

Does the Size of the Legislature Affect the Size of Government?

Evidence from Two Natural Experiments*

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Abstract

This paper addresses the question of whether the size of the legislature matters for the size of government. Previous empirical studies have found a positive relationship between the number of legislators and government spending but those studies do not adequately address the concerns of endogeneity. In contrast, this paper uses variation in council size induced by statutory council size laws to estimate the causal effect of legislature size on government size. These laws create discontinuities in council size at certain known thresholds of an underlying continuous variable, which make it possible to generate “near experimental” causal estimates of the effect of council size on government size. In contrast to previous findings, I find a negative relationship between council size and government size: on average, spending and revenues are decreased by roughly 0.5 percent for each additional council member.

Key words: government size, legislature, legislative policy-making, distributive politics, regression-discontinuity design, natural experiment

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1. Introduction

A central feature of democratic policymaking is the allocation of resources by legislatures.¹ Consequently, a large literature has developed to understand decision making in legislatures. One of the most celebrated assertions in the literature is that the larger the number of budget-decision makers the larger is the size of government (Weingast, Shepsle, and Johnsen 1981). The argument is that legislators will try to benefit their constituents at the expense of the general community through pork barrel spending and other distributive policies which would lead to excessive government spending. There are some recent empirical work (e.g. Baqir 2001, Bradbury and Crain 2001, Bradbury and Stephenson 2003, Gilligan and Matsusaka 1995, 2001 and Perotti and Kontopoulos 2002) that has tried to test this hypothesis. They typically find a positive relationship between the number of legislators and government spending consistent with theory.

The above results can, however, be questioned from both a theoretical and an empirical point of view. Primo and Snyder (2005) show theoretically that the result of a positive relationship between government size and legislature size is not robust to alterations of the standard model. Specifically, they show that the relationship can also be negative. On the empirical side, it is questionable whether previous studies have identified a causal effect. For example, the correlations between council size and the size of government could be caused by an omitted third variable such as voter preferences (e.g., Poterba 1996). The argument is that budget institutions no longer suiting a majority of voters will be overturned and therefore these institutions will simply reflect the preferences of the electorate.² Reverse causality is also likely to be a concern since government size may influence legislature size, that is, a large public sector may require a larger number of legislators to participate in the budget process due to the increased complexity of budget matters. To convincingly address these types of endogeneity problems require an exogenous source of variation in legislature size.

¹ There is a voluminous literature about policy making in legislatures. Two excellent overviews of the literature are Shepsle and Weingast (1995) and Persson and Tabellini (2000).

² See Aghion et al. (2004, 2005) for work on endogenous political institutions.

The main contribution of this paper is to make use of two exogenous sources of variation in the number of legislators, namely to use variation in council size provided by statutory council-size laws in Finland and Sweden. In Finland, the council size of local governments is determined solely by population size. For example, if a local government has a population between 4001 and 8,000, the council must consist of 27 members, but if its population is between 8,001 and 15,000 the council must have 35 members. Thus, the law creates a discontinuity in council size at the threshold of 8001 inhabitants. The Swedish council-size law also induces discontinuities in the council size at certain known values of a continuous variable (e.g., number of eligible voters). These specific features of the Finnish and Swedish council-size laws make it possible to implement a *regression discontinuity design*, i.e., to compare the outcomes for units (e.g., local governments) whose value of an observed covariate (e.g., population in Finland and eligible voters in Sweden) is “just below” and “just above” a known threshold (e.g., 8001 inhabitants in the example above). In other words, those units slightly below the threshold will provide the counterfactual outcome for those units slightly above since treatment status will be “as good as randomly assigned” in a neighborhood of the treatment threshold. Therefore the causal inference from a regression discontinuity analysis can be as credible as from a randomized experiment (Lee 2003).

In the cross-section, I show that there is a positive correlation between the council size and the size of government consistent with the work listed above. However, the results from the two regression discontinuity analyses show that this is not a causal effect. On the contrary, results from both the natural experiments show that there is a negative relationship between the council size and the size of government. The estimated effect of adding one additional member to the council is to decrease total spending with 0.5 percent. The total effect on the budget is, however, much larger since the change in council size is usually between 6-10 members and therefore the total effect is in the order of 3-5 percent. Taken together, these results suggest that previous studies might be subject to omitted-variables and simultaneity bias.

The outline of the paper is as follows. Section 2, describes the local governments in Finland and Sweden and the council size laws. Section 3 presents the empirical strategy that will be used to estimate the causal relationship between the size of the

legislature and the size of government. Section 4 presents the results. Section 5 discusses the findings while section 6 concludes.

2. Local governments and council size laws

This section describes the local governments in Finland and Sweden with focus on the council size laws that provide the source of variation in council size used for estimating the effect of legislature size on government size.

Finnish local governments

Finland is currently divided into 444 local governments or municipalities, which cover the entire country. Local governments play an important role in the Finnish economy. They are, for example, responsible for the provision of social welfare, health services, education and local infrastructure. To emphasize their economic importance, it is sufficient to note that their share of spending out of GDP is 18 percent and they employ roughly 20 percent of the total Finnish workforce. They have the constitutional right of self-government. Specifically, they have independent taxation rights and decide on their own budgets. The bulk of revenues are raised through own taxation and only 15 percent comes from grants. The local election schedule is fixed and elections are held every fourth year on the fourth Sunday of October. Voter turnout has been between 70 to 80 percent and voters cast their vote both for political parties and individual candidates. The distribution of council seats is based on proportional representation in multi-seat constituencies, which encourages a multitude of political parties. Four major parties have dominated the postwar political arena (e.g., Social Democrats, the Agrarian Party/Centre Party, the Democratic Union of the Finnish People/the Left Alliance, the Coalition Party) and 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. Consistent with the consensus mode of politics, there are small effects of partisanship variables on fiscal policy outcomes at the local level in Finland as discussed by Moisio (2002). The decision-making body in each of the municipalities is an elected municipal council. Municipal governments have no legislative or judicial powers so decisions are carried out by means of ordinances. The decisions are taken by simple majority of the council

members, but before 1995 decisions relating to financial or budgetary questions usually required two-thirds majorities in council votes.

A statutory law prescribes a specific number of council members in relation to the population size per May 31st in an election year.³ This law is displayed in Table 1 and it states if a municipality's population is less or equal to 2,000 the council must consist of 17 members; if the population is larger than 2,000 but less or equal to 4,000 the law states that council size must be 21, etc. The law can now induce nine discontinuities between population size and the size of the council at population levels or thresholds: 2,001, 4,001, 8,001, 15,001, 30,001, 60,001, 120,001, 250,001 and 400,001. For instance, suppose that one locality had 2,000 inhabitants and thus had to have 17 council members. Suppose further that the population increased with one, i.e., to 2,001, then the locality is forced by the law to increase its council size to 21. In other words, a small change in population size causes a discontinuous jump in council size. The change in council size can only take place the year after an election year and since elections are held every fourth year this implies that the council size can only be changed every fourth year. During the period of investigation, 1977-2002, there have been six elections: 1980, 1984, 1988, 1992, 1996, and 2000. Thus, changes in council size have only taken place in the years 1981, 1985, 1989, 1993, 1997 and 2001. Table 2 shows the number of law-induced changes in council size across these years, namely those changes in council size due to that the population of municipalities crossed one of the nine population thresholds. As will become clear below, these changes in council-size constitute the source of variation that is going to be used for identifying the causal relationship between the council size and the size of government. For example, Table 2 shows that in year 1981 one municipality increased its population size above 2,001, and therefore it had to change its council size from 17 to 21, while four municipalities decreased their population size below 2,001 and thus they had to reduce their council size from 21 to 17. Table 2 also reveals there are a total of 135 changes in council size during the sample period: two municipalities had three changes, 12 had two changes, while 105 had one change. Table 2 also shows that 1977 is the year with smallest number changes, namely 16, whereas 2001 is the year with the largest number of changes, namely 36.

³ Before 1996, the effective population size was determined per January 1st.

Swedish local governments

Sweden is currently divided into 290 local governments or municipalities, which cover the entire country. The Swedish local governments are in many respects similar to the Finnish counterparts. For example, they perform quite similar tasks and have the same significant role in the economy. The election schedule is also fixed and elections are held every fourth year on the third Sunday of September.⁴ Voter turnout has been high; close to 90 percent, in the local elections in Sweden. The Swedish election system is also based on proportional representation with the existence of several political parties. But in contrast to 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 quite large differences in fiscal policy outcomes between the two blocs at the local level as discussed in Pettersson-Lidbom (2003b).

The decision-making body in each of the Swedish municipalities is an elected municipal council where the number of council members is regulated by law. The Swedish council size law prescribes a minimum requirement of council size in relation to the number of eligible voters as can be seen from Table 3.⁵ 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 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 over 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 12,001, 24,001 and 36,001.

Table 4 shows the actual size of the local council grouped by segments with a minimum requirement of council sizes of 31, 41, 51 and 61. The table reveals that many municipalities have chosen to have more council members than required by law. This is particularly true for the ones with a requirement of at least 31 members. On average, this

⁴ As from 1994, elections are held every fourth year.

⁵ Until 1997 the 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).

group had 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 the ones who will help identify the council size effect. Table 5 presents data on those municipalities that passed one of the three thresholds: 12,001, 24,001 or 36,001, during the sample period. Only one municipality was forced to change its council-size at the lowest threshold, whereas 12 and 7 municipalities had to change its number of seats for the middle and highest cutoffs, respectively.

3. Empirical framework

The question of whether there exists a causal effect of the size of the legislature on government size could be convincingly addressed if we could randomize the number of legislators across political entities since randomization assures that there is no systematic difference between political entities with legislatures of different sizes. Although we cannot conduct such an experiment we can still try to approximate the evidence generated by a randomized experiment, namely to use a quasi-experiment or a natural experiment. In this paper, the council size laws in Finland and Sweden provide the opportunity to implement a regression discontinuity design for estimating the impact of council size on government size.

In the *sharp* regression-discontinuity design, treatment status is a deterministic function of some underlying continuous variable, that is,

$$(2) \quad T_i = T(x_i) = 1[x_i \geq \bar{x}],$$

where $1[.]$ is an indicator function and x is the continuous variable or the assignment variable, and \bar{x} is a treatment threshold separating the units into two mutually exclusive groups: those units receiving treatment and those who do not. The idea is to compare the outcomes for units whose value of the underlying targeting variable is “just below” and “just above” the threshold \bar{x} since they on average will have similar characteristics except for the treatment. In other words, those units slightly below the threshold will provide the counterfactual outcome for those units slightly above since the treatment status will be randomized in a neighborhood of \bar{x} . Hence, the causal inference from a regression

discontinuity analysis can be as internally valid as those drawn from a randomized experiment.⁶

In practice, the regression-discontinuity design can be implemented in a number of ways. The simplest possible approach is just to compare average outcomes in a small neighborhood on either side of the treatment threshold. This approach could, however, produce very imprecise measures of the treatment effect since the regression-discontinuity method is subject to a large degree of sampling variability and therefore this procedure would require very large sample sizes. An *equivalent* but much more efficient method is to use all available data and regressing the outcome of interest, say Y_i , on a low-order polynomial in the treatment-determining covariate, $f(x_i, \varphi)$, and the binary treatment indicator T_i , i.e.,

$$(3) \quad Y_i = \alpha + \beta T_i + f(x_i, \varphi) + v_i$$

This procedure would yield an unbiased estimate of the treatment effect β since x_i is the *only* systematic determinant of T_i and therefore will capture *any* correlation between T_i and the population error term v_i .

In principle, we could estimate equation (3) at each of the nine treatment thresholds (e.g., at population sizes: 2001, 4001, 8001, 15001, 30001, 60001, 120001, 250001 and 400001) and thus getting 9 estimates of the council size effect when using the Finnish data. However, since the regression-discontinuity analysis require lots of data to get a precise measure of the treatment effect and since there are too few municipalities close to each individual threshold I will use the following specification instead to produce a single estimate of the council size effect:

$$(4) \quad Govsize_{it} = \pi Csize_{it} + f(pop_{it}) + \alpha_i + \lambda_t + \varepsilon_{it}$$

where $Govsize$ is a measure of government size, $Csize$ is the council size and $f(\cdot)$ is a low order polynomial function of population size pop , α_i is a local government specific effect and λ_t is a time-specific effect. The parameter of interest is π – the council size effect –

⁶ See Lee (2003) for a proof of this claim.

which measures the effect of including one more council member on government size. The reasons for including the fixed effects are twofold. First, if some of the unobserved determinants of *Govsize* are persistent over time for a given municipality i or if they are common shocks to all municipalities for a given t , the inclusion of α_i and λ_t will greatly reduce the error variance and thus produce a more precise estimate of the council size effect.⁷ Second, and more importantly, the inclusion of the fixed municipality effects makes it possible to pool information from *all* the nine treatment thresholds to produce a single estimate of the council size effect. Consequently, any constant difference across the municipalities or the treatment thresholds will not contribute to identifying the council-size effect. The idea with this setup is to mimic block randomization or a stratified randomized experiment, i.e., council size is randomly assigned in a neighborhood around each treatment threshold and the assignment probability is allowed to differ from one treatment threshold to the next. Here it is important to stress that although equation (4) looks almost like a standard fixed effect specification except for the control function $f(pop)$, the source of identifying variation of the council size effect in a regression-discontinuity analysis is very different from the standard fixed effect approach. Nevertheless, if one should exclude the control function from equation (4), then the identification would be based solely on the within variation. The final comment on specification (4) is that we have to assume that the council size effect is linear in order to get a single estimate of the council size effect, but this assumption seems to be supported by data as discussed below.

Turning to the Swedish natural experiment where the size of the local council is only *partly* determined by statutory law requires a different empirical approach since the regression discontinuity design is not sharp as in the Finnish case. One approach in the “fuzzy” regression-discontinuity case is to use the method of instrumental variables as explained in the following.⁸ The Swedish council size law as displayed in Table 3 states

⁷ The R^2 from OLS regressions on the policy outcomes used in the empirical analysis and the fixed-municipality and time effects are about .9. In other words, these fixed effects explain a large amount of the variation in the policy outcomes.

⁸ 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

that the number of council members must be at *least* 31, 41, 51 and 61 depending on whether the number of eligible voters in a local government falls into one of four intervals. Thus, the law potentially induces three discontinuities in the council size at values 12,001, 24,001 and 36,001. The idea is to use these discontinuities as instrumental variables, that is, to divide the municipalities into 4 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, if we again are willing to assume that the council-size effect is linear, as in equation (4), we can use a Two-Stage-Least-Square (TSLS) procedure to form a single TSLS estimate. The TSLS estimate is a weighted average of each of the instrumental variables estimates obtained taking the instruments one by one. Another useful way of thinking about this particular way of constructing instrumental variables is to make a comparison with a randomized experiment were there is only partial compliance to the treatment protocol. Since the council size can be partly chosen by the municipalities there is only going to be partial compliance to the treatment protocol (see also Table 4). In such a case the *assigned* treatment level can serve as an instrumental variable for the *actual* treatment level, which is precisely the reason for why the council-size law can be used to construct instrumental variables for council size. The instrumental variable approach can now be formally expressed by two equations:

$$(5) \quad Govsize_{it} = \theta Csize_{it} + f(vot_{it}) + \alpha_i + \lambda_t + \varepsilon_{it},$$

$$(6) \quad Csize_{it} = \psi_{41}Z_{41it} + \psi_{51}Z_{51it} + \psi_{61}Z_{61it} + f(vot_{it}) + \alpha_i + \lambda_t + \xi_{it},$$

exogeneity assumption of the instruments alone is usually not sufficient to identify 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 to 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).

where equation (5) is the structural equation and equation (6) is the reduced form or the “first stage” equation for the endogenous variable $Csize_{it}$. Here equation (5) is similar to equation (4) except for that the assignment variable in the Swedish case is number of eligible voters, vot . The previous discussion about the specification of equation (4) therefore automatically transfers to equation (5) while equation (6) 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 $f(\cdot)$, λ_t , and α_i this will partial out any other effects between the instruments and the size of government. The requirement of relevance of the instruments is going to 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 TSLS. This first-stage F -statistic should exceed 10 to avoid the weak instrument problem as discussed by Staiger and Stock (1997).

In principle, there is no need to include additional covariates in the regression-discontinuity approach other than the control function to get an unbiased estimate of the treatment effect. In practice, however, there may still be reasons for including other regressors. We have already mentioned efficiency as a reason for including additional covariates since it reduces the variance of the error term, which can be quite important since the regression-discontinuity method has such large sampling variability. For example, including fixed-municipality effects as we did in the above specifications might greatly enhance efficiency if there are some unobserved determinants of policy outcome that are persistent over time for a given municipality. Another reason to include additional covariates is that we can test whether the treatment is randomly assigned as in true randomized experiment. Since council size should be as good as randomly assigned conditional on the control function, the inclusion of additional covariates should not have a significant influence on the estimate of the council size effect. However, it is important not to include outcomes of the treatment as additional covariates since these will bias the estimate of the treatment effect. (e.g., Rosenbaum 1984) For example, including lagged values of policy outcomes among the covariates is not advisable since the council itself has determined these outcomes. The additional variables that I will make use of in my

analysis are municipality income, the proportion of population age 0 to 15, and the proportion of population age 65 since these variables are probably not influenced by the treatment. These covariates are also considered a standard set of controls in the local public finance literature.

Turning to the measures of the size of government, two different variables will be used in the empirical analysis: total spending and total revenues. Spending and revenues are expressed in per capita terms and in 1995 prices.⁹ Table 6 presents summary statistics for the dependent variables. Table 6 also presents summary statistics for population size, municipality income, the proportion of population age 0 to 15, and the proportion of population age 65 and above. All the Finnish and Swedish data used are publicly available and were obtained from Statistics Finland (Tilastokeskus) Statistics Sweden (SCB) or its publications.¹⁰

4. Results

In this section, I present results on the relationship between council size and government size. Before showing the results from the two regression-discontinuity analyses from Finland and Sweden, I present results for simple bivariate regressions of spending and revenues on council size. These results may be seen as a benchmark for assessing potential biases in previous work.

4.1 Bivariate regressions

Table 7 presents the results from simple bivariate regressions of spending and revenues on council size. I follow the usual approach of reporting Huber-White robust standard errors. However, because there could be serial dependence in the errors within municipalities, I also report (within brackets) the more conservative Huber-White standard errors clustered at the municipality level following the suggestions of Bertrand et al. (2004) and Kézdi (2002). The estimates from all four bivariate regressions are positive and statistically significantly different from zero. The estimated effects for Finland are 147 and 160 FIM per capita for spending and revenues respectively, which

⁹ I have used the implicit GDP deflator. The deflator is constructed by taking the ratio of GDP at current market prices to GDP at fixed market prices using data from the World Bank.

¹⁰ The publications used from SCB are Local government finance, and Statistical yearbook of administrative districts of Sweden.

means that spending increases with about 0.7 percent of average spending (i.e., 20,181 per capita) and revenues increases with 1.5 percent of average revenues (i.e., 10,884 per capita) for each additional council member. The estimate effects for Sweden are SEK 83 and 91 SEK per capita respectively, which is 0.2 percent average spending (i.e., 34,166 per capita) and 0.3 percent of average revenues (i.e., 34,035 per capita). These results are in line with the previous findings of a positive relationship between council size and government size. However, as discussed previously this positive statistical association does not necessarily reflect a causal relationship since we have not isolated an exogenous variation in council size, which I attempt to do in the following.

4.2 Regression-discontinuity analyses

In this section, I present empirical evidence of the council size effect using the regression-discontinuity analysis as discussed in section 3. I first present the results from Finland and then from Sweden.

Finnish natural experiment

In this subsection, I present results from the Finnish natural experiment. Table 8 shows results from spending while Table 9 presents the results for revenues. Column 1 shows the results from a pure fixed-effect specification, while columns 2 and 3 include a linear and a quadratic control function (i.e., polynomials in population size), respectively. Columns 4, 5 and 6 also add additional covariates: municipality income, the proportion of population age 0 to 15, and the proportion of population age 65 and above, to each one of the specifications in columns 1-3.

Starting with spending as the outcome of interest, Table 8 shows that estimated council size effect is negative and significantly different from zero for all specifications. However, the standard errors allowing for arbitrary serial correlation within municipalities are roughly two times larger than the usually reported heteroskedastic-robust standard errors suggesting that the latter may not be valid. The size of the council size effect is quite similar across the specifications including control functions. The estimated effect is -117 FIM per capita or 0.6 percent of mean spending for the specification with a linear control function and -96 for the one with quadratics in population size. The similarity of these two estimates suggests that the council size

estimate is not particularly sensitive to the parameterization of the control function. Moreover, adding additional covariates have a small impact on these estimates as can be seen by comparing column 2 with 4 and column 3 with 6. The fact that adding these observable covariates does not change the estimated council-size effect by much makes it plausible that council size is as good as randomly assigned (conditional on the assignment variable) since this is one way of testing for random receipt of treatment as discussed in section 3.

Another specification check is to estimate the council size effect separately for those municipalities who increased the council size from those who decreased it. These results are shown in columns 7 and 8 and the estimates for those who increased their council sizes is -119 FIM per capita while the estimate is -67 for those who decreased their size. Both these specifications also include a linear control function.

A final specification check is to restrict the sample close to the treatment thresholds since the source of identifying information of the council size effect comes from the discontinuity that the council size law induces at population sizes of 2,001, 4,001, 8,001, 15,001, 30,001, 60,001, 120,001, 250,001 and 400,001. The idea is that observations close to these cutoffs are more representative of a random experiment and therefore any misspecification of the control function might be avoided. However, restricting the sample comes at a cost, namely that the council size effect will be less precisely measured, as discussed in section 3. I will present results restricting the sample to ± 5 , that is, only municipalities with a population size that is within 5 percent from the different cutoffs are being used, namely those in the set $\{[1900, 2100], [3800, 4200], [7600, 8400], [14250, 15750], [28500, 31500], [57000, 63000], [114000, 126000], [237500, 262500], [380000, 420000]\}$. If the discontinuity sample is narrow enough there is not necessary to include the control function since such a situation will be a very close approximation to a randomized trial. Hence, I do not include any parametric population controls for the discontinuity analysis but I still include the fixed municipality and fixed time effects as to increase efficiency and to allow for that the probability of treatment to be different across the nine treatment thresholds as discussed in section 3. In other words, this set up should mimic block randomization or a stratified randomized experiment since the resulting estimator is constructed from simple comparison of means around each

discontinuity. Columns 9 show the result for the discontinuity samples. It is interesting to note the great reduction in the number of observation for the discontinuity sample as compared to the full sample. The total sample has 10,874 observations while the ± 5 sample only has 1,627 observations. The council size estimate is percent -73 for the ± 5 sample. The point estimate is similar to the estimates in the control function approach. The fact that these estimates are similar across the control function approach and the discontinuity method suggests that a quadratic specification of the control function is sufficient to purge the estimate of the council size effect of any bias.

Turning to the other measure of government size namely revenues, Table 9 reveals that the results are consistent with those for spending. In all specifications there is a negative and statistically significant estimate of the council size effect. The estimate is in range -31 to -44 FIM per capita, which is about 0.4 percent of average revenues (10,884). The various estimates of the council size effect are quite similar across all the specifications. Thus, all the comments about the spending regressions in Table 8 are also valid for the revenue regressions in Table 9.

A final comment about the council size estimates concerns its size. Since the municipalities are forced to change the council size by 4, 6, 8 or 10 members depending on the specific threshold, the total effect on the size of government is much larger than the effect from adding one more council member as presented in Tables 9 and 10. For example, if a municipality increased its population size from 8,000 to 8,001, this would cause a reduction in spending of about 4 percent since the municipality has to include 8 additional members to its council.

Swedish natural experiment

In this section, I present the results from the Swedish natural experiment. As discussed in section 3, I will use an instrumental variable approach since the council size can partly be chosen by the municipalities in Sweden and therefore the regression-discontinuity design is no longer sharp, as in the Finnish case, but fuzzy instead. Tables 10 and 11 show the results from TSLS regressions of spending and revenues on council size, namely estimating equation (6) by TSLS using the predicted council size from equation (5) as a regressor instead of *Csize* and calculate the correct standard errors for the TSLS procedure. As before, I will present both heteroskedastic-robust standard errors and the

more conservative standard errors allowing for arbitrary serial correlation within municipalities (within brackets). In all specification, I also include fixed municipality and fixed time effect for reasons explained previously. In columns 2 and 3, I also include different polynomials in the number of eligible voters since this is the assignment variable. In columns 4-6, I also add municipality income, the proportion of population age 0 to 15, and the proportion of population age 65 and above to the specification in columns 1-3. Finally, column 7 shows the estimates of the council size effect for the discontinuity sample: ± 5 (i.e., those municipalities with number of eligible voters in the set $\{[11,400-12,600], [22,800-25,200], [34,200-37,800]\}$), similar to the Finnish analysis. All the specifications in Tables 10 and 11 show a negative estimate of the council size effect but the effect is not precisely measured.

4.3 Robustness checks

In this section, I relax the linear council size effect assumption made previously. I begin with the Finnish case, which is followed by the Swedish one.

In order to relax the linear council size effect in the Finnish setting, I construct 10 dummy variables such that they correspond to the different council size levels in the council size law: $D_{17}=1$ if council size=17, and zero otherwise; $D_{21}=1$ if council size=21, and zero otherwise; $D_{27}=1$ if council size=27, and zero otherwise; $D_{35}=1$ if council size=35, and zero otherwise; $D_{43}=1$ if council size=43, and zero otherwise; $D_{51}=1$ if council size=51, and zero otherwise; $D_{59}=1$ if council size=59, and zero otherwise; $D_{67}=1$ if council size=67, and zero otherwise; $D_{75}=1$ if council size=75, and zero otherwise; and $D_{85}=1$ if council size=85, and zero otherwise. If we now estimate the following regression

$$(7) \quad Govsize_{it} = \alpha_i + \lambda_t + \beta_{21}D_{21} + \beta_{27}D_{27} + \dots + \beta_{85}D_{85} + u_{it},$$

where I have arbitrary omitted category D_{17} , we can construct different council size effect estimates, i.e., “Wald” type estimates, at each treatment threshold along the lines suggested by Angrist (1991). For example, dividing the estimate of β_{21} by 4, the difference between council size 21 and 17, produces an estimated of the council size effect at the population threshold 2,001, while dividing the estimate of β_{27} by 10, the

difference between the council size 27 and 17, we will get an estimate of the council size effect at the threshold 4,001. Table 13 shows the results from estimation of equation (6) and the corresponding Wald estimates. We can note here that we have only seven different estimates of the council size effect, which are less than the nine treatment thresholds. The reason for this is that we include fixed municipality effects and therefore we cannot identify any council size effect for these two thresholds since there were no changes in council size at the treatment thresholds 250,001 and 400,001 as can be seen from Table 2. For spending, all the council size estimates are between -116 FIM per capita and -195 while for revenues they are in the range -9 FIM per capita to -49. Thus, all the estimates are negative and they are broadly of similar magnitude, which provides support to that council size effect is roughly linear. As discussed by Angrist (1991), the estimates in column 1 of Tables 8 and 9, i.e., -148 for spending and -40 for revenues, are effectively a linear combination of the seven different Wald estimates in Table 13.

We can also construct different Wald estimates for the Swedish data and since there are three treatment thresholds we can construct three distinct estimates. Table 14 shows these results. Column 1 and 2 show the reduced form estimates of spending and revenues on instruments, while column 3 show the reduced form estimates of council size on instrument or the “first stage” in the TSLS procedure. The Wald estimates is constructed by dividing the estimates in column 1 and 2 by the corresponding estimates from column 3. For example, dividing -1009 by 1.32 produces one Wald estimate for the spending regression namely -764 as displayed in column 4. The two other estimates for the spending regression are -632 and -478. These three estimates are roughly similar and suggest that a linear specification of the council size effect is not a bad first-order approximation. The same conclusion can also be made for the revenue regression since also these Wald estimates are similar as can be seen from column 5. Finally, Table 14 also reveals that all three instruments are positively and highly significantly related to the number council seats. A test of instrument relevance shows that these instruments are not “weak”. The heteroskedastic-robust F -statistic is 189 and the heteroskedastic- and autocorrelation-robust F -statistic 13, both of which is higher than 10, the rule of thumb value suggested by Staiger and Stock (1997).

Another robustness check that one could potentially consider is to include partisanship variables; such as left and right majority government indicators or the share of seats for the parties included in the ruling coalition, as additional control variables. However, these variables are not pretreatment characteristics and should not to be included since they may cause the estimate of the treatment to be biased (Rosenbaum 1984). Nevertheless, I have re-estimated the specifications in column 5 in Tables 8-11 with partisanship variables included. In the Finnish case I include the relative share of the three main political parties (e.g., Socialists, Centre Party and Coalition Party) in the municipal council following Moisio (2002), and for the Swedish case I include an indicator variable for left-wing majority and the share of left-wing voters following my own paper Pettersson-Lidbom (2003b) which uses a regression discontinuity approach to the question whether party control matters for fiscal policy choices. For ease of comparison, odd numbered columns reproduce the results from column 5 in Tables 8-11 while even numbered include the different partisanship variables. As can be seen from Table 14, the previous results do not change at all. From columns 6 and 8, we can also note that party control matters for both spending and revenues for the Swedish data, which is consistent with the results in Pettersson-Lidbom (2003b) where I use the same data but over a shorter time span (e.g., 1974-1994) than in this paper (e.g., 1977-2002)

5. Discussion

This paper has established empirically a negative relationship between the size of legislature and the size of government. There are strong reasons to believe that these findings are internally valid, i.e., council size is causally related to government size for the population being studied, since the sources of variation used for identifying the council-size effect are likely to be exogenous. In fact, specification tests support that council size is as good as randomly assigned.

The fact that I find a negative effect in both the Finnish and Swedish settings also bolster claims to external validity, i.e., the inference and conclusions can be generalized from the population and setting studied to other populations and settings. This finding together with the result of a positive association between council size and government size in both the Swedish and Finnish settings when all variation in council size is being

used cast doubt on a causal interpretation of results in the studies by Baqir (2001), Bradbury and Crain (2001), Bradbury and Stephenson (2003), Gilligan and Matsusaka (1995, 2001) and Perotti and Kontopoulos (2002). All these studies also find a positive council-size effect when they use variation in council size that is likely to be endogenous.

A potential critique that can be raised against using Swedish and Finnish data for testing the predictions from the models of distributive politics by Weingast et al. (1981) and Primo and Snyder (2005) is that these models does not apply to the Finnish and Swedish settings since the political systems are based on proportional representation, whereas the models are developed for a first-the-post-system with single member electoral districts. However, the same critique can be raised against all the previous empirical studies since they all have used the number of seats in the legislature and not the number of districts in their empirical specifications.¹¹ Moreover, even for those studies based on U.S. data the mapping between the number of districts and the number of legislators is far from one to one.¹² For example, in Baqir (2002) more than 80 percent of the cities have council members elected from *multimember* districts (at-large systems).¹³ Finally and more importantly, it is not the number of districts per se that it is the crucial ingredient to models of distributive politics but rather that economic policy decisions create benefits for well-defined groups, with the cost diffused in society at large, as discussed by Persson and Tabellini (2000). Thus, one cannot a priori to rule out that these models do not apply to Finnish and Swedish local governments since they do raise the bulk of revenues through a proportional income tax (i.e., dispersed costs) and that individual members of the council may cater to particular constituencies (i.e., concentrated benefits).

¹¹ The only exception is Perroti and Kontopoulos (2002). They use the number of spending ministers in the cabinet as a measure of N .

¹² There are also a number of U.S. states that have multimember districts.

¹³ Among the 17 percent with only district electoral system, there are cities with both single member and multimember districts. Unfortunately, Baqir treats all cities as having single-member districts since his data does not allow him to separate them apart.

6. Conclusion

Previous empirical studies have found a positive relation between the size of the legislature and the size of government. Those, studies, however, do not adequately address the concerns of simultaneity bias and omitted-variable bias. To isolate exogenous variation in legislature size of the, this paper exploits statutory council size laws in Finland and Sweden. These laws create discontinuities in the council size at certain known values of an observable covariate which can be used to generate “near experimental” causal estimates of the effect of council size on government size. In contrast to previous findings, the results show an increase of the council size to induce a significant and substantial decrease in spending and revenues. On average, spending and revenues go down by about 0.5 percent for each additional council member.

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Table 1. Council size law: Finnish local government

Population size	Number of council members
0-2,000	17
2,001-4,000	21
4,001-8,000	27
8,001-15,000	35
15,001-30,000	43
30,001-60,000	51
60,001-120,000	59
120,001-250,000	67
250,001-400,000	75
400,000-	85

Table 2 Changes in council in Finnish local governments

Year of change	1981	1985	1989	1993	1997	2001	Σ
<i>Threshold: 2,001</i>							
17→21	1	2	2	2	0	0	7
21→17	4	2	5	3	3	6	23
<i>Threshold: 4,001</i>							
21→27	4	4	2	2	1	3	16
27→21	1	2	4	2	2	13	24
<i>Threshold: 8,001</i>							
27→35	6	5	2	3	1	1	18
35→27	1	1	4	0	3	6	15
<i>Threshold: 15,001</i>							
35→43	2	3	2	4	3	2	16
43→35	0	1	0	0	1	1	3
<i>Threshold: 30,001</i>							
43→51	2	2	0	1	2	3	10
51→43	0	0	0	0	0	0	0
<i>Threshold: 60,001</i>							
51→59	0	0	0	0	0	1	1
59→51	0	1	0	0	0	0	1
<i>Threshold: 120,001</i>							
59→67	1	0	0	0	0	0	1
67→59	0	0	0	0	0	0	0
<i>Threshold: 250,001</i>							
67→75	0	0	0	0	0	0	0
75→67	0	0	0	0	0	0	0
<i>Threshold: 400,001</i>							
75→85	0	0	0	0	0	0	0
85→75	0	0	0	0	0	0	0
Σ	22	23	21	17	16	36	135

Table 3. Council size law: Swedish local governments

Number of eligible voters	<u>Minimum</u> number of council members
0-12,000	31
12,001 – 24,000	41
24,001 – 36,000	51
36,000-	61

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

Table 4. Actual council size in Swedish local government

Minimum number of council members	Mean	St. Dev.	Min	Max
31	40.23	5.20	31	49
41	47.62	4.20	41	61
51	52.67	4.23	51	75
61	67.05	7.78	61	85

Table 5. Law-induced changes in council size in Swedish local governments

Thresholds	Number of potential changes in council size	Number of effective changes in council size
12,001	21	1
24,001	16	12
36,001	11	7

Table 6. Summary statistics

	Mean	St. Dev.	Min	Max
<i>Finland</i>				
Council size	28.66	10.34	17	85
Population size	11,531	31,269	240	559,718
Spending	20,181	5,640	6,537	58,344
Revenues	10,884	4,184	3,606	44,572
Income	44,730	12,782	16,723	174,557
The proportion of population aged 0 to 15	21.0	3.6	11.3	48.2
The proportion of population aged 65 or above	16.0	4.5	4.0	40.1
<i>Sweden</i>				
Council size	47.35	11.20	31	101
Number of eligible voters	23,348	43,858	2,099	615,490
Population size	30,094	53,782	2,639	754,948
Spending	34,166	7,318	17,620	78,234
Revenues	34,035	7,237	19,252	90,123
Income	87,586	17,844	17,378	234,625
The proportion of population aged 0 to 15	20.7	2.4	12.6	36.4
The proportion of population aged 65 or above	18.2	4.1	4.0	29.7

Note- Spending, revenues and income is expressed in per capita terms and in 1995 prices.

Table 7. Bivariate regressions

	Finland		Sweden	
	Spending	Revenues	Spending	Revenues
Council size	147 (5) [14]	160 (4) [9]	83 (9) [33]	91 (9) [33]
Number of observations	10,874	10,874	7,376	7,376

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.

Table 8. Spending: Finnish local governments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Council size	-148	-117	-96	-94	-81	-70	-119	-67	-73
	(12)	(12)	(13)	(12)	(12)	(13)	(15)	(17)	(19)
	[26]	[27]	[32]	[26]	[26]	[28]	[35]	[38]	[33]
Population size		-.13	-.23		-.11	-.17	-.13	-.13	
		(.02)	(.03)		(.01)	(.02)	(.02)	(.02)	
		[.05]	[.09]		[.03]	[.07]	[.05]	[.05]	
Population size^2			1.9e-07			1.2e-07			
			(3.4e-08)			(3.4e-08)			
			[1.0e-07]			[7.6e-08]			
Income				.06	.07	.07			
				(.01)	(.01)	(.01)			
				[.02]	[.02]	[.02]			
The proportion of population aged 0 to 15				328	305	296			
				(17)	(16)	(16)			
				[36]	[34]	[34]			
The proportion of population aged 65 or above				304	262	249			
				(19)	(18)	(19)			
				[42]	[41]	[44]			
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	10,874	10,874	10,874	10,820	10,820	10,820	9,442	9,490	1,627

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.

Table 9. Revenues: Finnish local governments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Council size	-40	-38	-32	-36	-34	-31	-29	-44	-33
	(8)	(9)	(9)	(8)	(8)	(8)	(12)	(11)	(13)
	[16]	[17]	[19]	[15]	[15]	[16]	[23]	[20]	[20]
Population size		-.01	-.04		-.01	-.04	-.001	-.01	
		(.01)	(.02)		(.01)	(.02)	(.01)	(.01)	
		[.03]	[.05]		[.02]	[.04]	[.02]	[.03]	
Population size ²			5.6e-08			4.2e-08			
			(3.2e-08)			(4.2e-08)			
			[5.3e-08]			[3.6e-08]			
Income				.13	.13	.13			
				(.01)	(.01)	(.01)			
				[.02]	[.01]	[.01]			
The proportion of population aged 0 to 15				61	58	55			
				(13)	(14)	(14)			
				[23]	[23]	[23]			
The proportion of population aged 65 or above				168	163	159			
				(15)	(15)	(15)			
				[20]	[21]	[22]			
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	10,874	10,874	10,874	10,820	10,820	10,820	9,442	9,490	1,627

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.

Table 10. Spending: Swedish local governments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Council size	-459 (47) [127]	-190 (61) [169]	-159 (68) [193]	-475 (49) [129]	-208 (60) [163]	-207 (67) [180]	-309 (115) [203]
Number of voters		-.39 (.05) [.12]	-.44 (.06) [.17]		-.38 (.04) [.12]	-.39 (.06) [.19]	
Number of voters ²			9.0e-08 (8.7e-08) [2.1e-07]			1.9e-08 (8.9e-08) [2.2e-07]	
Income				.12 (.02) [.05]	.12 (.02) [.05]	.11 (.02) [.05]	
The proportion of population aged 0 to 15				530 (52) [118]	422 (52) [110]	417 (54) [110]	
The proportion of population aged 65 or above				167 (58) [137]	-82 (53) [123]	-82 (52) [126]	
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	7,376	7,376	7,376	7,376	7,376	7,376	701

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.

Table 11. Revenues: Swedish local governments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Council size	-426 (47) [126]	-182 (63) [167]	-142 (69) [188]	-443 (49) [128]	-205 (62) [163]	-192 (68) [177]	-345 (110) [201]
Number of voters		-.35 (.05) [.12]	-.41 (.06) [.16]		-.34 (.05) [.12]	-.37 (.06) [.17]	
Number of voters ²			1.1e-07 (8.6-08) [1.9e-07]			5.0e-08 (8.8e-08) [2.0e-07]	
Income				.11 (.02) [.05]	.11 (.02) [.06]	.11 (0.2) [.06]	
The proportion of population aged 0 to 15				496 (49) [115]	399 (49) [109]	388 (48) [106]	
The proportion of population aged 65 or above				182 (54) [123]	-40 (50) [117]	-41 (50) [119]	
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	7,376	7,376	7,376	7,376	7,376	7,376	701

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.

Table 12. Investigating the linear effect assumption in the Finnish data

	Spending	Revenues	Difference in council size from 17	Wald estimates for spending	Wald estimates for revenues
Dummy=21	-649 (177) [321]	-68 (119) [230]	4	-162	-17
Dummy=27	-1479 (219) [414]	-240 (145) [273]	10	-148	-24
Dummy=35	-2094 (262) [536]	-532 (183) [339]	18	-116	-30
Dummy=43	-4002 (336) [752]	-1004 (242) [520]	26	-154	-39
Dummy=51	-6625 (542) [1018]	-1672 (361) [742]	34	-195	-49
Dummy=59	-5338 (858) [1031]	-1896 (693) [723]	42	-127	-45
Dummy=67	-6978 (1142) [1049]	-432 (730) [724]	50	-140	-9
Fixed effects	Yes	Yes			
Time effects	Yes	Yes			
Number of obs.	10,874	10,874			

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.

Table 13. Investigating the linear effect assumption in the Swedish data

	Spending	Revenues	Council size (First stage)	Wald estimates for spending	Wald estimates for revenues
Instruments:					
<i>Z₄₁</i>	-1009 (252) [547]	-1033 (225) [459]	1.32 (.20) [.43]	-764	-783
<i>Z₅₁</i>	-3583 (395) [1098]	-3532 (385) [1064]	5.67 (.32) [1.17]	-632	-623
<i>Z₆₁</i>	-5459 (533) [1296]	-5118 (524) [1245]	11.43 (.50) [1.88]	-478	-448
Fixed effects	Yes	Yes	Yes		
Time effects	Yes	Yes	Yes		
Number of obs.	7,376	7,376	7,376		

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.

Table 14: Partisanship variables

	Finland				Sweden			
	Spending		Revenues		Spending		Revenues	
	1	2	3	4	5	6	7	8
Council size	-81 (12) [26]	-80 (12) [26]	-34 (8) [15]	-35 (8) [15]	-208 (60) [163]	-207 (61) [161]	-205 (62) [163]	-204 (62) [161]
Linear control function	-.11 (.01) [.03]	-.10 (.01) [.04]	-.01 (.01) [.02]	-.01 (.01) [.02]	-.38 (.04) [.12]	-.38 (.04) [.12]	-.34 (.05) [.12]	-.34 (.05) [.12]
Income	.07 (.01) [.02]	.07 (.01) [.02]	.13 (.01) [.01]	.13 (.01) [.01]	.12 (.02) [.05]	.12 (.02) [.05]	.11 (.02) [.06]	.11 (.02) [.05]
The proportion of population aged 0 to 15	305 (16) [34]	307 (16) [34]	58 (14) [23]	58 (14) [23]	422 (52) [110]	417 (52) [109]	399 (49) [109]	393 (49) [109]
The proportion of population aged 65+	262 (18) [41]	262 (18) [41]	163 (15) [21]	161 (15) [21]	-82 (53) [123]	-69 (54) [125]	-40 (50) [117]	-27 (50) [117]
Socialists (%)		5 (6) [12]		-2 (5) [8]				
Coalition Party (%)		38 (6) [12]		-2 (4) [8]				
Centre Party (%)		38 (5) [11]		5 (4) [6]				
Left-wing majority						715 (212) [347]		698 (206) [332]
Share of left-wing voters						12 (14) [30]		18 (14) [29]

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.