Does the Size of the Legislature Affect the Size of Government?
Evidence from a Natural Experiment*

Per Pettersson-Lidbom♣

First version: December 18, 2001
This version: February 8, 2003

Abstract

Previous empirical studies have found a positive relationship between the size of the legislature and the size of government. Those studies, however, do not adequately address the concerns of endogeneity. In contrast, this paper exploits an exogenous variation in the size of the legislature induced by a statutory law linking council size to the number of eligible voters in Swedish local governments. The statutory law can potentially create discontinuities between number of eligible voters and council size at certain known values. These discontinuities are used to construct instrumental variable estimates of the effect of council size on government size. In contrast to previous findings, the results show that an increase of the council size leads to a statistically and economically significant decrease in spending and revenues. On average, spending and revenues are decreased by roughly 0.5-0.8 percent for each additional council member.

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

Key words: government size, legislature, regression-discontinuity design, natural experiment

---

* The author gratefully acknowledges helpful comments from Torsten Persson, Jakob Svensson and seminars participants at the University of California at Berkeley, Harvard University, Princeton University, University of Pennsylvania, and Uppsala University. The views expressed in the paper are mine, as is the responsibility for any mistakes. The first draft of this paper was written while the author was a Fellow at CBRSS Harvard University. Financial support from CBRSS and Jan Wallander's Foundation is gratefully acknowledged.

♣ Department of Economics, Stockholm University, S-106 91 Stockholm, Sweden; e-mail: pp@ne.su.se.

1. Introduction

Recent empirical research has found a positive relationship between the number of legislators and the size of government.[1] But these studies raise problems of endogeneity. A potential omitted variable problem is that both council size and the size of government could be related to voter preferences. Riker (1980) argues that political institutions no longer suiting a majority of voters will be overturned and therefore all institutions simply reflect the "congealed preferences" of the electorate and contain no information other than on voter preferences. Put differently, this is an example of a potential self-selection problem on the basis of unobserved characteristics.[2] There may also be a simultaneity problem since both the size of government and the size of the legislature are results of choices made by legislatures. The omitted variable problem can potentially be solved by including controls for voter preferences in the regression of interest. But preferences are unobserved and generically difficult to measure. Thus, to convincingly address the endogeneity problem requires a source of exogenous variation in the number of legislators making possible an instrumental variable approach.

The first contribution of this paper is to make use of an exogenous source of variation in the number of legislators induced by a statutory law linking council size to the number of eligible voters in Swedish local governments. The law prescribes a minimum requirement of council size in relation to the number of eligible voters: 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 larger than 12,000 but less or equal to 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 levels of 12,000, 24,000 and 36,000 of eligible voters. For instance,

[1] The studies by Baqir (2001), Bradbury and Crain (2001), and Gilligan and Matsusaka (1995, 2001) all find a positive effect on the number of legislators and government size. The work by Perotti and Kontpoulos (2002) also find a positive effect but they use number of ministers in the cabinet instead of the number of legislators.

[2] The general selection problem is subject of an extensive literature. For example, see Heckman and Robb (1985) or Manski (1989).

suppose that one locality had 12,000 eligible voters and choose to have 31 council members, i.e., the smallest required size. Suppose further that the number of eligible voters increased with one, i.e., to 12,001, then the locality is forced by the law to increase its council size to at least 41. In other words, a small change in the number of eligible voters causes a discontinuous jump in the council size. The idea is now to use these discontinuities, as the exogenous source of variation, to explicitly construct instrumental variables. The logic behind this identification strategy is that there is a discontinuous relationship between council size and number of eligible voters at certain known levels whereas there is a smooth relationship between the number of eligible voters and the size of government.

A second contribution of this article is to explicitly introduce the language and conceptual framework of randomized experiments and to illustrate how we may combine three different kinds of empirical identification strategies: instrumental variables, regression-discontinuity design and fixed effects in an attempt to approximate the evidence generated by a true experiment. In other words, the council size law provides something close to a random assignment, or a so called "natural experiment" which gives rise to interesting methodological and practical issues in implementing empirical strategies.

Apart from the natural experiment provided by the council size law there are other attractive features of using Swedish local governments as a testing ground. As Sweden is a unitary state local governments operate under the *same* constitutional and institutional setting. Thus, the variation due to the council size law is not confounded with variation of other institutional factors that might be related to both council size and the size of government since these are implicitly "held constant". We also have access to a huge panel data set, consisting of 288 local governments from 1974 to 1998, which means that we can both use the cross-sectional and time-series variation to identify the effect of council size on government size. Swedish local governments also have the constitutional right of self-government, they have no restrictions on borrowing, and they have no balanced budget rules.[3] Moreover, only 20 percent of their revenues are from grants, whereas the bulk comes from a proportional income tax, which each municipality can set

[3] However, as from 2000 there is a balanced budget rule in place.

freely. In other words, they have a large degree of freedom in fiscal policy, which has resulted in quite large differences in policy outcomes across the local governments.[4] Finally, Swedish local governments also play a significant role in the Swedish Economy. During the sample period their total expenditures amounted to roughly 20-25 percent of GDP. This makes them more economically significant than most other sub-national government around the world.

When using a standard regression control methodology, I find a positive association between the size of the legislature and the size of government, as did previous studies. But I find a negative relationship when the instrumental variable method is used in order to account for unobserved heterogeneity and simultaneity. These two results together suggest that the previous studies might be subject to omitted-variable and simultaneity problems.

This paper is related to a broader literature about the empirical relationship between political institutions, broadly defined as the rules by which decisions are made and economic or policy outcomes.[5] However, few of these studies discuss the potential problem of endogenous institutions, and very rarely try to address it.[6]

This paper is also related to the literature about social interactions.[7] The goal of this literature is to provide an explanation of group behavior, which emerges from interdependences across individuals. The interactions among budget decisions makers are a specific example of such interdependences. This literature also provides a thorough discussion about the inferential problems in detecting the existence and estimating the magnitude of social interactions. The empirical approach put forward in this paper suggests one way to circumvent these problems.

The outline of the paper is as follows. Section 2, presents a simple model of why we should expect the number of legislature to be positively related to the size of government. Section 3 discusses the problem of causal inference and how this relates to the previous studies. This section also present the empirical strategy used to estimate the causal relationship between the size of the legislature and the size of government. Section

[4] In a series of papers of mine, Pettersson-Lidbom (2001, 2002a, 2002b) and Dahlberg and Pettersson-Lidbom (2002), I also find strong support for Swedish local policy discretion.
[5] See Poterba and Von Hagen (1999) and Besley and Case (2002) for surveys.
[6] See, however, Persson and Tabellini (2002).
[7] See Brock and Durlauf (2001) for a survey of this literature.

4 describes the data used in the analysis, while section 5 presents the results. Section 6 discusses the interpretations of the findings and section 7 concludes.

2. Model

Why should we expect the size of the legislature to be related to the size of government? The literature about the "common-pool" problem suggests one answer.[8] To illustrate the common-pool problem we use a simple model by Persson and Tabellini (2000). Let N denote the number of legislators assuming each legislator l to have quasi-linear preferences

$$w_l = c_l + H(g_l),$$

where c_l denotes the consumption of private goods, g_l the per capita supply of a publicly provided good, and H is an increasing and concave function with $H(0)=0$. Thus, in this set-up public good g_l only benefits legislator l. Each legislator represents a certain group of people, where groups can be defined according to preferences, occupation, geographical location, age or any other personal attributes. All legislators have the same income y. A unit of income can be costlessly converted into one unit of any of the N publicly provided goods. We assume that all these goods are financed out of a common pool of tax revenues, where each group contributes the same amount τ (i.e., a lump sum tax). We also assume that each legislator decides freely on the supply of each public good, whereas the tax rate is residually determined.[9] This creates a distorted incentive for each individual legislator since all groups share the cost of financing the public good. Put differently, each legislator would like to overspend on her particular good since she shares the cost of providing this good with the other legislators.

This type of incentive problem that arise from centralized financing can be illustrated more formally by first deriving a condition for the utilitarian optimum g^u by maximizing $\Sigma_l \, w_l$, subject to the resource constraint $\Sigma_l \,(c_l + g_l) = Ny$:

$$H_g\,(g^u) = 1. \qquad (1)$$

Thus, the equilibrium condition is to set the marginal benefit of each good equal to the marginal cost of unity. In contrast, when each legislator decides freely on the supply of

[8] The classic reference is Weingast, Shepsle and Johnson (1981).
[9] This collective choice procedure is known as universalism (e.g., Weingast 1979). This type of collective choice procedure has been criticized to be somewhat unsatisfactory from a game-theoretic point of view.

"her own" public good but the taxes are residually determined each legislator maximizes $y\text{-}\tau + H(g_l) = y\text{-}1/N\ \Sigma_l\, g_l + H(g_l)$ with respect to g_l taking equilibrium spending by all other legislators as given. In this case equilibrium spending satisfies

$$H_g(g^c) = \frac{1}{N} \qquad (2)$$

This equilibrium is characterized by overspending compared to utilitarian optimum, i.e., $g^c > g^u$. This result is the well-known common-pool problem where each legislator fully internalizes the benefit of its own good, but she internalizes only the fraction 1/N of the social marginal cost of higher taxes. Thus, the prediction from this model is that the larger the number of legislators the larger is the size of government.

Obviously, this model of the budget making process is highly stylized. In particular, individual legislators make all budget decisions without taking into account the role of political parties. However, the main prediction from the model: the larger the number of decision makers in the budget process the larger the size of government; should also be valid in a strong party system such as the Swedish one.[10] This issue is further discussed in section 6.

3. Causal inference and empirical strategy

In the previous section, I presented a simple model of why the size of the legislature can have an effect on the size of government. In this section, I discuss the empirical conditions that need to be fulfilled to conclude that there exists a *causal relationship* between the size of the legislature and the size of government, and how earlier studies have tried to tackle the issue of causality. Then I present the empirical strategy used in this paper to address the whether the number of legislators is *causally* related to the size of government.

The equilibrium condition from the common pool model, as expressed in equation 2, predicts a positive relationship between the size of government and the number of

[10] Inman and Fitts (1990) incorporate parties into the common pool model and show that the result of an overspending bias still holds, although it is mitigated.

legislators. If we make a linear approximation to the f.o.c,[11] we have the following outcome equation of interest

$$g_i = \alpha + \delta N_i + X_{it}\beta + \varepsilon_i, \tag{3}$$

where i indexes a unit such as countries, states or cities. X is a vector of observable variables that might be both related to g_i and N_i. The parameter of interest is the council-size effect δ, which measures the *ceteris paribus* effect of adding an additional legislator on the size of government.

We can make causal inference if the error term ε_i is independent of the size of the legislature N_i, a condition which can be stated as $E[\varepsilon \mid N] = 0$. In applied research this condition usually fails in one of four ways: omitted variables, selection, measurement error, or simultaneity. In our case omitted variables, selection and simultaneity are the prime concerns. For example, in the cross section study by Baqir (2002) unobserved city characteristics such as voter preferences might be related both to the size of government and the size of the city council. Using a panel of cities and controlling for fixed city effects would solve any *time invariant* omitted-variable problem. This is the estimation strategy used by Bradbury and Crain (2001) for a cross-country data set, and Gilligan and Matsusaka (1995) for the U.S. states. However, their estimation strategy only works if there is any non-trivial variation across time in the size of the legislature, since the inclusion of fixed unit effects makes it impossible to identify any time invariant institutional factor. The fixed-effect estimation strategy, however, does not solve the problem of simultaneity, namely that the size of the legislature and the size of government could be determined simultaneously. Nor does a fixed-effect estimation strategy solve a *time varying* omitted variables problem, such as a change in voter preferences which might give rise to the problem of endogenous institution pointed out by Riker (1980). These empirical problems also make it difficult to predict the sign of the potential bias. For example, the omitted-variable problem due to unobserved voter preferences could generate both upward and downward biased estimates depending on

[11] Of course, the f.o.c is non-linear, but all previous studies have implicitly assumed a linear regression model with constant coefficients and I will also retain these assumptions. Nevertheless, I will also discuss the implication for the empirical analysis when we relax the assumptions.

the true correlation between voter preferences and the size of government and the size of the legislature respectively.

One solution to the endogeneity problems is to conduct an experiment in which the size of the legislature is randomly assigned. Random assignment would eliminate any dependence between the size of legislature and the error term. Even though we cannot conduct a randomized experiment, we can use a natural experiment to generate instrumental variables that effectively do the same thing.[12]

An instrumental variable Z_i should fulfill two requirements: instrument *relevance* and instrument *exogeneity*. The first condition states that the instrument must be *partially* correlated with the size of the legislature N_i once all other exogenous variables included in equation (1) have been netted out. This condition can be formally expressed as $N_i = \theta_0 + \theta_1 Z_i + X_i\theta + u_i$, and $\theta_1 \neq 0$. The second condition states that the instrument must be *independent* of the regression error, i.e., $E[\varepsilon | Z] = 0$.[13]

Baqir (2002) is the only previous study that uses an instrumental variable approach to solve the endogenity problems. He uses the size of the city council in 1960, i.e., $Z_i=N_{i,t-30}$, as the instrument. There is a problem with this instrument if unobserved city characteristics are persistent over time. If such a variable are not accounted for in the regression the instrument and the error term become correlated, i.e., $E[\varepsilon | Z] \neq 0$.[14]

In this paper, I will use an attractive instrumental variable to solve the endogenity problem. In Swedish local governments, the size of the local council is partly determined by statutory law. The law prescribes a minimum council size in relation to the number of eligible voters. Table 1 shows the relationship between council size and number of eligible voters. The law 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 size of the council: at the number of 12,000, 24,000 and 36,000 of eligible voters. The

[12] See Angrist and Kreuger (2001) for an introduction of using natural experiments to construct instrumental variables.

[13] If the structural relationship of interest is linear then the instruments need only to be uncorrelated with the error term.

[14] In more general terms, Baqir's instrument is an example of using institutions that date back in time as the exogenous source variation. The argument is that those institutions should be less susceptible to concerns about endogeneity than more recent institutions. However, there is no compelling reason to believe that this is the case since there may be omitted variables correlated with this institution.

idea is to use these discontinuities as instrumental variables. One way of constructing the instrumental variables is by dividing the municipalities into 4 groups and use a set of dummy variables to indicate each group, i.e., Z_{31}=1[v≤12,000], Z_{41} =1[12,000<v≤24,000], Z_{51} =1[24,000<v≤36,000], and Z_{61} =1[v>36,000] where v 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 (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 are willing to assume that the council-size effect is linear and constant across all units i (which is implicitly what we have done in equation 3) 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. This is the most efficient TSLS estimator in homoscedastic regression models with constant coefficients (Newey 1990).

The use of multiple instruments can also have drawbacks. In case that the second condition for instrument validity is violated, namely if the instruments are only weakly correlated with the endogenous regressor, the IV estimator will be biased.[15] The larger the number of instruments the larger is the bias. The instrumental variable estimate will be biased towards the OLS estimate (Sawa 1969). Put differently, there is going to be a trade-off between bias and efficiency as the number of instruments increases.

One potential solution to the weak instrument problem is to use combine the dummy instruments into a single instrument. In this case the bias is approximately zero since the number of instruments is equal to the number of endogenous variables (Angrist and Kreuger 2001). This instrument is most naturally constructed as linear combination of the previous instruments: $Z=1*Z_{31}+2*Z_{41}+3*Z_{51}+4*Z_{61}$ (e.g. equivalent to $Z=31*Z_{31}+41*Z_{41}+51*Z_{51}+61*Z_{61}$), since these are discrete and uniformly distributed.

A useful way of thinking about this particular way of constructing instrumental variables is to make a comparison with the treatment literature. We can think of

[15] See Stock, Wright and Yogo (2002) for a recent survey of the literature about weak instruments.

legislature size as a multi-valued treatment.[16] Since the treatment can be partly chosen by the municipalities there can only be partial compliance to the treatment protocol. 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 I use the council-size law to construct instrumental variables.

The use of instrumental variables to solve the endogenity problem also raises the issue of the interpretation of the estimated parameter of interest, i.e., the council-size effect. Here, we can again 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).[17] It turns out that these effects coincide if the treatment effect is linear and constant across all units, as we have assumed above. 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.[18]

An important issue in constructing instrumental variables is whether the assumption about instrument exogeneity is plausible. In other words, we need to isolate the part of variation in council size that is arguably exogenous, or the "as if" random source of variation. Here, we will make use of an empirical strategy known as a *regression-discontinuity* design to isolate the random variation. If the council size was entirely determined by the council-size law we would not need to use an instrumental variable approach since we could just compare the average outcome of municipalities slightly above a certain discontinuity with those slightly above. The reason for this is that one can think of regression-discontinuity design as mimicking a tie-breaking randomized experiment, namely to toss a fair coin at some known pre-treatment value. For example,

[16] The potential values are $N \in \{31, 33, 35, \ldots, J\}$

[17] See Heckman (2001) for a discussion of the different effects.

[18] See Angrist and Imbens (1991) for a discussion of this claim.

if we could toss a coin when the number of eligible voters is exactly at 12,000, and assign municipalities with head 31 council members, and municipalities with tail 41 members. Now we can compare the average size of government across the two different treatment groups. If there is a difference in the average outcome we could causally attribute it to the size of the legislature since the treatment is randomly assigned.[19] However, since the council-size law only influences but not entirely determines the actual size we will estimate the council-size effect by instrumental variable regression, where the "as if" random source of variation provides the instrumental variable. It is the three discontinuities at 12,000, 24,000 and 36,000 eligible voters that provides the "as if" random variation in council size, i.e., small changes in the number of eligible voters potentially induce large changes in the council size at these predetermined levels due to the law. Thus, these discontinuities are used to construct instrumental variable estimates of the effect of council size on government size. Practically, we can implement the regression-discontinuity design in a number of ways.[20] In this paper, I will use two different regression-discontinuity methods. The first will only use within-municipality variation to identify the council-size effect on government size. The second method is to restrict the sample around the points of discontinuities, which means that the inference will be based on the cross-sectional information instead.

Within-unit method

The within-regression-discontinuity method amounts to use observational units with a rule-triggered change in the variable of interest for to identification. Hoxby (2000) argues that the this method is "more accurate and less prone to bias than the cross-section method." One way of implementing the within-unit method is to include fixed-municipality effects, i.e., only the within-municipality variation is being used for identification of the parameter of interest. In addition to the fixed municipality effects, a full set of time dummies will also be included since I do not want to attribute behavioral significance to any across-municipality correlations that are really due to common national influences such as the effect of the national business cycle. The identifying

[19] Formally, this is a conditional mean independence assumption.

[20] Angrist and Lavy (1999), Hoxby (2000), and Pettersson-Lidbom (2002) are examples of some recent studies that have used a regression discontinuity approach.

assumption behind the within-municipality regression-discontinuity method can be expressed formally by first rewriting (3) as:

$$g_{it} = \mu_i + \lambda_t + \delta N_{it} + X_{it}\beta + u_{it}, \tag{4}$$

where i indexes a local government and t corresponds to time, μ_i is the fixed municipality effect, λ_t is the fixed time effect, X_{it} is a vector of other covariates, u_{it} is an i.d.d. error term, g_{it} is the measure of the size of government, and N_{it} is the council size. The council-size parameter δ is the structural parameter of interest. The vast variation in legislature-size is probably endogenous since it is the result of choices made by the legislatures themselves. Therefore, we need to isolate the part this is arguably exogenous. We do this by using the statutory law to create instrumental variables as previously defined. The instrumental variable approach can now be formally explained by writing the reduced form or the "first stage" equation for the endogenous variable N_{it} as:

$$N_{it} = \mu_i + \lambda_t + \psi_{41}Z_{41it} + \psi_{51}Z_{51it} + \psi_{61}Z_{61it} + X_{it}\pi + \xi_{it}, \tag{5}$$

where the error term ξ_{it} is defined as the residual from the population regression of N_{it} on X_{it}, λ_t, μ_i and the instruments: Z_{41it}, Z_{51it}, and Z_{61it} . The validity of the instruments can now be stated as follows: once we control for X_{it}, λ_t, and μ_i, this will partial out any other effects between the instruments and the size of government. Since instrumental validity is the key to get unbiased estimate of the parameter of interest δ some comments about the empirical specification is warranted.

The advantage of using the within-municipality variation, as discussed above, is that the council size is only identified when there is a rule-triggered change in the size of the council. In other words, this means that the council-size parameter will *only* be identified when a municipality actually was forced to change its council size because the number of eligible voters passed one of the three thresholds: 12,000, 24,000 or 36,000. This will turn out to be important piece of information in bolstering a causal interpretation of my findings. At the thresholds of 12,000, no municipality had to change its council size due to the law. This particular fact gives an opportunity of refuting a causal interpretation of my findings. We would expect to see no relationship between council-size and the size of government at this particular threshold. The null hypothesis of no effect can be tested empirically by looking at the links (i.e. the reduced forms) from the instruments to government size and number of council members.

The instruments are explicitly constructed from information about the relationship between the number of eligible voters and the minimum requirement of the council-size members stated in the law. This is distinct from a conventional instrumental variable approach. For this reason it might be necessary to include a smooth function of number of eligible voters in the vector of covariates X_{it}. In our case, one may believe there to be economies of scale in the production of local public goods. Therefore, we may expect the number of eligible voters to be related to the size government as the number of eligible voters is highly correlated with population size. Because of this concern, I control for a cubic in the number of eligible voters. In other words, I assume that there is a smooth relationship between the number of eligible voters and the size of government, which is sufficiently captured by a third order polynomial function.

Finally, I include a number of covariates in X_{it} considered a standard set of controls in the local public finance literature: proportion of people of age 0 to 15, proportion of people older than 65, population size, income, and grants-in-aid.

Cross-section method

The most common regression-discontinuity approach is to rely exclusively on information in the cross-section. However, the drawback of this method is that we need to get really close to the discontinuity to avoid any bias, as discussed by Hoxby (2000). This method is also subject to more sampling variability than the within-unit method.[21] The cross-section method used here treats the panel data as though they were cross-section data and actual changes in the council size were not observed. A crucial question is thus how close the observations must be to the discontinuities to avoid bias. For example, Angrist and Lavy (1999) restrict the sample to less than 12.5 percent from the

[21] In the within-unit approach we are using fixed effects and they will effectively reduce the variance of the error term leading to more precise estimates. In practice this could be quite important since the regression-discontinuity method has such large sampling variability due to that is a correlated design. The standard errors in a correlated design can be much larger compared to an uncorrelated design, i.e., a randomized experiment. The larger is the correlation between the assignment variable and the treatment indicator the larger the variance of the estimate. 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)

discontinuities in their preferred regressions. To avoid any potential bias I will restrict the empirical analysis to intervals that are quite close to the discontinuities. I will work with three different sub samples. In the first sub sample I have restricted to +/-500 (i.e., {[11,500-12,500], [23,500-24,500], [35,500-36,500]}) eligible voters around the discontinuities. However this will lead to relatively more observations in the intervals around the smaller discontinuities. Therefore, in the second sub sample, I restrict the sample to 5 percent (i.e., {[11,400-12,600], [22,800-24,200], [34,200-37,800]}) around the discontinuities. In the third sub sample, I restrict the samples to 3 percent (i.e., {[11,640-12,360], [23,280-24,720], [34,920-37,080]}) from the discontinuities. Since all observations in the samples are quite close to the discontinuities, the instrumental variables should now be valid even without controlling for the number of eligible voters or any other covariates. The only controls are time effects and indicators for the interval around each discontinuity. Since there are three discontinuities there need to be only two indicator variables. For example, in the first sub-sample the two indicator variables will be defined as $I_1=1[11{,}500 \leq v \leq 12{,}500]$ and $I_2=1[23{,}500 \leq v \leq 24{,}500]$. In this setup any constant difference across the thresholds will not contribute to identifying the council-size effect. In other words, I control for fixed-threshold effects and therefore this cross-section estimator is constructed from simple comparison of means around each discontinuity. The idea with this setup is to mimic block randomization (i.e., stratified randomized experiment), i.e., council size is randomly assigned within each group (interval) defined by the indicator variables and the assignment probability is allowed to differ from one group to the next.

4. Data

Before turning to the description of the data it is perhaps helpful to digress briefly on the workings of Swedish local governments. Local governments play an important role in the Swedish economy, both in terms of the allocation of functions among different levels of government and economic significance. They are, for example, responsible for the provision of day care, education, care of the elderly, and social welfare services. To quantify their economic importance, we can note that in the 1980s and 1990s their share of spending out of GDP was in the range 20 to 25 percent and they employed roughly 20 percent of the total Swedish workforce. Swedish local governments also have a large degree of autonomy. They have the constitutional right of self-government, they have no restrictions on borrowing, and they have no balanced budget rules.[22] Moreover, the bulk of revenues are raised trough a proportional income tax, which each municipality is allowed to set freely,[23] and only 20 percent of the revenues come from intergovernmental grants.

There is also a considerable variation in both expenditures and revenues across municipalities. For example, during the period 1974 to 1998, real expenditure per capita was on average SEK 29,174 ($ 4862), the standard deviation 6,015 ($1,000), the minimum value 14,392 ($ 2,400), and maximum value 70,032 ($ 11,672).[24] The data used in the empirical analysis consists of 288 municipalities between 1974 and 1998.[25] The statutory law regulating the minimum council-size requirement has however only been in affect since 1977, and it was not after the election in year 1979 that municipalities had to comply with it. Therefore, some of the empirical analysis is restricted to the period 1980 to 1998. In particular, this concerns the instrumental variable regressions. Table 2 shows the descriptive statistics of the actual size of the local council grouped by segments with a minimum requirement of council sizes of 31, 41, 51 and 61. We can see 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 discussed in the previous section, 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 parameter in the within-unit regression-discontinuity approach. Table 3 presents data on those municipalities that passed one of the three thresholds: 12,000, 24,000 or 36,000 of eligible voters, during the sample period. No municipality was forced to change its council-size at the lowest threshold, whereas 12 and 6 municipalities had to change its number of seats for the middle and highest cutoffs, respectively. Thus, 18 municipalities, which constitute 6.25 percent of the

[22] However, from year 2000 there is a balanced budget rule.
[23] From 1991 to 1993, however, the central government imposed a temporary tax cap.
[24] The expenditures are expressed in 1991 prices using the implicit GDP deflator. The deflator is constructed by taking the ratio of GDP at current market prices to GDP at fixed market prices. The equivalent amount in dollars is shown within parentheses.
[25] Due to a large amalgation of municipalities in 1974 it is not possible to go further back in time

entire sample, will identify the council-size effect in the within-unit method. In the cross-section discontinuity method too, the number of useful observations will also be quite small: around 5-8 percent of all available observation. This large reduction in the number of observations used for identifying the effect of interest is typical for the regression-discontinuity design and is the "price we have to pay" for isolating an exogenous source of variation.

As discussed previously, that no municipality changed its council-size at 12,000 creates an opportunity of refuting a causal interpretation of the relationship between council-size and size of government. In other words, there should be no association between the size of government and the council-size at this particular discontinuity.

Table 4 present summary statistics for the whole sample. All the data used are publicly available and were obtained from Statistics Sweden (SCB) or its publications.[26]

5. Results

In this section I present results on the relationship between council size and government size. Before showing the results for the two regression discontinuity methods described in section 3, I present results for a few methods that have previously been used, despite the inferential problems associated with these approaches. These results may bee seen as a benchmark for assessing potential biases in previous work.

A. Results from previously used methods of identification

Table 5 presents OLS estimates without controlling for unobserved heterogeneity. These estimates show a strong positive correlation between council size and the size of government. The estimates are very precisely measured when only time effects are included as controls. Columns 1 and 2 show that spending and revenues increase with SEK 130 per capita (i.e., 0.5 percent of total spending or revenues) for each additional council member. Including a full set of covariates, the estimated council-size effect decreases to SEK 30 per capita, but are still statistically significant. These results are therefore consistent with previous estimates in the literature that rely on cross-section variation and not taking heterogeneity or simultaneity into account.

[26] The publications used are: How much do local public services cost in Sweden, Local government finance, and Statistical yearbook of administrative districts of Sweden.

Table 6 shows the OLS estimates when we control for unobserved heterogeneity by including fixed municipality effects. In contrast to Table 5, the estimated council-size effects are now all negative. Without any control variables, they are large (SEK -200 per capita) and significant, but controlling for the additional covariates makes them much smaller and not significantly different from zero.

B. Results from the within-unit method

In this section, I present reduced-form results between the instruments and council-size and between the instruments and spending and revenues. These reduced-form estimates provide evidence of the strength of the instruments and whether the instruments can be considered as valid. Table 7 presents the results of both the dummy instruments (the first three columns) as well as the single instrument (the last three columns) since there is a trade off between bias and efficiency when more than one instrument is being used, as discussed in section 3. Table 7 also presents results when controlling for a smooth function of the number of eligible voters by using a third order polynomial.

In the dummy instruments case (the first column), we can see that the instruments are strongly related to council size except for Z_{41}. The instruments, Z_{51} and Z_{61} are positively and highly significantly (with t-values of 10 and 14 respectively) related to the number council seats. A test of instrument relevance shows that these instruments are not "weak". The F-statistic is 86.10, which is much higher than 10, the rule of thumb value suggested by Staiger and Stock (1997). In the single instrument case, the F-statistic is 14, which is also suggestive of a strong instrument. Column 3 shows that the rule-triggered change in council size at the discontinuity 24,000 was 2.9, whereas at the discontinuity 36,000 the change was 7.6. For the single instrument, column 4 shows that the average change was 0.24 members.

Columns 2, 3, 5 and 6 in Table 7 also reveal a large and negative relation between the instruments and the policy outcomes except for Z_{41}. In the dummy instruments case, the estimate of Z_{51} is significantly different from zero at the 1 percent level and the estimate of Z_{61} is almost significant at 10 percent level. However, the estimate of Z_{41} is small and not significantly different from zero. As previously noted, no municipality was forced to change its council size at the threshold of 12,000 eligible voters. Thus, the absence of an effect at this threshold strengthen a causal interpretation of the relationship between council size and size of government since the specification test suggests that factors other than council-size law are not responsible for the correlation between the size of government and the other instruments. As discussed in section 3, we can construct simple Wald-type or IV estimates. For example, dividing the spending and revenue effects in column 2 and 3 by the council-size effect in column 1 leads to an estimated council-size effect on spending and revenues of -1,422/2.93= -485 and –1,472/2.93= -502 respectively when Z_{51} is used as an instrument. Using Z_{61} as an instrument leads to an estimate of –985/7.57= -130 and –869/7.57= -114 on spending and revenues respectively. Thus, it seems that the relationship might be nonlinear since the estimates differ with respect to the instrument being used. However, even if the structural relationship between the council size and the government size is nonlinear, using all instruments produces a linear combination of the Wald estimates and captures an average effect of economic interest (e.g., see Angrist et al., 2000, and Heckman and Vytlacil, 1999). To summarize, the reduced form results from Table 7 suggest a direct and negative relationship between council size and the size of government, but we now turn to two-stage least squares regressions.

Table 8 shows the results from the two-stage least squares estimates. The first four columns show the estimate of the effect of the council-size on spending and revenues from both single and dummy instrument without controlling for the number of eligible voters, whereas the last four columns show the result from using a qubic in eligible voters. The point estimates are roughly SEK –130 per capita for both spending and revenues in both the single and dummy instruments regressions, whereas the estimates are in the range of -150 to -220 when controlling for a third-order polynomial in the number of eligible voters. Thus, it seems that the council-size effect is robust the parameterization of the variable that generates the discontinuities since the council-size estimates are roughly similar across the specifications. Table 8 also reveals that the standard errors are always larger in the single instrumental case compared to the dummy instrument case and that the point estimates of the council-size effect is typically larger than in the dummy-instruments regressions. Thus, this observation suggests a bias-efficiency trade off of using multiple instruments as discussed in section 3. The point estimate from a 2SLS regression is biased towards the OLS-estimate when we increase the number of instruments. By comparing the 2SLS estimate in Table 8 with the OLS estimate from the last two columns in Table 6, we can get a sense of the bias versus efficiency trade off. Table 8 reveals that the council-size estimates from the multiple-instrument regressions might be more biased than the estimates from the single instrument regressions since the estimates from the former are closer to the OLS estimates shown in Table 6, but the estimates are also less precisely measured in single-instrument regressions compared to the estimates from the multiple-instrument regressions. Nevertheless, the estimates of the council-size effects from the various 2SLS regressions are never statistically significantly different from each other. Moreover, all the 2SLS estimates are significantly different from zero at conventional levels.

The interpretation of the relation between council-size and government size as causal, relies on the identification assumption that there are no omitted time varying and municipality specific effects correlated with the discontinuities induced by statutory council-size law. One potential important factor that could produce such a correlation is party control. As noted in section 2, the simple model in this section abstracts from political parties in the budget process. However, since strong parties may have an affect on the common pool problem we also may need to control for party identity. I include an indicator for partisanship (e.g. left or right majority) and polynomials in vote shares. In other words, I add the same covariates used by Pettersson-Lidbom (2002b) in his tests for whether party control matters for fiscal policy choices using a sharp regression-discontinuity design. Table 9 presents a two-stage least square specification that includes these set of controls. The point estimates of the council-size effect are *unaffected* by the inclusion of partisanship. This finding is quite important since the instruments should be as good as randomly assigned and therefore unrelated to both observed and unobserved factors related to the size of government. Thus, this finding lends further credibility to that the instruments are exogenous.

C. Results from the cross-section method

Table 10, present the results from the cross-sectional regression-discontinuity method, i.e., when the sample has been restricted around the discontinuity points: 12,000, 24,000 and 36,000. All specifications only include fixed threshold effects and time effects as controls. As discussed in section 3, there is an issue of how close one needs to be to the discontinuity in order to get unbiased estimates. I have therefore experimented with a number of intervals around the discontinuities: +/-500, 5 % and 3 %. As can be seen from table 10, in each sub sample the estimated effect on the council size on the size of government is always negative. The estimated effect is basically in the range of –200 to –500 SEK per capita, for both spending and revenues. The effects are also statistically significant in the 5% sample and for spending in the 500-sample. Thus, the negative estimates of the council-size effect are consistent with the results from the within-unit regression discontinuity method, although some of the estimates are twice as large. This finding also consistent with that the estimates from the regression discontinuity method based on the cross section information is much less precisely measured than the estimates from the within method as was discussed in section 3.

6. Discussion

The last section established empirically a negative relation between the size of legislature and the size of government using data from Swedish local governments. I interpret the negative council-size effect as causal. In other words, I claim that my findings to be internally valid since I am using a source of exogenous variation to identify the council-size effect. The negative relationship between the number of legislators and the size of government implies rejection of the prediction from the common-pool model in section 2. A potential critique that can be raised against my results is that the common-pool model does not apply to the Swedish political system, since it is based on proportional representation, whereas the classic work by Weingast, Shepsle and Johnson (1981) was developed for a first-the-post-system with one-seat 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, I believe the common-pool model could still be expected to apply in the Swedish context.

The two key factors in a common-pool model are the number of participants in the budget decision and generalized taxation, and not the number of districts per se.[27]

[27] Perotti and Kontopoulos (2002) identify two basic determinants of the degree of overspending bias due to the common pool problem. The number of decisions-makers and the structure of the process were they interact. Since there is no institutional variation in the budget process across the municipalities, this factor could not lead to an omitted variable bias in the estimated council-size effect.

Swedish local governments do raise the bulk of revenues through a proportional income tax, i.e., a broad based tax base, and the budget must be approved by majority of the council members. Thus, as long as individual members of the council can marginally influence the budget outcome and they have different preferences about public goods, it is quite likely that the members might view the tax base as a common pool, and thus give rise to an overspending bias. This in turn raises the issue whether a political system with strong parties as the Swedish one allows for individual policymakers to affect the budget. The result presented in this paper provides support that they do have an impact *independent* of party control. Support for this claim is also evidenced by the fact that the inclusion of party control does not affect the council-size estimate at all, which is to be expected since the instruments constructed from the council-size law are based on the assumption that they should be as good as randomly assigned and therefore be uncorrelated with both observed and unobserved determinants of the outcome. Moreover, since each party gets seats in proportion to its vote share, the relative strength between the ruling majority and its opposition is held constant when the council size is changed. In other words, the balance of power between different political parties in a council is not affected when additional council members is added to the council. Thus, this feature of the Swedish proportional election system gives an additional argument why the size of the council size and strength of parties should be unrelated. [28]

Whether we can interpret the negative relationship between council size and government size as evidence against the common pool model obviously hinges on the interpretation of *N*. However, if one were to raise concerns about the use of the number of seats as a measure for *N* in my study, the same critique must also be raised against all the previous studies since they have equated *N* with the number of seats in the legislature.[29] In addition, 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.[30] For example, in Baqir (2002) less than 17 percent of the cities have council members elected from different

[28] This feature of the council size law avoids the problem, pointed out by Inman and Fitts (1990), of having to rely on a particular hypothesis about how legislature's do their business, i.e., whether legislatures operate under a norm of universalism (leading to 1/*N*), or whether strong parties internalize fiscal spillovers across party members (giving M/N where M is the number of members in the majority party).
[29] The only exception is Perroti and Kontpoulos (2002). They use the number of spending ministers in the cabinet as a measure of *N*.
[30] There are also a number of U.S. states that have multimember districts.

districts (ward systems),[31] whereas the majority of cities instead have at-large systems. Baqir also finds that the council size effect does not depend on the type of election system.

To generalize my findings to other settings one need not only tie my findings to theory, but also establish whether the range of variation used to identify the effect of council size on the size of government is similar to other settings as well. The range of variation used to identify the effect is between 31 and 61, with an average size of 47. This is much larger than city councils in the U.S., which have an average of 7 members. However, cities like New York, NY (36), Stamford, CT (40) and Chicago, IL (50) are in the relevant range. For the U.S states, the average size of the upper house is 40 which put them in the relevant range, whereas the lower house is off the mark with an average of 116 legislators. Most countries also have larger legislatures: an average of 122 in the upper house and an average of 281 in the lower house, but countries like Australia, Norway, Austria and Switzerland, all have upper houses of similar sizes to Swedish local governments.

To summarize, my result seem to be at odds with common-pool model. The question is now whether we can we find an alternative explanation that could explain the negative relationship between the size of the legislature and the size of government? Unfortunately, I am not aware of such models so my results constitute a challenge for future theoretical work. Perhaps, ideas or models from the social interactions literature may give some clues for possible explanations of the negative relationship since this literature explicitly deals with group interactions.[32] The council effect identified in this paper is an example of such a group effect.

7. 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

[31] There are cities with both single member and multimember districts among the 17 percent with district electoral system. Unfortunately, Bagir treats all cities as having single-member districts since his data does not allow him to separate them apart.

[32] See Brock and Durlauf (2001) and the references cited therein.

address the concerns of simultaneity, selection, and omitted-variable bias. To isolate an exogenous variation in the size of the legislature, this paper exploits a statutory law linking council size to the number of eligible voters in Swedish local governments. The statutory law creates discontinuities between number of eligible voters and council size, which are used to construct instrumental variable 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 0.5-0.8 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., and K., Graddy, and G. Imbens (2000), “Nonparametric Demand Analysis with an Application to the Demand for Fish,” *Review of Economic Studies*, 67, 499-527.

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

Angrist, J. and G. Imbens (1995), “Two-Stage Least Squares Estimation of Average Causal Effects in Models with Variable Treatment Intensity,” *Journal of the American Statistical Association*, 430, 431-442.

Angrist, J. and A. Kreuger (2001), “Instrumental Variables and the Search for Identification: From supply and Demand to Natural Experiments,” *Journal of Economic Perspectives*, 15, Fall, 69-85.

Angrist, J., and V. Lavy (1999), “Using Mamonides’ Rule to Estimate the Effect of Class Size on Scholastic Achievement,” *Quarterly Journal of Economics*, 114, 533-75.

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

Besley, T., and A. Case (2002), “Political Institutions and Policy Choices: Empirical Evidence from the United States,” mimeo, London School of Economics.

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

Brock, W., and S., Durlauf (2001), “Interactions-based models,” in Heckman J., and E. Leamer (eds.), *Handbook of Econometrics*, Volume 5, North-Holland.

Dahlberg, M., and P. Pettersson-Lidbom (2002), “The Bailout Problem: An Empirical Approach,” mimeo, Stockholm University.

Gilligan, T. and 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., and 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.

Heckman, J., and E. Vytlacil (1999) “Instrumental Variable Methods for the Correlated Random Coefficients Model: Estimating the Average Rate of Return to Schooling when the Return is Correlated with Schooling,” *Journal of Human Resources*, 33, 974-987.

Heckman, J., (2001), Micro Data, Heterogeneity, and the Evaluation of Public Policy: Nobel Lecture, *Journal of Political Economy*, 109, 673-748.

How much do local public services cost in Sweden (Vad kostar verksamheten i din kommun) Örebro: SCB, Publikationstjänsten; Stockholm: Kommentus.

Hoxby, C., (2000) “The Effects of Class Size on Student Achievements: New Evidence from Population Variation,” *Quarterly Journal of Economics*, 115, 1239-85.

Inman, R. and M., Fitts (1990), “Political Institutions and Fiscal Policy: Evidence from the U.S. Historical Record,” *Journal of Law, Economics and Organization*, 6, 79-132.

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

Manski, C. (1989), “Anatomy of Selection Problems,” *Journal of Human Resources*, 24, 341-360.

Newey, W. (1990), “Efficient Instrumental Variable Estimators of Nonlinear Models,” *Econometrica*, 58, 809-838.

Perotti R., and Y. Kontopoulos (2002), “Fragmented Fiscal Policy,” *Journal of Public Economics*, 86, 191-222.

Persson, T., and G. Tabellini (2002), *The Economic Effect of Constitutions: What do the Data Say?*, forthcoming at MIT Press, 2003.

Pettersson-Lidbom, P. (2001), “An Empirical Investigation of the Strategic Use of Debt,” *Journal of Political Economy*, 109, 570-84.

Pettersson-Lidbom, P. (2002a), “A Test of the Rational Electoral-Cycle Hypothesis,” mimeo, Stockholm University.

Pettersson-Lidbom, P. (2002b), “Do Parties Matter for Fiscal Policy Choices? A Regression-Discontinuity Approach,” mimeo, Stockholm University.

Poterba, J., and J. Von Hagen (1999), *Fiscal Institutions and Fiscal Performance*, Chicago: University of Chicago Press and NBER.

Riker, W. (1980), “Implications for the Disequilibrium of Majority Rule for the Study of Institutions,” *American Political Science Review*, 74, 432-446.

Sawa, T. (1969), “The Exact Sampling Distribution of Ordinary Least Squares and Two-Stage Least Squares Estimators,” *Journal of the American Statistical Association*, 64, 923-37.

Staiger, D., and 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.

Stock, J., Wright, J., and M. Yogo (2002), “A Survey of Weak Instruments and Weak Identification in Generalized Method of Moments,” *Journal of Business and Economic Statistics*, 20, 518-29.

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

Weingast, B., Shepsle K., and 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. Minimum council size and the number of eligible voters

Minimum requirement of council size	Number of eligible voters
31	Less than 12,000
41	12,001 – 24,000
51	24,001 – 36,000
61	More than 36,000
Stockholm (the capital) is required to have at least 101 council members	

Table 2. Actual council size

Minimum requirement of council size	Average council size	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
101	101	0	101	101

Table 3. Information about the discontinuities 1980-1998

Discontinuities (Number of eligible voters)	Number of municipalities that crossed a discontinuity	Number of municipalities that had to change their council size due to the law
12,000	15	0
24,000	17	12
36,000	9	6

Table 4. Descriptive statistics

Variables	Mean	Standard d.	Min	Max
Council size	47.55	11.06	31	101
Number of eligible voters	22,818	42,430	2,286	562,591
Spending	29,174	6,015	14,392	70,032
Revenues	29,083	5,929	15,515	71,699
Proportion of young, 0-15	21.05	2.69	12.65	36.69
Proportion of old, 65+	17.79	4.22	3.27	28.14
Income	72,624	12,357	15,945	162,962
Population size	29,923	53,074	2,865	727,339
Grants-in-aid	2,589	2,598	-4,749	19599

Spending, revenues, income and grants-in-aid are all expressed in per capita terms and in 1991 prices.

Table 5. Council size and the size of government: OLS estimates

	Spending	Revenues	Spending	Revenues
Council size	129	130	31	34
	(7.52)	(7.50)	(9.65)	(9.56)
Population 0-15			-765	-747
			(43)	(42)
Population 65+			-422	-402
			(33)	(33)
Population size			.030	.030
			(.004)	(.004)
Income			.128	.133
			(.011)	(.011)
Grants			1.37	1.37
			(.037)	(.035)
Time effects	Yes	Yes	Yes	Yes
R2	0.3705	0.3725	0.6100	0.6183
Number of observations	7,051	7,050	7,051	7,050

Notes: Estimates are based on Swedish municipality data for 1974-1998 Robust standard errors are in parentheses.

Table 6. Council size and the size of government: fixed-effect estimates

	Spending	Revenues	Spending	Revenues
Council size	-205	-202	-16	-15
	(21)	(21)	(24)	(24)
Population 0-15			189	162
			(41)	(41)
Population 65+			-178	-142
			(43)	(41)
Population size			-.229	-.215
			(.024)	(.021)
Income			.102	.117
			(.019)	(.021)
Grants			.48	.55
			(.05)	(.05)
Municipality effects	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes
R2 within	0.6409	0.6436	0.6799	0.6856
Number of observations	7,051	7,050	7,051	7,050

Notes: Estimates are based on Swedish municipality data for 1974-1998. Robust standard errors are in parentheses.

Table 7. Reduced form estimates

	Council size	Spending	Revenues	Council size	Spending	Revenues
Z_{41}	-0.15	70	119			
	(.092)	(327)	(312)			
Z_{51}	2.93	-1422	-1472			
	(.29)	(505)	(488)			
Z_{61}	7.57	-985	-870			
	(.53)	(641)	(620)			
Single instrument (Z_{31}+2*Z_{41}+3*Z_{51}+4*Z_{61})				.237	-52	-51
				(.017)	(22)	(21)
Eligible voters	0.00021	-0.140	-0.232	.000325	-.148	-.240
	(.000069)	(.116)	(.110)	(.000071)	(.116)	(.109)
Squared	-2.43e-09	-2.59e-06	-2.26e-06	2.87e-09	-2.57e-06	-2.25e-06
	(2.61e-10)	(7.35e-07)	(7.22e-07)	(2.73e-10)	(7.31e-07)	(7.19e-07
Cubic	2.08e-15	2.79e-12	2.31e-12	2.51e-15	2.76e-12	2.28e-12
	(2.51e-16)	(8.12e-13)	(7.88e-13)	(2.61e-16)	(8.08e-13)	(7.85e-13)
Population 0-15	-0.047	309	277	.046	300	268
	(.037)	(63)	(63)	(.039)	(63)	(63)
Population 65+	0.168	-321	-250	.205	-315	-243
	(.033)	(66)	(62)	(.034)	(66)	(62)
Population size	.00022	-.038	-.024	.000215	-.030	.033
	(.000059)	(.068)	(.063)	(.000059)	(.068)	(.064)
Income	-0.000054	0.149	0.169	-.000055	.153	.174
	(.000021)	(.026)	(.029)	(.000021)	(.026)	(.029)
Grants	-0.00008	0.388	0.470	-.000069	.391	.474
	(.000021)	(.0544)	(.051)	(.000021)	(.054)	(.051)
Municipality effects	Yes	Yes	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.9860	0.8316	0.8316	0.9853	0.8310	0.8375
Number of observations	5,403	5,389	5,389	5,403	5,389	5,389

Notes: Estimates are based on Swedish municipality data for 1980-1998. Robust standard errors are in parentheses.

Table 8. Council size and the size of government: Two-Stage Least Square estimates

	Dummy IV		Single IV		Dummy IV		Single IV	
	Spending	Revenues	Spending	Revenues	Spending	Revenues	Spending	Revenues
Council size	-124	-129	-128	-135	-152	-146	-221	-216
	(59)	(56)	(76)	(73)	(67)	(65)	(92)	(89)
Eligible voters					-.112	-.206	-.077	-.170
					(.124)	(.116)	(.130)	(.123)
Squared					-2.91e-06	-2.58e-06	-3.21e-06	-2.87e-06
					(7.90e-07)	(7.78e-07)	(8.43e-07)	(8.28e-07)
Cubic					3.05e-12	2.55e-12	3.31e-12	2.82e-12
					(8.51e-13)	(8.25e-13)	(8.95e-13)	(8.70e-13)
Population	352	329	352	329	308	276	311	278
0-15	(64)	(67)	(65)	(66)	(64)	(64)	(64)	(65)
Population	-362	-293	-362	-292	-286	-216	-270	-199
65+	(67)	(63)	(68)	(64)	(69)	(64)	(71)	(67)
Population size	-.250	-.229	-.250	-.228	.00008	.062	.017	.079
	(.039)	(.034)	(.039)	(.034)	(.075)	(.070)	(.079)	(.074)
Income	.132	.146	.132	.146	.144	.165	.141	.162
	(.034)	(.039)	(.034)	(.038)	(.028)	(.032)	(.029)	(.032)
Grants	.375	.461	.375	.460	.380	.463	.375	.46
	(.055)	(.052)	(.056)	(.052)	(.054)	(.051)	(.055)	(.051)
Municipality effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	5,389	5,389	5,389	5,389	5,389	5,389	5,389	5,389

Notes: Estimates are based on Swedish municipality data for 1980-1998. Robust standard errors are in parentheses.

Table 9. Council size and the size of government: adding party control and vote shares

	Dummy IV		Single IV		Dummy IV		Single IV	
	Spending	Revenues	Spending	Revenues	Spending	Revenues	Spending	Revenues
Council size	-129	-134	-137	-148	-154	-147	-233	-234
	(59)	(57)	(76)	(73)	(67)	(65)	(92)	(89)
Controls:	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
See Table 8								
Municipality effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	5,389	5,389	5,389	5,389	5,389	5,389	5,389	5,389

Notes: Estimates are based on Swedish municipality data for 1980-1998. Robust standard errors are in parentheses.

Table 10. Results for the discontinuity samples: Two-Stage Least Square estimates

Discontinuity-samples	+/- 500				5 percent				3 percent			
	Dummy IV		Single IV		Dummy IV		Single IV		Dummy IV		Single IV	
	Spending	Revenues	Spending	Revenues	Spending	Revenues	Spending	Revenues	Spending	Revenues	Spending	Revenues
Council size	-545	-391	-512	-341	-295	-298	-355	-339	-240	-227	-368	-330
	(301)	(265)	(306)	(268)	(109)	(106)	(117)	(110)	(247)	(234)	(264)	(245
I_1	-10399	-7965	-9966	-7265	-5817	-5897	-6636	-6464	-5799	-5739	-7450	-7082
	(4190)	(3709)	(4231)	(3721)	(1591)	(1569)	(1699)	(1639)	(3243)	(3146)	(3512)	(3335)
I_2	-6786	-4820	-6518	-4388	-4358	-4104	-4940	-4506	-4488	-4057	-5552	-4922
	(2620)	(2299)	(2634)	(2299)	(1130)	(1109)	(1210)	(1152)	(2081)	(2024)	(2254)	(2127)
Time effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	302	302	302	302	593	593	593	593	342	342	342	342

Notes: Estimates are based on Swedish municipality data for 1980-1998. Robust-standard errors are in parentheses.