VATT-KESKUSTELUALOITTEITA
VATT DISCUSSION PAPERS

350

DOES THE SIZE OF THE LEGISLATURE AFFECT THE SIZE OF GOVERNMENT? EVIDENCE FROM TWO NATURAL EXPERIMENTS*

Per Pettersson-Lidbom♣

Valtion taloudellinen tutkimuskeskus
Government Institute for Economic Research
Helsinki 2004

* 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 Torsten Persson, David Strömberg, and Jakob Svensson for numerous discussion and comments. I also thank Antti Moisio and seminar participants at UC Berkeley, Harvard University, University of Pennsylvania, Uppsala University, Stockholm School of Economics, and Institute for International Economics (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.

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

ISBN 951-561-520-8 (nid.)
ISBN 951-561-521-6 (PDF)

ISSN 0788-5016 (nid.)
ISSN 1795-3359 (PDF)

Valtion taloudellinen tutkimuskeskus

Government Institute for Economic Research

Arkadiankatu 7, 00100 Helsinki, Finland

Email: pp@ne.su.se
Email: etunimi.sukunimi@vatt.fi

Oy Nord Print Ab

Helsinki, November 2004

PER PETTERSSON-LIDBOM: DOES THE SIZE OF THE LEGISLATURE AFFECT THE SIZE OF GOVERNMENT? EVIDENCE FROM TWO NATURAL EXPERIMENTS. Helsinki, VATT, Valtion taloudellinen tutkimuskeskus, Government Institute for Economic Research, 2004, (C, ISSN 0788-5016 (nid.), ISSN 1795-3359 (PDF), No 350). ISBN 951-561-520-8 (nid.), ISBN 951-561-521-6 (PDF).

Abstract: Previous empirical studies have found a positive relationship between the size of legislature and the size of government. Those studies, however, do not adequately address the concerns of endogeneity. In contrast, this paper uses variation in legislature size induced by statutory council size laws in Finland and Sweden 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, regression-discontinuity design, natural experiment

JEL classification: C9, D7, E6, H0, H1, H3, H7, K1, P16

Tiivistelmä: Aiempien tutkimusten mukaan julkisten päätöksenteko-organisaatioiden koon kasvu aiheuttaa julkisten menojen kasvua. Siten kansanedustajien tai kunnanvaltuutettujen määrän kasvu lisäisi julkisia menoja. Tutkimuksissa ei kuitenkaan ole huomioitu sitä, että suuri julkinen sektori kasvattaa myös päätöksenteko-organisaatioiden kokoa. Tämän endogeenisuusongelman poistaminen on tämän tutkimuksen keskeinen tavoite. Tutkimuksen aineisto koostuu Suomen ja Ruotsin kunnanvaltuustojen kokotiedoista. Valtuustojen kokoa säätelevät lait, joiden mukaan tietyn asukaskokoluokan kunnissa on oltava tietty määrä valtuutettuja. Säännöstö antaa mahdollisuuden testata valtuustojen koon muutoksen vaikutusta julkisiin menoihin tilanteessa, jossa kunnan asukasluku kasvaa vain vähän. Tulosten mukaan valtuuston koon ja menojen välillä on negatiivinen yhteys. Siten yhden uuden valtuutetun tulo valtuuston jäseneksi pienentää kunnan menoja ja tuloja keskimäärin noin 0,5 prosenttia.

Asiasanat: Julkiset menot, julkinen päätöksenteko, kvasikoe, regression-discontinuity -menetelmä

Contents

1. Introduction

Recent empirical research (e.g. Baqir 2001, Bradbury and Crain 2001, Bradbury and Stephenson 2003, and Gilligan and Matsusaka 1995, 2001) has found a positive relationship between the size of the legislature and the size of government.[1] While these findings have been interpreted as providing strong support for one of the standard economic models of budget decision-making within legislatures (e.g. Weingast et al. 1981), question about causality remains. As discussed by Poterba (1996), the correlations between council size and the size of government could be caused by an omitted third variable such as voter preferences. 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. Simultaneous causality is also likely to be a concern since government size may have an influence on 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.

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., 8.001 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).

[1] Perotti and Kontpoulos (2002) use the number of ministers in the cabinet instead of the number of legislators and they find that the number of spending ministers is positively associated with government size.

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 an emphasis 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.[2] 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

[2] Before 1996, the effective population size was determined per January 1st.

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.

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.[3] 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 lead to quite large differences in fiscal policy outcomes between the two blocs at the local level as discussed in Pettersson-Lidbom (2003b).

[3] As from 1994, elections are held every fourth year.

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.[4] 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 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.

[4] 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).

3. Regression-discontinuity design and data

In an ideal randomized controlled trial, treatment (e.g., the number of council members) would be randomly assigned to subjects (e.g., political jurisdictions) from some population of interest. The causal effect (e.g., the council size effect) is defined as the expected effect of the outcome of interest of the treatment and the causal effect can be estimated by the difference in the sample average outcomes between the treatment and control groups. In such a case, one can run the following regression

$$(1) \qquad Y_i = \beta_0 + \beta T_i + u_i,$$

where Y is an outcome measure, T is a treatment indicator, and u is an error term capturing all other factors that are related to the outcome. Random assignment of treatment implies that treatment will be distributed independently of all omitted factors in u whether they are observed or not. Thus, random assignment of T makes the zero mean assumption, i.e., E(Yi | Ti)=0 to be valid and therefore β will measure the causal effect of the treatment, namely the expected difference in outcomes between the treatment and control groups, i.e., E(Yi | Ti =1)- E(Yi | Ti =0). In other words, the attractiveness of a randomized experiment is that it bypasses the need to specify elaborate behavioral models, and to use complicated estimation strategies with arbitrary assumptions to estimate the parameter of interest. This is the reason why an ideal randomized controlled experiment is usually considered to be the gold standard to establish causality.

Randomized experiments are, however, quite rare in the field of political economics and we are therefore left with drawing inference from non-experimental data. Nevertheless, 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,

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

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 expect 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.[5]

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.[6] An equivalent method but that is much more efficient is to use a "control function" approach (Goldberger 1972, Heckman and Robb 1985). In this case, one includes the conditional mean function E[ui | xi] as an additional regressor in equation (1) making it possible to use all available observations. For example, if the true population conditional mean function would be linear, the equation to be fitted is:

$$Y_i = \alpha + \delta T_i + \theta x_i + \varepsilon_i. \quad (3)$$

The inclusion of the control function will now free Ti from the contamination which leads to bias since it will capture any correlation between Ti and εi, and therefore δ will be an unbiased measure of the treatment effect. This is known as conditional mean independence assumption, i.e., E[εi | Ti, xi] = E[εi | xi].[7] Under the conditional mean independence assumption, the observed or unobserved characteristics in the error term εi, may be correlated with xi, but given xi the

[5] See Lee (2003) for a proof of this claim.

[6] The regression discontinuity method is a correlated design, which implies that the standard errors will be larger than compared to an uncorrelated design, i.e., a randomized experiment. The larger is the correlation between the control function and the treatment indicator the larger is the variance of any estimates of the treatment effect. In other words, much more observations are needed in the regression-discontinuity design to give the same precision as in an experiment. A detailed discussion of efficiency of the regression-discontinuity method is provided in Goldberger (1972)

[7] Conditional mean independence is also known as "selection on observables" or "ignorability of treatment"

conditional mean of the error term does not depend on the treatment Ti. In this case, the parameter δ will be the causal effect of treatment, that is, the difference in conditional expectations: E(Yi| Ti =1, xi)- E(Yi| Ti =0, xi). This difference is also the causal effect defined by the experiment where the units with a given xi are randomly assigned to treatment. Since the causal treatment effect does not depend on xi,[8] it is also the causal effect of treatment for a randomly selected subject from the population. A caveat with the control function approach is that we do not know the functional form of the population conditional mean function. A common approach is therefore to specify a flexible parametric control function as to avoid functional form misspecification. However, if we include a too flexible functional form, the control function will have sharp jumps or "spikes", which will create a problem for the regression-discontinuity method because the identifying variation for estimating the treatment effect comes from the discontinuities that the assignment rule induces at certain known values. Put differently, we must assume that there is a smooth relationship between the assignment variable and the outcome of interest otherwise the treatment effect would not be identifiable.[9]

In principle, we could estimate equation (3) at each of the nine treatment thresholds (e.g., at population sizes: 2.001, 4.001, 8.001, 15.001, 30.001, 60.001, 120.001, 250.001 and 400.001) 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:

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

where Govsize is a measure of government size, Csize is the council size and f(.) is a low order polynomial function of population size pop, αi is a local government specific effect and λt is a time-specific effect. The parameter of interest is π - the council size effect 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 αi and λt will greatly reduce the error

[8] This is true if the treatment effect is constant.

[9] That continuity is a requirement for identification in the regression-discontinuity approach is discussed by Hahn et al (2001).

variance and thus produce are more precise estimate of the council size effect.[10] Second, 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 but fuzzy instead. One approach in the fuzzy case is to use the method of instrumental variables as explained in the following.[11] The Swedish council size law as displayed in Table 3 states 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., Z31=1[vot≤12.000], Z41 =1[12.000<vot≤24.000], Z51 =1[24.000<vot≤36.000], and Z61 =1[vot>36.000] where vot is the number of eligible voters and the sub-

[10] 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.

[11] 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 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).

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:

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

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

where equation (5) is the structural equation and equation (6) is the reduced form or the “first stage” equation for the endogenous variable Csizeit. Here equation (5) is similar to equation (4) except for that the assignment variable in the Swedish case is 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: Z41, Z51, and Z61, are exogenous and relevant. The requirement that the instruments should be exogeneous implies that once we control for f(.), λt, and μi 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 above 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 include 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.[12] 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.[13]

[12] 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.

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

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 bee 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 bivarate 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 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 from a two discontinuity samples: the ±10 and ±5 discontinuity samples. In the ±10 discontinuity sample only municipalities with a population size that is within 10 percent from the different cutoffs are being used, namely those in the set {[1.800, 2.200], [3.600, 4.400], [7.200, 8.800], [13.500, 16.500], [27.000-33.000], [54.000, 66.000], [108.000, 132.000], [225.000, 275.000], [360.000, 440.000]}, and similarly for the ±5 discontinuity sample, i.e. those municipalities in the set {[1.900, 2.100], [3.800, 4.200], [7.600, 8.400], [14.250, 15.750], [28.500-31.500], [57.000, 63.000], [114.000, 126.000], [237.500, 262.500], [380.000, 420.000]}. 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 and 10 show these results 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 ±10 sample has 3.069 observations and the ±5 sample only has 1.627 observations. The council size estimates are -86 for the ±10 percent sample and -73 for the ±5 sample. These point estimates are similar to the estimates in the control function approach. The fact that these estimates are broadly similar across the control function approach and the discontinuity method suggests that a linear 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 significantly estimate of the council size effect. The estimate is in range -31 to -40 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, columns 7 and 8 show the estimates of the council size effect for the two discontinuity samples: ±10 (i.e., those municipalities with number of eligible voters in the set {[10.800-13.200], [21.600-26.200], [32.600-39.400]}) and ± 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]}) samples, 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 the 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: D17=1 if council size=17, and zero otherwise; D21 =1 if council size=21, and zero otherwise; D27 =1 if council size=27, and zero otherwise; D35 =1 if council size=35, and zero otherwise; D43 =1 if council size=43, and zero otherwise; D51 =1 if council size=51, and zero otherwise; D59 =1 if council size=59, and zero otherwise; D67 =1 if council size=67, and zero otherwise; D75 =1 if council size=75, and zero otherwise; and D85 =1 if council size=85, and zero otherwise. If we now estimate the following regression

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

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 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 revels 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 Caolition 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 Kontpoulos (2002). All these studies also find a positive council-size effect when they use variation in council size that is likely to be endogenous.

The result of this paper also contradicts one of the predictions from the model of budget decision making within legislatures as developed by Weingast et al (1981). The model predicts that the larger is the size of the legislature (say N number of legislators) the larger is the scale of government since each legislator is going to fully internalizes the benefit of her own public good but will only internalizes a fraction (i.e. $1/N$) of the social marginal cost of higher taxes. In other words, this model (which is often called the "Law of $1/N$") asserts that government spending is driven by a common-pool problem in the fiscal revenues pool.[14] A potential critique that can be raised against interpreting my results as evidence against the Law $1/N$ is that the common-pool model does not apply to the Finnish and Swedish political systems since they are based on proportional representation, whereas the model by Weingast et al (1981) was developed for a first-the-post-system with single member electoral districts. In other words, in their model N was referring to the number of districts, which happens to coincide with the number of legislators in a first-past-the post system. However, the same critique can be raised against all the previous empirical studies since they also have equated N with the number of seats in the legislature.[15] 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.[16] For example, in Baqir (2002)

[14] Other recent papers that have the same common-pool mechanism at their heart include Chari et al. (1997) and Velasco (1999)

[15] The only exception is Perroti and Kontpoulos (2002). They use the number of spending ministers in the cabinet as a measure of N.

[16] There are also a number of U.S. states that have multimember districts.

less than 17 percent of the cities have council members elected from single member districts (ward systems),[17] whereas the majority of cities instead have at-large systems. In addition, as discussed by Persson and Tabellini (2000), it is not the number of districts per se that it is the crucial ingredient to create a common pool problem but rather that economic policy decisions create benefits for well-defined groups, with the cost diffused in society at large. Thus, it is difficult a priori to rule out that the common pool problem is not at work in 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).

The result of this paper does not, however, support the prediction from the common pool model. The question is now whether we can we find an alternative explanation that explains the negative relationship between the size of the legislature and the size of government. To my knowledge there exists no such models and therefore the precise causal mechanism behind the negative relationship is left for future research.

[17] There are cities with both single member and multimember districts among the 17 percent with district electoral system. 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 exploit 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.

References

Angrist, J. (1991): Grouped Data Estimation and Testing in Simple Labor Supply Models, Journal of Econometrics, 47, 243-266.

Angrist, J. – G. Imbens (1991): Sources of Identifying Information in Evaluation Models, NBER Technical working paper no. 117, National Bureau of Economic Research.

Baqir, R. (2002): Districting and Government Overspending, Journal of Political Economy, 110, 1318-1354.

Besley, T. – A. Case (2003): Political Institutions and Policy Choices: Empirical Evidence from the United States, Journal of Economic Literature, 41, 7-73.

Bradbury, C. – M. Crain (2001): "Legislative Organization and Government Spending: Cross-Country Evidence," Journal of Public Economics, 82, 309-325.

Bradbury, J. – F. Stephenson (2003): Local Government Structure and Public Expenditures, Public Choice, 115, 185-198.

Chari, V. V. – Jones, L. – R. Marimon (1997): The Economics of Split-Ticket Voting in Representative Democracies. American Economic Review, 87, 957–76.

Gilligan, T. – J. Matsusaka (1995): Deviations from Constituent Interest: the Role of Legislative Structure and Political Parties in the States, Economic Inquiry, 33, 383-401.

Gilligan, T. and J. Matsusaka (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.

Goldberger, A. (1972): Selection Bias in Evaluating Treatment Effects: Some Formal Illustrations, Discussion paper 123-72, Madison. IRP.

Heckman, J. – R. Robb (1985): Alternative Methods for Evaluating the Impact of Interventions, in Longitudinal Analysis of Labor Market Data, eds J. Heckman and B. Singer. Cambridge: Cambridge University Press.

Lee, D. (2003): Randomized Experiments from Non-random Selection in U.S. House Elections, mimeo, UC Berkeley.

Local government finance (Kommunernas finanser), Stockholm: SCB; Örebro: Publikationstjänsten, SCB.

Moisio, A. (2002): Determinants of Expenditure Variation in Finnish Municipalities, VATT Discussion paper 269, Government Institute for Economic Research, Helsinki.

Perotti R. – Y. Kontopoulos (2002): Fragmented Fiscal Policy, Journal of Public Economics, 86, 191-222.

Pettersson-Lidbom, Per (2003a): Does the Size of the Legislature Affect the Size of Government? Evidence from a Natural Experiment, Working paper no 18, Stockholm University.

Pettersson-Lidbom, Per (2003b): Do Parties Matter for Fiscal Policy Choices? A Regression-Discontinuity Approach, Working paper no 15, Stockholm University.

Poterba, J. (1996): Budget Institutions and Fiscal Policy in the U.S. States, American Economic Review, 86, 395-400.

Staiger, D. – J. Stock (1997): Instrumental Variables Regressions when the Instruments are Weak, Econometrica, 65, 557-586.

Statistical yearbook of administrative districts of Sweden (årsbok för Sveriges Kommuner), Stockholm: SCB; Örebro: Publikationstjänsten, SCB.

Velasco, A (1999): A Model of Endogenous Fiscal Deficit and Delayed Fiscal Reforms. In Fiscal Institutions and Fiscal Performance, edited by James Poterba and Jurgen von Hagen. Chicago: Univ. Chicago Press (for NBER).

Weingast, B., (1979): A Rational Choice Perspective on Congressional Norms, American Journal of Political Science, 23, 245-262.

Weingast, B. – Shepsle K. – C. Johnsen (1981): The Political Economy of Benefits and Costs; A Neoclassical Approach to Distributive Politics, Journal of Political Economy, 89, 642-64.

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	∑
			Threshold: 2.001				
17→21	1	2	2	2	0	0	7
21→17	4	2	5	3	3	6	23
			Threshold: 4.001				
21→27	4	4	2	2	1	3	16
27→21	1	2	4	2	2	13	24
			Threshold: 8.001				
27→35	6	5	2	3	1	1	18
35→27	1	1	4	0	3	6	15
			Threshold: 15.001				
35→43	2	3	2	4	3	2	16
43→35	0	1	0	0	1	1	3
			Threshold: 30.001				
43→51	2	2	0	1	2	3	10
51→43	0	0	0	0	0	0	0
			Threshold: 60.001				
51→59	0	0	0	0	0	1	1
59→51	0	1	0	0	0	0	1
			Threshold: 120.001				
59→67	1	0	0	0	0	0	1
67→59	0	0	0	0	0	0	0
			Threshold: 250.001				
67→75	0	0	0	0	0	0	0
75→67	0	0	0	0	0	0	0
			Threshold: 400.001				
75→85	0	0	0	0	0	0	0
85→75	0	0	0	0	0	0	0
∑	22	23	21	17	16	36	135

Table 3. Council size law: Swedish local governments

Number of eligible voters	Minimum 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
		Finland		
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
		Sweden		
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)	(10)
Council size	-148	-117	-96	-94	-81	-70	-119	-67	-86	-73
	(12)	(12)	(13)	(12)	(12)	(13)	(15)	(17)	(14)	(19)
	[26]	[27]	[32]	[26]	[26]	[28]	[35]	[38]	[28]	[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)			(3.4-08)				
			[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	Yes
Time effects	Yes	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	3.069	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)	(10)
Council size	-40	-38	-32	-36	-34	-31	-29	-44	-23	-33
	(8)	(9)	(9)	(8)	(8)	(8)	(12)	(11)	(11)	(13)
	[16]	[17]	[19]	[15]	[15]	[16]	[23]	[20]	[19]	[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	Yes
Time effects	Yes	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	3.069	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)	(8)
Council size	-459	-190	-159	-475	-208	-207	-391	-309
	(47)	(61)	(68)	(49)	(60)	(67)	(118)	(115)
	[127]	[169]	[193]	[129]	[163]	[180]	[212]	[203]
Number of voters		-.39	-.44		-.38	-.39		
		(.05)	(.06)		(.04)	(.06)		
		[.12]	[.17]		[.12]	[.19]		
Number of voters^2			9.0e-08			1.9e-08		
			(8.7e-08)			(8.9e-08)		
			[2.1e-07]			[2.2e-07]		
Income				.12	.12	.11		
				(.02)	(.02)	(.02)		
				[.05]	[.05]	[.05]		
The proportion of population aged 0 to 15				530	422	417		
				(52)	(52)	(54)		
				[118]	[110]	[110]		
The proportion of population aged 65 or above				167	-82	-82		
				(58)	(53)	(52)		
				[137]	[123]	[126]		
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	7.376	7.376	7.376	7.376	7.376	7.376	1.247	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)	(8)
Council size	-426	-182	-142	-443	-205	-192	-285	-345
	(47)	(63)	(69)	(49)	(62)	(68)	(125)	(110)
	[126]	[167]	[188]	[128]	[163]	[177]	[219]	[201]
Number of voters		-.35	-.41		-.34	-.37		
		(.05)	(.06)		(.05)	(.06)		
		[.12]	[.16]		[.12]	[.17]		
Number of voters^2			1.1e-07			5.0e-08		
			(8.6-08)			(8.8e-08)		
			[1.9e-07]			[2.0e-07]		
Income				.11	.11	.11		
				(.02)	(.02)	(0.2)		
				[.05]	[.06]	[.06]		
The proportion of population aged 0 to 15				496	399	388		
				(49)	(49)	(48)		
				[115]	[109]	[106]		
The proportion of population aged 65 or above				182	-40	-41		
				(54)	(50)	(50)		
				[123]	[117]	[119]		
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observa-tions	7.376	7.376	7.376	7.376	7.376	7.376	1.247	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	-68	4	-162	-17
	(177)	(119)			
	[321]	[230]			
Dummy=27	-1479	-240	10	-148	-24
	(219)	(145)			
	[414]	[273]			
Dummy=35	-2094	-532	18	-116	-30
	(262)	(183)			
	[536]	[339]			
Dummy=43	-4002	-1004	26	-154	-39
	(336)	(242)			
	[752]	[520]			
Dummy=51	-6625	-1672	34	-195	-49
	(542)	(361)			
	[1018]	[742]			
Dummy=59	-5338	-1896	42	-127	-45
	(858)	(693)			
	[1031]	[723]			
Dummy=67	-6978	-432	50	-140	-9
	(1142)	(730)			
	[1049]	[724]			
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

Instruments:	Spending	Revenues	Council size (First stage)	Wald estimates for spending	Wald estimates for revenues
Z_{41}	-1009	-1033	1.32	-764	-783
	(252)	(225)	(.20)		
	[547]	[459]	[.43]		
Z_{51}	-3583	-3532	5.67	-632	-623
	(395)	(385)	(.32)		
	[1098]	[1064]	[1.17]		
Z_{61}	-5459	-5118	11.43	-478	-448
	(533)	(524)	(.50)		
	[1296]	[1245]	[1.88]		
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	-80	-34	-35	-208	-207	-205	-204
	(12)	(12)	(8)	(8)	(60)	(61)	(62)	(62)
	[26]	[26]	[15]	[15]	[163]	[161]	[163]	[161]
Linear control function	-.11	-.10	-.01	-.01	-.38	-.38	-.34	-.34
	(.01)	(.01)	(.01)	(.01)	(.04)	(.04)	(.05)	(.05)
	[.03]	[.04]	[.02]	[.02]	[.12]	[12]	[.12]	[.12]
Income	.07	.07	.13	.13	.12	.12	.11	.11
	(.01)	(.01)	(.01)	(.01)	(.02)	(.02)	(.02)	(.02)
	[.02]	[.02]	[.01]	[.01]	[.05]	[.05]	[.06]	[.05]
The proportion of population aged 0 to 15	305	307	58	58	422	417	399	393
	(16)	(16)	(14)	(14)	(52)	(52)	(49)	(49)
	[34]	[34]	[23]	[23]	[110]	[109]	[109]	[109]
The proportion of population aged 65 or above	262	262	163	161	-82	-69	-40	-27
	(18)	(18)	(15)	(15)	(53)	(54)	(50)	(50)
	[41]	[41]	[21]	[21]	[123]	[125]	[117]	[117]
Share of Socialists		5		-2				
		(6)		(5)				
		[12]		[8]				
Share of Coalition Party		38		-2				
		(6)		(4)				
		[12]		[8]				
Share of Centre Party		38		5				
		(5)		(4)				
		[11]		[6]				
Left-wing majority						715		698
						(212)		(206)
						[347]		[332]
Share of left-wing voters						12		18
						(14)		(14)
						[30]		[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.

VATT-KESKUSTELUALOITTEITA / DISCUSSION PAPERS ISSN 0788-5016 - SARJASSA ILMESTYNEITÄ

292. Montén Seppo – Tuomala Juha: Alueellinen työttömyys ja pitkäaikaistyöttömyys 1990-luvulla. Helsinki 2003.

293. Lyytikäinen Teemu: Pienituloisuuden dynamiikka Suomessa. Helsinki 2003.

294. Aulin-Ahmavaara Pirkko – Jalava Jukka: Pääomapanos ja sen tuottavuus Suomessa vuosina 1975-2001. Helsinki 2003.

295. Vaittinen Risto: Maatalouskaupan vapauttaminen – kansainväliset vaikutukset ja merkitys EU:lle. Helsinki 2003.

296. Haataja Anita: Suomalaiset mikrosimulointimallit päätöksenteon valmistelussa ja tutkimuksessa. Helsinki 2003.

297. Kangasharju Aki – Korpinen Liisa – Parkkinen Pekka: Suomessa asuvat ulkomaalaiset: Esiselvitys. Helsinki 2003.

298. Hietala Harri – Lyytikäinen Teemu: Työn, pääoman ja kulutuksen verorasituksen mittaaminen. Helsinki 2003.

299. Räisänen Heikki: Rekrytointiongelmat ja työvoimapotentiaali lääkärien, lastentarhanopettajien, farmaseuttien ja proviisorien ammateissa. Helsinki 2003.

300. Kröger Outi: Pääoma- ja yritystulojen verotus – uusi suunta? Helsinki 2003.

301. Kari Seppo – Liljeblom Eva – Ylä-Liedenpohja Jouko: Snedvridande beskattning av utländska investeringar: Reell och finansiell aktivitet inducerad av skattearbitrage. Helsinki 2003.

302. Pekkala Sari: Is Little Brother Nothing but Trouble?: Educational Attainment, Returns to Schooling and Sibling Structure. Helsinki 2003.

303. Vaittinen Risto: Liberalisation of Agricultural Trade – Global Implications and what it Means for the EU. Helsinki 2003.

304. Kangasharju Aki – Venetoklis Takis: Do Wage-subsidies Increase Employment in Firms? Helsinki 2003.

305. Räisänen Heikki: How to Cope with Labour Market Policy Tools in Economic Downturn: Finnish Evidence. Helsinki 2003.

306. Ruotoistenmäki Riikka – Siivonen Erkki: Tiehankkeiden rahoitusvajeen ratkaisu? Helsinki 2003.

307. Hjerppe Reino: Social Capital and Economic Growth Revisited. Helsinki 2003.

308. Honkatukia Juha – Kangasharju Aki – Vaittinen Risto: Suuren aluepolitiikan ja hajasijoittamisen vaikutuksia Keski-Suomessa. Helsinki 2003.

309. Luukkonen Antti: Palkkadiskriminaatio Suomen teollisuussektorin toimihenkilöillä vuonna 2000. Helsinki 2003.

310. Pekkala Sari: What Draws People to Urban Growth Centers: Jobs vs. Pay? Helsinki 2003.

311. Rantala Juha – Romppanen Antti: Ikääntyvät työmarkkinoilla. Helsinki 2003.

312. Hämäläinen Kari: Education and Unemployment: State Dependence in Unemployment Among Young People in the 1990s'. Helsinki 2003.

313. Berghäll Elina – Kiander Jaakko: The Finnish Model of STI Policy: Experiences and Guidelines. KNOGG Thematic Network WP4 Country Report – Finland. Helsinki 2003.

314. Kilponen Juha – Sinko Pekka: Does Centralised Wage Setting Lead into Higher Taxation? Helsinki 2003.

315. Järviö Maija-Liisa: Julkisesti tuettu hammashuolto vuosina 1994-2000. Helsinki 2003.

316. Ollikainen Virve: The Determinants of Unemployment Duration by Gender in Finland. Helsinki 2003.

317. Kari Seppo – Lyytikäinen Teemu: Efektiivinen veroaste eri sijoitusmuodoissa. Helsinki 2003.

318. Peltola Mikko – Soininen Jarno: Lasku- ja kasvualojen työmarkkinat 1990-luvulla. Helsinki 2003.

319. Sinko Pekka: Subsidizing vs. Experience Rating of Unemployment Insurance in Unionized Labor Markets. Helsinki 2003.

320. Korkeamäki Ossi – Kyyrä Tomi: Explaining Gender Wage Differentials: Findings from a Random Effects Model. Helsinki 2003.

321. Luukkonen Antti: Sukupuolten palkkaero yksityisissä palveluammateissa. Helsinki 2003.

322. Hjerppe Reino: Uncovering the Dimensions of the Common Good – Problems of Measurement of the Size of the Public Sector. Helsinki 2003.

323. Perrels Adriaan – Ahlqvist Kirsti – Heiskanen Eva – Lahti Pekka: Kestävän kulutuksen potentiaalia etsimässä – esitutkimus –. Helsinki 2004.

324. Tukiainen Janne: Access to Computer, Internet and Mobile Phone at Home in Finland, Ireland, Netherlands and Sweden. Helsinki 2004.

325. Räty Tarmo – Luoma Kalevi – Aronen Pasi: Palvelusetelit kuntien sosiaalipalveluissa. Helsinki 2004.

326. Parkkinen Pekka: Hoiva- ja hoitopalvelumenot tulevaisuudessa. Helsinki 2004.

327. Korkeamäki Ossi – Kyyrä Tomi – Luukkonen Antti: Miesten ja naisten palkkaerot yksityisellä sektorilla. Helsinki 2004.

328. Mäkelä Pekka: Kariutuneet kustannukset ja omaisuudensuoja päästökaupassa. Helsinki 2004.

329. Honkatukia Juha: Päästöoikeuksien jakotapojen kustannusvaikutukset. Helsinki 2004.

330. Moisio Antti: Julkisen rahan liikkeet Uudenmaan ja muun Suomen välillä. Helsinki 2004.

331. Laine Veli: Eläkejärjestelmän kannustinvaikutukset. Helsinki 2004.

332. Kari Seppo – Kröger Outi – Rauhanen Timo – Ulvinen Hanna: Beskattning av småföretag i Finland. Helsinki 2004.

333. Leppälehto Jenni: Naapurialueiden vaikutus veroprosentin määräytymisessä paikallistasolla. Helsinki 2004.

334. Pekkala Sari: Maahanmuuton taloudelliset vaikutukset. Helsinki 2004.

335. Perrels Adriaan: The Basic Service Quality Level of Transport Infrastructure in Peripheral Areas. Helsinki 2004.

336. Kiander Jaakko: Growth and Employment in Nordic Welfare States in the 1990s: a Tale of Crisis and Revival. Helsinki 2004.

337. Kari Seppo – Ylä-Liedenpohja Jouko: Effects of Equalization Tax on Multinational Investments and Transfer Pricing. Helsinki 2004.

338. Hietala Harri – Kari Seppo – Rauhanen Timo – Ulvinen Hanna: Laskelmia yritys- ja pääomaverouudistuksesta. Helsinki 2004.

339. Koskela Erkki – Virén Matti: Government Size and Output Volatility: New International Evidence. Helsinki 2004.

340. Räty Tarmo: Palvelusetelit sosiaalipalveluissa 2004. Helsinki 2004.

341. Honkatukia Juha – Antikainen Riikka: Väylähankkeiden kansantaloudellinen merkitys. Helsinki 2004.

342. Mustonen Esko: Välittömän verotuksen progressiivisuus. Helsinki 2004

343. Kiander Jaakko: Onko Suomessa liian vähän yrittäjiä? Helsinki 2004

344. Kiander Jaakko: The Evolution of the Finnish Model in the 1990s: from Depression to High-tech Boom. Helsinki 2004.

345. Riihelä Marja – Sullström Risto: Välittömien verojen ja tulonsiirtojen vaikutus tulonsaajajärjestyksen ja tuloerojen muutoksiin Suomessa. Helsinki 2004.

346. Kyyrä Tomi – Wilke Ralf: Reduction in the Long-Term Unemployment of the Elderly. A Success Story from Finland. Helsinki 2004.

347. Kröger Outi: Kansainvälinen yhteistyö haitallisen verokilpailun estämiseksi. Helsinki 2004.

348. Honkatukia Juha: Sähköntuotannon voitot päästökaupan yhteydessä. Helsinki 2004.

349. Sinko Pekka: Progressive Taxation under Centralised Wage Setting. Helsinki 2004.