Public Choice
DOI 10.1007/s11127-017-0484-2

The effect of legislature size on public spending: evidence from a regression discontinuity design

Daniel Höhmann[1]

Received: 14 February 2017 / Accepted: 21 October 2017

Abstract What is the effect of legislature size on public spending? An answer to this question is provided by Weingast et al. (J Polit Econ 89(4):642–664, 1981), whose "law of 1/*n*" posits that an increase in the number of elected representatives always leads to an increase in public spending. Because elected politicians regard the tax base as a common pool from which they can finance specific projects for their constituencies, and these specific constituencies internalize the full benefits of the projects, but only bear a fraction of the costs (projects are financed from the common tax base), fiscal inefficiency will increase with the number of representatives. In this paper, I test the validity of the "law of 1/*n*" using a dataset of 9325 German municipalities between 2008 and 2010. Through the application of a regression discontinuity design, many of the methodological pitfalls of previous studies can be avoided and a valid estimation of the causal effect of legislature size on public spending for German municipalities can be determined. The results do not corroborate the positive findings of previous studies, which generally supported the implications of the "law of 1/*n*". For the years 2008–2010, I find a negative effect of legislature size on public spending in German municipal councils.

Keywords Legislature size · Public spending · Law of 1/*n* · Regression discontinuity design

JEL Classification D72 · H72 · R50

1 Introduction

Since the publication of Weingast et al.'s (1981) seminal work on costs and benefits of distributive policies, the relationship between legislature size (number of parliament members) and government spending has attracted much interest from scholars within the

✉ Daniel Höhmann
daniel.hoehmann@uni-bamberg.de

1 University of Bamberg, Feldkirchenstrasse 21, 96052 Bamberg, Germany

Published online: 28 October 2017

field of political economy (e.g., Bradbury and Crain 2001; Perotti and Kontopoulos 2002; Fiorino and Ricciuti 2007; Schaltegger and Feld 2009; Pettersson-Lidbom 2012; Egger and Koethenbuerger 2010). Based on earlier studies from Tullock (1959) and Olson (1965), Weingast et al. (1981) formalized the "law of 1/*n*", which posits that an increase in the number of elected representatives always leads to an increase in public spending. According to their theory, elected politicians regard the tax base as a common pool from which they can finance specific projects for their constituencies. Because the specific constituencies internalize the full benefits of the projects, but only bear a fraction of the costs (projects are financed from the common tax base), fiscal inefficiency will increase with the number of representatives. In other words, if n stands for the number of legislative districts and each district is represented by one delegate, the fraction of the costs falls to $1/n$, leading to the common problem of overutilization if every representative tries to approve projects that benefit his or her respective constituency (Weingast et al. 1981).

Previous studies overwhelmingly confirm the expected positive relationship between legislature size and public spending. However, from a methodological point of view, almost all reported findings and interpretations have to be called into question. Because the majority of the previous studies on the validity of the "law of 1/*n*" conducts simple regression models of public spending on the number of representatives, strong endogeneity problems may occur. In particular, those findings may be biased owing to the pitfalls of omitted confounding variables as well as the possibility of reverse causality (Acemoglu 2005; Primo and Snyder 2008).

Therefore, this paper applies a more sophisticated statistical model that is able to overcome the methodological weaknesses of previous studies and, thus, provides a valid estimation of the causal effect of legislature size on public spending for German municipal councils. Concretely, I utilize a regression discontinuity design to test the validity of the theoretical assumptions of the "law of 1/*n*" using data from 9325 German municipal councils between 2008 and 2010. The value-added of this paper is twofold: First, the application of a regression discontinuity design avoids many of the methodological shortcomings of previous studies and, thus, provides a valid answer to the question whether an increase in the number of elected representatives leads to an increase in public spending in German municipalities. In Germany, the size of municipal councils is a positive but discontinuous function of a jurisdiction's population size. This means that the number of representatives increases at certain population thresholds. As a consequence, municipalities with marginally smaller or larger populations can differ significantly on the basis of their legislature sizes (cf. Egger and Koethenbuerger 2010). This quasi-experimental method has the great advantage that the assignment of the treatment is nearly random and, consequently, that a difference in the outcome variable between treatment and control groups can be attributed solely to the causal effect of the independent variable. Second, the present analysis expands the rather narrow scope of previous research on the "law of 1/*n*", which focuses mainly on the United States. It thereby extends the study by Egger and Koethenbuerger (2010), whose analysis is limited to local councils in Bavaria,[1] and provides the first test of the validity of the "law of 1/*n*" for all cities in Germany.

The results of the analysis do not confirm Weingast et al.'s theory. For the years 2008–2010, I find a negative effect of legislature size on public spending for the German municipal councils. An increase in elected representatives at the population threshold causes a reduction in public spending by roughly 3%. This negative effect might be explained by a tense economic and financial situation, which gives municipalities little

[1] Bavaria is one of the 16 German *Länder* (comparable to the US states).

scope for enacting expensive pork-barrel projects. Moreover, strong and independent mayors might be able to internalize the costs of the proposed projects and to impose fiscal discipline on the council members.

The remainder of the paper is structured as follows. The following section summarizes previous research on the "law of 1/*n*". The subsequent theoretical section elaborates the assumptions and mechanisms underlying the "law of 1/*n*". The fourth section presents the regression discontinuity design and dataset used in the empirical analysis. Afterwards, I present the main empirical results and conclude with a discussion of economic and institutional conditions under which the "law of 1/*n*" might not apply.

2 Previous research on the "law of 1/*n*"

Previous empirical tests of Weingast et al.'s "law of 1/*n*" are rather limited and focus mainly on the United States. On the sub-national level, Gilligan and Matsusaka (1995, 2001) analyze US states for the 1960–1990 and 1902–1942 periods and report a positive relationship for the upper chamber in each case. Primo (2006), in general, confirms those results in his analysis of the US states for the years 1969–2000, although he finds that the effect is negative for the lower legislative chambers (an increase in the number of elected politicians leads to less public spending). At the local level of government, Bradbury and Stephenson (2003) report a significant and positive relationship for the counties in Georgia (1992–1997). Baqir (2002) generally confirms this positive effect in a cross-sectional analysis of city governments in the United States. However, the positive relationship disappears if powers are concentrated in the office of the mayor. In an international comparative study of 35 democracies, Bradbury and Crain (2001) find a significant increase in public spending for every additional representative. The effect is weaker for countries with bicameral parliaments, in line with the reasoning of Buchanan and Tullock (1996 [1962], Chapter 16).

From a methodological point of view, however, the robustness of these results has to be challenged as all of these studies estimate simple regression models with cross-sectional data to gauge the effects of legislature size on public spending. The existing models are prone to problems of endogeneity because omitted variables will lead to biased estimates of the causal effect. It is possible, for example, that the cases studied differ not only with respect to parliamentary sizes, but also on some unmeasured confounding variables that are correlated with the number of representatives as well as with the level of public spending. Even the inclusion of a comprehensive list of control variables in the model cannot completely preclude all of the alternative explanatory variables that might have an impact on the relationship of interest. Thus, legislature size and public spending could be positively correlated, even if no causal relation exists between the two variables. A further weakness is the potential problem of reverse causality. Contrary to the theoretical expectations of Weingast et al. (1981), it might also be possible that substantial government spending is used to finance additional seats in the legislature.

In response to this methodological criticism, MacDonald (2008) conducts a longitudinal analysis of US cities from 1980 to 2000. Once fixed-effects are entered into the models to control for unmeasured confounding characteristics of the cities, the results indicate that council size no longer is significantly related to the amount of public spending. Two further exceptions are the studies by Pettersson-Lidbom (2012) and Egger and Koethenbuerger (2010). Both studies apply a regression discontinuity design, which allows them to conduct a quasi-experimental test of the relationship between legislature size and public spending. In contrast to the previous findings, Pettersson-Lidbom (2012) finds that public spending declines with every additional representative for the local councils in Finland and Sweden (1977–2002). Egger and Koethenbuerger (2010) studied the local councils in Bavaria, Germany, for the 1984–2004 period and found the expected positive relationship between number of elected representatives and the amount of public spending. Motivated by the results from MacDonald and Pettersson-Lidbom, the present study puts the "law of $1/n$" to a rigorous empirical test by estimating a valid causal effect of legislature size on public spending and extending the analysis from Egger and Koethenbuerger (2010) to all municipalities in Germany.

3 Theory—the assumptions of the "law of $1/n$"

The question to what extent institutional arrangements can influence the decisions and policy outcomes of political systems has attracted much interest from scholars in the fields of political science and political economy (for a good overview of the literature, see Acemoglu 2005). In their seminal article, Weingast et al. (1981) ask why legislatures frequently enact policy programs that are economically inefficient, meaning that the costs of the respective programs exceed the associated benefits. Wasteful public spending leads to an unnecessarily inflated budget and an inefficient utilization of public financial resources (Weingast et al. 1981, p. 643). On the basis of these observations, the authors develop a model that explains the inefficiencies of legislative decision-making. The key explanatory variable that influences the amount of public spending is the size of a parliament (number of representatives). According to their model, an increase in the number of elected representatives will always lead to an increase in public spending. That mechanism generally became known as the "law of $1/n$".

The basic model of the "law of $1/n$" applies to distributive politics, defined as political decisions that benefit exclusively a clearly defined geographical region or special-interest group, but whose costs are financed by revenues from a common tax base and, thus, are borne by the taxpaying public as a whole (Weingast et al. 1981, p. 644; Weingast 1994).

If policy programs are financed by a common pool of tax revenue, representatives can internalize the full benefits from projects that are targeted towards their constituencies in their respective districts; the costs, however, are not borne exclusively by taxpayers in that very district, but by all taxpayers. The perceived cost for every representative (or district) declines with every additional delegate in the parliament.

Formally, this can be explained as follows: If $b_i(x)$ is a function of the benefits that result from public spending in the amount of x for a specific project (e.g., construction of a new swimming pool) targeted towards district i of representative i, and $c_i(x)$ represents the tax burden that has to be borne for this project by the inhabitants of district i, then every representative prefers spending up to x so that

$$b'(x) = c_i'(x) \tag{1}$$

All decisions over distributive policies by means of Eq. (1) would result in an efficient allocation of the financial resources available, because marginal costs and benefits are equal. In reality, however, costs are borne not only by revenue from district i, but from the whole population in all districts combined. Let n stand for the number of districts that are each represented by one delegate in the legislature; every representative then is responsible only for $1/n$ of the total costs for his constituency in district i. The equation then reads as follows:

$$b_i'(x) = (1/n) * c'(x) \tag{2}$$

Consequently, an increase in the number of electoral districts (i.e., an increase in the number of elected representatives) leads to a reduction in the perceived costs for each district (the cost share for each district declines to $1/n$) (Weingast et al. 1981, p. 646ff.; Bradbury and Stephenson 2003, p. 187; Bradbury and Crain 2001, p. 311). If we assume that representatives are primarily concerned about reelection and that they act rationally, then every member of the legislature strives for legislative approval of additional projects for her electoral district because her constituency bears only a fraction of the total costs, but each representative will gain credit for all of the projects' benefits on the next Election Day (Battaglini and Coate 2007, p. 118ff.; Del Rossi 1995, p. 285; Baron 1991, p. 57ff.; Ferejohn 1974).

Weingast et al. (1981) assume further that all decision-making processes over distributive policies are characterized by a so-called universalism norm. In the absence of strong party discipline, representatives will build super-majority coalitions that very often embrace all members of a legislature, thereby contradicting the theory of Riker (1962) that all decisions within a legislature will produce minimal wining coalitions (MWC). Shepsle and Weingast (1981) have identified the problem that MWCs are unstable and that coalition membership can change from decision to decision. Consequently, great uncertainty faces each representative, who may not know if she will be part of the MWC and if she will be able to pass a project for her district. Weingast and Shepsle, therefore, assume that representatives prefer a universalism norm and build coalitions that include all delegates, because such a strategy ensures a specific project for every district and representative (Collie 1988, p. 874; Shepsle and Weingast 1981, p. 100; Weingast 1979, p. 252).

Thus, the following hypothesis for the empirical analysis can be deduced:

$\mathbf{H}_1$ An increase in the number of elected representatives always leads to an increase in government spending.

4 Institutional background, methods and data

4.1 Municipal politics in Germany

Germany is currently divided into 11,993 municipalities, which are proper subsets of the *Länder* (roughly comparable to the US states), but enjoy a considerable degree of legal and fiscal autonomy. Municipalities are responsible for the provision of many important public services like elementary schools, kindergartens and sports facilities, as well as museums and theatres. Because the provision of these services has to be financed from the local budget, municipalities have the right to raise and set property taxes and a business tax (these own-source taxes make up about 30% of total municipal revenues). In addition, municipalities receive earmarked income transfers from the *Länder* and the national budget. Municipalities also have the right to borrow if they want to increase public spending. However, the amount of borrowing is not allowed to exceed infrastructure expenditures (Egger and Koethenbuerger 2010).

The council size is determined by the number of municipal inhabitants on a specific date prior to the next local election. The number of council members increases at certain legally defined population thresholds, which differ from *Land* to *Land*. Table 5 in the appendix shows the different thresholds for 11 German states.

The political and administrative organization of almost all municipalities conforms to a mayor-council system in which the council and the mayor are elected directly. The councils have the right to adopt statutes, implement important control functions and pass the annual budgets of the municipalities. However, the mayor-council system also is characterized by a strong mayor, who is the chief executive officer of the municipality and (in most cases) the chair of the council (Egger and Koethenbuerger 2010).

4.2 The regression discontinuity design[2]

In order to estimate the causal effect of legislature size (number of members of parliaments) on public spending, I implement a regression discontinuity design (RDD). The RDD was developed originally by Thistlethwaite and Campbell (1960) to estimate the effect of scholarships on the professional careers of former college students. Recently, RDD is growing in popularity and many studies have been published that apply this quasi-experimental design (Angrist and Lavy 1999; Butler and Butler 2006; Hainmüller et al. 2006; Imbens and Lemieux 2008b; Lee 2008; Pettersson-Lidbom 2008; Butler 2009; Becker et al. 2010; Bloom 2012, p. 45).

To explain the assumptions and advantages of a RDD as a quasi-experimental design, it is useful to apply the Rubin causal model (Rubin 1974, 2005). If a researcher is interested in the effect of binary treatment variable, Rubin's causal model uses the notion of *potential outcomes,* which defines Y_1 as the outcome with treatment, and Y_0 as the outcome without treatment, to determine the causal effect of an independent variable. For the present analysis, Y_0 would be the amount of public spending with a given number of representatives, and Y_1 would stand for the amount of public spending of the same parliament at the same point of time, but with a larger number of elected representatives. According to Holland's (1986) fundamental problem of causal inference, however, it is never possible to observe Y_1 and Y_0 for the same subject at the same point in time. Therefore, the individual treatment effect $(Y_1 - Y_0)$ is a non-observable counterfactual because for every subject we can observe only either Y_1 or Y_0. Under specific conditions, however, it is possible to estimate an average treatment effect (ATE) of the exogenous variable. For this purpose, one compares the observable results of the treatment and control groups. If D stands for the binary treatment variable, we get Y^1 if $D = 1$ and Y^0 if $D = 0$. The average treatment effect is then defined by the following equation:

$$ATE = E\left(Y^1 \middle| D = 1\right) - E\left(Y^0 \middle| D = 0\right) \tag{3}$$

The essential prerequisite for Eq. (3) to hold is the assumption that the subjects in the treatment and control group do not differ systematically on characteristics other than the treatment variable. In classical experimental studies, this requirement is achieved through the random assignment of subjects into the two groups (Shadish et al. 2002). As the randomization of subjects is very often not feasible, the advantages of the RDD, which is based on a detailed knowledge of the assignments of the subjects into treatment and control group in a real-world setting, become apparent (Hainmüller et al. 2006). As a quasi-experimental design, the RDD allows for a valid estimation of causal effects whenever the assignment of a treatment variable is a discontinuous function of an underlying running

[2] For a detailed introduction to regression discontinuity designs, see Lee and Lemieux (2010), Imbens and Lemieux (2008a), Gelman and Hill (2007), Shadish et al. (2002) and Trochim (1984).

variable and is determined by the crossing of a specific threshold on this variable (Imbens and Lemieux 2008a; Trochim 1984). If X is the running variable with the threshold c, than all subjects with $x_i > c$ would get the treatment, and all subjects with $x_i < c$ would be in the control group. For the analysis of the effect of legislature size on public spending, these prerequisites are fulfilled: In Germany, the size of municipal councils is a positive but discontinuous function of a jurisdiction's population size (see Table 5 in the appendix). This means that, owing to legal provisions, the number of representatives increases at certain population thresholds (about 2–4 seats). As a consequence, municipalities with marginally smaller or larger populations can differ significantly according to their legislature sizes (cf. Egger and Koethenbuerger 2010).

It is assumed herein that municipalities that are very close to the population threshold are randomly assigned to either side of the threshold and do not differ systematically according to other characteristics. The assignment of the treatment (in this case, an increase in the number of representatives) can be seen as nearly random, because it depends only on whether a municipality has marginally more or less inhabitants. Consequently, a difference in the outcome variable between treatment and control group can be attributed solely to the causal effect of the independent variable. The local average treatment effect (LATE) is estimated by comparing the values of the outcome variable of subjects just above and just below the threshold. If a discontinuity of the level of public spending at the population threshold occurs, this "jump" can be interpreted as the causal effect of a change in legislature size on public spending. Owing to the random assignment, possible third variable biases can be eliminated (Lee and Lemieux 2010; Cook and Wong 2008; Hahn et al. 2001). Therefore, the RDD allows for a valid estimation of the causal effect of legislature size on public spending and avoids the aforementioned methodological pitfalls of previous studies.

In Germany, the elections for municipal councils are under the jurisdiction of the *Länder*, so population thresholds differ from *Land* to *Land*. Therefore, it is necessary to standardize the multiple thresholds in a way that centers them around a single threshold. Following Egger and Koethenbuerger (2010), this is achieved as follows:

If X_i stands for the population of municipality i, and X_c denotes the respective threshold, then the standardized population threshold is defined as $\tilde{X} = X_i/X_c$. Consequently, the natural log of $\tilde{X} = 0$, if the number of inhabitants of a municipality equals the threshold. Hence, D_i can be introduced as an indicator variable for the assignment into treatment and control group:

$$D_i = \begin{cases} 1 & \mathit{if}\ \ln\left(\tilde{X}_{\mathrm{i}}\right) > 0\,(\mathit{Treatment}) \\ 0 & \mathit{if}\ \mathit{ln}\left(\tilde{X}_{\mathrm{i}}\right) < 0\,(\mathit{Control}) \end{cases} \tag{4}$$

By now, most RDD studies have applied a parametric approach to estimate the LATE. The equation for the RDD model then reads as follows:

$$Y_i = \alpha + \beta D_{\mathrm{i}} + k\left(\tilde{X}_{\mathrm{i}}\right) + \varepsilon_{\mathrm{i}}, \tag{5}$$

where Y_i denotes the public spending of municipality i, $k\left(\tilde{X}_{\mathrm{i}}\right)$ stands for a polynomial function between X (number of inhabitants) and Y, α is the constant and ε_{i} is the error term. In Eq. (5), β is the estimated coefficient of interest, indicating the causal effect on spending of an increase in elected representatives at the threshold D. A serious disadvantage of this parametric approach, however, is identifying the correct functional form of $\mathrm{k}(\tilde{X})$. If the relationship between X and Y is specified wrongly, the model will produce biased results, and the LATE will either be over- or underestimated (Angrist and Pischke 2009, p. 253ff.; Berk 2010). In this paper, I will therefore follow the example of Hahn et al. (2001), as well as Imbens and Lemieux (2008a), and estimate a nonparametric model. This can be accomplished either by kernel regressions or by estimating local linear (or polynomial) regressions. Because a RDD estimates a regression equation for one single point (the threshold) and that point also represents a boundary point, local linear regressions should be preferred. The equation is then estimated for a certain bandwidth above and below the threshold (see Hahn et al. 2001, p. 206; Imbens and Lemieux 2008a, p. 623). For the estimation of the local regressions, the window around the threshold should be as small as possible so as to limit the analysis to only those subjects that fall very closely above or below the threshold. If the chosen window is too wide, the assumption of a random assignment can no longer be justified and a systematic difference between the municipalities in treatment and control group can occur.

For the analysis of the German municipal councils, a further distinction between sharp and fuzzy RDDs is necessary. In a sharp RDD, the assignment of the treatment is deterministic, meaning that with the crossing of the threshold, the probability of receiving the treatment jumps from 0 to 1. For the local German parliaments, however, this deterministic function does not apply to each *Land.* In some of the *Länder* municipalities are permitted to either increase or reduce the number of elected representatives with the agreement of a simple majority of the legislators.

The assignment to treatment or control there no longer is deterministic, and must be understood as a probabilistic function. Such cases require application of a fuzzy RDD (Imbens and Lemieux 2008a, p. 619). The LATE is then estimated according to the following equation:

$$LATE = \frac{\lim_{x\downarrow c} \mathrm{E}(Y|X=x) - \lim_{x\uparrow c} \mathrm{E}(Y|X=x)}{\lim_{x\downarrow c} \mathrm{E}(D|X=x) - \lim_{x\uparrow c} \mathrm{E}(D|X=x)} \tag{6}$$

Equation (6) shows that in a fuzzy RDD, the LATE is defined as the ratio of the jump in the outcome variable (public spending) to the jump in the treatment variable (number of representatives) at the threshold. The estimation equals a local Wald-estimator with the jump in the outcome as the numerator and the jump in the treatment as denominator (Battistin and Rettore 2002, p. 42ff.; Nichols 2007, p. 527ff).

4.3 Data

I analyze data from all German municipal councils between 2008 and 2010. Owing to a lack of comparable data, the city states (Berlin, Hamburg and Bremen) as well as municipalities from Mecklenburg-Vorpommern and Lower Saxony are not included in the analysis.[3] The independent cities (municipalities that are not associated with a county) are excluded from the analysis because they additionally have to provide public services that usually are the responsibility of the counties. Public spending in those cities therefore is not comparable to the other municipalities in Germany. All data were obtained from the Federal Statistical Office Germany (*Statistisches Bundesamt*) and the statistical offices of the German *Länder.* The dependent variable for the analysis is logged per capita gross

[3] The city states do not have separate municipal councils that are comparable to the local parliaments in other German *Länder*. Mecklenburg-Vorpommern and Lower Saxony collect data on public expenditures only for the counties, but not for individual municipalities.

expenditure of the German municipalities (in euros). Gross spending combines the running administrative budget (labor costs, current material costs) as well as the capital budget (fixed investments, amortization). Legislature size (the dependent variable) is measured in seats per council and is determined by the number of inhabitants prior to the latest local election (see Table 5).

Altogether, panel data for 9325 municipalities could be gathered for the 2008–2010 period. After the exclusion of observations with missing data, this makes a total of 26,873 observations. Of course, the number of observations is reduced drastically when the analysis is restricted to municipalities that are close to the respective population thresholds. All in all, however, the sample provides a sufficient amount of observations around the population thresholds to obtain statistically meaningful estimates. Moreover, the data paint a current picture of local politics in Germany and reflect the present problems of the municipalities (e.g., large public debts). The descriptive statistics for all important variables are shown in Table 1.[4]

5 Results

For the estimation of the RDD, I am using a 10% window around the normalized threshold, which yields a total sample of 7824 observations (3906 above, and 3918 below the threshold). In order to calculate the local linear regressions, I apply a triangular kernel[5] and use Imbens and Kalyanaraman's (2012) method to find the optimal bandwidth for the estimator. Owing to the application of a fuzzy-RDD, the local average treatment effect (LATE) equals a local Wald-estimator with the jump in public spending as the numerator and the jump in legislature size as the denominator (Battistin and Rettore 2002, p. 42ff.; Nichols 2007, p. 527ff., 2011).

Table 2 summarizes the main results of the analysis. In addition to reporting the LATE, the table includes information on the number of observations above and below the threshold that were used for the estimation of the LATE. Table 2 also includes the average increase in council seats from below to above the threshold as well as two additional models with different window sizes (15% and 5%) that test if the estimation of the LATE depends on the in- or exclusion of additional municipalities around the threshold.

The main results of the analysis of the German municipalities (Model 1) indicate that, in contrast to the theoretical assumptions of the "law of $1/n$", an increase in legislature size has a negative effect on public spending. Using a 10% window, the legislature size rises by on average about 3.1 seats (treatment) at the population threshold. The estimated local average treatment effect associated with that jump in the number of council seats equals ($-$ 0.03) and is statistically significant ($p < .05$). The standard errors are clustered by municipalities to adjust for dependencies between observations from the same municipality at different points in time. Substantively, the average increase in elected representatives at the population threshold causes a reduction in public spending by 3%: For every additional seat in the local council, public spending per capita decreases by about 1%. Model 2 and Model 3 in Table 2 test whether the results are sensitive to different widths of the window around the threshold. Using either a 15% (Model 2) or a 5% window (Model 3) has almost no influence on the estimation of the LATE and does not change the statistical significance of the results from Model 1.

4 The datasets analyzed during the current study are available from the author on reasonable request.

5 The results are not sensitive to the choice of different kernels (Epanechnikov, rectangle kernel).

Table 1 Descriptive statistics. German municipalities, 2008–2010 (without independent cities)

	Mean	SD	Min	Max
Inhabitants	4289.7	7920.4	5	151,254
Number of representatives in local council	14.7	7.7	6	59
Public spending (in thsd. €)	7595.9	16,558.8	7.6	341,544
Public spending per capita (in €)	1511.8	760.7	391.5	19,224.5

Author's calculation. Data were obtained from the Federal Statistical Office Germany and the Statistical Offices of the German Länder; N = 26,873

Table 2 The causal effect of legislature size on public spending: results of a regression discontinuity design for German local councils, 2008–2010

	Model 1 (10% window around threshold)	Model 2 (15% window around threshold)	Model 3 (5% window around threshold)
LATE	− 0.03** (0.01)	− 0.03** (0.01)	− 0.04** (0.02)
Average increase in seats (from below to above threshold)			
Absolute	3.1	2.7	3.3
In percent	21.5	18.6	23.2
Observations			
Total	7824	11,824	3873
Total within window above population threshold	3906	5925	2009
Total within window below population threshold	3918	5899	1864
95%-confidence interval for LATE	(− 0.05) to (− 0.004)	(− 0.06) to (− 0.005)	(− 0.07) to (− 0.005)

Dependent variable: log expenditure/capita. The estimation of the LATE equals a local Wald-estimator at the threshold with the jump in the outcome as the numerator and the jump in the treatment (council size) as denominator. Population data have been normalized around a single threshold (see Sect. 4.2). Standard errors (clustered by municipalities) in parentheses

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

To illustrate the results from Model 1 graphically, Fig. 1 shows local polynomial regression plots on both sides of the normalized population threshold. The data points indicate bin averages of log expenditure per capita (bin size 0.004, calculated as proposed in Calonico et al. 2014). The local polynomial smooths on both sides of the threshold apply triangular kernels and second-order polynomials. The vertical line indicates the normalized population threshold, and the gap between the two black lines shows the causal effect of legislature size on the amount of log public spending for the German municipalities.

All in all, the results of the RDD lead to the conclusion that Hypotheses 1 is not supported and that the assumptions of the "law of $1/n$" are not valid for the German local councils.

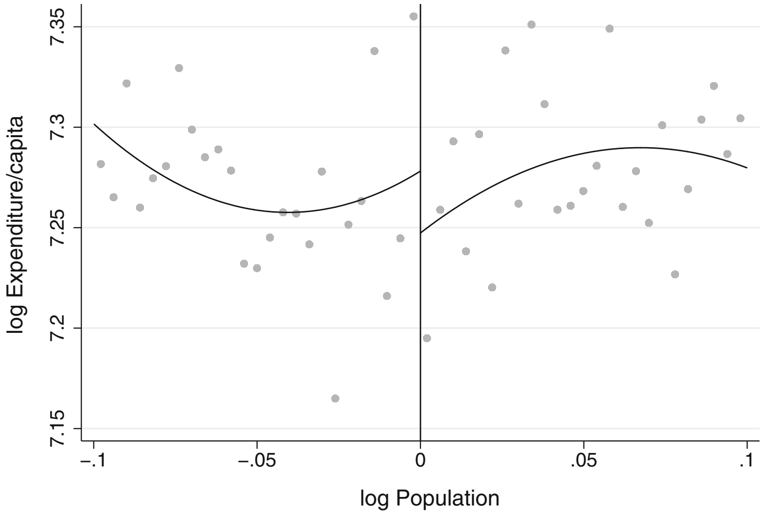

Fig. 1 The Effect of legislature size on public spending. Regression discontinuity analysis of the German local councils, 2008–2010. Data points are averaged within bins of 0.004. Triangular kernel and second-order polynomial. Population data have been normalized around a single threshold (see Sect. 4.2). N = 7824

6 Robustness checks

The application of population thresholds in RD designs has been criticized for various reasons (Eggers et al. 2017; Ade and Freier 2011). One potential pitfall is that not only council size increases at the population thresholds, but that many other institutional arrangements and policies change at the same time. For example, many of the thresholds also are used to determine the status (paid or unpaid) and the salary of the mayor, the number and the statuses of deputy mayors as well as the classification of municipalities as larger cities. To be as transparent as possible about these changes, Table 5 in the appendix lists all other policies that change at the same thresholds at which council size increases. To show that the reduction in public spending actually is driven by the increase in council size and not by any other of the institutional changes, I repeat the analysis using only those observations that are close to the thresholds at which no other policy changes take place. That procedure leads to the exclusion of 64 of the 178 different thresholds and drastically reduces the number of observations from 7824 to 4787 (Model 1). The results reported in Table 3 indicate that the effect of council size declines slightly; however, it remains negative and statistically significant. The same applies for Model 3. Only Model 2, which uses a 15% window around the normalized threshold, fails to reach a conventional level of significance. Thus, the reduction in public spending is driven mainly by the increase in council size and not by a pooled treatment effect of all other policy changes at the thresholds.

One of the strongest assumptions of the RD design is the continuity of the running variable (population size) at the threshold. If cities are able to modify or manipulate their population sizes to end up above or below a certain threshold of interest, the assignment into treatment and control groups no longer would be truly random and the estimation of the causal effect would be biased (Eggers et al. 2017; Ade and Freier 2011). If population

Table 3 Robustness check: results of a regression discontinuity design for German local councils at thresholds with no other policy changes, 2008–2010

	Model 1 (10% window around threshold)	Model 2 (15% window around threshold)	Model 3 (5% window around threshold)
LATE	− 0.02** (0.01)	− 0.02 (0.01)	− 0.03* (0.017)
Average increase in seats (from below to above threshold)	2.8	2.5	2.9
Observations			
Total	4787	7334	2411
Total within window above population threshold	2356	3667	1231
Total within window below population threshold	2431	3667	1180
95%-confidence interval for LATE	(− 0.05) to (− 0.002)	(− 0.04) to (0.004)	(− 0.07) to (0.000)

See Table 1

figures have been manipulated by city officials, we should observe clustering of cities either just above or just below the population threshold (McCrary 2008). To validate that no sorting occurs around the normalized threshold, Fig. 2 plots the frequency distribution of municipalities within the 10% window surrounding the threshold. The graphical evidence shows an almost equal number of observations on both sides of the threshold and no obvious discontinuity at the cut-off point, suggesting that the manipulation of population estimates is not a common practice among German municipalities.[6] Besides this graphical evidence, I also implement a formal statistical check to confirm that the no-sorting assumption is not violated. Eggers et al. (2017) show that the standard McCrary density test may be biased if applied to population data. Therefore, I use an automatic manipulation test developed by Cattaneo et al. (2017). To estimate whether a discontinuity in the density of observations occurs near the threshold, the authors develop a test based on local polynomial density estimators that use kernel functions to choose different bandwidths and—in contrast to the McCrary test—do not require pre-binning of the data.[7] The implementation of the manipulation test with a triangular kernel, a second-order polynomial and a data-driven bandwidth selector (see Cattaneo et al. 2017 for details), gives us a test statistic (T-value) of 0.74 with a p value of 0.46. This result indicates no significant jump in the density of observations above and below the threshold.

An alternative explanation for why spending does not increase in cities with larger legislatures is that many of these municipalities have added additional council members only recently because they have just exceeded the population threshold. Not enough time may have elapsed for spending to adjust because new legislators need to gain experience in the municipal council political game. To test this explanation, I drop all observations from

[6] Eggers et al. (2017) test the no-sorting assumption for municipalities in Germany, Italy and France. The authors find weak evidence of sorting in Germany which, however, is by far less severe than in Italy or France. Deviating from the present analysis, Eggers et al. (2017) use data for the years 1998-2007 and analyze population thresholds at which either the council size or the mayor's salary changes.

[7] This test can be implemented in Stata 14 with the *rddensity*-command.

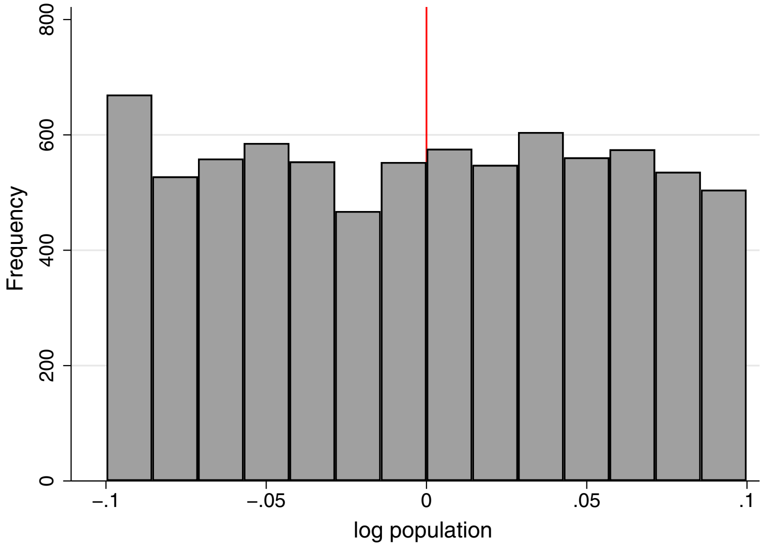

Fig. 2 Robustness check: Distribution of municipalities around the normalized threshold. Bin = 14; width = .014

the analysis that have crossed the population threshold within the study period (2008–2010) and then re-estimate the original models. In that way, only those municipalities are analyzed that have had the same number of council members for at least two consecutive years (i.e., only cities with relatively stable population sizes). For Model 1 this procedure reduces the sample size by 355 observations. As can be seen in Table 4, however, the magnitude, direction, and significance level of the LATE does not change. The same holds for Models 2 and 3.

7 Discussion, conclusion and outlook

This study analyzed the effect of legislature size on public spending and tested the validity of the "law of $1/n$" (Weingast et al. 1981), which posits that an increase in the number of representatives leads to an increase in public spending, for more than 9000 German municipal councils between 2008 and 2010. Through the application of a regression discontinuity design, I was able to overcome many previous methodological shortcomings and provide a valid estimation of the causal effect of legislature size on public spending. The results do not corroborate the positive findings of previous studies. In contrast, in German municipalities, each additional representative in a city council leads to a significant decline in public spending. Thus, by using a sophisticated statistical approach as well as by extending the empirical tests of the "law of $1/n$" to the German municipal councils, new evidence could be presented that questions the theoretical predictions of Weingast et al. (1981).

How can we explain these negative findings and how do they fit with other studies that have failed to confirm the "law of $1/n$" empirically? Have previous papers only been able to confirm the "law of $1/n$" because they are methodologically flawed or because some specific circumstances in the German municipalities help us modify Weingast et al.'s

Table 4 Robustness check: results of a regression discontinuity design for German local councils with a stable number of council seats, 2008–2010

	Model 1 (10% window around threshold)	Model 2 (15% window around threshold)	Model 3 (5% window around threshold)
LATE	− 0.03** (0,01)	− 0.03** (0,01)	− 0.04**(0.02)
Average increase in seats (from below to above threshold)	3.0	2.5	3.1
Observations			
Total	7523	11,502	3643
Total within window above population threshold	3843	5857	1960
Total within window below population threshold	3680	5645	1683
95%-confidence interval for LATE	(− 0.07) to (− 0.007)	(− 0.06) to (− 0.005)	(− 0.07) to (− 0.007)

See Table 1. All observations that have crossed the population threshold during the period of investigation have been dropped

theory? The regression discontinuity design is certainly a rigorous empirical test that provides a valid estimation of causal effects. However, the results should be treated with caution as there are limitations inherent in the RDD that must be mentioned. In particular, the degree to which the results can be generalized is tightly constrained. First of all, the research design includes only a short time frame of 3 years and we cannot assume that all parameters of the model are stable over that period and that the results can be extrapolated to other points in time. Moreover, the estimation of the causal effect in a RDD is restricted to those municipalities that are close to the population threshold. Unless these cities are completely identical to those municipalities that are further away from the threshold, the results should not be generalized or treated as valid for all municipal councils in Germany. It follows, that one cannot infer with certainty that the results also are generalizable to other governmental levels there or even to other countries. Therefore, I do not intend to claim that the positive results of many of the previous papers on the "law of 1/n" are definitely null and void. However, owing to the strengths of the RDD, the empirical analysis herein presents strong evidence that the assumed positive relationship between legislature size and public spending is absent in the case of German municipalities. This finding gives us a promising opportunity to develop new ideas about this relationship and to specify conditions under which the "law of $1/n$" does not apply.

The findings suggest that pork-barrel politics, which are the prerequisite for the "law of $1/n$", are not common in the German municipal councils. In contrast, Germany seems to be an example of the "reverse law of $1/n$" (Primo and Snyder 2008, p. 478) in which an increase in legislature size leads to a reduction in public spending. To make sense of this finding, the recent economic situation of the German municipalities has to be taken into consideration. Because of the worldwide economic crisis and tight public finances at home (remember that municipalities have the right to borrow if they want to increase public spending, but that the amount of borrowing is not allowed to exceed infrastructure expenditures), little scope remains for the municipalities to enact expensive public projects. In particular, the enactment of costly pork-barrel projects seems to be off the table because simply not enough money is available for financing them. In economic downturns, it is more likely that politicians try to appear fiscally responsible and that they try to keep the tax burden as low as possible to gain votes from their constituents. That explanation relates pretty well to the results from Pettersson-Lidbom (2012), who finds a significant decline in public spending with every additional representative on the local councils in Finland and Sweden (1977–2002). In particular, by looking at operating expenditures and local public employment, Petersson-Lidbom shows that bureaucrats tend to increase public spending in order to maximize their budgets, whereas politicians try to cater to fiscally conservative voters. Pettersson-Lidbom concludes that if the number of council members increases, politicians can more effectively control the bureaucracy and the level of public spending therefore falls. Even though appearing as fiscally responsible seems to be particularly important during economic downturns (as is the case for the German municipalities between 2008 and 2010), the longitudinal results from Pettersson-Lidbom (1977–2002) demonstrate that politicians also tend to reduce public spending during prosperous times in order to gain votes from fiscally conservative constituents.

Another institutional feature of German municipalities, that might mitigate the underlying common pool problem of overspending, is the strong position of the mayor. As pointed out above, cities in Germany conform to the mayor-council system, characterized by a directly elected mayor who is the chief executive officer of the municipality endowed with the right to veto decisions of the city council. Related to the literature on budget institutions (e.g., Alesina and Perotti 1999; Persson and Tabellini 2003) we can expect that a strong and independent mayor is able to internalize the total costs of the projects proposed by each of the council members and to impose fiscal discipline on the council. That institutional feature reduces the overspending bias and, hence, breaks the positive relationship between legislature size and public spending. Similar results have been found by Baqir (2002) for city governments in the United States. Using cross-sectional data from 1990, he can show that in cities with strong mayors public spending does not increase with a growing number of council members.

MacDonald (2008), who also fails to confirm the assumptions of the "law of 1/*n*" for US cities between 1986 and 2002, speculates that this result might be explained by the relationship between group size and decision-making costs. If the number of council members becomes too large, vote trading and logrolling may become too complex to reach unanimous agreements. This reasoning seems to be a plausible alternative explanation for the negative results of the present analysis. Several authors have shown that in German local politics the importance of political parties increases if cities and councils get bigger (e.g., Holtkamp 2006; Naßmacher and Naßmacher 2007). However, if parties become the main parliamentary actors and the party leadership chooses what projects to pursue, then it becomes more difficult for every individual legislator to pass a project for her specific constituency (MacDonald 2008, p. 470). This result contradicts Weingast's universalism-norm assumption and shows that strong parties might be able to mitigate common-pool problems in public spending.[8]

As this discussion demonstrates, a number of studies now find a negative relationship between legislature size and public spending. Those results do not seem to be country-specific as they occurred in a diverse range of different countries (Sweden, Finland, the United States and Germany). However, the negative effect seems to be particularly evident for municipal councils. The present discussion tried to develop some new ideas about the

[8] Related to this argument, McCormick and Tollison (1981, p. 33ff.) show that legislature size also has an effect on the rate of return that interests groups can gain through lobbying activities in the legislative arena. The authors argue that the influence of each individual legislator on the parliamentary decision-making process will diminish if legislature size increases. Thus, although it becomes cheaper for interests groups to buy individual votes in larger legislatures, rents for interests groups are higher in smaller legislatures.

conditions under which the applicability of the “law of $1/n$” should be qualified. Unfortunately, given the fact that city councils have restricted budget authorities as well as rather narrow scopes of action limited to the local level, it is not possible to say whether the results of the study reported herein are generalizable to higher governmental levels. Several studies of sub-national and national parliaments have shown that the assumptions of the “law of $1/n$” do not apply to the lower chambers of bicameral legislatures (Gilligan and Matsusaka 1995, 2001; Primo 2006). Nevertheless, further studies of regional, national, or even international parliaments (e.g., the European Parliament) are needed to develop a broader understanding of the “law of $1/n$” and to investigate if different causal effects might occur at different levels of state hierarchies. Moreover, based on the data of the present analysis, it cannot be said with certainty which of the potential mechanisms actually explains the negative relationship between council size and public spending. Further qualitative studies of single municipal councils are necessary to find out more about these specific conditions under which the “law of $1/n$” does not apply.

Acknowledgements I gratefully acknowledge helpful comments and suggestions by Matt Schoene, Peter Selb, Susumu Shikano, Ulrich Sieberer, Sophia Wallace, Geoffrey Wallace, the journal’s editors, two anonymous referees as well as participants at seminars in Konstanz and Rutgers. All errors and shortcomings are my own.

Compliance with ethical standards

Conflict of interest The author declares that he has no conflict of interest.

Appendix

See Table 5.

Table 5 Local council size and other institutional changes as a function of population size in the German *Länder*

State	Threshold	Council size	Other institutional changes
Baden-Württemberg	<1000	8	–
	1000–2000	10	Additional members council for top-secret issues
	2001–3000	12	Mayor status changes from honorary to full-time
	3001–5000	14	Wage of mayor increases
	5001–10,000	18	Wage of mayor increases
	10,001–20,000	22	Quota requirement for petition for citizen requests decreases
			Election of full-time deputy mayors
			Additional member council for top-secret issues
			Wage of mayor increases
	20,001–30,000	26	Status of a larger city
			Wage of mayor and deputy mayor increases
	30,001–50,000	32	Additional members council for top-secret issues
			Wage of mayor and deputy mayor increases
	50,001–150,000	40	Wage of mayor and deputy mayor increases
	150,001–400,000	48	–
	>400,000	60	–

Table 5 continued

State	Threshold	Council size	Other institutional changes
Brandenburg	<700	8	–
	701–1500	10	–
	1501–2500	12	–
	2501–5000	16	–
	5001–10,000	18	–
	10,001–15,000	22	Wage of mayor increases
	15,001–25,000	28	Election of one or several deputy mayors
			Wage of mayor increases
	25,001–35,000	32	Wage of mayor increases
	35,001–45,000	36	Status of a larger city
	>45,000	40	–
Bavaria	<1000	8	–
	1001–2000	12	–
	2001–3000	14	–
	3001–5000	16	–
	5001–10,000	20	Full-time mayor
			Accounting agency
	10,001–20,000	24	Quota requirement for open council decreases
			Full-time council members
			Wage of mayor increases
	20,001–30,000	30	–
	30,001–50,000	40	Status of a larger city
			Wage of mayor increases
	50,001–100,000	44	Status of a county-free city
			Wage of mayor increases
	100,001–200,000	50	–
	200,001–500,000	60	–
Hesse	<3000	15	–
	3001–5000	23	–
	5001–10,000	31	Full-time mayor
	10,001–25,000	37	Wage of mayor increases
	25,001–50,000	45	–
	50,001–100,000	59	Quota requirement for referenda decreases
			Quota requirement for petition for citizen requests decreases
			Accounting agency
			Wage of mayor increases
	100,001–250,000	71	Quota requirement for referenda decreases
			Quota requirement for petition for citizen requests decreases from 5 to 3%
			Wage of mayor increases
	250,001–500,000	81	–
	500,001–1000,000	93	Wage of mayor increases
	>1,000,000	105	–

Table 5 continued

State	Threshold	Council size	Other institutional changes
North Rhine-Westfalia	<5000	20	–
	5001–8000	26	–
	8001–15,000	32	–
	15,001–30,000	38	–
	30,001–50,000	44	Quota requirement for petition for citizen requests decreases
			Wages of mayor and deputy mayor increase
	50,001–100,000	50	Quota requirement for petition for citizen requests decreases
			Quota requirement for referenda decreases Status of a larger city
	100,001–250,000	58	Quota requirement for petition for citizen requests decreases
			Quota requirement for referenda decreases
			Wages of mayor and deputy mayor increase
	250,001–400,000	66	Wages of mayor and deputy mayor increase
	400,001–550,000	74	–
	550,001–700,000	82	–
	>700,000	90	–
Saarland	<10,000	27	–
	10,001–20,000	33	Number of deputy mayors increases to three
			Wage of mayor increases
	20,001–30,000	39	Qualification requirement for mayor
			Number of deputy mayors increases to four
			Full-time deputy mayors
			Full-time women's officer
			Accounting agency
			Wage of mayor and deputy mayor increases
	30,001–40,000	45	Status of a larger city
			Wage of mayor and deputy mayor increases
	40,001–60,000	51	Number of deputy mayors increases to five
			Wage of mayor and deputy mayor increases
	60,001–100,000	57	Wage of mayor and deputy mayor increases
	>100,000	63	Number of deputy mayors increases to seven
Rhineland-Palatinate	<300	6	–
	301–500	8	–
	501–1000	12	–
	1001–2500	16	–
	2501–5000	20	–
	5001–7500	22	–
	7501–10,000	24	–
	10,001–15,000	28	Quota requirement for referenda decreases
			Wage of mayor increases
	15,001–20,000	32	One full-time deputy mayor
			Wage of mayor and deputy mayor increases

Table 5 continued

State	Threshold	Council size	Other institutional changes
	20,001–30,000	36	Wage of mayor and deputy mayor increases
	30,001–40,000	40	Quota requirement for referenda decreases
			Wage of mayor and deputy mayor increases
	40,001–60,000	44	Four additional deputy mayors
			Wage of mayor and deputy mayor increases
	60,001–80,000	48	Wage of mayor and deputy mayor increases
	80,001–100,000	52	One additional deputy mayor
	100,001–150,000	56	Quota requirement for referenda decreases
			Wage of mayor and deputy mayor increases
	>150,000	60	–
Saxony	<500	8	–
	501–1000	10	–
	1001–2000	12	–
	2001–3000	14	–
	3001–5000	16	–
	5001–10,000	18	Full-time mayor
	10,001–20,000	22	One full-time deputy mayor
			Wage of mayor increases
	20,001–30,000	26	Full-time equal opportunity commissioner
			Accounting agency
			Wage of mayor increases
	30,001–40,000	30	Financial support for parliamentary party groups
			Additional full-time deputy mayor
			Wage of mayor increases
			Wage of mayor increases
	40,001–50,000	34	–
	50,001–60,000	38	Additional full-time deputy mayor
	60,001–80,000	42	Wage of mayor increases
	80,001–150,000	48	–
	150,001–400,000	54	–
	>400,000	60	Additional full-time deputy mayor
Saxony-Anhalt	<100	4	–
	101–500	8	–
	501–1000	10	–
	1001–2000	12	–
	2001–3000	14	–
	3001–5000	16	–
	5001–10,000	20	Number of members in the city district councils increases to 19
			Wage of mayor increases
	10,001–20,000	28	Quota requirement for referenda decreases
			Wage of mayor increases

Table 5 continued

State	Threshold	Council size	Other institutional changes
	20,001–30,000	36	Quota requirement for referenda decreases
			Wage of mayor increases
	30,001–50,000	40	Quota requirement for referenda decreases
			Wage of mayor increases
	50,001–150,000	50	Quota requirement for referenda decreases
			Formation of a PPG requires at least 3 representatives
			Wage of mayor increases
	150,001–300,000	56	–
	>300,000	60	–
Thuringia	<500	6	–
	501–1000	8	Two additional members in city district councils
	1001–2000	12	General committee
			Two additional members in city district councils
	2001–3000	14	Two additional members in city district councils
	3001–5000	16	Full-time mayor
	5001–10,000	20	Wage of mayor increases
	10,001–20,000	24	Accounting agency
			Wage of mayor increases
	20,001–30,000	30	Equal opportunity commissioner
			Wage of mayor and deputy mayor increases
	30,001–50,000	36	Wage of mayor and deputy mayor increases
	50,001–100,000	42	Additional deputy mayor
	100,001–200,000	46	Additional deputy mayor
			Wage of mayor and deputy mayor increases
	>200,000	50	Additional deputy mayor
Schleswig-Holstein	<200	7	–
	201–750	9	–
	751–1250	11	–
	1251–2500	13	–
	2501–5000	17	–
	5001–10,000	19	–
	10,001–15,000	23	Quota requirement for referenda decreases
			Status of a larger city
			Wage of mayor increases
	15,001–25,000	27	Full-time equal opportunity commissioner
			Wage of mayor increases
	25,001–35,000	31	–
	35,001–45,000	35	–
	>45,000	39	–

References

Acemoglu, D. (2005). Constitutions, politics, and economics: A review essay on Persson and Tabellini's. *The Economic effects of constitutions. Journal of Economic Literature, 43*(4), 1025–1048.

Ade, F., & Freier, R. (2011). When can we trust population thresholds in regression discontinuity designs? *DIW Discussion Papers, 1136,* 1–27.

Alesina, A., & Perotti, R. (1999). Budget deficits and budget institutions. In J. M. Poterba & J. von Hagen (Eds.), *Fiscal institutions and fiscal performance*. Chicago: University of Chicago Press.

Angrist, J. D., & Lavy, V. (1999). Using maimonides' rule to estimate the effect of class size on scholastic achievement. *The Quarterly Journal of Economics, 114*(2), 533–575.

Angrist, J. D., & Pischke, J. S. (2009). *Mostly harmless econometrics: An empiricists companion*. Princeton: Princeton University Press.

Baqir, R. (2002). Districting and government overspending. *Journal of Political Economy, 110*(6), 1318–1354.

Baron, D. P. (1991). Majoritarian incentives, pork barrel programs, and procedural control. *American Journal of Political Science, 35*(1), 57–90.

Battaglini, M., & Coate, S. (2007). Inefficiency in legislative policymaking: A dynamic analysis. *The American Economic Review, 97*(1), 118–149.

Battistin, E., & Rettore, E. (2002). Testing for programme effects in a regression discontinuity design with imperfect compliance. *Journal of the Royal Statistical Society, 165*(1), 39–57.

Becker, S., Egger, P., & von Ehrlich, M. (2010). Going NUTS: The effect of EU structural funds on regional performance. *Journal of Public Economics, 94*(9), 578–590.

Berk, R. (2010). Recent perspectives on the regression discontinuity design. In A. R. Piquero & D. Weisburd (Eds.), *Handbook of quantitative criminology* (pp. 563–579). Berlin: Springer.

Bloom, H. S. (2012). Modern regression discontinuity analysis. *Journal of Research on Educational Effectiveness, 5*(1), 43–82.

Bradbury, J. C., & Crain, M. W. (2001). Legislative organization and government spending: Cross-country evidence. *Journal of Public Economics, 82*(3), 309–325.

Bradbury, J. C., & Stephenson, F. E. (2003). Local government structure and public expenditures. *Public Choice, 115*(1/2), 185–198.

Buchanan, J. M., & Tullock, G. (1996) [1962]. *The calculus of consent: Logical foundations of constitutional democracy*. Ann Arbor: University of Michigan Press.

Butler, D. M. (2009). A regression discontinuity design analysis of the incumbency advantage and tenure in the US house. *Electoral Studies, 28*(1), 123–128.

Butler, D. M., & Butler, M. J. (2006). Splitting the difference? Causal inference and theories of split-party delegations. *Political Analysis, 14*(4), 439–455.

Calonico, S., Cattaneo, M. D., & Titiunik, R. (2014). Robust data-driven inference in the regression-discontinuity design. *The Stata Journal, 14*(4), 909–946.

Cattaneo, M. D., Jansson, M. & Ma, X. (2017). Rddensity: Manipulation testing based on density discontinuity. *The Stata Journal*, 1–22 (**forthcoming**).

Collie, M. P. (1988). Universalism and the parties in the US house of representatives, 1921-80. *American Journal of Political Science, 32*(4), 865–883.

Cook, T. D., & Wong, V. C. (2008). Empirical tests of the validity of the regression discontinuity design. *Annals of Economics and Statistics, 91*(92), 127–150.

Del Rossi, A. F. (1995). The politics and economics of pork barrel spending: The case of federal financing of water resources development. *Public Choice, 85*(3/4), 285–305.

Egger, P., & Koethenbuerger, M. (2010). Government spending and legislative organization: Quasi-experimental evidence from Germany. *American Economic Journal: Applied Economics, 2*(4), 200–212.

Eggers, A., Freier, R., Grembi, V., & Nannicini, T. (2017). Regression discontinuity designs based on population thresholds: Pitfalls and solutions. *American Journal of Political Science*. doi:10.1111/ajps.12332.

Ferejohn, J. A. (1974). *Pork barrel politics: Rivers and harbors legislation, 1947–1968*. Stanford: Stanford University Press.

Fiorino, N., & Ricciuti, R. (2007). Legislature size and government spending in Italian regions: Forecasting the effects of a reform. *Public Choice, 131*(1), 117–125.

Gelman, A., & Hill, J. (2007). *Data analysis using regression and multilevel hierarchical models*. Cambridge: Cambridge University Press.

Gilligan, T. W., & Matsusaka, J. G. (1995). Deviations from constituent interests: The role of legislative structure and political parties in the states. *Economic Inquiry, 33*(3), 383–401.

Gilligan, T. W., & Matsusaka, J. G. (2001). Fiscal policy, legislature size, and political parties: Evidence from state and local governments in the first half of the 20th century. *National Tax Journal, 54*(1), 57–82.

Hahn, J., Todd, P., & van der Klaauw, W. (2001). Identification and estimation of treatment effects with a regression-discontinuity design. *Econometrica, 69*(1), 201–209.

Hainmüller, J., Kern, H., & Bechtel, M. (2006). Wahlkreisarbeit zahlt sich doppelt aus. Zur Wirkung des Amtsinhaberstatus einer Partei auf ihren Zweitstimmenanteil bei den Bundestagswahlen 1949 bis 1998. In T. Bräuninger & J. Behnke (Eds.), *Jahrbuch für Handlungs- und Entscheidungstheorie* (Vol. 4, pp. 11–46). Wiesbaden: VS Verlag für Sozialwissenschaften.

Holland, P. W. (1986). Statistics and causal inference. *Journal of the American Statistical Association, 81*(396), 945–960.

Holtkamp, L. (2006). Parteien und Bürgermeister in der Repräsentativen Demokratie. Kommunale Konkordanz- und Konkurrenzdemokratie im Vergleich. *Politische Vierteljahresschrift, 47*(4), 641–661.

Imbens, G., & Kalyanaraman, K. (2012). Optimal bandwidth choice for the regression discontinuity estimator. *The Review of Economic Studies, 79*(3), 933–959.

Imbens, G. W., & Lemieux, T. (2008a). Regression discontinuity designs: A guide to practice. *Journal of Econometrics, 142*(2), 615–635.

Imbens, G. W., & Lemieux, T. (2008b). Special issue editors' introduction: The regression discontinuity design—Theory and applications. *Journal of Econometrics, 142*(2), 611–614.

Lee, D. S. (2008). Randomized experiments from non-random selection in US house elections. *Journal of Econometrics, 142*(2), 675–697.

Lee, D. S., & Lemieux, T. (2010). Regression discontinuity designs in economics. *Journal of Economic Literature, 48*(2), 281–355.

MacDonald, L. (2008). The impact of government structure on local public expenditures. *Public Choice, 136*(3/4), 457–473.

McCormick, R. E., & Tollison, R. D. (1981). *Politicians, legislation, and the economy. An inquiry into the interest-group theory of government*. Boston: Martinus Nijhoff Publishing.

McCrary, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics, 142*(2), 698–714.

Naßmacher, H., & Naßmacher, K. (2007). *Kommunalpolitik in Deutschland*. Wiesbaden: VS Verlag für Sozialwissenschaften.

Nichols, A. (2007). Causal inference with observational data. *Stata Journal, 7*(4), 507–541.

Nichols, A. (2011). rd 2.0: Revised stata module for regression discontinuity estimation. http://ideas.repec.org/c/boc/bocode/s456888.html.

Olson, M. (1965). *The logic of collective action: Public goods and the theory of groups*. Cambridge: Harvard University Press.

Perotti, R., & Kontopoulos, Y. (2002). Fragmented fiscal policy. *Journal of Public Economics, 86*(2), 191–222.

Persson, T., & Tabellini, G. (2003). *The economic effects of constitutions*. Cambridge: MIT Press.

Pettersson-Lidbom, P. (2008). Do parties matter for economic outcomes? A regression-discontinuity approach. *Journal of the European Economic Association, 6*(5), 1037–1056.

Pettersson-Lidbom, P. (2012). Does the size of the legislature affect the size of government? Evidence from two natural experiments. *Journal of Public Economics, 96*(3–4), 269–278.

Primo, D. M. (2006). "Stop us before we spend again: Institutional constraints on government spending. *Economics and Politics, 18*(3), 269–312.

Primo, D. M., & Snyder, J. M. (2008). Distributive politics and the law of 1/n. *The Journal of Politics, 70*(2), 477–486.

Riker, W. H. (1962). *The theory of political coalitions*. New Haven: Yale University Press.

Rubin, D. B. (1974). Estimating causal effects of treatments in randomized and nonrandomized studies. *Journal of Educational Psychology, 66*(5), 688–701.

Rubin, D. B. (2005). Causal inference using potential outcomes: Design, modeling, decisions. *Journal of the American Statistical Association, 100*(469), 322–331.

Schaltegger, C. A., & Feld, L. P. (2009). Do large cabinets favor large governments? Evidence on the fiscal commons problem for Swiss cantons. *Journal of Public Economics, 93*(1), 35–47.

Shadish, W. R., Cook, T. D., & Campbell, D. T. (2002). *Experimental and quasi-experimental designs for generalized causal inference*. Boston: Houghton Mifflin.

Shepsle, K. A., & Weingast, B. R. (1981). Political preferences for the pork barrel: A generalization. *American Journal of Political Science, 25*(1), 96–111.

Thistlethwaite, D. L., & Campbell, D. T. (1960). Regression-discontinuity analysis: An alternative to the ex post facto experiment. *Journal of Educational Psychology, 51*(6), 309–317.

Trochim, W. M. K. (1984). *Research design for program evaluation: The regression-discontinuity approach*. Beverly Hills: Sage.

Tullock, G. (1959). Problems of majority voting. *The Journal of Political Economy, 67*(6), 571–579.

Weingast, B. R. (1979). A rational choice perspective on congressional norms. *American Journal of Political Science, 23*(2), 245–262.

Weingast, B. R. (1994). Reflections on distributive politics and universalism. *Political Research Quarterly, 47*(2), 319–327.

Weingast, B. R., Shepsle, K. A., & Johnsen, C. (1981). The political economy of benefits and costs: A neoclassical approach to distributive politics. *The Journal of Political Economy, 89*(4), 642–664.