



# Does the size of the legislature affect the size of government? Evidence from two natural experiments<sup>☆</sup>

Per Pettersson-Lidbom

Department of Economics, Stockholm University, S-106 91 Stockholm, Sweden

## ARTICLE INFO

### Article history:

Received 26 March 2010

Received in revised form 17 June 2011

Accepted 10 July 2011

Available online 1 August 2011

### Keywords:

Government spending

Legislature size

Legislative policy-making

Distributive politics

Regression-discontinuity design

Panel data

## ABSTRACT

This paper makes use of regression discontinuity designs to estimate the effect of the number of legislators on the size of government. The results indicate a negative effect, i.e., the larger the size of the legislature the smaller is the size of government. This runs counter to conventional wisdom. One potential explanation is that more legislators can better control a budget maximizing bureaucracy. I present evidence that is consistent with the proposed mechanism.

© 2011 Elsevier B.V. All rights reserved.

## 1. Introduction

Legislative policy-making has received considerable attention by economists and political scientists alike. One institutional feature of legislative policy-making which has attracted particular interest is legislature size and its relation to government size or government spending.<sup>1</sup> In a seminal paper, Weingast et al. (1981) argued that legislators will try to benefit their constituents at the expense of the general community through pork barrel spending and other distributive policies. This would give rise to excessive government spending because

each legislator will internalize all the benefits from spending but only a fraction of the costs.

A number of empirical studies find support for this hypothesis but it is questionable whether they have identified a causal relationship since they have not properly addressed the endogeneity of legislature size.<sup>2</sup> For example, there may be a problem with reversed causality since a large public sector may require a large number of legislators to participate in the budget process due to the increased complexity of budget matters.<sup>3</sup>

Another potential concern is that previous studies conflate data from countries with very different election systems (e.g., Bradbury and Crain, 2001; Perotti and Kontopoulos, 2002), or that they combine data with both multi-member (i.e., at large election system) and single-member districts (e.g., Baqir, 2002 and Gilligan and Matsusaka, 1995, 2001), despite the fact that the theoretical prediction of a positive relationship between legislature size and government size is solely based on plurality rule and single member districts.<sup>4</sup> Nonetheless, all previous studies use the model by Weingast et al. (1981) to

<sup>☆</sup> Part of this work has been circulated in "Does the Size of the Legislature Affect the Size of Government: Evidence from a Natural Experiment". I thank Matthew Lindquist, Lena Nekby, Torsten Persson, David Strömberg, Jakob Svensson, Peter Skogman Thoursie and Björn Tyrefors for discussion and feedback. I also thank seminar participants at MIT, Princeton University, Stanford University, UC Berkeley, Harvard University, Yale University, Columbia University, New York University, University of Pennsylvania, University of Oslo, Uppsala University, Stockholm School of Economics, SOFI, IUI, FIEF, and IIES for useful comments. I am indebted to Antti Moisio at The Government Institute for Economic Research (VATT) for providing me with some of the Finnish data.

E-mail address: [pp@ne.su.se](mailto:pp@ne.su.se).

<sup>1</sup> Legislature size is also of perennial interest to policy makers. For example, one of the earliest discussions of legislature size appears in the Federalist Papers. The debate about the appropriate size is still ongoing in many countries. In England, for example, there is a current research project on council size and democracy on behalf of the Electoral Commission, which is an independent body set up by the UK Parliament. The outcome of this research is intended to provide the Boundary Committee for England with a robust basis for what might be the appropriate council size.

<sup>2</sup> Baqir (2002), Bradbury and Crain (2001), Gilligan and Matsusaka (1995, 2001) and Perotti and Kontopoulos (2002) all find a positive effect.

<sup>3</sup> There might also be an omitted variable problem due to unmeasured voter preferences since political institutions (legislature size) that are no longer preferred by a majority of voters will be overturned and therefore these institutions will simply reflect the preferences of the electorate.

<sup>4</sup> For example, in Baqir's (2002) study, more than 85% of the US cities have at large electoral systems, where candidates for office are elected from the entire jurisdiction. In the studies on US States by Gilligan and Matsusaka (1995, 2001), many U.S. State Legislatures also have multimember districts. For example, as of 1998, 13 states still had multimember districts in at least one of their legislative bodies.

motivate their findings. Moreover, even the conventional wisdom based on the Weingast et al. model can be questioned. In fact, [Primo and Snyder \(2008\)](#) show theoretically that the relationship between legislature size and government spending could be negative even under plurality rule and with single member districts. This type of theoretical ambiguity can only be solved empirically.

The main contribution of this paper is to estimate the causal effect of council size on government spending for jurisdictions with an identical electoral system. Naturally, the challenge of estimating a causal effect is to find some credible exogenous source of variation in legislature size. Generally, it is very difficult to find such variation since it is the policymakers themselves that decide on both size and policy, which makes legislature size endogenous to policy. Nonetheless, the national laws regulating the council-size in Finnish and Swedish local governments provide an unusually credible source of exogenous variations in legislature size. In Finland, the council size is a deterministic and discontinuous function of population size while in Sweden council size is a discontinuous but not deterministic function of the number of eligible voters. The Finnish council-size law thus fits with a sharp regression–discontinuity (RD) design while the Swedish law fits with a fuzzy RD design.

Results from both Finland and Sweden indicate a negative effect, i.e., the larger the size of the legislature, the smaller the size of government. The fact that this finding holds in two independent data sets and at multiple discontinuity points lends considerable credibility to the results being both internally and externally valid.

At first sight, a negative effect seems counterintuitive, at least if the explanation is based on the logic put forward in the model by [Weingast et al. \(1981\)](#) where policy decisions create benefits for well-defined groups (e.g., voters in an electoral district or ward) and with the costs dispersed in society at large. However, a negative relationship might arise, for example if there is a conflict between the legislature and the bureaucracy about the level of spending. Bureaucrats are often assumed to maximize their budgets (e.g., [Niskanen, 1971](#)) while policymakers cater to the voters, and voters are often characterized as being fiscally conservative (e.g., [Peltzman, 1992](#); [Besley and Case, 1995](#)). As a result, bureaucrats are therefore likely to prefer larger levels of spending than politicians.

The basic idea behind the agency problem is that elected legislators cannot make all policy decisions themselves since time constraints suggest that they must delegate some of their decision-making power to administrative officials (e.g., [Weingast and Moran, 1983](#)). Politicians therefore seek ways of monitoring and controlling how bureaucrats exercise this authority but monitoring and influencing bureaucrats' behavior also takes time. Thus, increasing the number of legislators may lead to a better monitoring and control of the public administration, in particular if politicians are severely time-constrained. Consequently, aggregate spending may be reduced if there is an agency problem between the legislature and the bureaucracy.

A potential agency problem between politicians and the public administration is likely to be particularly stark in Finland and Sweden for a number of reasons. For example, the local government sector is huge in both countries. It constitutes about 20% of GDP and employs nearly 20% of the total working population. Elected local politicians are also typically nonprofessionals that perform their duty in addition to their normal jobs. Thus, local politicians are likely to be severely-time constrained. Moreover, civil servants of local governments have strong positions with extensive decision-making powers in a number of policy areas.

One way of testing the proposed mechanism – the lack of political control of the bureaucracy – is to analyze outcomes that are more directly under the influence of the bureaucrats, such as public employment and operating expenditures, since the conflict of interest between the legislature and the public administration is likely to be particularly visible in areas where the administration has an extensive decision-making authority. I find evidence of local public employment

**Table 1**  
Local councils in Finland during the period 1977–2002.

Population thresholds	Number of council members required by law	Number of observations within an interval of one percent below cut-off	Number of observations within an interval of one percent above cut-off	Number of council changes
2000	17 below and equal to the threshold and 21 above	14	9	30
4000	21 below and equal to the threshold and 27 above	7	17	35
8000	27 below and equal to the threshold and 35 above	10	16	33
15,000	35 below and equal to the threshold and 43 above	7	1	16
30,000	43 below and equal to the threshold and 51 above	2	3	8
60,000	51 below and equal to the threshold and 59 above	0	0	0
120,000	59 below and equal to the threshold and 67 above	0	1	1
250,000	67 below and equal to the threshold and 75 above	0	0	0
400,000	75 below and equal to the threshold and 85 above	0	0	0

Notes: At the 2000 threshold there are altogether 23 observations from 18 different local governments, at 4000: altogether 24 observations from 18 different local governments, at 8000: altogether 26 observations from 21 different local governments, at 15,000: altogether 8 observations from 8 different local governments, at 30,000: altogether 5 observations from 5 different local governments, at 60,000: no observation, at 120,000: altogether one observation from one local government, at 250,000: no observation, at 400,000: no observation.

and operating expenditures being negatively related to the size of the legislature.<sup>5</sup>

In sharp contrast to the [Weingast et al. \(1981\)](#) prediction about the oversupply of public projects, I find little evidence that capital expenditure is affected by council size. This suggests that there is probably not much of pork-barrel politics in this type of settings.<sup>6</sup> Nonetheless, it is important to stress that the estimated council-size effect for aggregate spending still takes into account that some other sort of concentrated benefits/dispersed costs policies could emerge in

<sup>5</sup> An anonymous referee suggested the following alternative interpretation of the results. "If the councils are ultimately appropriating monies, then bureaucracies can't spend more than is appropriated. So the mechanism might be slightly different. When legislatures are smaller, monitoring is not quite as effective, so bureaucrats can more easily argue that more employees and more funds are required to accomplish a given task. As a result, the councils approve more spending, and bureaucrats devote it to areas like employment over which they have more control." However, it is not possible to discriminate between the two explanations with the available data.

<sup>6</sup> Council members in both Swedish and Finnish local governments are elected at large, that is, a council member represents all residents in a local government. The logic of the [Weingast et al. model \(1981\)](#) does not seem to apply in these settings which are based on the fact that politicians are elected from single-member districts. As result, the incentive for policymakers to overspend due to a common pool problem is likely to be more or less absent.

multi-member districts. In other words, the RD design provides reduced form estimates of the impacts of the treatment-council size. However, if there is any non-geographic pork-barrel politics at play, this would make it even harder to detect a negative council-size effect.

The result of this paper has a number of important implications for the literature on legislature size and the size of government. First, the negative council-size effect suggests that political context is of importance and therefore it casts some doubt on the validity of using the Weingast et al. model (1981) as a theoretical benchmark in political systems with at large elections and proportional representations. Second, all significant actors that can potentially affect economic policy must be brought into the analysis in order to understand how the size of the legislature affects economic policy. Specifically, previous research has neglected the role of the government administration in shaping the details of economic policy.<sup>7</sup> Finally, the results of this paper suggest that much more work is needed, both theoretical and empirical, to reconcile the conflicting findings in the current literature.<sup>8</sup>

More generally, this paper contributes to the literature on estimating the effects of political institutions on economic policy by exploiting unusually credible sources of variations in political institutions (e.g., Besley and Case, 2003, and Persson and Tabellini, 2003).<sup>9</sup> Specifically, this paper also adds to the recent work on regression-discontinuity designs in political economics that implements RD designs.<sup>10</sup> In fact, this paper is the first study to exploit population thresholds as an RD strategy at the local government level.<sup>11</sup> In many other countries such as Italy, Brazil, Germany, France, Sweden, Finland and Norway, the population size of a local government determines specific “treatments” or political institutions that can be used for the identification of key relationships in political economics.

The rest of the paper is structured as follows. Section 2 describes and analyzes the local government data set from Finland. Section 3 describes and analyzes the Swedish local governments. Section 4 provides suggestive evidence of the mechanism underlying the negative relationship between council size and spending while Section 5 concludes the paper.

## 2. Finland

### 2.1. Background

This section describes the local governments in Finland with a particular focus on the council-size laws of local governments that provide the exogenous sources of variations in council size.

Local governments play an extremely significant role in Finnish society. They are, for example, responsible for the provision of day care, education, care of the elderly and social welfare services.<sup>12</sup> As a result, their spending as a share of GDP is about 18% and they employ roughly 20% of the total workforce. They also have the constitutional right of self-government. For example, they can set their own proportional income tax rate. In 2008, the range in the proportional income tax rate was between 16 and 21%. They have no balanced budget requirements and

<sup>7</sup> Curiously, the standard reference in political economics, Persson and Tabellini (2000), does not deal with the bureaucracy as discussed in the introduction to their book.

<sup>8</sup> Ting (2010) develops a theory of bureaucratic influence on distributive politics.

<sup>9</sup> This literature faces very difficult identification problems as discussed by Besley and Case (2003) and Persson and Tabellini (2003). Acemoglu (2005), for example, “questions whether this research has successfully uncovered causal effects”.

<sup>10</sup> For example, Pettersson-Lidbom (2001a, 2008) were the first studies that exploited close elections as an RD strategy to answer whether parties matter for policy choices while Lee (2008) was the first to use them to estimate the incumbency advantage.

<sup>11</sup> Pettersson-Lidbom (2001b, 2004) were the first two studies exploiting treatment rules based on local governments' population sizes. This literature includes later work by, e.g., Bordignon et al. (2009), Brollo et al. (2009), Chamon et al. (2008), Ferraz and Finan (2009), Fujiwara (2008, 2010), Gagliarducci et al. (2008), Gagliarducci and Nannicini (2009) and Litschig and Morrison (2009).

<sup>12</sup> 48% of total spending is on social welfare and health care and 24% on education and culture.

**Table 2**  
Descriptive statistics for Finland.

	Mean	St. dev.	Min	Max
Council size	28.0	10.0	17	85
Spending per capita	20,053	5579	6537	58,345
Population size	10,916	31,844	240	559,718
The proportion of the population aged 0 to 15	20.88	3.30	11.35	38.64
The proportion of the population aged 65 and above	16.04	4.48	4.08	30.33
Income	44,314	12,607	16,723	174,557

Notes: Spending per capita is expressed in 1995 prices and in FIM for Finland (\$1 ≈ 5 FIM).

no restrictions on borrowing. Moreover, central government transfers account for less than one-fifth of all local government revenues.

The local election schedule is fixed and elections are held simultaneously across all local governments every fourth year (e.g., 1976, 1980, etc.) on the fourth Sunday in October. The election system is proportional representation (PR) in multi-seat constituencies. The PR system is an open and unordered list system. In Finland, this means that a voter only casts her vote on an individual candidate and not directly on a party, and that the central election committee compiles a combined list of candidates in which the candidates of all parties are enumerated in an order drawn by lot. The open and unordered list system thus implies that voters have a larger influence on who gets elected than under a closed list system. Voter turnout has been between 60 and 80% during the studied period 1977 to 2002. Three major parties dominate the political arena (the Social Democratic Party, the Centre Party, and the Coalition Party (right-wing)). Consensus has been the dominant mode of Finnish politics since the formation of a broadly based coalition government at the national level in the late 1960s.<sup>13</sup>

Finnish local governments have a council-manger government system.<sup>14</sup> The decision-making power of the local government is exercised by a council elected by the residents. The council is the highest decision-making body in the municipality. Decisions are taken by simple majority of the council members, but before 1995 decisions relating to financial or budgetary questions usually required two-third majorities in council votes. The municipal executive board is appointed by the council and it is chosen for a two-year period, albeit in practice it often remains the same for the full four-year term of the council. The board meets, on average, once a week. The manager is appointed by the council and her tenure may be either definite or indefinite. She is not a member of the Council, nor does she sit on the board. In other words, the manager is an employee, or the highest level civil servant within the municipality. She serves under the executive board. The manager is the head of the administration, financial management and other functions of the municipality.

A national law prescribes a specific number of council members in relation to the population size at a specific date during the election year.<sup>15</sup> Importantly, data on population size is based on the central government's administrative records, which means that the population is measured without errors and that it cannot be misreported by the local governments. Moreover, population data is registered on a yearly basis which offers a number of attractive features as compared to data sets only based on census years (typically every tenth year).<sup>16</sup>

<sup>13</sup> Studies that investigate the effect of partisanship variables on fiscal policy outcomes at the local level in Finland find little or no effects (e.g., Moisio, 2002), which are consistent with the consensus mode of politics.

<sup>14</sup> More than half of all American cities with populations of between 25,000 and 250,000 operate under council-manager governments (e.g., MacDonald, 2008).

<sup>15</sup> Until the election 1992, it was the population size on January 1st during an election year that determined the council size. From the election in 1996 it is the population size on May 31st.

<sup>16</sup> The population data used in this paper is measured on an annual basis on December 31st.

**Table 3**  
Estimates of council size effect in Finland.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLS		FE		RD				
Council size effect	.210*** (.023)	.157*** (.037)	-.199*** (.040)	-.169*** (.041)	-.159*** (.041)	-.147*** (.041)	-.090** (.039)	-.081** (.039)	-.088** (.038)
Degree of polynomial in population size	None	None	None	None	First	Second	Third	Fourth	Fifth
Controls	No	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each entry is a separate regression. The dependent variable is log per capita spending. Council size is also in log form. Data includes 391 local governments over the period 1977 to 2002, i.e., there are 10,166 observations. Huber-White standard errors allowing for clustering at the local government level are in parentheses. The control variables are population size, the proportion of people aged 65 or above, the proportion of people aged below 15, and per capita income. All regressions include time-fixed effects. Columns 3–9 include municipality-fixed effects.

\*\*\* Significant at the 1% level.

\*\* Significant at the 5% level.

\* Significant at the 10% level.

Most importantly, there are no other policies that change at any of these population thresholds since according to the legislation, all municipalities, whether large or small, have identical responsibilities.

The council size law is displayed in Columns 1 and 2 of Table 1 and it states that if a municipality's population is less or equal to 2000, the council must consist of 17 members; if the population is larger than 2000 but less or equal to 4000 the law states that the council size must be 21, etc. The law can now potentially induce nine discontinuities between population size and the size of the council at the following population levels or thresholds: 2000, 4000, 8000, 15,000, 30,000, 60,000, 120,000, 250,000 and 400,000. For instance, suppose that a municipality had 2000 inhabitants and thus had to have 17 council members. Suppose further that the population increased by one, i.e., to 2001, then the local government would be forced by law to increase its council size to 21.

## 2.2. Data and empirical framework

This paper uses data on Finnish local governments during the period 1977 to 2002. In 1977, there existed 464 local governments but due to voluntary amalgamations, this number was reduced to 448 in 2002. I will exclude all local governments that have amalgamated during the period and all local governments that belong to the autonomous region of Åland. This leaves us with a remaining sample of 391 local governments.

The empirical design will be based on a regression-discontinuity (RD) approach as noted above. However, the RD approach will differ from the traditional cross-sectional approach for reasons explained in the following. As an illustration of the problem with using a traditional RD design in the current application, all election years, namely the years 1976, 1980, 1984, 1988, 1992, 1996 and 2000, are pooled together. Columns 3 and 4 of Table 1 shows the number of observations within an interval of one percent around each of the cut-offs (e.g., the interval at the threshold 2000 ranges between population sizes 1980 and 2020). At each of these thresholds, the number of observations ranges from 0 to 26. The very small sample size creates problems for a traditional RD design since it is largely based on non-parametric identification (e.g., Hahn et al., 2001; Lee, 2008). In particular, when the sample size is severely limited, the distribution of the treatment-determining variable will not have continuous support (i.e., there will be intervals with few or no observations) which makes non-parametric estimation methods impractical since extrapolation is now also required outside the neighborhood of the discontinuity. Moreover, a limited sample size also means that the efficiency aspect of the estimation method becomes much more salient. To address these concerns, I will pool together data from all thresholds and use a fixed-effect approach combined with the RD set up, that is, I will estimate regressions of the form

$$Y_{it} = \alpha_i + \lambda_t + \beta \text{Csize}_{it} + f(x_{it}) + u_{it} \quad (1)$$

where  $Y_{it}$  is a measure of government size,  $\text{Csize}_{it}$  is the council size,  $f(\cdot)$  is a smooth function of the treatment-determining or forcing variable  $x_{it}$  (population size),  $\alpha_i$  is a fixed-municipality effect and  $\lambda_t$  is a year-fixed effect. The parameter of interest is  $\beta$  – the council-size effect – which is assumed to be linear across all treatment thresholds.

Although Eq. (1) clearly imposes more parametric assumptions than the traditional RD, it seems to be a sensible specification when the sample size is limited for two reasons. A traditional RD exploits the fact that local randomization ensures that the samples of subjects “just below” and “just above” the threshold are similar on average, both in terms of observed and unobserved characteristics. However, in practice, a very small sample size makes it questionable whether treatment and comparison groups are comparable, i.e., balanced on all pre-treatment characteristics, as recently discussed by Bruhn and McKenzie (2009).<sup>17</sup> In sharp contrast, a fixed-effect approach explicitly makes the treatment and comparison groups comparable *independently* of sample size, since the comparison is made within the same subject rather than between different subjects. A second reason for using a fixed-effect approach is that it can be more efficient than a pure cross-sectional approach if the error variance is greatly reduced by the inclusion of the fixed effects.

A within-subject approach also has the attractiveness that it can exploit the time-series variation in the data. For example, it is possible to estimate dynamic causal effects by using a distributed lag model of council size, i.e.,<sup>18</sup>

$$Y_{it} = \alpha_i + \lambda_t + \beta_1 \text{Csize}_{it+1} + \beta_2 \text{Csize}_{it} + \beta_3 \text{Csize}_{it-1} + f(x_{it}) + u_{it} \quad (2)$$

where  $\beta_2$  measures the impact effect,  $\beta_3$  the one-period dynamic multiplier effect, and  $\beta_1$  is the effect one-period before the treatment (council size) actually occurs. With Eq. (2), it is possible to test for how quickly the change in council size affects the outcome. For example, if the treatment effect occurs within the first time period, then  $\beta_3 = 0$ . Moreover, if  $\beta_1 \neq 0$ , this would cast some doubt on a causal interpretation of the relationship between council size and government size, since this means that the treatment effect arises before the treatment itself. In other words, this is a falsification test of the identification strategy.

Another way of exploiting the time-series data is to estimate Eq. (1) separately for those local governments that increased their council sizes and for those that decreased their council sizes. If these two estimates are similar this would further strengthen a causal

<sup>17</sup> Bruhn and McKenzie (2009) state that in samples of 300 or more, the method of randomization (e.g., pure randomization, stratification, and pair-wise matching) is of no importance in terms of achieving balance. However, for very persistent outcome variables and in smaller samples, the method of randomization is critical for achieving balance.

<sup>18</sup> For a textbook treatment of how to estimate dynamic causal effects, see Stock and Watson (2007).

**Table 4**  
Estimates of the council size effect in Finland: different window sizes around thresholds.

	Third-order polynomial in population size	Fourth-order polynomial in population size	Fifth-order polynomial in population size
Panel A: $\pm 25\%$ Council size effect	-.087** (.039)	-.092** (.038)	-.081** (.037)
Panel B: $\pm 10\%$ Council size effect	-.075* (.039)	-.078** (.040)	-.068 (.045)
Panel C: $\pm 5\%$ Council size effect	-.083* (.044)	-.083* (.044)	-.084* (.044)
Panel D: $\pm 2.5\%$ Council size effect	-.079 (.066)	-.079 (.067)	-.076 (.065)

Notes: The dependent variable is log per capita spending. Council size is also in log form. Huber-White standard errors allowing for clustering at the local government level are in parentheses. All regressions include municipality fixed effects, time effects and control variables.

\*\*\* Significant at the 1% level.

\*\* Significant at the 5% level.

\* Significant at the 10% level.

interpretation since it is then harder to explain these correlations by some omitted confounding factor.

Turning to a discussion about the time-series variation in council size in Finland, Table 1 shows the number of law-induced changes in council size across the years of investigation 1977–2002 for each of the nine population thresholds. There have been six elections and the change in council size can only occur the year after an election year. As a result, any changes in council size could only take place in the years 1981, 1985, 1989, 1993, 1997 and 2001. Column 5 of Table 1 shows that there are 30 or more changes at the first three thresholds (2000, 4000 and 8000). Table 1 also reveals that there are a total of 123 changes in council size during the sample period: two municipalities had three changes, 11 had two changes, while 95 had one change.

Total real spending per capita will be used as a measure of government size. In the regressions, both spending and council size will be expressed in logarithmic form. Thus, the council-size effect  $\beta$  will have an elasticity interpretation.

A number of other control variables than the treatment-determining variable will be included: income, the proportion of the population aged 0 to 15, and the proportion of the population aged 65 and above. In

**Table 5**  
Estimates of the council size effect in Finland: decreasing and increasing council sizes.

	(1)	(2)	(3)	(4)	(5)	(6)
	Council size increased			Council size decreased		
Council size effect	-.072 (.053)	-.059 (.053)	-.068 (.051)	-.090* (.047)	-.085* (.047)	-.090* (.047)
Degree of polynomial in population size	Third	Fourth	Fifth	Third	Fourth	Fifth
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is log per capita spending. Council size is also in log form. Huber-White standard errors allowing for clustering at the local government level are in parentheses. All regressions include municipality fixed effects, time effects and control variables.

\*\*\* Significant at the 1% level.

\*\* Significant at the 5% level.

\* Significant at the 10% level.

principle, there is no need to include additional covariates in the regression discontinuity approach other than the correct functional form of the treatment determining variable to get an unbiased estimate of the treatment effect. In practice, however, there may still be reasons for including other regressors so as to avoid a small sample bias and increase statistical efficiency.

Table 2 presents summary statistics for the Finnish data. All Finnish data is publicly available and was obtained from Statistics Finland (Tilastokeskus).

### 3. Results for Finland

In this section, I present results on the relationship between council size and government size from the RD designs in Finland. Initially, however, I present results from a conventional cross-sectional regression, i.e., OLS regressions with a number of control variables, as used by many of the previous studies. These results may be seen as a benchmark for assessing potential biases in previous work. Columns 1 and 2 of Table 3 present the results from OLS regressions of government spending on council size with and without controls for a number of confounding factors. I control for the proportion of people aged above 65, the proportion of people aged below 15, income per capita, and population size, since these are considered to be a standard set of control variables in the empirical literature investigating the determinants of policy choices of state and local governments (e.g., Besley and Case, 2003). I also add a full set of time-fixed effects since I pool the data across many years. Huber-White standard errors clustered at the municipality level are reported following the suggestions of Bertrand et al. (2004). This turns out to be an important consideration since the standard errors allowing for arbitrary serial correlation within municipalities are roughly two times larger than the heteroskedastic-robust standard errors suggesting that the latter are not valid.<sup>19</sup>

Column 1 shows that the relationship between spending and council size is positive and highly significant in the specifications without any controls. For example, the estimated council size effect is 0.21. Since this effect is expressed as an elasticity measure – spending and council size are in log forms – this implies that a 10% increase in council size increases spending by about 2.1%. This suggests that, in practice, total spending is increased between 4.2 and 6.3% since most of the changes in council size are between 20 and 30%. When including control variables in column 2, the estimated council-size effect decreases somewhat to 0.16.

As previously noted, a positive statistical association does not necessarily reflect a causal relationship since we have not isolated any exogenous variations in council size. This is what I attempt to do in the following using results from the RD design, i.e., Eq. (1). Since a major concern in a regression discontinuity design is whether the control function  $f(x_{it})$  is correctly specified, I will use a number of different specifications of the control function. Specifically, I will go from a first-order up to a fifth-order polynomial in population size as a way of testing whether the estimate of the council-size effect is sensitive to the different specifications of the control function.

I will also include fixed-municipality and time effects in all specifications. The inclusion of the fixed-municipality effects ensures that we only compare within-municipalities which are of great importance since otherwise the treatment and control groups are not likely to be comparable due to a problem with small sample sizes as previously discussed.

Table 3 shows the results. In Column 3, the results from a pure fixed effect (FE) regression are displayed. In Column 4, I add the proportion of

<sup>19</sup> The statistical significance of the results from the papers by Bradbury and Crain (2001), Gilligan and Matsusaka (1995, 2001) and Perotti and Kontopoulos (2002) are probably grossly overstated since they do not account for serial correlation in their panel data regressions even though legislature size and spending are quite persistent over time.

**Table 6**  
Estimates of the council size effect in Finland: dynamic specifications.

	(1)	(2)	(3)	(4)	(5)	(6)
Council size <sub>t</sub>	−.084* (.044)	−.123*** (.039)	−.091** (.038)	−.082** (.037)	−.070** (.034)	−.081** (.039)
Council size <sub>t-1</sub>		.060 (.049)	.029 (.038)	.031 (.038)	.033 (.045)	.040 (.040)
Council size <sub>t-2</sub>			.054 (.071)	.054 (.056)	.048 (.054)	.043 (.053)
Council size <sub>t-3</sub>				−.010 (.051)	−.015 (.051)	−.016 (.053)
Council size <sub>t+1</sub>					−.016 (.038)	.015 (.037)
Council size <sub>t+2</sub>						−.047 (.036)

Notes: The dependent variable is log per capita spending. Council size is also in log form. Huber-White standard errors allowing for clustering at the local government level are in parentheses. All regressions include municipality fixed effects, time effects and control variables.

\*\*\* Significant at the 1% level.  
\*\* Significant at the 5% level.  
\* Significant at the 10% level.

people aged above 65, the proportion of people aged below 15, and income per capita to the specification. Columns 5–9 of Table 3 show the result from the regression-discontinuity (RD) specifications when I step-wise add more flexible polynomials in population size: first to fifth order. In sharp contrast to the conventional OLS results, all estimated council size effects are negative, i.e., a larger council size decreases spending. It is important to note that the estimated council size effect is basically similar in the last three columns (7–9) since all estimates are in the narrow range of −0.081 to −0.090. Thus, the insensitivity of the estimated council-size effect to polynomials of degree 3 or higher suggests that the control function is not misspecified. It is also noteworthy that a pure fixed effect analysis or controlling for too few polynomials in population size would result in a larger estimate of the council-size effect.

As an additional specification check, the council-size effect is estimated only using observations that are close to the treatment thresholds since the estimate from such a “discontinuity” sample should be equal (apart from sampling variability) to the estimate from the control function approach, unless the control function  $f(x_{it})$  is misspecified. Table 4 reports results from samples with four different window sizes around the discontinuities:  $\pm 25$ ,  $\pm 10$ ,  $\pm 5$ , and  $\pm 2.5\%$ , respectively. In addition, different specifications of  $f(x_{it})$  are being used, namely in Column 1 it is a third-order polynomial, in Column 2 it is a fourth-order polynomial, and in Column 3 it is a fifth-order polynomial. Most of the council-size estimates in Table 4 are strikingly similar suggesting that  $f(\cdot)$  is not misspecified.

As another specification check is to exploit time series changes in council size, namely rule-based increases and decreases in council size over time as discussed above. Table 5 reports specifications for those local governments that were required to increase their council size and for those that were forced to decrease their size. In each case, three different specifications of the control function are used: namely a third-, fourth- and fifth-order polynomials in population size. Once more, all specifications include control variables together with fixed municipality and time effects. The estimates for those with an increasing council size are about −.07 and for those with a decreasing council size about −.09. Although these estimates are less precisely estimated than those in Table 3 (the standard errors are about 30% larger), they are still broadly similar which lends credibility to a causal interpretation of the estimated council size effect.

Another way of exploiting time series changes in council sizes is to estimate dynamic causal effects, i.e., a distributed lag model relating council size and its lags (and leads) to spending (see Eq. (2)). Table 6 shows the results from such specifications where the  $\pm 5\%$  sample with a fifth-order polynomial is being used. For comparison purposes, Column 1 displays the estimate from the  $\pm 5\%$  sample which is taken from Panel C in Table 4. In the subsequent columns of Table 6, results are reported where lags and leads of council size are added to the specification in Column 1. It is noteworthy that it is only the coefficient on the contemporaneous value of council size that is significant since neither the lags nor the leads are significantly different from zero in any

of the specifications in Table 6. The insignificant estimates of all lags of council size therefore suggest that the effect of a change in council size takes place within the first year of a change in council size, while the insignificant estimates of the leads lend support to the credibility of the identification strategy.

## 4. Sweden

### 4.1. Background

Sweden is currently divided into 290 local governments or municipalities, which cover the entire country. Local governments (or municipalities) play a very important role in the Swedish economy, both in terms of the allocation of functions among different levels of government and economic significance. They are, for example, responsible for the provision of day care, education, care of the elderly, and social welfare services. In trying to quantify their economic importance, it can be noted that during the 1980s and 1990s, their share of spending out of GDP was 25% and they employed roughly 20% of the total Swedish workforce. Swedish local governments also have a large degree of autonomy. They have the constitutional right of self-government, they have no restrictions on borrowing, and they have no balanced budget rules.<sup>20</sup> Moreover, less than 25% of their income comes from grants, whereas the rest mostly comes from a proportional income tax, which each municipality can set freely.

The election schedule is fixed and elections are held every fourth year on the third Sunday in September.<sup>21</sup> Voter turnout has been high, close to 90%, in many of the local elections in Sweden. The Swedish election system is based on closed list proportional representation with the existence of several political parties. However, in contrast to the consensus mode of politics in Finland, the political map in Sweden has been characterized by a clear dividing line between socialist and non-socialist parties leading to a quite stable two-bloc system. The two-bloc feature has also led to relatively large differences in fiscal policy outcomes between the two blocs at the local level as discussed by Pettersson-Lidbom (2001a, 2008).

The system of local government in Sweden is in many respects similar to a city commission government and a cabinet-style council government. A legislative municipal assembly is elected from party-list proportional representation at municipal elections. The assembly, in turn, appoints a municipal executive committee from its council members. The executive committee is headed by its chairman. The structure of the local public administration in Sweden is similar to the Finnish structure discussed above.

The Swedish council size law prescribes a minimum requirement of council size in relation to the number of eligible voters as can be seen

<sup>20</sup> From 2000, there is a “weak” balanced budget rule in force.

<sup>21</sup> Since 1994, elections are held every fourth year.

**Table 7**  
Local councils in Sweden during the period 1977–2002.

Number of eligible voters	Number of council members required by law	Mean	St. dev.	Min	Max
0–12,000	At least 31	40.5	5.0	31	49
12,001–24,000	At least 41	47.6	4.1	41	61
24,001–36,000	At least 51	52.8	4.7	49	75
36,000–	At least 61	67.2	8.0	51	85

Notes: Stockholm (the capital) is required to have at least 101 council members.

from Columns 1 and 2 of Table 7.<sup>22</sup> The law states that if the number of eligible voters is less or equal to 12,000, the council must consist of at least 31 members; if the number of eligible voters is between 12,000 and 24,000, the law states that the council size must be no less than 41; if the number of eligible voters is over 24,000 but less or equal to 36,000, then the size must be at least 51, and finally if the number of eligible voters is more than 36,000, the size must be at least 61. The law can now potentially induce three discontinuities between the number of eligible voters and the size of the council at the thresholds of 12,000, 24,000 and 36,000.

Table 7 shows the actual size of the local council grouped by segments with a minimum requirement of council sizes of 31, 41, 51 and 61. This table also reveals that many municipalities have chosen to have more council members than what is required by law. This is particularly true for those with a requirement of at least 31 members. On average, this group has slightly more than 40 seats. As will be discussed below, the municipalities that were forced to change their council size due to the statutory law are those who will help identify the council size effect. At the 12,000 threshold only one municipality was forced to change its council size at the lowest threshold, whereas 12 and 7 municipalities had to change their number of seats for the middle and highest cutoffs, respectively.<sup>23</sup>

#### 4.2. Data and empirical framework

This paper uses data on Swedish local governments during the period 1977 to 2002. In 1977, there existed 277 local governments but due to voluntary splits, this number was increased to 289 in 2002. I will exclude all local governments that have been engaged in splits during the period. This leaves us with a remaining sample of 265 local governments.

In the Swedish setting, where the size of the local council is only partly determined by statutory law, a different empirical approach than in the Finnish case is required since the regression discontinuity design is not sharp but rather “fuzzy”. One approach in the fuzzy regression-discontinuity case is to use the method of instrumental variables (e.g., Angrist and Pischke, 2009 and Hahn et al., 2001) as explained in the following.<sup>24</sup>

<sup>22</sup> Until 1997, eligibility to vote was based on information pertaining to July 1st the year before the election year, but since then it is based on information from the previous election (i.e., four years back in time).

<sup>23</sup> The number of potential changes at each of was larger: 21 at the lowest threshold, and 15 and 11 for the middle and highest cutoffs, respectively.

<sup>24</sup> The use of instrumental variables raises the issue of the interpretation of the estimated parameter of interest, namely the council-size effect. Here, we can draw on the treatment literature. This literature has defined four different causal effects: average treatment effect (ATE), treatment on the treated effect (TT), local average treatment effect (LATE) and marginal treatment effect (MTE). It turns out that these effects coincide if the treatment effect is linear and constant across all units. However, if this is not the case, the exogeneity assumption of the instruments alone is usually not sufficient for identifying a meaningful treatment effect. Instead, one needs to make additional assumptions about how the instrument affects the participation or selection into treatment. For example, random assignment into treatment and control groups and full compliance with the treatment protocol identifies the ATE. In our case, if the constant treatment assumption fails, the council size effect will be identified as TT since there is a population of municipalities that is denied to take certain treatments because of the council size law as discussed by Angrist and Imbens (1991).

**Table 8**  
Descriptive statistics for Sweden.

	Mean	St. dev.	Min	Max
Council size	47.5	10.9	31	101
Spending per capita	34,068	7273	19,253	90,123
Population size	29,991	54,878	3132	754,948
The proportion of the population aged 0 to 15	20.68	2.41	12.65	36.42
The proportion of the population aged 65 and above	18.25	3.96	3.97	27.37
Income	87,404	17,745	38,674	234,625

Notes: Spending per capita is expressed in 1995 prices and in SEK for Sweden. (\$1 ≈ 8 SEK).

The Swedish council size law, as displayed in Table 7, states that the number of council members must be at least 31, 41, 51 or 61 depending on in which of four intervals the number of eligible voters in a local government falls. Thus, the law potentially induces three discontinuities in council size at values 12,000, 24,000 and 36,000. The idea is to use these discontinuities as instrumental variables, that is, to divide the municipalities into four groups and use a set of dummy variables to indicate each group, i.e.,  $Z_{31} = 1[vot \leq 12,000]$ ,  $Z_{41} = 1[12,000 < vot \leq 24,000]$ ,  $Z_{51} = 1[24,000 < vot \leq 36,000]$ , and  $Z_{61} = 1[vot > 36,000]$  where *vot* is the number of eligible voters and the sub-indices refer to the minimum required council size within each group. Since the instruments are mutually orthogonal indicator variables, it is possible to construct distinct IV or Wald estimates of the council-size effect (e.g., Angrist, 1991). Thus, it is possible to construct three different estimates of the council-size effect since there are three linearly independent dummy variables. However, we can use a Two-Stage-Least-Square (2SLS) procedure to form a single 2SLS estimate. The 2SLS estimate is a weighted average of each of the instrumental variables estimates obtained taking the instruments one by one.<sup>25</sup> The instrumental variable approach can now formally be expressed by two equations:

$$Y_{it} = \alpha_i + \lambda_t + \theta Csize_{it} + f(x_{it}) + \varepsilon_{it}, \tag{3}$$

$$Csize_{it} = \alpha_i + \lambda_t + \psi_{41}Z_{41it} + \psi_{51}Z_{51it} + \psi_{61}Z_{61it} + g(x_{it}) + \xi_{it}, \tag{4}$$

where Eq. (3) is the structural equation and Eq. (4) is the reduced form or the “first-stage” equation for the endogenous variable *Csize<sub>it</sub>*. Here, Eq. (3) is similar to Eq. (1) except for the treatment-determining variable in the Swedish case being the number of eligible voters, *vot*, rather than population size. The previous discussion about Eq. (1) therefore automatically transfers to Eq. (3) while Eq. (4) requires some additional comments about instrument validity, that is, whether the instruments:  $Z_{41}$ ,  $Z_{51}$ , and  $Z_{61}$ , are exogenous and relevant.

The requirement that the instruments should be exogeneous implies that once we control for  $f(\cdot)$ ,  $\lambda_t$ , and  $\alpha_i$ , this will partial out any other effects between the instruments and the size of government. The requirement of relevance of the instruments will be checked by computing the *F*-statistics testing the hypothesis that the coefficients on the instruments are all zero in the first-stage regression of 2SLS. This first-stage *F*-statistic should exceed 10 to avoid the weak instrument problem as discussed by Staiger and Stock (1997).

In contrast to the sharp RD design, in the fuzzy design it is problematic to take dynamic treatment effects, i.e. include leads and lags of the council size, into account since the fuzzy design is based on an

<sup>25</sup> Another useful way of thinking about this particular way of constructing instrumental variables is to make a comparison with a randomized experiment where there is only partial compliance with the treatment protocol. Since the council size can be partly chosen by the municipalities, there will only be partial compliance with the treatment protocol. In such a case, the assigned treatment level can serve as an instrumental variable for the actual treatment level, which is exactly the reason why the council-size law can be used to construct instrumental variables for council size.

**Table 9**  
Estimated council size effect in Sweden.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLS		FE		RD				
Council size effect	.123 (.046)	-.048 (.057)	-.688** (.198)	-.389 (.253)	-.264 (.263)	-.136 (.275)	-.376 (.291)	-.143 (.318)	-.102 (.321)
Degree of polynomial in number of eligible voters	None	None	None	None	First	Second	Third	Fourth	Fifth
Controls	No	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F-test			12.8	9.1	8.6	9.3	9.9	10	10
J-test (chi-sq(2)) P-value within parenthesis			0.59 (0.74)	1.65 (0.44)	2.04 (0.36)	2.29 (0.32)	2.21 (0.33)	2.21 (0.33)	2.19 (0.34)

Notes: The dependent variable is log per capita spending. Council size is also in log form. Data includes 265 local governments over the period 1977 to 2002. There are 6876 observations. Huber-White standard errors allowing for clustering at the local government level are in parentheses. The control variables are population size, the proportion of people aged below 65 or above, the proportion of people aged below 15, and per capita income. All regressions include time-fixed effects. Columns 3–9 include municipality-fixed effects.

\*\*\* Significant at the 1% level.

\*\* Significant at the 5% level.

\* Significant at the 10% level.

**Table 10**  
Estimates of council size effect in Finland: operating expenditures.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Council size effect	-.177*** (.046)	-.139** (.047)	-.129** (.046)	-.122** (.047)	-.077* (.044)	-.070 (.043)	-.078* (.042)
Degree of polynomial in population size	No	No	First	Second	Third	Fourth	Fifth
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is log per capita operating expenditures. Council size is also in log form. Huber-White standard errors allowing for clustering at the local government level are in parentheses. All regressions include municipality fixed effects, time effects and control variables.

\*\*\* Significant at the 1% level.

\*\* Significant at the 5% level.

\* Significant at the 10% level.

instrumental variable approach where the identification of many endogenous variables with only a few available instruments is infeasible.

Table 8 shows the summary statistics for the Swedish data. All Swedish data is publicly available and was obtained from Statistics Sweden.

## 5. Results for Sweden

In this section, I present the results from the Swedish natural experiment. However, I first start with the results from conventional cross-sectional specifications which are displayed in Columns 1 and 2 of Table 9. The OLS specifications in Table 9 are identical to the OLS specification in Table 3 for Finland. Similarly to the Finnish results, there is a positive relation between council size (Column 1) and spending but the council-size effect becomes negative but insignificant when controls are added to the specification for Sweden (Column 2).

Turning to the instrumental variable approach (i.e., Eqs. (1) and (2)), Table 9 shows the results from two-stage least squares specifications. The first-stage F test is about 10 for most of the specifications, suggesting that the instruments are not particularly weak. All council-size estimates in columns 3–9 are very imprecisely measured which is not surprising given the very few numbers of law-induced council size changes in Sweden (the standard errors are 6–8 times larger for Sweden as compared to Finland). Nonetheless, it is still noteworthy that all estimates are negative. Moreover, the elasticity of the council-size effect in Columns 8 and 9, the two most general specifications, is nearly identical to the elasticity results in Finland (e.g., see Table 3).

Taken together, the close similarity between the Swedish and the Finnish results suggests that the same mechanism may be at work in both settings. In other words, this suggests that the negative council-size effect of the RD designs does not only have internal validity but also some external validity.

## 6. Mechanism

In this section, I provide suggestive evidence on the mechanism that might underlie the negative council-size effect for total spending in Finland and Sweden. As noted in the introduction, the agency problem between politicians and bureaucrats is likely to be most noticeable in policies over which the administration has considerable influence.

One policy outcome that the local administration is likely to have considerably discretion over is operating expenditures since it is generally the bureaucrats that are involved in most of the day-to-day activities of the local governments. Unfortunately, operating expenditures are not available for Sweden. Table 10 presents the results for Finland using the same RD specification as in Table 3. All estimates are negative and all of them except one are significantly different from zero at the 10% level. Moreover, the size of the council-size effect is nearly identical to those in Table 3.

Another local policy over which bureaucrats may have considerably influence is local public employment. The number of local government employees per 1000 inhabitants is 68 for Sweden and 46 for Finland. These numbers are comparable with the U.S. average of 52 for state employees and local government employees in 2003, which once more underscores the economic importance of local governments in Finland and Sweden. Table 11 shows the results for public employment,<sup>26</sup> as measured by the number of employees per capita: Columns 1–3 show the results for Finland while Columns 4–6 display the results for Sweden. These RD specifications are more parsimonious than previous ones. This is because employment in Finland is only available for a shorter period of time (i.e., 1980–2002); there are lots of missing values,

<sup>26</sup> I am grateful to Matz Dahlberg for providing me with the employment data for Finland.



**Table 11**  
Estimates of council size effect in Finland and Sweden: public employment.

	(1)	(2)	(3)	(4)	(5)	(6)
	Finland			Sweden		
Council size effect	-.034*** (.012)	-.029** (.012)	-.025** (.012)	-.145*** (.043)	-.037 (.053)	-.021 (.060)
Degree of polynomial in number of eligible voters	No	First	Second	No	First	Second

Notes: The dependent variable is employment per capita. Huber-White standard errors allowing for clustering at the local government level are in parentheses. All regressions include municipality fixed effects, time effects and control variables.

\*\*\* Significant at the 10% level.

\*\* Significant at the 5% level.

\* Significant at the 10% level.

and employment is a highly persistent outcome. It is noteworthy that the estimates are negative for both Finland and Sweden, and of similar magnitudes. All estimates for Finland are also statistically significant from zero at conventional levels.

Finally, I test the Weingast et al. (1981) prediction about the oversupply of public projects. Capital expenditure should be a good measure of the size of public projects. I only have data on capital expenditure from Finland. Table 12 shows that capital expenditure is not related to the council size for any of the seven specifications. Moreover, the council-size estimate also switches signs, i.e., it is sometimes positive and sometimes negative. These results therefore suggest that there is not much of pork-barrel politics in Finland.

## 7. Conclusion

This paper empirically investigates the relationship between council size and government size. It exploits unusually credible sources of exogenous sources of variation in council size in Finland and Sweden. In Finland, council size is a deterministic and discontinuous function of population size while in Sweden council size is a discontinuous but not deterministic function of the number of eligible voters. The Finnish council-size law thus fits with a sharp regression-discontinuity (RD) design while the Swedish law fits with a fuzzy RD design.

The results indicate a negative effect i.e., the larger the size of the legislature the smaller is the size of government in both settings. This runs counter to conventional wisdom based on the model by Weingast et al. (1981).

**Table 12**  
Estimates of council size effect in Finland: capital expenditures.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Council size effect	.064 (.108)	-.030 (.115)	-.030 (.115)	-.010 (.115)	.048 (.115)	.060 (.116)	.039 (.116)
Degree of polynomial in population size	No	No	First	Second	Third	Fourth	Fifth
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is log per capita capital expenditures. Council size is also in log form. Data includes 391 local governments over the period 1977 to 2002, i.e., there are 10,166 observations. Huber-White standard errors allowing for clustering at the local government level are in parentheses. All regressions include municipality fixed effects, time effects and control variables.

\*\*\* Significant at the 1% level.

\*\* Significant at the 5% level.

\* Significant at the 10% level.

I argue that the negative council size effect may be due to a potential conflict between the legislature and the bureaucracy about spending. Bureaucrats are likely to prefer larger spending than politicians since they want to maximize their budgets (Niskanen, 1971) while politicians would like to cater to “fiscally conservative” voters (Peltzman, 1992). I present evidence that is consistent with the proposed mechanism.

## 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, 1025–1048.
- Angrist, J., 1991. Grouped data estimation and testing in simple labor supply models. *Journal of Econometrics* 47, 243–266.
- Angrist J., Imbens, G., 1991. Sources of Identifying Variation in Evaluation Models, NBER Technical working paper, No 115.
- Angrist, Joshua D., Pischke, Jörn-Steffen, 2009. *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton University Press.
- Baqir, R., 2002. Districting and government overspending. *Journal of Political Economy* 110, 1318–1354.
- Bertrand, M., Duflo, E., Mullainathan, S., 2004. How much should We trust difference-in-differences estimates. *Quarterly Journal of Economics* 119, 249–275.
- Besley, T., Case, A., 1995. Does electoral accountability affect economic policy choices? Evidence from gubernatorial term limits. *Quarterly Journal of Economics* 110, 769–798.
- Besley, T., Case, A., 2003. Political institutions and policy choices: empirical evidence from the United States. *Journal of Economic Literature* 41, 7–73.
- Bordignon, M., Nannicini, T., Tabellini, G., 2009. Moderating Political Extremism: Single Round vs Runoff Elections under Plurality Rule. Mimeo, Bocconi University.
- Bradbury, C., Crain, M., 2001. Legislative organization and government spending: cross-country evidence. *Journal of Public Economics* 82, 309–325.
- Brollo, F., Nannicini, T., Perotti, R., and G. Tabellini (2009), “The Political Resource Curse”, mimeo, Bocconi University.
- Bruhn, Miriam, McKenzie, David, 2009. In pursuit of balance: randomization in practice in development field experiments. *American Economic Journal: Applied Economics* 1 (4), 200–232.
- Chamon, M., de Mello, J.M.P., Firpo, S., 2008. “Electoral Rules, Political Competition and Fiscal Spending: Regression Discontinuity Evidence from Brazilian Municipalities,” Discussion Paper 559. PUC-Rio.
- Ferraz, C., Finan, F., 2009. “Motivating Politicians: The Impacts of Monetary Incentives on Quality and Performance,” NBER Working Paper No. 14906.
- Fujiwara, T. (2008), “A regression discontinuity test of strategic voting and Duverger's Law,” Mimeo, University of British Columbia.
- Fujiwara, T. (2010), “Voting technology, political responsiveness, and infant health: evidence from Brazil,” Mimeo, University of British Columbia.
- Gagliarducci, S., Nannicini, T., 2009. “Do Better Paid Politicians Perform Better? Disentangling Incentives from Selection,” IZA Working paper no. 4400.
- Gagliarducci, S., Nannicini, T., Naticchioni, P., 2008. “Electoral Rules and Politicians' Behavior: A Micro Test,” IZA Working Paper No. 3348.
- Gilligan, T., Matsusaka, J., 1995. Deviations from constituent interest: the role of legislative structure and political parties in the states. *Economic Inquiry* 33, 383–401.
- Gilligan, T., Matsusaka, J., 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* 35, 57–82.
- Hahn, J., Todd, P., Van der Klaauw, W., 2001. Identification and estimation of treatment effects with a regression-discontinuity design. *Econometrica* 69, 201–209.
- Lee, D., 2008. Randomized experiments from non-random selection in U.S. House elections. *Journal of Econometrics* 142, 675–697.
- Litschig, Stephan and K. Morrison (2009), “Electoral effects of fiscal transfers: quasi-experimental evidence from local executive elections in Brazil, 1982–1988”, Mimeo, Columbia University.
- MacDonald, L., 2008. The impact of government structure on local public expenditures. *Public Choice* 136, 457–473.
- Moisio, A., 2002. “Determinants of Expenditure Variation in Finnish Municipalities”, VATT-Discussion paper 269. Government Institute for Economic Research, Helsinki.
- Niskanen, A., 1971. *Bureaucracy and Representative Government*. Aldine-Atherton, New York.
- Peltzman, S., 1992. Voters as fiscal conservatives. *Quarterly Journal of Economics* 107 (2), 327–361.
- Perotti, R., Kontopoulos, Y., 2002. Fragmented fiscal policy. *Journal of Public Economics* 86, 191–222.
- Persson, T., Tabellini, G., 2000. *Political Economics: Explaining Economic Policy*, Cambridge: MIT Press.
- Pettersson-Lidbom, P (2001a), “Do parties matter for fiscal policy choices? A regression-discontinuity approach,” Mimeo, Stockholm University.
- Pettersson-Lidbom, P (2001b), “Does the size of the legislature affect the size of government? Evidence from a natural experiment,” Mimeo, Harvard University.
- Pettersson-Lidbom, 2004. “Does the size of the legislature affect the size of government? Evidence from two natural experiments,” VATT-Discussion paper 350. Government Institute for Economic Research, Helsinki.

- Pettersson-Lidbom, P., 2008. Do parties matter for economic policy outcomes? A regression-discontinuity approach. *Journal of the European Economic Association* 6 (5), 1037–1056.
- Primo, D., Snyder, J., 2008. Distributive politics and the law of 1/n. *Journal of Politics* 70 (2), 477–486.
- Staiger, D., Stock, J.H., 1997. Instrumental variable regression with weak instruments. *Econometrica* 65 (5), 557–586 May.
- Stock, J., Watson, M., 2007. *Introduction to Econometrics*, Addison Wesley and Longman. Amsterdam.
- Ting, M., (2010), "Legislatures, Bureaucracies and Distributive Spending," mimeo. Columbia University.
- Weingast, B., Moran, M.J., 1983. Bureaucratic discretion or congressional control? Regulatory policymaking by the federal trade commission. *Journal of Political Economy* 91, 765–800.
- Weingast, B., Shepsle, K., Johnsen, C., 1981. The political economy of benefits and costs; a neoclassical approach to distributive politics. *Journal of Political Economy* 89, 642–664.