, the usage of community level data in this paper assures that only political units at a similar stage of development and with an identical institutional framework are compared.

While our RDD estimates suggest that the type of government matters for personnel expenditures, we do not find significant effects for material spending and investment expenditures. OLS estimates differ substantially from the RDD estimates. Besides using the RDD, this study also employs fixed effects and clustered standard errors that should account for serial correlation of the standard errors. This is the first empirical study dealing with the influence of the type of government on spending that combines the RDD with a fixed effect analysis and clustered standard errors. Our results suggest that significant results in past research might exist because one of these factors(or the combination of them) has been neglected.

The remainder of the paper is structured as follows: Section 2 entails a description of the institutional framework in NRW and the data used in this paper. Section 3 presents the empirical strategy. In section 4, we present the results and check their sensitivity. Finally, section 5 concludes.

2. Institutional framework and data

2.1 Institutional framework

In Germany, municipalities are the lowest administrative unit of government. We regard a panel of 396 municipalities over the years 1985-2004 in Germany's most populous state North Rhine-Westphalia. The years 1985-2004 consist of four legislative terms, each with a length of five years. 23 of the 396 municipalities are cities with county status(*kreisfreie Städte*), while the rest of the cities belongs to a certain county(*kreisangehörige Städte*). Cities with county status have a larger autonomy and thus possibly larger expenditures than comparable municipalities belonging to a county.

NRW has a closed list, proportional election system[3]. In the period of investigation, the number of seats in the municipal council was determined by population size, ranging from 21 to 91 seats. Moreover, until the election in 1999, parties faced a barrier to entry into municipal parliaments that took the form of a minimum vote share of 5 %. This barrier to entry was abolished in 1999(which was part of a large reform of the municipal election system in the 1990s that will be further described below) which raised the number of parties in parliaments and might have changed voter behaviour at the polls because votes for parties with less than 5 % of the vote share are considered in the seat allocation process after the reform. Surprisingly, although this reform has led to a rise in the number of parties in the municipal councils, the proportion of coalition governments has not changed very much. There are even less coalition governments in the term 1999-2004 than in the term 1989-1994 in which the 5%-threshold has been in place.

[3] To be precise, the election system is a mixed-member proportional system, but this is not of importance in the rest of the paper.

Table 1 shows the proportion of coalition governments for the four legislative terms. Elections in NRW take place every fifth year in the second half of the year.

Traditionally, the North German council constitution was in place in NRW. The North German council constitution can be classified as the local constitution that provides the municipal council with the strongest position relative to the mayor(in contrast, for example, to the South German council constitution, see Egger and Köthenbürger, 2010). The mayor was elected by the council and was rather a representative of the municipality than endowed with widespread political power. Besides abolishing the 5%-threshold, the municipal election reform in the 1990s made the North Rhine-Westphalian system more similar to the South German council constitution: Since the election in 1999, the mayor is directly elected by the constituency and has a relatively strong position. Moreover, the municipal electoral reform in the 1990s integrated elements of direct democracy like initiatives and referendums into the municipal election system. These features of the electoral system have been in place since 1994. Therefore, the institutional settings in place across different legislative terms are only partly comparable to each other. We use time-fixed effects to capture different institutional settings as well as to capture common shocks that affect all municipalities.

Municipalities in Germany are of considerable economic importance: Municipalities are responsible for roughly one-third of the total German government spending and employ 40% of all state employees. Municipalities have the right to self-government which is guaranteed by the German constitution(see Scherf, 2010). Moreover, municipalities are free to set three different tax rates: a tax on business profits(*Gewerbesteuer*), a tax on agricultural land(Property Tax A) and a tax on business and private land(Property tax B). Additionally, they receive grants from higher tiers of government and parts of the

income tax and the VAT tax revenue. In return, the revenue of the tax on business profits does not completely accrue to the municipalities because municipalities have to give a fraction of these revenues to higher levels of government(*Gewerbesteuerumlage).* Besides raising taxes and receiving grants, municipalities finance themselves through debt, fees and financial contributions. The difference between fees and financial contributions lies in the fact that fees can only be charged for persons that effectively use a service provided, while financial contributions can already be charged if the citizen might use a service provided by the municipality. It is important to note that the local constitution for municipalities in NRW(*Gemeindeordnung)* forces the municipalities to rely as strongly as possible on fees and financial contributions to finance the services they provide. Only if this is not possible anymore, taxes should be raised(see Karrenberg and Münstermann, 1994). Thus, citizens participate at least partially in financing the services they use.

The responsibilities of municipalities can be classified into two groups(see Scherf, 2010): Mandated spending(*Auftragsangelegenheiten)* and autonomy spending (*Selbstverwaltungsangelegenheiten)*. The tasks in the group of mandated spending consist of responsibilities that the state or federal government has handed over to the municipalities for execution. The state or federal government determines in detail how the municipalities have to solve these mandated tasks. Examples are the construction supervision agency and disaster prevention. The group of autonomy spending can be further divided into limited autonomy spending(*pflichtige Selbstverwaltungsangelegenheiten*) and voluntary spending (*freiwillige Selbstverwaltungsangelegenheiten*). Municipalities have to fulfil the tasks belonging to the group of limited autonomy spending by law, but it is not exactly specified *how* they have to fulfil them. Examples for these tasks are social assistance, the construction of elementary schools and the maintenance of municipal

roads. For tasks belonging to the group of voluntary spending, municipalities are not restricted by the federal or state government. Thus, they are free to decide *whether and how* they want to pursue these tasks. Examples are sports grounds, culture and economic promotion.

Besides using total expenditures as outcome variable, we will also repeat our analysis for different types of expenditures: Following Egger and Köthenbürger(2010) using German data and Pettersson-Lidbom(2012) using data from Sweden and Finland both in the context of the effect of council size on government expenditures, we will investigate whether different types of expenditures, namely current and investment expenditures, depend on the type of government. The type current expenditures can further be divided into material spending and personnel expenditures. By looking at different types, we take into account that not all expenditure types can be influenced by a (local) government in the short run. We suppose that current expenditures react more strongly to the type of government, because they contain positions of relatively small magnitude and can be adjusted by a local government in the short run, while investment expenditures can only be adjusted with some time lag. Egger and Köthenbürger(2010) point out that this might be due to legal planning requirements and co-financing modalities with higher levels of government in the case of investment expenditures.

2.2 Data

For this paper, we use electoral and financial data from North-Rhine Westphalia that originates from the Federal Statistical Office of NRW. We consider a panel that includes the yearly total expenditures per capita of the 396 municipalities in the period 1985-2004. Furthermore, we regard different types of expenditures. In this period, there

have been four legislative terms which can be seen in Table 1. The elections for these legislative terms were in 1984, 1989, 1994 and 1999. Because elections are held towards the end of the year, it is reasonable to assume that each newly elected government can only influence the expenditures in the year after the election for the first time, i.e. the relevant years for the legislative term 1984-1989 are 1985,1986,1987,1988 and 1989. The expenditure data from the Federal Statistical Office is on a yearly basis. We express all expenditures in per capita terms in constant 2005 prices(EUR). Moreover, all per capita expenditure variables are expressed in logarithms such that the influence of a coalition government on the total expenditures will have a percentage interpretation.

In the election data, we find that in 829 of the 1584 cases, there is a single-party government. In most cases, either the middle-right Christian Democratic Union(CDU) or the middle-left Social Democratic Party(SPD) is the strongest party. There are only very few cases in which a third party gains the most seats. From the financial data, we can infer that governments with a coalition spent on average 2033 Euro per capita per year, while single-party governments spent on average 1976 Euro per capita. A t-test strongly rejects the null hypothesis that the means of both samples are identical. But it still remains an open question whether the form of government has any influence on expenditures as the treatment effect is measured at the 50%-threshold of the seat share. Table 2 shows summary statistics of the main variables used in this paper. The minimum of yearly per capita investment expenditures seems to be quite small, but it is due to the fact that it is possible that there are years in which spending on investment is close to zero for some municipalities, showing their influence on this expenditure category.

3. Empirical Strategy

We make use of a regression discontinuity design to estimate the causal effect of the type of government on fiscal outcomes. A regression discontinuity design exploits that the treatment status is a discontinuous function of a covariate S_i. This makes it possible to estimate the causal effect of the treatment, because any effect on the outcome at the threshold can be attributed to the treatment status. In our case, the treatment-determining covariate is the seat share of the strongest party: if a party holds the majority of the seats in a municipal council, there will be a single-party government.[4] In any other case, there will be a coalition government. Therefore, the treatment status is a *deterministic* discontinuous function of the seat share of the strongest party and we can use a sharp RDD. Note that it does not matter which parties will form a coalition as we only want to estimate the effect of a coalition on spending per se. Equivalently, it does not matter which party is the strongest party in parliament. In general, all pre-treatment characteristics should be balanced on both sides of the threshold.

Note that in this study, we use the seat share, i.e. the number of seats held by a party divided by the total number of seats in the council, instead of the commonly used vote share(see e.g. Pettersson-Lidbom, 2008) as the determinant of control over the council. In proportional election systems, whether a party gains more than 50% of the votes or not does not precisely determine whether a party has control over the council or not. Especially due to the characteristics of certain seat allocation functions, it is for example

[4] We assume that a party always prefers to rule alone and that there are no strategic considerations that leads a party to build a coalition although it already has a majority alone.

possible that a party gains less than 50% of the votes but more than 50% of the seats.[5] Thus, the vote share might be a misleading measure of party control. Instead, the majority of the seats determines which policy will be realized. The fact that the election system in NRW involved barriers to entry(at least in the first three legislative terms of investigation) makes this point even more clear: because parties with less than 5% of the votes are not considered in the seat allocation process, it is possible that parties with less than 50% of the votes gain a majority in seats. The possibility of excess seats (*Überhang- und Ausgleichsmandate*) in the NRW municipal councils works in a similar direction. For the RDD to be valid, it is necessary that political units are unable to precisely manipulate the assignment variable. We argue that the seat share of the strongest party fulfils this requirement: Parties cannot precisely determine how much seats they will gain in an election. Figure 1 shows a histogram of the distribution of the assignment variable, ruling out concerns about a manipulation of the assignment variable.

The assignment variable should measure how far a party is from gaining (or losing) control over the municipal council. It is calculated by dividing the seats gained by the strongest party by the total number of seats in a council. For example, gaining 10 seats in a municipal council of size 21 yields an assignment variable of 47.62 %. Gaining 40 seats in a municipal council of size 81 yields an assignment variable of 49.38 %. One could argue that in both cases, the strongest party needs just one additional seat to gain control over the council and that therefore both assignment variables should be the same. This argument, however, neglects that a party needs, on average, relatively more

[5] In the first three legislative terms of investigation, seats in municipal parliaments in NRW were allocated using the method of D'Hondt which is known to discriminate small parties, thus making single-party government more likely. After the election in 1999, seats were allocated using the method of Hare-Niemeyer.

votes in a small municipal council to gain one additional seat than in a large one(see Lijphart and Gibberd, 1977 for payoff functions of seat allocation methods).

Besides the usage of the seat share instead of the vote share as treatment-determining covariate, this paper is related to, but distinct from the literature on estimating the party effect on fiscal policies using the RDD in the following point(see Ferreira and Gyourko, 2009, Pettersson-Lidbom, 2008): The literature on the party effect focuses on electoral systems that are bipartisan(or are treated as being bipartisan systems even though they are not) and defines two treatments: left-wing and right-wing majority. In these papers, the treatment status changes discontinuously at 50% of the vote share of the left-wing block. In the present paper, we focus on the treatments coalition or single-party government, without looking at which specific party or political direction builds a coalition or rules alone. Therefore, the treatment status changes discontinuously at 50% of the seat share of the strongest party.

The traditional RDD uses variation between units to estimate the effect of interest. By contrast, we follow Pettersson-Lidbom(2012) and use a within-unit design: Because the sample size includes rather few (independent) observations, it is not clear whether all (observable and unobservable) pre-treatment characteristics are really balanced on each side of the threshold with the effect that the treatment and control groups may well be not comparable. By contrast, a within-unit design, i.e. using a full set of municipality-fixed effects, only uses variation within one municipality, making the treatment and control groups comparable independently of the sample size. We also add additional control variables to the regression framework: population size, the proportion of people aged below 15, the proportion of people aged above 65 and party control(i.e. dummy

variables indicating whether the middle-right CDU, the middle-left SPD or a third party holds the most seats).

Basically, estimation of the treatment effect in an RDD can be done parametrically or non-parametrically. In the parametric approach, one uses all observations, even those far away from the threshold, and tries to capture the true functional relationship using, for example, a polynomial of p-th order. The non-parametric approach uses observations only in a close neighbourhood of the threshold. For the non-parametric approach to work, many independent observations are necessary which are not available here. Therefore, in this paper we make use of the parametric estimation approach. To be precise, we estimate the pooled regression

$$\begin{aligned} Y_{igt} = \alpha_i + \lambda_{gt} + \tau D_{ig} + \beta_1 \tilde{S}_{ig} + \beta_2 \tilde{S}_{ig}^2 + ... + \beta_p \tilde{S}_{ig}^p + \\ \beta_{p+1} \tilde{S}_{ig} D_{ig} + \beta_{p+2} \tilde{S}_{ig}^2 D_{ig} + ... + \beta_{2p} \tilde{S}_{ig}^p D_{ig} + \gamma' X_{igt} + \varepsilon_{igt} \end{aligned} \quad (1)$$

with $\tilde{S}_{ig} = S_{ig} - 0.5$, where centering S_{ig} around the threshold 0.5 ensures that τ measures the treatment effect at $S_{ig} = 0.5$ (see Angrist and Pischke, 2009) . Note that D_{ig} is a dummy variable indicating whether municipality i has a coalition government(in which case the dummy variable has the value zero) in legislative term g or a single-party government(in which case the dummy variable has the value one) and Y_{igt} is the fiscal outcome variable, for example the logarithm of per capita spending. $t_g = 1,...,5$ denotes the year in legislative term g from which the observations comes. α_i and λ_{gt} denote municipality- and time-fixed effects, respectively. Therefore, if the corresponding treatment effect τ takes a positive value, this will mean that a single-party government spends more than a coalition government, thus contradicting the theoretical hypothesis. X_{igt} is a vector of the additional control variables mentioned above.

For statistical inference in the presence of panel data, we have to consider two points: First, we have to take into account that the standard errors might be serially correlated. Following the suggestions of Bertrand et al.(2004), we cluster the standard errors at the municipality level to take this into consideration. Most of the existing literature on the influence of coalition governments on spending does not take this into account which is why we should expect that the standard errors in these studies are understated. Therefore, we will compare our standard errors in the case of clustered standard errors with the standard errors that would have resulted if we did not cluster our errors, showing the potential consequences of making the (possibly) erroneous assumption of no serial correlation in the standard errors. Second, since the treatment is the same during a legislative term g, one should also account for this. One possibility is to also cluster the standard errors at the level of the legislative terms, but due to the small number of clusters(there are only four legislative terms), this is not advisable. Another possibility lies in aggregating the data such that we estimate the effect of the treatment on total spending per capita per legislative term, i.e. we estimate

$$\begin{aligned} Y_{ig} = \alpha_i + \lambda_g + \tau D_{ig} + \beta_1 \tilde{S}_{ig} + \beta_2 \tilde{S}_{ig}^2 + ... + \beta_p \tilde{S}_{ig}^p + \\ \beta_{p+1} \tilde{S}_{ig} D_{ig} + \beta_{p+2} \tilde{S}_{ig}^2 D_{ig} + ... + \beta_{2p} \tilde{S}_{ig}^p D_{ig} + \gamma' X_{ig} + \varepsilon_{ig} \end{aligned} \tag{2}$$

In the robustness checks of the results, we also report results from this specification. We will see that the estimates of the treatment effect are very similar and conclusions concerning the statistical significance of the results do not change.

If the chosen parametric functional form is misspecified, the estimate of the treatment effect will in general be biased. Therefore, in this paper, we use polynomials of different order to estimate the treatment effect and explore the sensitivity of the treatment effect

to the chosen order. Additionally, we present the Akaike-Informations-Criterion(AIC) as a benchmark of which specification might be appropriate.

4. Results

4.1 Results from the Regression Discontinuity Design

In this section, we present the results on the distinction of single- and multi-party governments and its influence on fiscal policies. First, we look at the influence of the type of government on total expenditures. Second, we will investigate whether different types of expenditures, namely current and investment expenditures, depend on the type of government. The subcategory of current expenditures can further be divided into material spending and personnel expenditures.

In Table 3, we present the estimates for equation (1) using total expenditures as outcome variable. The first column contains the estimate of the treatment effect and its standard error for the simple OLS regression, thus only regressing the logarithm of per capita expenditure on the dummy variable, a full set of time fixed effects and control variables. For this simple OLS specification, we find a positive but insignificant treatment effect. According to this estimate, a change from a single-party government to a coalition government *lowers* government spending by 0.95%, thus contradicting the theoretical hypothesis. In Table 4, we present estimates for equation (2) without clustering the standard errors. Neglecting the suggestions of Bertrand et al.(2004) leads to standard errors that are only approximately half as large as those with clustering. Therefore it is not surprising that Table 4 contains more significant results than Table 3. As argued above, the effect of the simple OLS specification, even with controls, is likely to be biased. Using municipality-fixed effects to control for time-invariant unobservables, there is an even smaller positive treatment effect. For the RDD estimates with a poly-

nomial of first and second degree, we also find positive but insignificant treatment effects. The polynomial specifications ranging from the third to the fifth order display negative treatment effects, from which only the fourth is slightly significant at the 10%-level. According to the AIC, the model with the polynomial of fourth order is the most appropriate one, saying that a coalition government spends 3.29% per cent more than a single-party government which can be interpreted as a small evidence supporting the theoretical hypothesis. This might be due to the fact that the total expenditures consist of different types of expenditures. For some of them, the theoretical relationship might hold, while for others, this might not be the case.

To assess which characteristics of an expenditure type might lead to a violation of the Law of 1/n, Primo and Snyder(2008) point out the distinction between full and partial cost sharing and show that under partial cost sharing, the Law of 1/n need not hold for spending on projects and the effect of government fragmentation on fiscal outcomes is ambiguous: even a reverse Law of 1/n, that is, a negative relationship between the degree of fragmentation and spending, can hold. Full cost sharing means that a good provided for one specific interest group is completely financed from a common pool, while partial cost sharing implies that the interest group to which the good is targeted finances a part of it on its own. In local governments in NRW, it seems likely that for many goods partial cost sharing applies, because municipalities are required to first rely on user fees to finance themselves and only in a second step raise taxes. Thus, interest groups have to pay at least partly for the services they use.

Under the assumption of partial cost sharing, Primo and Snyder(2008) show that the effect of the degree of fragmentation on spending may be positive, constant or negative, depending among other things on the curvature of the benefit function and congestion,

i.e. the "publicness" of the provided good, with the two extremes of a pure public and a pure private good. Other things being equal, the more public a good is, the more likely it is that the Law of 1/n does not hold. Moreover, the smaller the deviation from full cost sharing, the higher has to be the degree of congestion for the Law of 1/n not to hold. Since different types of expenditures might exhibit a different degree of congestion and cost sharing, it makes sense to take a closer look at subcategories.

As a first expenditure type, we regard personnel expenditures as one part of current expenditures. Results can be found in Table 5(with clustering of the standard errors) and Table 6(without clustering as a robustness check). In column 1 of table 5, we see that the naïve OLS estimation leads to a very small negative insignificant treatment effect, while using municipality-fixed effects yields a positive treatment effect which is close to zero. Looking at the RDD results and using different degrees of the polynomial in the seat share of the strongest party leads (with exception of the polynomial of first degree) to relatively high negative treatment effects from which the ones derived with the polynomial of third and fourth order, respectively, are significant. Without clustering of the standard errors, all of the treatment effects in the last four columns are significant. According to the AIC, we should select the model with the polynomial of third degree, leading to a treatment effect that is significant at the 5%-level and thus supports the theoretical hypothesis. Comparing Table 5 with Table 6, the importance of clustering gets obvious: Standard errors that are not clustered seem to be grossly understated.

The results show that simple OLS estimates, even with clustered standard errors, yield totally different conclusions: The naïve OLS estimate suggests that the theoretical model of Weingast et al.(1981) does not hold and the type of government does not matter for spending decisions. But using more sophisticated econometric techniques shows that

there is an evidence that the type of government can matter, at least in the case of personnel expenditures. How can we interpret this finding? In light of the findings of Primo and Snyder(2008), for personnel expenditures it might be the case that full cost sharing applies such that the Law of 1/n has to hold. For most public employment, it seems to be unrealizable to let citizens directly pay for the services they use.

The second type of expenditures that belong to the group of current expenditures is material spending. Results are shown in Table 7(with clustered standard errors) and Table 8(without clustered standard errors). In Table 7, we see that the simple OLS estimate is not in line with the theoretical prediction: It takes a very small negative value and is insignificant. Using fixed effects, however, leads to a negative treatment effect that is significant at the 5%-level. Thus, the simple OLS estimate seems to have neglected time-invariant variables that are both correlated with spending and the type of government. Using the RDD leads to estimates that are larger in magnitude than the simple OLS estimate, but still very small and never significant at conventional levels. Moreover, the treatment effect is robust across different specifications of the polynomial. According to the AIC, we should prefer the model with the polynomial of fifth degree. Except for the polynomial of first degree, using Huber-White standard errors yields no different conclusions.

The expenditure type material spending contains positions of relatively small magnitude such that it seems reasonable to believe that benefits from material spending only accrue to a small group of users. Therefore, it might be the case that for this expenditure type, only partial cost sharing applies which might be the explanation for why the theoretical prediction of the Weingast et al.(1981) model does not hold. Although there seems to exist a correlation between the type of government and spending as indicated by the

fixed effect OLS estimate, this effect disappears in all RDD specifications. Thus, there seems to be no causal effect of the type of government on material spending. We can rather conclude that there are unobserved, time-variant third factors that influence both spending and the type of government. For example, voter preferences for spending might be reflected in the number of parties in government.

Table 9 and 10 display the results for investment expenditures. The naïve OLS estimate suggests that there is a strong correlation between the type of government and capital expenditures: single-party governments spend approximately 7.5% *more* than coalition governments. This effect is significant at the 1%-level. A possible interpretation of this finding might be that investment projects, which are relatively large in magnitude, require consensus and stability, which is more the case in single-party governments. Moreover, the simple OLS estimate does not take into account the seat share of the ruling party. This result might be due to single-party governments that have gained so much seats in the last election that they do not fear to be re-elected and thus are more willing to investment in projects of large magnitude. In contrast, the RDD estimate takes this into account by putting the more weight on an outcome the closer an election was (see Lee, 2008).

As a consequence, the OLS estimate might be, for example, due to voter preferences: voters might wish for a single-party government because they want large investment projects to be realized. Including municipality-fixed effects to eliminate time-invariant omitted variables shows that the treatment effect is smaller as in column 1, indicating that the simple OLS estimate again overstates this effect. However, the treatment effect is still significant at the 5%-level.

Using the RDD approach with a polynomial of first order, the treatment effect is no longer significant. Comparing the results across polynomial specifications from first to fifth order, we see that the treatment effect strongly varies and even takes negative values. It is only slightly significant at the 10%-level for the polynomial of fourth order. Again, this result suggests that the naïve estimation procedure(even with fixed effects and clustered standard errors) leads to different conclusions than an econometric strategy that accounts for the possible endogeneity of the type of government. The bottom line of these results is that there seems to be no clear evidence for an effect of the type of government on investment expenditures.

This result is in line with the result of Pettersson-Lidbom(2012) who uses a similar econometric technique and measures government fragmentation in the form of council size: For Finnish local governments, he also finds no significant effect for capital expenditures, but varying signs of the treatment effect depending on the polynomial specification. In the light of the theoretical results of Primo and Snyder(2008), a possible explanation for the result that the Law of 1/n does not hold could be that investment goods provided by the municipality might often be very close to pure public goods and therefore even with a small deviation from full cost sharing, the Law of 1/n need not hold. It might be the case that investment expenditures are more close to full cost sharing than material spending, because it might be more difficult to charge fees for large projects. Still, it should also be the case that large investment expenditures are more close to a pure public good than material spending. In general, it seems worthwhile to investigate in how far cost sharing applies and whether there is a positive correlation between the degree of cost sharing and the "publicness" of the provided goods in German municipalities.

4.2 Robustness checks

As argued above, a concern about the robustness of our findings is that the treatment status is the same during one legislative term, whereas the outcome variable varies each year. To check the validity of the results derived without accounting for this fact when calculating the standard errors, we present estimates of equation (2) for the four outcome variables in the Tables 11-14(each with clustering of the standard errors at the municipality level to account for serial correlation). For this, we use the mean of the population size as well as of the proportion of people aged under 15 and above 65 for one legislative term. We see that estimating equation (2) does not change the results substantially and the overall conclusion mostly stay the same. Only the effect of the type of government on investment expenditures in the RDD specification with a polynomial of fourth changes from being slightly significant to non-significant at conventional levels. By contrast, clustering the standard errors to account for possible serial correlation changed the standard errors and thus the significance of the results strongly as we have seen in the last section. Standard errors seem to be grossly understated when we do not take the possible serial correlation into consideration.

5. Conclusion

In this paper, we estimate the causal effect of the form of government(coalition vs. single-party government) on government spending using community level data from Germany. In the existing political economy literature, it has been often claimed that the type of government matters for government spending. While our simple OLS and fixed effect estimates indicate highly significant effects of the type of government on material spending and investment expenditures but not on personnel expenditures, using a regression discontinuity design which takes the endogeneity of the type of government into account shows that the reverse seems to be the case: The form of government seems

to have a significant influence on personnel expenditures but not on material spending and investment expenditures. We combine the RDD with fixed effects and clustered standard errors. This is the first empirical study on this topic that combines these factors. Our results indicate that only using fixed effects as done in past research does not solve the problems of reverse causality and omitted variable bias.

In light of the contribution by Primo and Snyder(2008), our results suggest that the validity of the predictions of the Weingast et al.(1981) model depends on the characteristics of the specific expenditure type that is considered, for example on the degree of cost sharing that is applied and on the publicness of the provided good. Primo and Snyder(2008) show that the Law of 1/n for total spending on projects must hold in the case of full cost sharing, while it need not hold if only partial cost sharing is applied. It seems likely that for personnel expenditures, partial cost sharing is not realizable such that the Law of 1/n must hold. This might explain our results. For material spending and investment expenditures, municipalities might rely on charging user fees such that only partial cost sharing is applied. Our results thus also suggest to take into consideration how political units finance themselves as well as to consider different types of expenditures with presumably different degrees of cost sharing when testing for the effect of the type of government. In past research, only Perotti and Kontopoulos(2002) have used different types of expenditures: They find that only for transfers, the number of parties has an influence on expenditures. As transfers are not financed by the group who benefits from them but rather by the whole society, their result also fits into the framework of Primo and Snyder(2008). Thus, in future research the revenue side of the political unit should be taken into consideration when testing for the Law of 1/n.

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Tables

Table 1: Election Periods and Election Results

Election Period	Number of single-party governments	Number of coalition governments	Fraction of coalition governments
1984-1989	254	142	35.86%
1989-1994	196	200	50.51%
1994-1999	185	211	52.28%
1999-2004	194	202	51.01%

Table 2: Summary statistics

	Mean	Std. Deviation	Minimum	Maximum
Total expenditures per capita	2003.40	483.74	1038.63	6608.72
Personnel expenditures per capita	372.80	122.07	147.95	972.88
Material spending per capita	295.91	80.16	88.54	1244.37
Investment expenditures per capita	278.80	148.15	3.92	2556.35
Seat share of the strongest party	51.51	7.51	33.33	85.71
Population size	44305.37	87222.85	3752	968639
Proportion of people below 15	0.1738	0.0208	0.1125	0.2642
Proportion of people above 65	0.1477	0.0243	0.0787	0.2587

Note: Expenditure variables are expressed in constant 2005 prices.

Table 3: Single-party effect for total expenditures(clustered standard errors)

	OLS	OLS	RDD	RDD	RDD	RDD	RDD
Single-party effect	0.0095 (0.0106)	0.0042 (0.0079)	0.0136 (0.0098)	0.0145 (0.0124)	-0.0102 (0.0157)	-0.0329* (0.0199)	-0.0269 (0.0249)
Degree of polynomial in seat share	None	None	First	Second	Third	Fourth	Fifth
Municipality-fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
AIC	-4850.07	-10987.61	-11004.39	-11001.03	-11022	-11032.36	-11029.38
Number of observations	7920	7920	7920	7920	7920	7920	7920

Notes: Each column is a separate regression. The dependent variable is log per capita spending. The data set consists of 396 municipalities over the period 1985-2004. Standard errors clustered at the municipality-level are reported in parentheses. All regressions include population size, the proportion of people aged below 15 and above 65 and the identity of the party with the most seats as control variables. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

Table 4: Single-party effect for total expenditures(unclustered standard errors)

	OLS	OLS	RDD	RDD	RDD	RDD	RDD
Single-party effect	0.0095** (0.0042)	0.0042 (0.0041)	0.0136** (0.0055)	0.0145** (0.0070)	-0.0102 (0.0089)	-0.0329*** (0.0109)	-0.0269** (0.0134)
Degree of polynomial in seat share	None	None	First	Second	Third	Fourth	Fifth
Municipality-fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
AIC	-4850.07	-10987.61	-11004.39	-11001.03	-11022	-11032.36	-11029.38
Number of observations	7920	7920	7920	7920	7920	7920	7920

Notes: Each column is a separate regression. The dependent variable is log per capita spending. The data set consists of 396 municipalities over the period 1985-2004. Heteroscedasticity-robust standard errors are reported in parentheses. All regressions include population size, the proportion of people aged below 15 and above 65 and the identity of the party with the most seats as control variables. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level

Table 5: Single-party effect for personnel expenditures(clustered standard errors)

	OLS	OLS	RDD	RDD	RDD	RDD	RDD
Single-party effect	-0.0011 (0.0142)	0.0006 (0.0071)	0.00003 (0.0089)	-0.0135 (0.0113)	-0.0347** (0.0135)	-0.0282* (0.0164)	-0.0217 (0.0196)
Degree of polynomial in seat share	None	None	First	Second	Third	Fourth	Fifth
Municipality-fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
AIC	-2655.16	-16564.78	-16569.92	-16584.74	-16611.16	-16610.9	-16110.31
Number of observations	7920	7920	7920	7920	7920	7920	7920

Notes: Each column is a separate regression. The dependent variable is log per capita personnel expenditures. The data set consists of 396 municipalities over the period 1985-2004. Standard errors clustered at the municipality-level are reported in parentheses. All regressions include population size, the proportion of people aged below 15 and above 65 and the identity of the party with the most seats as control variables. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

Table 6: Single-party effect for personnel expenditures(unclustered standard errors)

	OLS	OLS	RDD	RDD	RDD	RDD	RDD
Single-party effect	-0.0011 (0.0048)	0.0006 (0.0029)	0.00003 (0.0038)	-0.0135*** (0.0050)	-0.0347*** (0.0063)	-0.0282*** (0.0076)	-0.0217** (0.0094)
Degree of polynomial in seat share	None	None	First	Second	Third	Fourth	Fifth
Municipality-fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
AIC	-2655.16	-16564.78	-16569.92	-16584.74	-16611.16	-16610.9	-16110.31
Number of observations	7920	7920	7920	7920	7920	7920	7920

Notes: Each column is a separate regression. The dependent variable is log per capita personnel expenditures. The data set consists of 396 municipalities over the period 1985-2004. Heteroscedasticity-robust standard errors are reported in parentheses. All regressions include population size, the proportion of people aged below 15 and above 65 and the identity of the party with the most seats as control variables. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

Table 7: Single-party effect for material spending(clustered standard errors)

	OLS	OLS	RDD	RDD	RDD	RDD	RDD
Single-party effect	-0.0032 (0.0135)	-0.0249** (0.0102)	-0.0194 (0.0120)	-0.0066 (0.0156)	-0.0093 (0.0197)	-0.0052 (0.0245)	-0.0062 (0.0312)
Degree of polynomial in seat share	None	None	First	Second	Third	Fourth	Fifth
Municipality-fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
AIC	-2212.31	-9619.90	-9619.02	-9626.12	-9623.15	-9627.09	-9630.61
Number of observations	7920	7920	7920	7920	7920	7920	7920

Notes: Each column is a separate regression. The dependent variable is log per capita material spending. The data set consists of 396 municipalities over the period 1985-2004. Standard errors clustered at the municipality-level are reported in parentheses. All regressions include population size, the proportion of people aged below 15 and above 65 and the identity of the party with the most seats as control variables.*Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

Table 8: Single-party effect for material spending(unclustered standard errors)

	OLS	OLS	RDD	RDD	RDD	RDD	RDD
Single-party effect	-0.0032 (0.0050)	-0.0249*** (0.0044)	-0.0194*** (0.0060)	-0.0066 (0.0077)	-0.0093 (0.0097)	-0.0052 (0.0119)	-0.0062 (0.0146)
Degree of polynomial in seat share	None	None	First	Second	Third	Fourth	Fifth
Municipality-fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
AIC	-2212.31	-9619.90	-9619.02	-9626.12	-9623.15	-9627.09	-9630.61
Number of observations	7920	7920	7920	7920	7920	7920	7920

Notes: Each column is a separate regression. The dependent variable is log per capita material spending. The data set consists of 396 municipalities over the period 1985-2004. Heteroscedasticity-robust standard errors are reported in parentheses. All regressions include population size, the proportion of people aged below 15 and above 65 and the identity of the party with the most seats as control variables. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

Table 9: Single-party effect for investment expenditures(clustered standard errors)

	OLS	OLS	RDD	RDD	RDD	RDD	RDD
Single-party effect	0.0749*** (0.0226)	0.0483** (0.0241)	0.0447 (0.0303)	0.0215 (0.0396)	-0.0508 (0.0481)	-0.1001* (0.0607)	-0.0953 (0.0734)
Degree of polynomial in seat share	None	None	First	Second	Third	Fourth	Fifth
Municipality-fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
AIC	10623.65	7486.91	7490.47	7491.65	7471.84	7469.74	7473.58
Number of observations	7920	7920	7920	7920	7920	7920	7920

Notes: Each column is a separate regression. The dependent variable is log per capita investment expenditures. The data set consists of 396 municipalities over the period 1985-2004. Standard errors clustered at the municipality-level are reported in parentheses. All regressions include population size, the proportion of people aged below 15 and above 65 and the identity of the party with the most seats as control variables. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

Table 10: Single-party effect for investment expenditures(unclustered standard errors)

	OLS	OLS	RDD	RDD	RDD	RDD	RDD
Single-party effect	0.0749*** (0.0112)	0.0483** (0.0131)	0.0447** (0.0175)	0.0215 (0.0226)	-0.0508* (0.0287)	-0.1001*** (0.0350)	-0.0953** (0.0429)
Degree of polynomial in seat share	None	None	First	Second	Third	Fourth	Fifth
Municipality-fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
AIC	10623.65	7486.91	7490.47	7491.65	7471.84	7469.74	7473.58
Number of observations	7920	7920	7920	7920	7920	7920	7920

Notes: Each column is a separate regression. The dependent variable is log per capita investment expenditures. The data set consists of 396 municipalities over the period 1985-2004. Heteroscedasticity-robust standard errors are reported in parentheses. All regressions include population size, the proportion of people aged below 15 and above 65 and the identity of the party with the most seats as control variables. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

Table 11: Single-party effect for total expenditures per legislative term

	OLS	OLS	RDD	RDD	RDD	RDD	RDD
Single-party effect	0.0102 (0.0106)	0.0039 (0.0079)	0.0128 (0.0098)	0.0140 (0.0125)	-0.0113 (0.0159)	-0.0344* (0.0201)	-0.0271 (0.0251)
Degree of polynomial in seat share	None	None	First	Second	Third	Fourth	Fifth
Municipality-fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
AIC	-1380.03	-3321.71	-3326.14	-3322.49	-3329.44	-3331.63	-3328.19
Number of observations	1584	1584	1584	1584	1584	1584	1584

Notes: Each column is a separate regression. The dependent variable is log per capita expenditures in each legislative term. The data set consists of 396 municipalities over the 4 legislative terms 1984-1989,1989-1994, 1994-1999 and 1999-2004. Standard errors clustered at the municipality-level are reported in parentheses. All regressions include population size, the proportion of people aged below 15 and above 65 and the identity of the party with the most seats as control variables. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

Table 12: Single-party effect for personnel expenditures per legislative term

	OLS	OLS	RDD	RDD	RDD	RDD	RDD
Single-party effect	-0.0010 (0.0142)	-0.0016 (0.0071)	-0.0013 (0.0090)	-0.0148 (0.0114)	-0.0365*** (0.0137)	-0.0302* (0.0167)	-0.0233 (0.0200)
Degree of polynomial in seat share	None	None	First	Second	Third	Fourth	Fifth
Municipality-fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
AIC	-612.08	-3840.59	-3839.05	-3840.29	-3845.23	-3842.38	-3839.25
Number of observations	1584	1584	1584	1584	1584	1584	1584

Notes: Each column is a separate regression. The dependent variable is log per capita personnel expenditures in each legislative term. The data set consists of 396 municipalities over the 4 legislative terms 1984-1989,1989-1994, 1994-1999 and 1999-2004. Standard errors clustered at the municipality-level are reported in parentheses. All regressions include population size, the proportion of people aged below 15 and above 65 and the identity of the party with the most seats as control variables.*Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

Table 13: Single-party effect for material spending per legislative term

	OLS	OLS	RDD	RDD	RDD	RDD	RDD
Single-party effect	-0.0029 (0.0136)	-0.0255** (0.0102)	-0.0196 (0.0120)	-0.0067 (0.0157)	-0.0100 (0.0198)	-0.0050 (0.0244)	-0.0065 (0.0309)
Degree of polynomial in seat share	None	None	First	Second	Third	Fourth	Fifth
Municipality-fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
AIC	-678.19	-2669.31	-2666.53	-2666.18	-2662.58	-2661.56	-2660.19
Number of observations	1584	1584	1584	1584	1584	1584	1584

Notes: Each column is a separate regression. The dependent variable is log per capita material spending in each legislative term. The data set consists of 396 municipalities over the 4 legislative terms 1984-1989,1989-1994, 1994-1999 and 1999-2004. Standard errors clustered at the municipality-level are reported in parentheses. regressions include population size, the proportion of people aged below 15 and above 65 and the identity of the party with the most seats as control variables.*Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

Table 14: Single-party effect for investment expenditures per legislative term

	OLS	OLS	RDD	RDD	RDD	RDD	RDD
Single-party effect	0.0737*** (0.0220)	0.0510** (0.0235)	0.0443 (0.0297)	0.0270 (0.0391)	-0.0528 (0.0474)	-0.0928 (0.0603)	-0.0890 (0.0734)
Degree of polynomial in seat share	None	None	First	Second	Third	Fourth	Fifth
Municipality-fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
AIC	1359.04	196.53	200.13	203.42	195.01	197.17	201.16
Number of observations	1584	1584	1584	1584	1584	1584	1584

Notes: Each column is a separate regression. The dependent variable is log per capita investment expenditures in each legislative term. The data set consists of 396 municipalities over the 4 legislative terms 1984-1989,1989-1994, 1994-1999 and 1999-2004. Standard errors clustered at the municipality-level are reported in parentheses. regressions include population size, the proportion of people aged below 15 and above 65 and the identity of the party with the most seats as control variables.*Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

Figures

Figure 1: Histogram of the assignment variable