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Elected or Appointed? How the Nomination Scheme of the City Manager Influences the Effects of Government Fragmentation

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Elected or Appointed? How the Nomination Scheme of the City Manager Influences the Effects of Government Fragmentation

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Abstract:

Empirical work on the causal effect of government fragmentation finds diversified results. This might be explained by the fact that studies typically are settled in different institutional environments. To assess in how far the political system might shape the effects of fragmentation, this paper measures the causal effect of a change in the nomination scheme of the city manager on the council size effect. I utilize a combination of a Regression Discontinuity Design with a Difference-in-Difference Approach applied to a large panel data set of German municipalities. The paper finds that when the manager is appointed by the council, there is no significant council size effect, while there is negative effect when the manager is elected by the voters. This indicates that the political system indeed matters.

Key words: Government fragmentation, Council Size, Local government, City manager, Regression Discontinuity Design, Form of municipal government

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

1. Introduction

Empirical work that investigates the causal effect of government fragmentation on public spending behavior finds very diversified effects.¹ While most earlier studies find positive or insignificant effects (see, e.g., Roubini and Sachs, 1989; Bradbury and Crain, 2001; Perotti and Kontopoulos, 2002; Schaltegger and Feld, 2009), a recent paper (Pettersson-Lidbom, 2012) even finds significant negative effects. As a striking example for the diversity of the results found in the existing literature, both Egger and Köthenbürger (2010) as well as Pettersson-Lidbom (2012) utilize council size laws and apply a Regression Discontinuity Design (RDD) for identification, but find completely different results. This, of course, raises the question of the external validity of the results found so far. Besides the diversity of the results, a second characteristic of the existing literature is that it has measured the effect of fragmentation in completely different institutional environments. For example, institutional environments differ between single- and multiple-district jurisdictions, the form of local government (council-manager or mayor-council system) or the nomination scheme of the city manager/ mayor. Some studies even conflate data from different institutional environments. Given the above considerations, a question directly coming to one's mind is in how far the underlying institutional system is able to account for the diversity of the results found in the empirical literature.

This paper makes a step towards answering this question. To investigate in how far the underlying political system matters, the main contribution of this paper is to estimate the causal effect of a change in the underlying local government system on the effect of

¹ Also, the theoretical relationship between fragmentation and spending is ambiguous: While the model of Weingast et al. (1981) predicts a positive relationship of fragmentation on spending, Primo and Snyder (2008) theoretically derive that, depending on the degree of publicness of the provided good and the degree of cost sharing, there can even exist a negative relationship.

fragmentation.² Specifically, using the number of legislators as the measure of government fragmentation, I compare the effect of government fragmentation (i.e. the council size effect) when the city manager, the head of the public administration in the German state Hesse, is appointed by the municipal council to the case when he is elected by the voters.³

In the results, I indeed find that the effect of fragmentation differs depending on the political system in place. When the manager is appointed by the council, there is no relationship between the size of the municipal council and spending, while when the manager is elected by the voters, there is a highly significant negative council size effect, consistent with the results of Pettersson-Lidbom (2012). Thus, I find that the underlying political system might be a factor that explains why empirical studies typically find very different results.

How might these different council size effects be explained? When the manager is appointed by the council, he is completely dependent on the legislators. Assuming that the manager cares about staying in office, there will be, if any, a very restricted incentive to counteract the decisions of the council. When the manager is elected by the voters, his tenure in office should be less dependent on the council and his political position should be stronger: To secure his survival in office, he does not need a parliamentary majority behind him. This should create (or further strengthen) agency problems between the municipal council and the manager compared to the case when the manager is appointed

² An important predecessor of this paper is Baqir (2002). He compares the effects of government fragmentation in mayor-council and council-manager systems in US cities and finds that mayor-council systems break the relationship between fragmentation and spending. However, in his paper political units might self-select into specific forms of local governments such that it is questionable whether the estimated effects can be given a causal interpretation.

³ In the local constitution of Hesse, the city manager is labeled as *Bürgermeister* (mayor). However, his responsibilities clearly correspond to those of a city manager in a council-manager system whereas he lacks the competencies of the mayor in typical mayor-council system as I will discuss in the next section.

by the council. Therefore, in accordance with the explanation for negative effects of fragmentation proposed by Pettersson-Lidbom (2012), more council members are able to better monitor the public administration, thus weakening the agency problems and creating negative council size effects. To evaluate whether the different council size effects in these two different political systems are really due to agency problems, I perform the analysis also for different sub-categories of the budget as well as for local tax rates. While the above conclusions (negative council size effects when the manager is elected, no council size effects when the manager is appointed) hold for expenditure categories over which the public administration has the most discretion, they do not hold for other expenditures categories and local tax rates. This supports the proposed mechanism.

To estimate the causal effect of the nomination scheme on the relationship between fragmentation and spending, two natural experiments are necessary. First, council size is arguably not exogenous. To overcome a potential endogeneity bias, I use that in the German state Hesse, the council size is a discontinuous function of population size which allows the usage of a Regression Discontinuity Design (RDD). Second, there might be self-selection of municipalities into different nomination schemes of the manager based on unobserved municipal characteristics. However, in Hesse, there was a state-ruled switch in the nomination scheme which occurred in the 1990s. The timing of this switch was arguably not under the control of municipalities: Municipalities did not switch at a single date, but when the term of the last not publicly elected manager ended which differed across municipalities. This created a phase-in period of the new system with the length of six years, allowing to disentangle the effect of a change in the nomination scheme from general time effects. Importantly, neither the responsibilities of the

public administration nor those of the council have been changed through the reform. Only the nomination scheme of the manager has been changed.

This paper's results have important policy implications: If there indeed exist spending biases through government fragmentation, then one needs knowledge about which political system might solve or mitigate these biases.⁴ Moreover, if, as Baqir (2002) finds for a sample of US cities, mayor-council systems imply different council size effects than council-manager systems, it might be interesting to know which specific factor of such a system leads to a change in the council size effect.⁵ The direct voting of the head of the public administration is highly correlated with the existence of a mayor-council system, whereas in council-manager systems, the city manager is typically appointed by the council. Therefore, it is difficult to disentangle the change in the nomination scheme from a change in the whole administrative system. However, the unique institutional system setting in Hesse allows me to disentangle the nomination scheme from the underlying system, as there is only a change in the nomination scheme but not in the rest of the administrative system. Additionally, the results have important implications for the validity of cross-country studies in the government fragmentation literature: Finding that the council size effect differs depending on how the manager is nominated would illustrate the need to use data from a homogeneous institutional setting rather than conflating data from political units with different election systems as it is done in cross-country studies.

⁴ This is particularly true if this relationship also holds for other measures of fragmentation in local governments. Consider for example the case of government fragmentation due to coalition governments. National policy-makers are not able to determine whether a municipality is governed by a single-party or a coalition government as this is the result of the municipal elections. But if there are negative effects from one of these forms of governments, national policy-makers might be able to mitigate these negative effects by simply changing the nomination scheme of the public administration.

⁵ In the US as well as in German local governments, there are two predominant forms of local government systems: Council-manager and mayor-council systems. According to the Municipal Form of Government Survey conducted in 2011, more than 90% of all US local governments operate either under the council-manager or under the mayor-council system.

Besides the literature on government fragmentation, this paper contributes to the literature on fiscal policy determination in different forms of local governments (see, e.g., MacDonald, 2008; Egger et al., 2010; Coate and Knight, 2011). Moreover, the paper is related to a growing literature investigating the role of the public administration in the decision-making process in governments (see e.g., Alesina and Tabellini, 2007; 2008; Ting, 2012). Fourth, the paper is related to the literature in political economy that estimates causal effects by making use of variants of the Regression Discontinuity Design (see, e.g., Fujiwara, 2011; Gagliarducci et al., 2011; Tyrefors Hinnerich and Pettersson-Lidbom, 2012). To the best of my knowledge, this paper is the first to use the Regression Discontinuity Design in the context of measuring interaction effects of separate policy factors.

The rest of the paper is structured as follows. In Section 2, I describe the institutional framework in Hesse and the data set used. Section 3 presents the empirical strategy. In Section 4, I discuss the results and test their robustness. Finally, Section 5 concludes.

2. Institutional framework and data

2.1 Institutional framework

In Germany, municipalities are the lowest administrative unit of government. I have a panel data set consisting of 426 municipalities over the years 1985-2000 in Hesse which is one of the most populous German states. The years 1985-2000 consist of four legislative terms, each with a length of four years. Municipal council elections took place in 1985, 1989, 1993 and 1997 each time at the beginning of the year.

Hesse has a closed-list, proportional election system. Legislators in the municipal council are elected at large, i.e. the whole municipality forms one electoral district. Im-

portantly, due to the characteristics of the municipal election system in Hesse, excess seats are not possible. After 2000, a large reform of the election system became law: For example, the closed-list system was changed to an open-list system and the barrier to entry into the municipal council in the form of a minimum vote share was abolished. Because of this totally different institutional framework since 2000, the period of investigation ends at 2000.

The municipal constitution (*Gemeindeordnung*) in Hesse is the so-called magistrate constitution (see Elsner, 1956; Schneider, 1981; Dreßler, 2010): This local constitution can be described as one with a clear dividing line between the decision and the implementation level in the political process. The municipal council has the ultimate decision power in all affairs that concern the municipality and is responsible for monitoring the public administration whereas the decisions of the municipal council are implemented by a small magistrate (typically two members plus the manager) that leads the public administration in Hesse. The magistrate is presided by the manager and decides by majority. In case of a draw, the vote of the manager is pivotal. The manager has the right to allocate responsibilities to the other members of the magistrate. Thus, the manager can decide whether a member of the magistrate may handle a specific topic and can therefore design the working of the public administration in the way that is most compatible with his concepts. It is also important to note that the manager works full-time, while the other members of the magistrate typically work on an honorary basis. Thus, the other members are more time-constrained than the manager given that they have to do their normal jobs besides working in the public administration. This should create sizeable information advantages for the manager over the other members of the magistrate.

The size of the magistrate can be determined by the municipality on its own and in large cities, there are often more than two members besides the manager. Importantly, members of the magistrate have no political function. They are not allowed to be members of the municipal council and are employees of the municipality. Although labeled mayor, his position is equivalent to a manager in a council-manager system. Unlike all other German states after a large reform of the local government system in the 1990s, the manager in Hesse (as well as other members of the magistrate) has no voting rights in the municipal council and cannot veto against the decisions of the council.⁶ Accordingly, the decisions of the council also do not require approval by the magistrate. Thus, the magistrate is clearly not part of the decision, but of the implementation process.

However, for tasks that are necessary for the daily administration, the magistrate has some discretion over expenditures: The magistrate can freely decide to hire or dismiss municipal employees as long as this is in accordance with the employment plan of the municipal council and does not stand in contrast to the general employment policy of the council. This should give the magistrate some discretion in the case of personnel expenditures. Moreover, the magistrate does not require approval of the council for tasks that belong to the daily administration. These tasks can be defined as tasks that are executed repeatedly and are of small financial magnitude. This should give the magistrate some discretion for expenditures belonging to the expenditure type material spending given that this type consists of positions with these characteristics.

Before the reform, the members of the magistrate as well as the manager were appointed by the council with a 2/3 majority. After the reform, the manager was elected by the

⁶ An exception is given if a decision of the municipal council is probably against the law. In this case, the veto of the public administration has a delaying effect and the courts have to decide whether the decision of the council can be implemented or not.

voters in a runoff election: If no candidate receives the absolute majority of the votes in the first round of the election, the two candidates with the most votes in the first round stand for election in a second round with the winner becoming the manager. The magistrate, however, was still elected by the council after the reform. Thus, only the nomination scheme of the manager and neither the nomination scheme of the magistrate nor the responsibilities of any of the political actors changed after the reform.

The council has several possibilities to enforce his decisions: Most importantly, the council is able to remove members of the magistrate from office. Before the reform, the council could remove all members of the magistrate, including the manager, from office, while after the reform, removing the manager from office is now the right of the voters, while the council can still remove the rest of the members of the magistrate from office. Thus, the reform implied a strong decrease in the dependence of the manager on the council. This decrease leads me to expect an agency problem after the reform that could lead to the necessity of many legislators to monitor the administration.

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*). Be-

sides raising taxes and receiving grants, municipalities finance themselves through debt, fees and financial contributions.

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 fulfill the tasks belonging to the group of limited autonomy spending by law, but it is not exactly specified *how* they have to fulfill 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, the federal or state government does not restrict municipalities. Thus, they are free to decide *whether and how* they want to pursue these tasks. Examples are sports grounds, culture and economic promotion.

2.2 Data

For this paper, I use electoral, financial and population data from Hesse that originates from the Statistical Office of Hesse. In the period of investigation, there have been four legislative terms. The elections for these legislative terms were in 1985, 1989, 1993 and 1997. Elections were held at the beginning of the year. Therefore, I assume that the relevant years in which a council elected in 1985 can influence policy are 1985-1988. At

the beginning of 1989, a new council gets elected that can from then on influence policy in the years 1989-1992. The same assumption has been used for the other two legislative terms. The data from the Statistical Office of Hesse is on a yearly basis. I express all expenditures variables in per capita terms in constant 2005 prices (EUR). Moreover, all expenditure variables are expressed in logarithms, because histograms typically show a right-skewed distribution of the variables, as can be inferred from Figure 1. Therefore, the effect of the number of legislators in the council on these variables will have a percentage interpretation. I also use the multipliers of the three local tax rates the municipalities are free to set as outcome variables, but do not transform them into logarithms. Results are, however, also robust to using logarithms. Summary statistics for the main variables of this paper can be found in Table 1.

3. Identification Strategy

I am interested in the interaction between the nomination scheme of the manager and the size of the municipal council and the influence of this interaction on fiscal policy, i.e. the relationship I am interested in is given by

$$y_{it} = \alpha + \beta C_{it} + \gamma C_{it} D_{it} + \varepsilon_{it} \quad (1)$$

where y_{it} is a fiscal outcome of a municipality i in year t , C_{it} is the size of the municipal council and D_{it} is a dummy variable that is equal to one if the municipality has an elected manager and zero otherwise. For a manager appointed by the council, the council size effect I am interested in is given by β , while with an elected manager, the council size effect is given by $\beta + \gamma$. However, if one estimates equation (1) directly, the estimated parameters are likely to be biased. To solve potential problems of endogeneity and omitted variable bias, one thus needs two natural experiments that introduce exogenous variation in the terms C_{it} and $C_{it}D_{it}$, respectively.

First, in Hesse, I can use that the council size is a discontinuous function of the population size. Table 2 shows the council size law for Hesse according to paragraph 38(1) of the local constitution of Hesse. The idea of an RDD in this context is that municipalities with a population slightly below a population threshold are similar in all respects to municipalities with a population size slightly above a threshold. Any difference in outcomes between these municipalities can therefore be attributed to the council size.⁷

According to the local constitution in Hesse, the specific number of council members is determined by the **relevant population size** that the Statistical Office of Hesse had determined and published at the last regular date before the exact date of the election was scheduled by the state government.⁸ Importantly, as population size is reported by the Statistical Office of Hesse and not by the municipalities, population size should be measured without errors and local governments should not be able to strategically misreport their population size. Figure 2 shows the distribution of the relevant population size before elections. As can be seen, most municipalities have a population size below 25000, whereas there are very few municipalities with more inhabitants. In fact, only 29 of the 426 municipalities never had a relevant population size below 25000 before elections in the period of investigation. Because the RDD requires many observations near the threshold, I drop those observations with population size over 25000 and concentrate on the three discontinuities for municipalities with less than 25000 inhabitants: the 3000-, the 5000- and the 10000- threshold in the council size law.

⁷ An RDD using council size laws has recently been used by Egger and Köthenbürger (2010) and Pettersson-Lidbom (2012). For standard references on the RDD see Imbens and Lemieux (2008) and Lee and Lemieux (2011).

⁸ The corresponding population sizes for the four elections under investigation have been determined by the Statistical Office of Hesse on the following dates: June 30th, 1984; December 31st, 1987; March 31st, 1992; March 31st, 1996. Election dates have been: March 10th, 1985; March 12th, 1989; March 7th, 1993; March 2nd, 1997.

For the RDD to measure the effect of the council size law, it is necessary that there are no other changes at the 3000-, 5000- or 10000-threshold of the assignment variable (see Lee and Lemieux, 2011 and Ade and Freier, 2011 for a further discussion on this topic). In Hesse there are changes in the amount of fiscal equalization transfer at the 5000- and 10000-population threshold and in the salary of the manager at the 10000-threshold. However, the assignment variable for the salary of the manager and for the amount of fiscal equalization transfers differs from the assignment variable of the council size: The council size is determined by the **relevant population size** that is determined at specific dates before the start of the legislative term. The amount of fiscal equalization transfers and the salary of the manager are determined by the **current population size** and by the **current population size lagged one year**, respectively. Thus, at the three thresholds of the relevant population size, only the council size changes which helps us to disentangle the council size effect from other confounding policy factors.

Suppose that there were no change in the nomination scheme of the manager. Then, using the council size law, one could estimate the council size effect for the whole period of investigation by

$$y_{it} = \alpha + \beta C_{it} + \delta' \mathbf{X}_{it} + f(Pop_{it}) + \varepsilon_{it}, \quad (2)$$

where f is a polynomial function of the treatment-determining variable Pop , the **relevant population size** before an election, and \mathbf{X} is a set of potential control variables.

To generate exogenous variation in $C_{it}D_{it}$, I use that the introduction of the election of the manager was implemented gradually over six years with a timing of the first direct manager election for each municipality that is arguably independent of observable and

unobservable municipal characteristics.⁹ To be concrete, the decision of a change in the nomination scheme in Hesse evolved as follows: In 1991, the state government of Hesse asked citizens in a referendum whether the manager in municipalities in Hesse should be elected instead of appointed. The citizens of Hesse voted for an election of the manager. For the implementation of this reform which should become law from 1993 on, the state government had to deal with the following issue: The date at which the term of the last appointed manager ended differed across municipalities and has not been under control of the state or local decision-makers in the year of the change in law, 1991. Because the date differed across municipalities, it was easier for Hesse to implement such a phase-in mechanism of the new election than to let all municipalities switch to the new system at a single date (as it has been done for reforms of the local constitution in some other German states). Therefore, I can use those municipalities that have not been treated yet as counterfactual outcomes for those that already have an elected manager. I can therefore disentangle general state-wide time effects from the introduction of the elected manager.

I code the variable D_{it} as follows: It is equal to one for the first time when the elected manager is likely to be able to effectively lead the public administration and from then on stays one until the year 2000. Manager elections take place at all times in the year. If the first election of the manager takes place in the first half of the year, I assume that the elected manager is able to lead the public administration the first time in the year of the election. If the first election takes place in the second half of the year, I assume that the manager can effectively lead the administration from the next year on. For example, if the first election of the manager is in the first half of 1995, D_{it} is equal to one for the

⁹ For empirical studies also using a similar phase-in period of the switch to a mayor-council system see in a different context Egger et al. (2010, 2011) in the case of the state Lower-Saxony.

first time in 1995. If the first manager election is in the second half of 1995, D_{it} is equal to one for the first time in 1996. Table 3 shows a distribution of the 426 municipalities concerning the switch in the nomination scheme.

I estimate a combination of a Regression Discontinuity Design with a Difference-in-Difference Approach:

$$y_{it} = \alpha + \beta C_{it} + \gamma C_{it} D_{it} + \delta' \mathbf{X}_{it} + \rho D_{it} + f(\text{Pop}_{it}) + f(\text{Pop}_{it}) D_{it} + \lambda_t + \varepsilon_{it} \quad (3).$$

Thus, I take into account direct effects of the nomination scheme on fiscal policies by including D_{it} directly in the regression equation and allow the polynomial function of the treatment-determining covariate to differ before and after the change in the nomination scheme.¹⁰ I use polynomials ranging from the third to the fifth order as a way of investigating whether the estimated council size effect is sensitive to the choice of the control function. In addition, I estimate the council size effect only using observations that are close to the population threshold. I choose three different window sizes around the discontinuity points: ± 25 , ± 20 and ± 10 percent, respectively. In all specifications, I include year-fixed effects λ_t and the proportion of people aged below 15 and above 65 in the specific municipality as control variables.¹¹ I cluster the standard errors at the municipality level to allow for arbitrary serial correlation within a municipality (see Bertrand et al., 2004).

¹⁰ The estimated model is similar to the Difference-in-Discontinuities Design estimated by Grembi et al. (2012) and Lalive et al. (2012). However, while their motivation is to disentangle one policy factor from other confounding policy factors, my aim is to estimate the combined effect of two features of the municipal system. Accordingly, they test for interactive effects between their policy factor of interest and other confounding policy factors as the presence of such interactions would invalidate their approach. By contrast, as I am interested in a combined effect of two policy factors, interactions between these policy factors do not invalidate my approach, but are at the core of the analysis.

¹¹ I also checked the robustness of the results by additionally including the seat shares of the two most important local parties, the middle-right CDU and the middle-left SPD as well as a dummy for absolute majorities (as a proxy for single-party governments) as control variables, although these are not pre-treatment variables in a strict sense. The inclusion of these three additional control variables did not change the results and therefore I omit them to save space. The results are available from the author upon request.

Before investigating the change in the council size effect, I measure the direct causal effect of the change in the nomination scheme of the manager on the outcome variables. If the switch in the nomination scheme of the manager results in higher spending for those expenditure categories over which the public administration has the most discretion, there is evidence that the manager prefers different policies than the council and is indeed able to implement these policies after his independence from the council has increased. This is an important requirement for the existence of agency problems. To measure the causal effect of a change in the nomination scheme, I implement the following difference-in-difference estimation approach:

$$y_{it} = \mu_i + \rho D_{it} + \lambda_t + \delta' \mathbf{X}_{it} + \varepsilon_{it}$$

where μ_i is a municipality-fixed effect. For the difference-in-difference approach, I use the following covariates: council size¹², population size, the proportion of people aged below 15 and above 65, the seat shares of the two major parties, the center-left SPD and the center-right CDU, as well as a dummy variable for absolute majorities in the municipal council. The identifying assumption for the difference-in-difference approach is that the timing of the change in the nomination scheme is exogenous to observable and unobservable municipal characteristics. Evidence that this assumption holds is presented in Section 4.3.

¹² Note that I allow the coefficient on the council size to differ before and after the reform as the analysis below clearly shows that this is appropriate. This way, I am able to measure the effect that is due to a change in the nomination scheme without confounding it with the effect that is due to a changing relationship between fragmentation and fiscal policies after the change in the nomination scheme.

4. Results

4.1 The direct effect of a change in the nomination scheme

Table 4 presents the results from the difference-in-difference approach. Total expenditures, personnel expenditures and material spending significantly increase when the manager is elected instead of appointed, while most other outcome variables are not affected. Only for the multiplier of the property tax B , I find a significant negative effect. The effects for the outcome variables over which the public administration has the most discretion are highly significant and economically relevant. These results provide evidence for the interpretation that the manager has different spending priorities than the municipal council and that he can use his increased independence from the council after the change in the nomination scheme to influence these expenditure categories. These results thus point to agency problems between the public administration and the municipal council. Note again that these results are due to incentives created by the nomination scheme and cannot explained by other factors as there is only a change in the nomination scheme.

4.2 The change in the council size effect

In this section, I provide evidence on how the council size effect changes when there is a change in the nomination scheme by estimating the empirical model (3). Table 5 presents the results for the council size effect before the change in the nomination scheme of the manager (β) was introduced and after it was introduced ($\beta + \gamma$). OLS results differ depending on the outcome variable and no clear pattern emerges neither regarding the significance nor the sign of the council size effect before and after the change in the nomination scheme. The comparison with the RDD results leads to the conclusion that the OLS estimates are severely biased.

Using the RDD estimation approach from (3), for total expenditures I find no significant council size effect when the manager is appointed by the council, while after the introduction of the election of the manager, the estimates suggest that there is a significant negative council size effect. Thus, for the elected manager the results correspond to those from Pettersson-Lidbom (2012). The estimated effects are very robust across polynomial specifications. This insensitivity to the choice of the polynomial order suggests that the control function is not misspecified. Thus, a directly elected manager wants to increase spending according to the results in Section 4.1., while this increase is smaller the larger the council is. This is strong evidence in favor of the interpretation that a larger council has more possibilities to restrict the spending preferences of the manager. The manager can use this increasing independence from the council after the introduction of the direct election better if he cannot be monitored by many councilors.

To further investigate the mechanism underlying the results, I consider the council size effect for different types of expenditures. If the negative council size effect after the introduction of elections is really due to the role of the public administration, then I should expect to find these negative council size effects for types of expenditures over which the public administration has the most discretion. This should be the case for personnel expenditures and material spending, while I do not expect to find negative council size effects for capital expenditures. I indeed find highly significant negative council size effects for material spending and personnel expenditures for the period with an elected manager, but not for the period with a manager appointed by the council. The estimates for the period with an elected manager are of a larger magnitude in the case of material spending than in the case of total expenditures. For investment expenditures, I do not find any significant effect for either period. Again, the council size effects are very robust across polynomial specifications. Other outcome variables over which the

public administration has no discretion are the multipliers of the three local tax rates that municipalities are free to set. Therefore, I should not find (more) negative council size effects after the change in the nomination scheme for these outcome variables. Indeed, using the three local tax rates as outcome variables reveals no significant council size effects before and after the reform.

As a specification check, the council size effect is estimated using only observations that are close to the treatment thresholds. Tables 6-8 report the results from the ± 25 , ± 20 and ± 10 window widths around the thresholds, respectively. These results are very similar to the estimation results obtained with the full sample. Most of the results stay significant and the magnitude of the point estimates roughly stays the same. This is further evidence that the results are very robust against misspecification.

4.3 Robustness checks

In this section, I discuss the robustness of the results. A concern that may render the RDD invalid is that units strategically manipulate the assignment variable in order to self-select into the treatment or control group. While I argue that local governments do not have the possibility to misreport their population size, it may be - at least theoretically - possible that citizens strategically migrate into or out of municipalities to change the council size. Evidence for this kind of sorting would be given if there is a discontinuity in the distribution of the assignment variable at the threshold (see Lee and Lemieux, 2011). In Figure 3, I show histograms of the assignment variable at the three thresholds. These histograms do not provide any evidence for sorting around the thresholds as the number of local governments slightly below each of the thresholds is always nearly the same as the number of local governments slightly above the thresholds. More formally, I can test the null hypothesis of no discontinuity with a McCrary-test (see

McCrary, 2008). Figure 4 reports the results of the McCrary-test at the three thresholds for the relevant population size before the four elections. At none of these thresholds, the null hypothesis of no discontinuity can be rejected. Moreover, I conducted the McCrary-test separately for the relevant population size before the reform and after the reform. It might be possible that the potential benefits of a specific council size differ between the two possible nomination schemes such that sorting of political units might only occur before or after the reform. However, I do also not find any evidence for a discontinuity at the thresholds if I consider both time periods separately. Results are omitted here, but are available upon request.

A second concern with the RDD is that council size is not randomly assigned at the threshold. To test for this, I use council size as dependent variable and run a regression on the observable covariates and the control function (see Pettersson-Lidbom, 2008). I run separate regressions for the periods with either nomination scheme of the manager. If the coefficients of the covariates are significantly different from zero, this would indicate that the council size depends on some covariates and that therefore council size is not randomly assigned. Tables 9 and 10 show the results for different polynomial orders in the control function. Neither are any of the individual coefficients significantly different from zero for either period nor are the coefficients jointly significant from zero as the F-test has p-values of 0.6151 for the period with an appointed and 0.7453 for the period with an elected manager (for the control function with a fourth-order polynomial). Another strategy to test whether the treatment status is randomly assigned is to look for a discontinuity in the distribution of a covariate at the threshold. That is, I use each of the observable covariates as a dependent variable in regression (3). A significant council size effect would indicate a discontinuity in the distribution of the covariate at the threshold. As Table 11 shows, I do not find any significant treatment effect no matter

which covariate I use as dependent variable.¹³ Importantly, local randomization holds for both periods. This is evidence that there seems to be nothing like dynamic sorting in response to a change in the nomination scheme in place. Thus, the balance tests suggest that the council size seems to be as good as randomly assigned at the thresholds and provide strong evidence in favor of a causal interpretation of the results from the RDD.

Although the order in which municipalities switch to an elected manager appears to be random, another robustness check is to test whether the timing of the transition from one nomination scheme to the other is really independent of observable covariates.

Therefore, I perform a duration analysis for the transition from one system to the other from the year 1993 on. Because I have grouped the timing of the switch into yearly data, I need to estimate a discrete duration model. I estimate the discrete complementary log-log model with a fully nonparametric baseline hazard (Allison, 1982). This model is the discrete time representation of a continuous time proportional hazard model. To estimate this model, I construct a panel data set in which the dependent variable is 0 for a municipality if it has not switched yet to the new system and 1 in the year of the transition (it is then the last observation for this municipality).¹⁴ The effects of the observable covariates on the hazard of switching to the new system are shown in Table 12. In short, I do not find any significant effect of neither the population variables nor the political variables on the hazard of the transition from the system of an appointed manager to a system with an elected manager. Although I can of course not perform such a test for unobservable covariates, the fact that the observed covariates do not have any influence

¹³ In Table 11, I use a fourth-order polynomial as control function and omit other orders of the polynomial to save space. The treatment effects are also not significant if I use different orders of the polynomial. Note that I also include the political variables as covariates for the robustness checks although these are potentially not pre-determined. However, leaving them out does not change the results.

¹⁴ Practically, estimating the discrete complementary log-log model is equivalent to estimating a logit model on the newly constructed panel data set.

on the timing of the first manager election suggests that the order of the switch was indeed random.

In Section 3, I have made clear that also the population weight to calculate the fiscal need of a municipality in the fiscal equalization law in Hesse as well as the salary of the manager are discontinuous functions of the population size, but that the weight as well as the salary do not depend on the relevant population size, but on the current population size and the population size lagged by one year, respectively. Although the relevant population size is not perfectly correlated with the assignment variables for the weights in the fiscal equalization law and for the salary of the manager, the correlation between these assignment variables might take up some effects that I consider to be due to the council size. To further investigate this issue, I first include the corresponding population weights from the fiscal equalization law into the regression and investigate whether this inclusion alters the council size effect. The fiscal equalization law is depicted in Table 13. Here, I can use that the thresholds in this law are not perfectly collinear with the council size law. There are more thresholds than in the council size law and not at all thresholds of the council size law, there is also a change of the population weights in the fiscal equalization law. Second, I investigate whether the change in the salary of the manager at the 10000-threshold of the lagged population size takes up some effects that I attributed to the council size effect. To be precise, I include a dummy variable that is equal to 1 if the lagged population size is above 10000 and zero otherwise into the regression. Again, I can use that the change in the salary of the manager is not perfectly collinear with the change in the council size.

Table 14 shows the corresponding results. I restrict the attention to the case of a fourth-order polynomial of the relevant population size in the control function. Other polyno-

mial orders show very similar results. Column 1 presents the results if I include the population weights from the fiscal equalization law into the regression. The general conclusions do not change: The council size effect after the change in the nomination scheme is much larger than the council size effect before the change in the nomination scheme. Column 2 shows the results when I include the change in the salary of the manager. Again, the results do not change. The coefficient estimates stay very similar to the ones before inclusion of the dummy variable and the general conclusions stay the same. Moreover, the coefficients on the population weights are never significant, while the coefficients on the salary for the manager are sometimes significant.¹⁵ Nevertheless, the inclusion does not change the magnitude and significance of the baseline results. Thus, I can be confident that the effects that I have estimated are really due to the council size effect.

5. Conclusion

Empirical research on government fragmentation has found diversified results. One reason for the discrepancy in the results might be that empirical studies are typically settled in totally different institutional environments and some studies even conflate data from different environments. In this paper, I investigate whether a different institutional setting can indeed change the relationship between fragmentation and fiscal policies. To this end, I compare the council size effect in two different political settings: when the head of the public administration is elected by the voters and when he is appointed by the municipal council. In contrast to existing empirical studies on factors that might shape fragmentation effects, in my setting there are two natural experiments such that it is possible to give the results a causal interpretation. I indeed find that the institutional background matters for the effect of fragmentation on spending: When the manager is

¹⁵ I do not report these coefficients here due to space considerations.

appointed by the council, there is no relationship between fragmentation and spending, while there is a significant negative council size effect when the manager is elected by the voters. Moreover, the most probable explanation for these effects of fragmentation are agency problems between the public administration and the municipal council as I find significant effects for expenditure categories over which the public administration has the most discretion, but not capital expenditures or the three local tax rates that the council is free to set. To summarize, the findings thus suggest that the negative effects of fragmentation found in the recent literature do not need to be a universal phenomenon. It seems to be the case that the sign and significance of the effects of fragmentation depend on the underlying political system and the incentives created from it. The results therefore provide evidence against the validity of empirical studies (in particular cross-country studies) in which data from different political systems are pooled together. Regarding fiscal policy determination in different forms of local governments, this paper suggests that a change in the nomination scheme of the public administration (without changing any competencies) is able to create severe agency problems between the municipal council and the public administration. In future research it might be interesting to investigate in how far variants of local governments are able to create or further strengthen incentive problems between the public administration and the municipal council.

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Tables

Table 1: Summary statistics

	Mean	Std. Deviation	Min	Max
Total spending	1131.6	371.1	279.1	11248
Personnel spending	285.4	74.6	24.8	833.1
Material spending	210.2	64.7	88.3	1023.2
Investment spending	341.3	184.3	18.4	2116.3
Property Tax A	257.7	43.1	0	450
Property Tax B	227.6	41	100	380
Business Tax	308.6	26	210	400
Council size	29.6	6.9	13	37
Proportion of old, 65+	0.154	0.024	0.082	0.272
Proportion of young, 0-15	0.163	0.016	0.106	0.227
Seat share CDU	0.320	0.124	0	0.710
Seat share SPD	0.444	0.135	0	0.867
Dummy: Absolute Majority	0.450	0.497	0	1

Notes: Expenditure variables are expressed in constant 2005 prices in per capita terms. The multipliers of the tax rates are expressed in percent points. The table only contains municipalities with a relevant population size less than 25000. There are 6308 observations that fulfil this requirement.

Table 2: The council size law

Relevant Population Size (pop)	Council size
pop<=3000	15
3000<pop<=5000	23
5000<pop<=10000	31
10000<pop<=25000	37
25000<pop<=50000	45
50000<pop<=100000	59
100000<pop<=250000	71
250000<pop<=500000	81
500000<pop<=1000000	93
pop>1000000	105

Table 3: Switch in the nomination scheme

Year	Number of municipalities with D=1 for the first time
1993	45
1994	77
1995	85
1996	77
1997	63
1998	66
1999	13
Sum	426

Table 4: The direct effect of a change in the nomination scheme

	ρ
	0.0915
Log (Total Expenditures)	(0.0340)***
	0.0657
Log (Personnel exp.)	(0.0297)**
	0.1031
Log (Material spending)	(0.0385)***
	-0.0007
Log (Capital expenditures)	(0.0975)
	-4.5423
Multiplier Property Tax A	(3.8705)
	-12.7461
Multiplier Property Tax B	(5.0712)**
	-4.2133
Multiplier Business Tax	(4.0642)
Observations	6308

Notes: Expenditure variables are expressed in constant 2005 prices. The tax rate multipliers are expressed in percent points. Standard errors are clustered at the municipality level. There are 397 clusters. The sample includes all municipalities with a relevant population size below 25000 in the time period 1985-2000. All regressions include year-fixed effects, municipality-fixed effects and the control variables mentioned in the text. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

Table 5: Results from the Empirical Model

		OLS	RDD	RDD	RDD
Log(Total Expenditures)	β	0.0067 (0.0014)***	-0.0053 (0.0036)	-0.0059 (0.0036)	-0.0059 (0.0037)
	$\beta + \gamma$	0.0016 (0.0015)	-0.0080 (0.0042)*	-0.0099 (0.0043)**	-0.0098 (0.0043)**
Log(Personnel exp.)	β	0.0089 (0.0015)***	-0.0041 (0.0036)	-0.0035 (0.0036)	-0.0029 (0.0035)
	$\beta + \gamma$	0.0050 (0.00)***	-0.0090 (0.0050)*	-0.0084 (0.0050)*	-0.0081 (0.0050)
Log(Material spending)	β	0.0040 (0.0014)**	-0.0045 (0.0044)	-0.0053 (0.0044)	-0.0051 (0.0044)
	$\beta + \gamma$	-0.0018 (0.0019)	-0.0136 (0.0056)**	-0.0141 (0.0059)**	-0.0137 (0.0057)**
Log(Capital expenditures)	β	-0.0061 (0.0024)**	-0.0016 (0.0060)	-0.0023 (0.0061)	-0.0017 (0.0063)
	$\beta + \gamma$	-0.0122 (0.0033)***	-0.0088 (0.0104)	-0.0114 (0.0105)	-0.0112 (0.0106)
Multiplier Property Tax A	β	-1.3987 (0.2897)***	0.0863 (0.7491)	0.2427 (0.7576)	0.2514 (0.7630)
	$\beta + \gamma$	-1.1915 (0.2789)***	0.4048 (0.8982)	0.6315 (0.8928)	0.6395 (0.8901)
Multiplier Property Tax B	β	-0.7804 (0.2619)***	1.0126 (0.7302)	1.0954 (0.7349)	1.0803 (0.7373)
	$\beta + \gamma$	-0.3830 (0.2545)	0.2526 (0.7367)	0.4850 (0.7403)	0.4627 (0.7426)
Multiplier Business Tax	β	0.7164 (0.1690)***	-0.4059 (0.3880)	-0.3574 (0.3907)	-0.3868 (0.3953)
	$\beta + \gamma$	0.8030 (0.2027)***	0.2881 (0.5327)	-0.1503 (0.5291)	-0.1829 (0.5296)
Degree of polynomial		None	Third	Fourth	Fifth

Notes: Expenditure variables are expressed in constant 2005 prices. The tax rate multipliers are expressed in percent points. Standard errors are clustered at the municipality level. There are 397 clusters. The sample includes all municipalities with a relevant population size below 25000 in the time period 1985-2000. There are 6308 observations. All regressions include year-fixed effects and the proportion of people aged below 15 and above 65 as control variables. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

Table 6: Window size $\pm 25\%$ around the threshold

		OLS	RDD	RDD	RDD
Log(Total Expenditures)	β	0.0025 (0.0018)	-0.0068 (0.0040)*	-0.0070 (0.0041)*	-0.0066 (0.0042)
	$\beta + \gamma$	-0.0014 (0.0017)	-0.0103 (0.0051)**	-0.0099 (0.0054)*	-0.0082 (0.0056)
Log(Personnel exp.)	β	0.0046 (0.0017)***	-0.0030 (0.0037)	-0.0028 (0.0037)	-0.0030 (0.0039)
	$\beta + \gamma$	0.0019 (0.0020)	-0.0111 (0.0054)**	-0.0111 (0.0056)**	-0.0119 (0.0057)**
Log(Material spending)	β	0.0000 (0.0022)	-0.0042 (0.0046)	-0.0037 (0.0046)	-0.0038 (0.0046)
	$\beta + \gamma$	-0.0030 (0.0021)	-0.0168 (0.0062)***	-0.0150 (0.0064)**	-0.0162 (0.0066)**
Log(Capital expenditures)	β	-0.0045 (0.0030)	0.0004 (0.0067)	0.0007 (0.0068)	0.0009 (0.0074)
	$\beta + \gamma$	-0.0087 (0.0040)**	0.0000 (0.0122)	0.0014 (0.0127)	0.0074 (0.0134)
Multiplier Property Tax A	β	-0.9219 (0.3736)**	-0.2472 (0.8949)	-0.3104 (0.9076)	-0.1111 (0.9414)
	$\beta + \gamma$	-0.8015 (0.3641)**	0.7981 (1.0379)	0.4890 (1.0720)	0.4304 (1.0767)
Multiplier Property Tax B	β	-0.4545 (0.3555)	0.0605 (0.8367)	0.0504 (0.8450)	0.1921 (0.8855)
	$\beta + \gamma$	-0.3546 (0.3452)	-0.4414 (0.9037)	-0.3105 (0.9051)	-0.5141 (0.9494)
Multiplier Business Tax	β	0.3672 (0.2109)*	-0.5301 (0.4398)	-0.5293 (0.4415)	-0.4191 (0.4595)
	$\beta + \gamma$	0.3194 (0.2473)	0.0576 (0.5298)	0.0141 (0.5391)	-0.0999 (0.5521)
Degree of polynomial		None	Third	Fourth	Fifth

Notes: Expenditure variables are expressed in constant 2005 prices. The tax rate multipliers are expressed in percent points. Standard errors are clustered at the municipality level. There are 298 clusters. The sample includes all municipalities with a population size in the $\pm 25\%$ window around the three thresholds. There are 4204 observations. All regressions include year-fixed effects and the proportion of people aged below 15 and above 65 as control variables. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

Table 7: Window size $\pm 20\%$ around the threshold

		OLS	RDD	RDD	RDD
Log(Total Expenditures)	β	0.0029 (0.0019)	-0.0067 (0.0040)*	-0.0067 (0.0041)*	-0.0064 (0.0042)
	$\beta + \gamma$	-0.0011 (0.0018)	-0.0093 (0.0053)*	-0.0094 (0.0054)*	-0.0077 (0.0056)
Log(Personnel exp.)	β	0.0053 (0.0018)***	-0.0031 (0.0037)	-0.0030 (0.0037)	-0.0026 (0.0039)
	$\beta + \gamma$	0.0016 (0.0021)	-0.0117 (0.0056)**	-0.0122 (0.0056)**	-0.0125 (0.0057)**
Log(Material spending)	β	0.0006 (0.0023)	-0.0028 (0.0046)	-0.0027 (0.0046)	-0.0037 (0.0046)
	$\beta + \gamma$	-0.0027 (0.0021)	-0.0159 (0.0064)**	-0.0147 (0.0065)**	-0.0148 (0.0065)**
Log(Capital expenditures)	β	-0.0034 (0.0032)	0.0034 (0.0069)	0.0036 (0.0069)	0.0022 (0.0074)
	$\beta + \gamma$	-0.0060 (0.0041)	0.0027 (0.0126)	0.0032 (0.0129)	0.0094 (0.0135)
Multiplier Property Tax A	β	-0.7479 (0.3883)*	-0.4319 (0.9033)	-0.4231 (0.9071)	-0.2153 (0.9309)
	$\beta + \gamma$	-0.5532 (0.3836)	0.8168 (1.0611)	0.7617 (1.0723)	0.4215 (1.0759)
Multiplier Property Tax B	β	-0.5839 (0.3729)	0.0165 (0.8539)	0.0291 (0.8568)	0.1827 (0.8809)
	$\beta + \gamma$	-0.5157 (0.3632)	-0.3568 (0.9232)	-0.3262 (0.9163)	-0.6830 (0.9329)
Multiplier Business Tax	β	0.2920 (0.2287)	-0.6594 (0.4390)	-0.6631 (0.4393)	-0.5010 (0.4510)
	$\beta + \gamma$	0.2763 (0.2654)	-0.0235 (0.5413)	0.0268 (0.5378)	-0.1312 (0.5526)
Degree of polynomial		None	Third	Fourth	Fifth

Notes: Expenditure variables are expressed in constant 2005 prices. The tax rate multipliers are expressed in percent points. Standard errors are clustered at the municipality level. There are 269 clusters. The sample includes all municipalities with a population size in the $\pm 20\%$ window around the three thresholds. There are 3460 observations. All regressions include year-fixed effects and the proportion of people aged below 15 and above 65 as control variables. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

Table 8: Window size $\pm 10\%$ around the threshold

		OLS	RDD	RDD	RDD
Log(Total Expenditures)	β	0.0019 (0.0025)	-0.0052 (0.0041)	-0.0060 (0.0043)	-0.0060 (0.0043)
	$\beta + \gamma$	-0.0035 (0.0025)	-0.0061 (0.0057)	-0.0069 (0.0059)	-0.0068 (0.0058)
Log(Personnel exp.)	β	0.0049 (0.0024)**	-0.0019 (0.0041)	-0.0015 (0.0041)	-0.0013 (0.0042)
	$\beta + \gamma$	-0.0009 (0.0034)	-0.0109 (0.0064)*	-0.0112 (0.0068)*	-0.0112 (0.0068)
Log(Material spending)	β	0.0026 (0.0029)	-0.0004 (0.0048)	0.0013 (0.0047)	0.0013 (0.0047)
	$\beta + \gamma$	-0.0015 (0.0029)	-0.0130 (0.0069)*	-0.0147 (0.0067)**	-0.0146 (0.0067)**
Log(Capital expenditures)	β	-0.0031 (0.0047)	0.0008 (0.0074)	-0.0002 (0.0077)	-0.0003 (0.0077)
	$\beta + \gamma$	-0.0064 (0.0067)	0.0107 (0.0154)	0.0041 (0.0152)	0.0042 (0.0152)
Multiplier Property Tax A	β	-1.2784 (0.5180)**	-0.6249 (0.9809)	-0.7592 (0.9442)	-0.7567 (0.9446)
	$\beta + \gamma$	-0.4182 (0.5720)	0.5113 (1.1640)	0.7942 (1.2041)	0.7663 (1.1914)
Multiplier Property Tax B	β	-0.6996 (0.5233)	0.2624 (0.9493)	0.4290 (0.9194)	0.4781 (0.9084)
	$\beta + \gamma$	-0.3784 (0.4916)	-1.0288 (0.9693)	-1.0684 (1.0024)	-1.0984 (0.9891)
Multiplier Business Tax	β	-0.0657 (0.3010)	-0.7949 (0.4549)*	-0.8391 (0.4565)*	-0.8418 (0.4594)*
	$\beta + \gamma$	0.2993 (0.3076)	0.4755 (0.5993)	0.5739 (0.6586)	0.5541 (0.6533)
Degree of polynomial		None	Third	Fourth	Fifth

Notes: Expenditure variables are expressed in constant 2005 prices. The tax rate multipliers are expressed in percent points. Standard errors are clustered at the municipality level. There are 175 clusters. The sample includes all municipalities with a population size in the $\pm 10\%$ window around the three thresholds. There are 1816 observations. All regressions include year-fixed effects and the proportion of people aged below 15 and above 65 as control variables. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level.

Table 9: Council size as dependent variable before change in nomination scheme

	(1) Council Size	(2) Council Size	(3) Council Size
Proportion of old, 65+	-0.0431 (0.0425)	-0.0416 (0.0413)	-0.0463 (0.0410)
Proportion of young, 0-15	-0.1040 (0.0649)	-0.0988 (0.0648)	-0.1017 (0.0647)
Dummy: Absolute majority	-0.0856 (0.2029)	-0.0229 (0.2075)	-0.0056 (0.2109)
Seat share CDU	-0.1172 (0.8907)	-0.3019 (0.9425)	-0.3869 (0.9779)
Seat share SPD	-0.4416 (0.9096)	-0.6637 (0.9873)	-0.7309 (1.0345)
Observations	4238	4238	4238
Degree of polynomial	Third	Fourth	Fifth
p-value F-Test	0.5584	0.6151	0.5800

Notes: Standard errors are clustered at the municipality level. There are 397 clusters. The sample includes all municipalities before the switch in the nomination scheme of the manager with a relevant population size below 25000. All regressions include year-fixed effects. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level

Table 10: Council size as dependent variable after change in nomination scheme

	(1) Council Size	(2) Council Size	(3) Council Size
Proportion of old, 65+	-0.0564 (0.0578)	-0.0669 (0.0571)	-0.0693 (0.0574)
Proportion of young, 0-15	-0.0109 (0.0859)	-0.0213 (0.0856)	-0.0164 (0.0856)
Dummy: Absolute majority	-0.1136 (0.2766)	-0.0284 (0.2814)	-0.0295 (0.2845)
Seat share CDU	-0.4797 (1.1319)	-0.5515 (1.1525)	-0.6082 (1.1806)
Seat share SPD	1.0521 (1.2058)	0.8932 (1.2929)	0.8989 (1.3377)
Observations	2070	2070	2070
Degree of polynomial	Third	Fourth	Fifth
p-value F-Test	0.7936	0.7453	0.7063

Notes: Standard errors are clustered at the municipality level. There are 393 clusters. The sample includes all municipalities after the switch in the nomination scheme of the manager with a relevant population size below 25000. All regressions include year-fixed effects. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level

Table 11: Covariates as dependent variables

	(1) Proportion of old	(2) Proportion of young	(3) Dummy: Abso- lute Majority	(4) Seat share CDU	(5) Seat share SPD
β	-0.0404 (0.0421)	-0.0331 (0.0211)	-0.0010 (0.0075)	-0.0007 (0.0024)	-0.0015 (0.0023)
$\beta + \gamma$	-0.0197 (0.0545)	0.0253 (0.0315)	0.0009 (0.0091)	-0.0004 (0.0028)	0.0030 (0.0025)
Observations	6308	6308	6308	6308	6308
R-squared	0.2732	0.3780	0.3712	0.2814	0.4601
Degree of polynomial	Fourth	Fourth	Fourth	Fourth	Fourth

Notes: Standard errors are clustered at the municipality level. There are 397 clusters. The sample includes all municipalities with a relevant population size below 25000. All regressions include year-fixed effects and the covariates not used as dependent variable as control variables. *Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level

Table 12: Results from the discrete complementary log-log model

	(1) D_{it}
Proportion old	0.0082 (0.0233)
Proportion young	0.0517 (0.0406)
Seat share SPD	0.7379 (0.5188)
Seat share CDU	-0.0336 (0.5274)
Abs. majority	-0.1392 (0.1250)
Population	-0.0000 (0.0000)
Observations	1446

Notes: Standard errors are clustered at the municipality level. There are 394 clusters. The sample includes all municipalities since 1993 that either have an appointed manager or have $D_{it} = 1$ for the first time. The baseline hazard is fully nonparametric.

*Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level

Table 13: Population weights in the fiscal equalization law

Current Population (Pop_cur)	Weight
≤ 5000	107
$5000 < \text{Pop_cur} \leq 7500$	114
$7500 < \text{Pop_cur} \leq 10000$	121
$10000 < \text{Pop_cur} \leq 15000$	124
$15000 < \text{Pop_cur} \leq 20000$	126
$20000 < \text{Pop_cur} \leq 30000$	127
$30000 < \text{Pop_cur} \leq 50000$	129
$\text{Pop_cur} > 50000$	130

Table 14: Results when controlling for confounding factors

		(1)	(2)
Log(Total Expenditures)	β	-0.0052 (0.0036)	-0.0050 (0.0037)
	$\beta + \gamma$	-0.0076 (0.0042)*	-0.0074 (0.0042)*
Log(Personnel exp.)	β	-0.0042 (0.0037)	-0.0041 (0.0037)
	$\beta + \gamma$	-0.0092 (0.0050)*	-0.0089 (0.0050)*
Log(Material spending)	β	-0.0045 (0.0045)	-0.0028 (0.0046)
	$\beta + \gamma$	-0.0136 (0.0055)**	-0.0112 (0.0055)**
Log(Capital expenditures)	β	-0.0016 (0.0061)	0.0006 (0.0063)
	$\beta + \gamma$	-0.0092 (0.0104)	-0.0059 (0.0105)
Multiplier Property Tax A	β	0.0517 (0.7567)	-0.1517 (0.7647)
	$\beta + \gamma$	0.3652 (0.9051)	0.0986 (0.8893)
Multiplier Property Tax B	β	1.0425 (0.7341)	0.9853 (0.7455)
	$\beta + \gamma$	0.2624 (0.7375)	0.1769 (0.7286)
Multiplier Business Tax	β	-0.4279 (0.3908)	-0.5705 (0.3908)
	$\beta + \gamma$	-0.2862 (0.5393)	-0.4923 (0.5297)
Degree of polynomial		Fourth	Fourth

Notes: Standard errors are clustered at the municipality level. There are 397 clusters. Column 1 includes the population weight taken from the fiscal equalization law. Column 2 includes a dummy variable for different salary categories of the city manager. There are 6308 observations.*Significant at the 10 percent level, **Significant at the 5 percent level, ***Significant at the 1 percent level

Figures

Figure 1 : Distribution of outcome variables

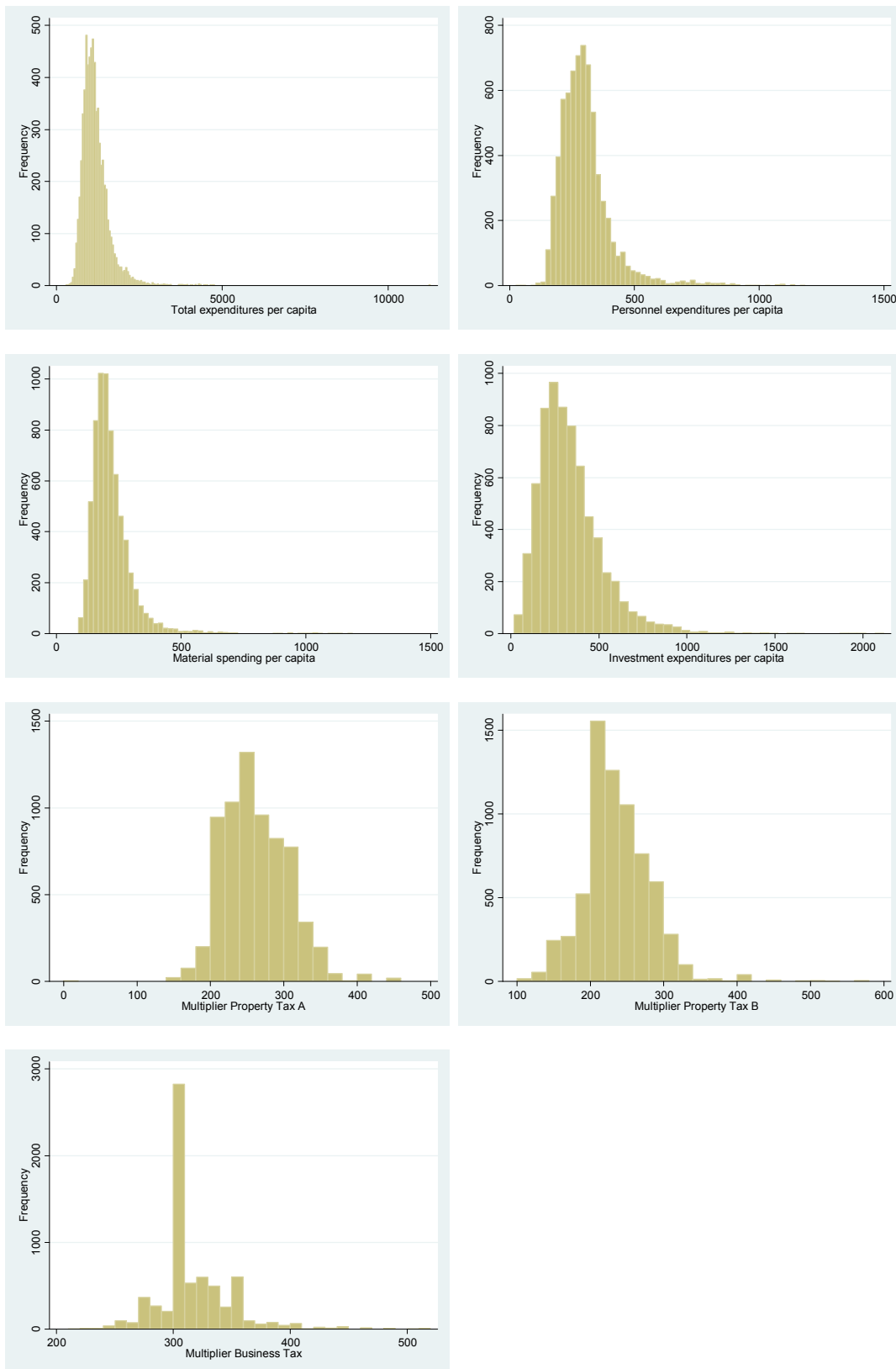
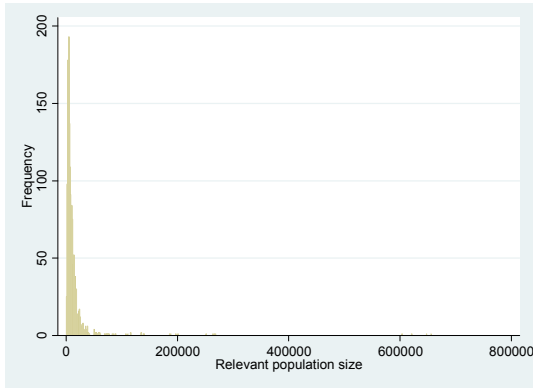
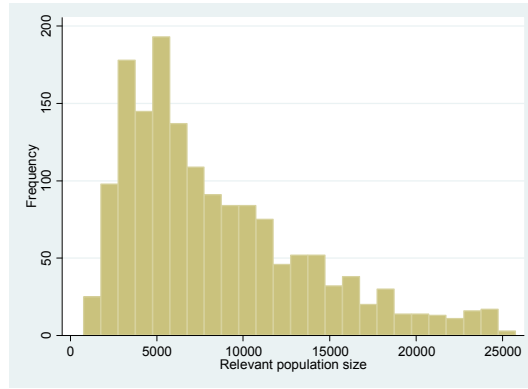


Figure 2: Histogram of relevant population size

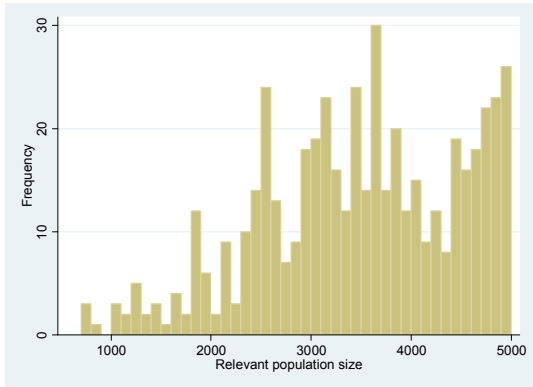


(a) All Municipalities

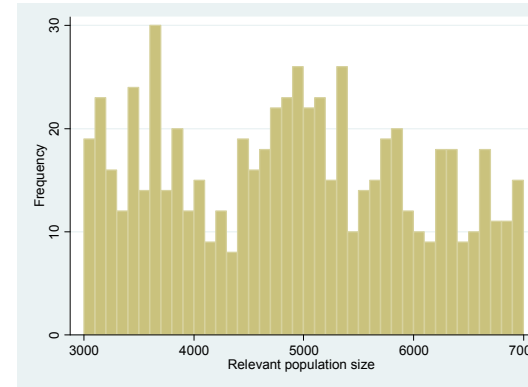


(b) Municipalities with Pop<25000

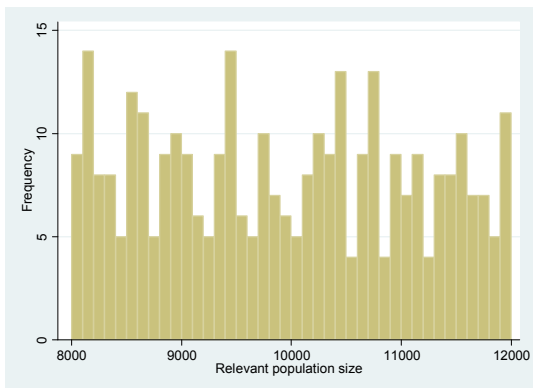
Figure 3: Histogram of relevant population size around the thresholds



(a) 3000-threshold

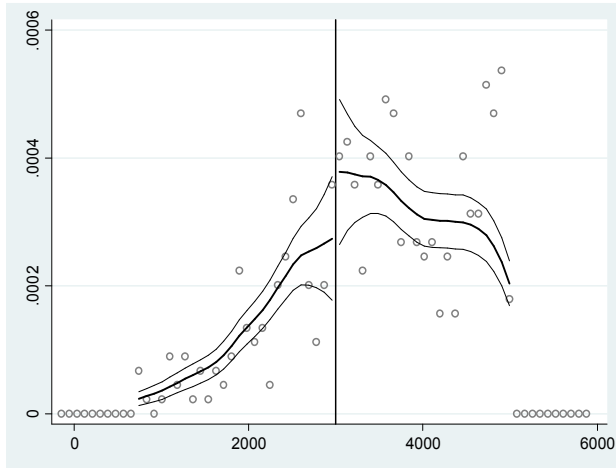


(b) 5000-threshold

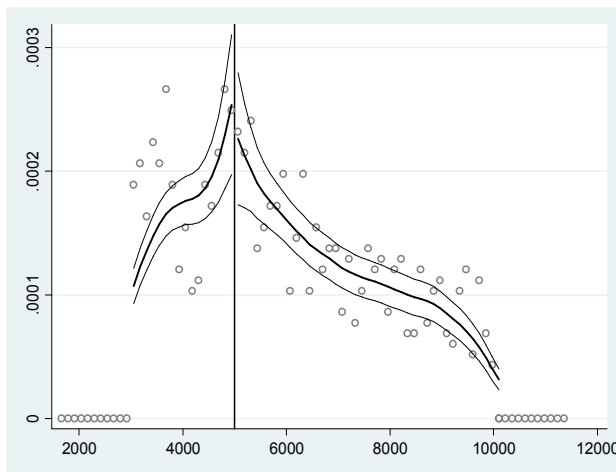


(c) 10000-threshold

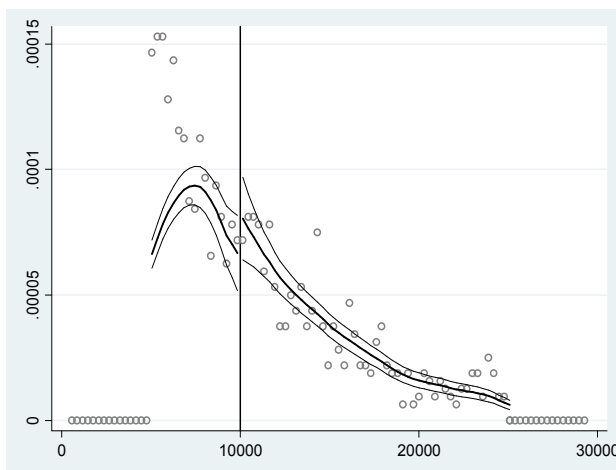
Figure 4: McCrary-test of the relevant population size for the full time period



Threshold of 3000 inhabitants. Point estimate of the discontinuity: 0.3349, se: 0.2662



Threshold of 5000 inhabitants. Point estimate of the discontinuity: -0.1356, se: 0.1821



Threshold of 10000 inhabitants. Point estimate of the discontinuity: 0.24, se: 0.1675

Notes: Weighted kernel estimation of the log density of the assignment variable, performed separately on either side of the each of the three population thresholds. The optimal bandwidth is computed as in McCrary (2008).