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**Working Paper**

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Working Paper, No. 2008:4

**Provided in Cooperation with:**

Department of Economics, Uppsala University

Suggested Citation: Dahlberg, Matz; Mörk, Eva (2008) : Is there an election cycle in public employment? Separating time effects from election year effects, Working Paper, No. 2008:4, Uppsala University, Department of Economics, Uppsala, <http://nbn-resolving.de/urn:nbn:se:uu:diva-15977>

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Working Paper 2008:4

*Department of Economics*

# Is there an election cycle in public employment? Separating time effects from election year effects

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Working paper 2008:4  
March 2008  
ISSN 1653-6975

IS THERE AN ELECTION CYCLE IN PUBLIC EMPLOYMENT?  
SEPARATING TIME EFFECTS FROM ELECTION YEAR EFFECTS

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# Is there an election cycle in public employment? Separating time effects from election year effects<sup>◦</sup>

Matz Dahlberg<sup>⊥</sup> and Eva Mörk<sup>\*</sup>

## Abstract

Do governments increase public employment in election years? This paper investigates this question by using data from Sweden and Finland, two countries that are similar in many respects but in which local elections are held at different points in time. We can thereby separate an election effect from other time effects. Our results indicate that there is a statistically significant election year effect in local public employment, a production factor that is highly visible in the welfare services provided by the local governments in the Scandinavian countries. The effect also seems to be economically significant; the municipalities employ 0.6 more full-time employees per 1,000 capita in election years than in other years (which correspond to an increase by approximately 1 percent).

Keywords: Election cycle, Public employment, Exogenous elections

JEL-codes: D72, H72, P16

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<sup>◦</sup> This paper has benefited from comments from Per Pettersson-Lidbom, Patrik Hesselius, Jon Fiva, Gissur Erlingsson and from seminar participants at IUI, IFAU, Ratio and at the 62th IIPF Congress in Paphos. We are also grateful to Per Pettersson-Lidbom for the idea to combine Swedish and Finnish data, to Heléne Lundqvist for excellent research assistance, and to Antti Moisio for supplying us with Finnish data and for answering our many questions on institutional matters for Finland. Remaining errors are of course ours.

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# 1 Introduction

There is a large and growing empirical literature studying the effects of elections on public policy (see, e.g., Shi and Svensson, 2006, for a recent study and for references to earlier work). The major problem with the existing studies<sup>1</sup> is however that they have typically used data in which the timing of the election is determined by other factors that are potentially affected by public policy, implying that the estimated election effect is quite likely to be biased. There are at least two reasons to why elections might be endogenous in this sense: first, both the timing of elections and fiscal policies could be influenced by a number of (unobserved) variables, such as crises or social unrest; second, the incumbent may strategically choose the timing of elections conditional on fiscal policy outcomes. Since all variables that are likely to affect the choice of election date are as likely to affect the chosen policy, there does not exist any good instrumental variables for endogenous elections, implying that the endogeneity problem is not likely to be solved through IV estimation.<sup>2</sup>

One solution to the problem would be to use data from one country in which the election dates are pre-determined by constitution (and hence strictly exogenous); this could be done either by using a long time series on national elections or by using panel data on local government elections. The problem with the first approach is that one gets rather few observations.<sup>3</sup> The latter approach, which has a major advantage over national time series data in that it generates

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<sup>1</sup> See, e.g., Alesina *et al* (1993), Brender & Drazen (2005), Shi & Svensson (2002, 2006) using cross-country data of developed as well as developing countries, Andrikopoulos *et al* (2004) investigating EU countries, Blais & Nadeau (1992), Reid (1998) and Kneebone & McKenzie, (2001) all using data from Canadian provinces, and Akhmedov & Zhuravskaya (2004) using regional Russian data.

<sup>2</sup> The nature of the endogeneity problem when estimating potential election effects is well discussed by Shi and Svensson (2006).

<sup>3</sup> The earliest empirical studies on election effects aimed at testing for electoral cycles in national output, unemployment and inflation using aggregate time series data. The general conclusion from these studies is that there is little empirical evidence of a political business cycle in inflation and unemployment (see, e.g., McCallum, 1978, Golden & Poterba, 1980, Beck, 1987 and Alesina, 1989). One reason for the weak evidence might be the few election years present in aggregate data, yielding little variation in data.

more observations on election periods, has been adopted by Drazen & Eslava (2005), investigating Columbian local governments, Pettersson-Lidbom (2003), investigating Swedish municipalities, Galli & Rossi (2002), investigating Western German Länder, and Baleiras & da Silva Costa (2004), Coelho *et al* (2006), and Veiga & Veiga (2007), investigating Portuguese local governments. The approach adopted in these latter studies is however problematic since it is very hard to separately identify an election effect from other time effects. Optimally, one would like to include time dummies in the econometric specification to control for general time effects. However, the election effect is not identified when time dummies are included. Instead time-trends, macro-economic indicators, or period-dummies have been used. The problem with the first two approaches (i.e., time-trends and macro-economic indicators) is that one imposes restrictions on the time effect by imposing a specific pattern for it. The use of period-dummies might, *ex ante*, seem like a more valid approach than the other two. However, the results are quite likely to be very sensitive to the exact specification of period-dummies.

In this paper, we use an approach that allows us to separate an election effect from other time effects.<sup>4</sup> The idea is to use data from two or more countries with fixed election dates, countries that are very similar when it comes to municipal decision making and economic environment, but in which local elections are held at different points in time. Thereby, the time effect in the country where no election is held can be used as a counterfactual to the time effect that would have been present in the country in which an election is held if there had not been an election. In this paper we use data from Sweden and Finland, two Scandinavian countries in which the local governments have very important and similar roles but in which elections to the local council are

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<sup>4</sup> The best approach so far in the literature is the one adopted by Akhmedov & Zhuavskaya (2004) on Russian data. For most of the local elections in their data, the election dates are fixed. They do however not have a completely clean case since some of the election dates are chosen by the local politicians, implying that they still might have some unsolved endogeneity problems in their paper. A similar approach to solve the endogeneity problem was adopted by Shi & Svensson (2006) and Brender & Drazen (2005). They conducted separate estimations for countries where the elections were held within six months before or after the scheduled election date, which they argue can be considered as pre-determined. However, much can happen in six months. Also, when using data from many countries, it is difficult to properly control for all institutional differences that might be correlated both with public policies and with election dates.

held in different years. We look for election effects in municipal employment. This is a variable that is highly visible for the voters. This is especially true in the Scandinavian countries where the local governments are mainly responsible for welfare services that are highly labor intensive, such as schooling, child care, and care for the elderly and disabled. These services are also economically important in the sense that they constitute a large part of the local governments' responsibilities; for a further discussion on this issue, see Section 3. Visibility of a policy to the voters must be considered a minimum requirement if a politician wants to use a policy tactically (see also the theoretical discussion in Drazen & Eslava, 2005, 2006).

Besides making it possible to separate between election effects and other time effects we argue that there are at least two advantages with using data from Sweden and Finland. First, when using cross-section data it is very hard to control for all factors that differ across very different countries and that might be correlated with both the election dummy and the public policy. The existing studies typically control for country-dummies but it is questionable whether these are enough to control for the very different settings the different countries work under. Second, the data in traditional cross-country studies is often of poor quality and there is often a lot of work with the data before estimations.<sup>5</sup> These problems do not occur with data from countries like Sweden and Finland.

We find a significant election effect in local government employment rates; municipalities employ 0.6 more full-time employees per 1,000 capita in election years than in other years. This corresponds to an increase by approximately 1 percent. Since several of the earlier studies have looked at election effects in total spending (see, e.g., Andrikopoulos *et al*, 2004, Blais & Nadeau, 1992, and Pettersson-Lidbom, 2003) and tax rates (see, e.g., Andrikopoulos *et al*, 2004, Kneebone & McKenzie, 2001, and Pettersson-Lidbom, 2003), we thought it could be interesting to use our strategy also on these, for the voters,

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<sup>5</sup> Both Shi & Svensson (2006) and Brender & Drazen (2004) do for example use data from International Financial Statistics, published by the International Monetary Fund, which consists of fiscal data that are well known to be noisy (see the discussion in Brender & Drazen, 2005). Brender & Drazen (2005) do for example tamper with the data by filling in missing observations from other sources and by dropping or replacing outliers that they argue are unreliable data points.

less visible outcomes. We find a significant election effect also in the local tax rate and in local total consumption. This is interesting since it contrasts with the results in Brender & Drazen (2004) and Shi & Svensson (2006) who do not get any election effects for their sub-samples of developed countries (in which Sweden and Finland are included).<sup>6</sup>

## 2 Theoretical background

The early papers discussing political business cycles study the behavior of central governments and macroeconomic aspects like inflation and unemployment. These papers provided a rather naïve view of the voters, where they are described as myopic and non-rational (see, e.g., Nordhaus, 1975, and Lindbeck, 1976). An incumbent government can increase its re-election possibilities by lowering unemployment or by taking expansionary actions now at the cost of higher inflation tomorrow.

These early papers have been criticized on theoretical grounds. Why do voters care about short-term fluctuations during election years, knowing that the pattern will be changed after the election when the re-elected government cleans up the economy? Are voters really that myopic? As a response to this critique, Rogoff & Sibert (1988) and Rogoff (1990) present models where voters are rational and not myopic as in the models by Nordhaus and Lindbeck. These models assume that politicians care about policy outcomes as well as about personal ego-rents from holding office. Politicians differ in competence, affecting their ability to supply public services at lower tax rates. Whereas politicians observe competence directly, voters do only observe it with a one-year lag. In this setup, it is possible for competent governments to signal their competence in election years by boosting spending and cutting down taxes, and thereby postponing public investments until after the election. For non-competent incumbents, the cost of doing this will be too high, and, therefore, producing election cycles is only worthwhile for competent incumbents.<sup>7</sup>

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<sup>6</sup> Brender & Drazen (2004) and Shi & Svensson (2002, 2006) separate between new and mature democracies and find that the election cycle is present in new democracies only.

<sup>7</sup> Persson & Tabellini (2000) presents a similar model as Rogoff, studying the same macroeconomic variables as Nordhaus did. Besley & Case (1995) study the phenomena of



Recently, Drazen & Eslava (2005) have presented a model where politicians are predicted to increase some types of spending and decrease others, rather than increasing total spending. They can thereby impose an election cycle in spending without raising deficits. The critical assumptions in this model are that politicians have unobserved, persistent, preferences over groups of voters or types of expenditures and that voters lack perfect information of these preferences as well as of the voting pattern in the population. Voters can therefore not judge whether high targeted expenditures before an election is caused by the incumbent having preferences for the group's preferred type of spending or by their group having many swing voters. They show that there exists a Perfect Bayesian Equilibrium in which voters rationally respond to election-year expenditures and politicians allocate expenditures across groups on the basis of this behavior. They further show that even if voters are aware of being a swing-group, a political cycle may still be present.

### 3 Institutional background

This paper builds on the assumption that Sweden and Finland are two similar countries, at least when it comes to the role played by the local governments (municipalities). In this section we will therefore devote some time at describing, and comparing, the municipalities in the two countries.

In both countries, the municipalities have strong, constitutionally regulated, independence, and are the key suppliers of welfare services, including primary and secondary education, day care, care for the elderly, care of the handicapped and the mentally ill, social assistance etc. In Finland, the municipalities are also responsible for health care and higher education, whereas the first is a responsibility of the counties in Sweden (except for basic health care for elderly which is a municipal responsibility also in Sweden), and the second of the

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election cycles in a somewhat different setup. As opposed to Rogoff, they assume that voters *dislike* spending, since high spending is an indication of a rent-seeking behaviour among politicians. Politicians will keep down rent-seeking during election years in order to get re-elected. However, this incentive is not present if the politician cannot run for office, which is the case in the presence of term-limits. Hence, they predict spending to be lower in election years if the incumbent is allowed to run for office again, a prediction that is supported by their empirical analysis.

central government. Since the Finnish municipalities typically are too small to supply hospitals, neighboring municipalities cooperate in joint municipal authorities. The municipalities in both countries are free to set their own tax rates and do not face any borrowing constraints.

There are 290 municipalities in Sweden and 432 in Finland. The Finnish municipalities are typically somewhat smaller, both in population size (see *Table 1* below for more details) and in geographical size. The inhabitants are unevenly spread in both countries with the capitals Stockholm and Helsinki being the largest (with respect to population) and the most densely populated situated in the north of the countries.

The municipalities are lead by municipal councils elected every third or fourth year. During the studied period, six local elections have been held in Sweden (on the third Sunday in September 1985, 1988, 1991, 1994, 1998 and 2002) and four in Finland (on the fourth Sunday in October 1988, 1992, 1996, and 2000).

The main source of municipal income comes from the local income tax (approximately 50–60 percent of municipal revenue).<sup>8</sup> Intergovernmental grants from the central government make up approximately 15–20 percent of municipal income in both countries (this figure varies a lot between municipalities and some are more reliant on grants than the average), and user fees make up approximately 5–10 percent. In 1993, major grant reforms took place in both Sweden and in Finland in which grants switched from being mainly targeted to becoming mainly general, thereby increasing the freedom of the municipalities.<sup>9</sup>

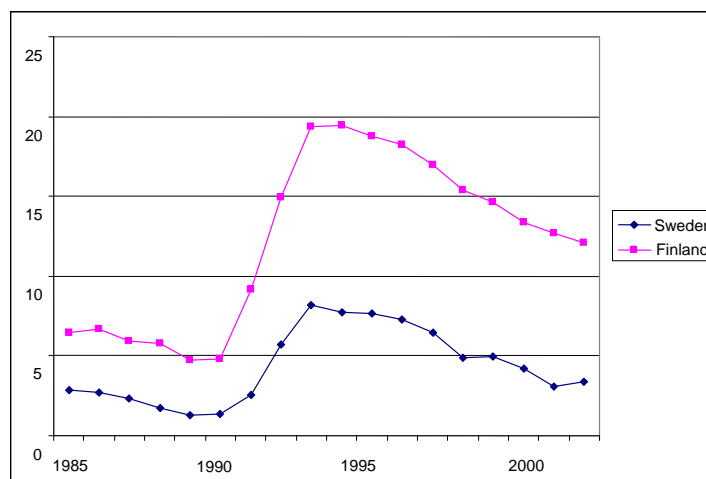
During the early 1990s, both Sweden and Finland went into economic recessions which of course affected the municipalities. As unemployment rose (see *Figure 1*), so did the costs for social assistance. Along with this, grants from the central governments decreased as the central governments were reconstructing the public finances which meant that the municipalities had to rely on own-source revenues to a larger extent.

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<sup>8</sup> In Finland the municipalities also raise property taxes whereas this is not the case in Sweden.

<sup>9</sup> For a thorough description of Finland and the Finnish grant reform, see Moisio (2002). For a description of the Swedish case, see, e.g., Bergström *et al* (2004).

**Figure 1** Average unemployment rate in Sweden and Finland, 1985–2002



Source: Statistics Sweden and VATT

## 4 Data

We have data on all Swedish and Finnish municipalities for the period 1985–2002. After excluding municipalities that have been engaged in mergers or splits during this period we end up with 276 Swedish municipalities and 411 Finnish municipalities.<sup>10</sup> *Table 1* below presents summary statistics for the two countries. Besides municipal employment we also present summary statistics for a number of variables that will be used as covariates in the empirical analysis. These are municipal wages, municipal tax base, intergovernmental grants from the central government, municipal population, and the shares of the

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<sup>10</sup> We also follow earlier studies on Finnish data and exclude 16 Finnish municipalities on the island Åland.

population aged 1–15 and older than 64 respectively.<sup>11</sup> The reason for including these population shares is that many of the services supplied by the municipalities are targeted to people in those age-intervals (child care, schooling, and care for the elderly).

The dependent variable that we will analyze is municipal employment<sup>12</sup>, which is somewhat higher in Sweden.<sup>13</sup> Turning to the other variables, we see from the table that municipal wages as well as intergovernmental grants are somewhat higher in Finland than in Sweden, but that there are no large differences.

*Figure 3* shows how the average employment rate has evolved over time. It can be noted that average employment increased quite dramatically in Sweden between 1990 and 1991, as well as between 1991 and 1992. These increases coincide with the decentralization of schooling (1991) and elderly care (1992) to the municipal sector. In Finland, a drop can be noted in 1993. This was a result of the economic recession during the 1990s when many municipalities discharged their part-time labor force and laid off full-time employees in order to reduce their salary expenditures (see Moisio, 2002). We will discuss how to handle these jumps in the next section when presenting the econometric specification.

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<sup>11</sup> Employment is expressed as employees per 1,000 capita, wages in euros per month, and tax base and intergovernmental grants in euros per capita.

<sup>12</sup> For Finland we include those that are employed by the joint authorities. An alternative way of defining municipal employment in Finland would be to only include those employed by the municipalities. However, since some of the joint authorities also provide all basic services such as education and social services, implying that some of the municipalities have zero employed persons, we argue that it is a better alternative to include than to exclude those that are employed by the joint authorities.

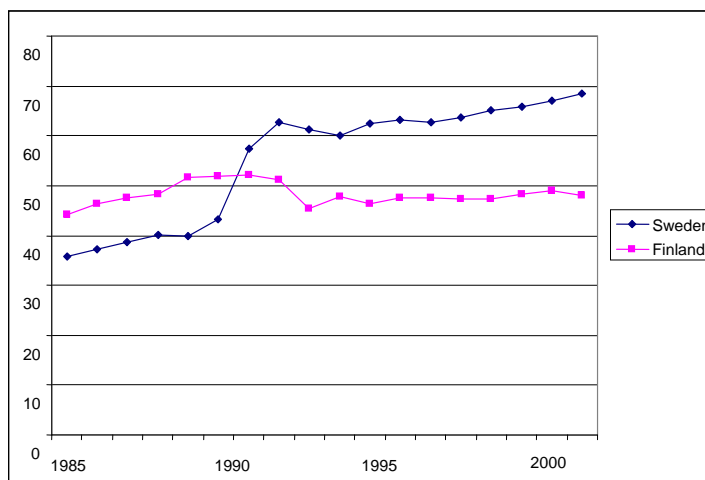
<sup>13</sup> One explanation for this might be that the Finnish data does not include part time employees whereas the Swedish data does.

**Table 1** Summary statistics, Mean, (st dev), [min max], 1985–2002

	Sweden	Finland
Municipal employment	55.29 (14.03) [19.67 101.3]	48.22 (16.23) [0.30 152.3]
Wage, municipal employment	1,202.7 (295.6) [664.5 1,918]	1,494.5 (308.2) [774.3 2,484]
Tax base	7,063 (2,158) [1,879 21,505]	7,859 (2,398) [2,752 31,713]
Intergovernmental grants	711.1 (318.9) [-1,068 2,317]	1,127 (501.6) [-376.7 3,387]
Population	26,721 (46,421) [3,629 754,948]	11,159 (31,848) [174 559,718]
Share of population 1–15	0.203 (0.020) [0.127 0.292]	0.208 (0.033) [0.099 0.381]
Share of population 65 +	0.186 (0.040) [0.051 0.297]	0.166 (0.044) [0.041 0.311]
Number of municipalities	276	411

**Notes:** Employment is expressed as employees per 1,000 capita, wages in euros per month, and tax base and intergovernmental grants in euros per capita.

**Figure 2** Average municipal employment in Sweden and Finland, 1985–2002



Source: The Swedish Association of Local Authorities and VATT

## 5 Econometric considerations

### 5.1 Econometric specification

When investigating if there is an election cycle in municipal employment, we will estimate the following baseline model:

$$y_{itc} = \phi ELECTION_{itc} + \theta' x_{itc} + f_{ic} + \tau_t + \varepsilon_{itc} \quad (1)$$

where  $y_{itc}$  is the local public employment rate (i.e., the number of persons employed by the local public sector per thousand capita) in municipality  $i$  in country  $c$ , in year  $t$ ,  $ELECTION_{itc}$  is a dummy variable taking on the value 1 if there is an election in country  $c$  in year  $t$ , zero otherwise,  $x_{itc}$  is observable char-

acteristics of the municipalities<sup>14</sup>,  $f_i$  are unobserved, municipality-specific fixed effects, and  $\tau_t$  are time-specific fixed effects.

The baseline model in equation (1) will then be tested against some more flexible specifications. First, acknowledging the upward shift in municipal employment in Sweden 1991 (which depends on teachers becoming municipal employees) and the downward shift in Finland in 1993, we will include two country-specific time-effects in the estimations; one for Sweden that takes the value 1 for the years 1991–2002 and zero otherwise, and one for Finland that takes the value 1 for the years 1993–2002 and zero otherwise. Thereafter, we will also allow for country-specific linear time-trends. Finally, since the two countries do not supply exactly the same services, implying that the demographic structure and other observable covariates might affect employment differently in the two countries, we will allow the parameter vector  $\theta$  to vary between the two countries. Hence, the most flexible equation that we will estimate is given by:

$$y_{itc} = \phi ELECTION_{itc} + \theta_c' x_{itc} + f_{ic} + \tau_t + trend_c + SWE91 + FIN\_93 + \varepsilon_{itc} \quad (2)$$

## 5.2 Correlated standard errors?

Criticisms have been raised in the literature regarding difference-in-differences models estimating treatment effects for large groups that are treated at the same time; see, e.g., Bertrand *et al* (2004). The fact that all individuals are treated at the same time makes it impossible to control for aggregate year-to-year shocks that affect all treated observations the same way. However, we argue that this type of criticism is less relevant in our set-up, since we have a case where the treatment (election) is repeatedly turned on and off, and its value yesterday says nothing about its value today. Hence, there exists no serial correlation in the treatment variable. In the Working Paper version of their paper, Bertrand *et*

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<sup>14</sup> These are municipal tax base, intergovernmental grants, population, share of population aged 1–15 and share of population older than 64. We allow share of population 1–15 and 65+ to have different effects in Sweden after the decentralization of schooling and care for elderly in 1991 and 1992 respectively than before.

*al* (2002) also present Monte Carlo evidence showing that when the serial correlation disappears, so does the over-rejection problem.

Having said this, there can however be other reasons why the standard errors might be correlated. First, since it is likely that municipalities may not adjust labour freely, due to labour market regulations and hiring costs, we would expect to see a sluggish adjustment in municipal employment.<sup>15</sup> If this is the case, it would induce a correlation in the standard errors over time for each municipality (implying that we would need to cluster on cross-sectional unit in the estimations in order to get unbiased estimates of the standard errors). Second, since, e.g., the elections to the central government are held on the same day as the municipal elections in Sweden, this might induce a correlation in the standard errors over municipalities for each year (implying that we would need to cluster on year in the estimations in order to get unbiased estimates of the standard errors). A correlation in the standard errors over municipalities for each year would also be the case if the municipalities interact strategically with each other when taking decisions on their outcome variables, or if there are any other country-year specific shocks.<sup>16</sup>

To deal with these issues, we will in the next section examine whether correlated standard errors seems to be a problem in our case, and, if so, try to find out what type of correlations that needs to be dealt with.

## 6 Results

### 6.1 Baseline results

Let us now turn to the question of interest: Is there an election year effect in local public employment? To start with some baseline results, *Table 2* presents

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<sup>15</sup> This is also what Bergström *et al* (2004) find in their study of municipal employment in Sweden. It is also in accordance with studies of dynamic decision-making in local jurisdictions (see, e.g., Holtz-Eakin and Rosen, 1991 on US data, Dahlberg and Johansson, 2000, on Swedish data, and Borge and Rattsø, 1996, on Norwegian data).

<sup>16</sup> Recent evidence shows that there seems to be strategic interactions among Swedish municipalities; see, e.g., Dahlberg & Edmark (2007) and Edmark & Ågren (2007).



estimated election year effects found in the four different specifications of the model discussed in the previous section.<sup>17</sup>

We start by estimating the model presented in equation (1). The result is presented in column i). We find a positive and statistically significant election year effect; municipalities employ almost 2 more people per 1,000 capita in election years, which corresponds to an increase by approximately 4 percent (average municipal employment is 51 employees per 1,000 capita).

Is this effect a result of the increased municipal employment in Sweden in 1991 (which was an election year in Sweden), or/and decreased employment in Finland in 1993 (which was not an election year in Finland) or is it really a pure election year effect? In order to investigate this, column ii) re-estimates the model allowing for a Sweden-specific time-effect for the years 1991–2002 and a Finland-specific time-effect for the years 1993–2002. Doing this we find that the size of the election year effect decreases to 1.6, but that it is still statistically significant. In column iii) we next allow for different underlying trends in the two countries. It turns out this does not alter our findings; the parameter estimates are almost identical in columns ii) and iii).

So far, we have assumed that  $\theta$  is the same for both countries. Given that the two countries do not supply exactly the same services (for example, in Finland local governments are responsible for health care, but not in Sweden) it might be the case that the demographic structure and other observable covariates affect employment differently in the two countries. We therefore finally estimate a flexible model where  $\theta$  is allowed to vary between the two countries, a specification that corresponds to equation (2). The result in column iv) shows that the estimated election effect decreases with 1 employee per 1,000 capita when allowing for country-specific parameters for the covariates. The effect is however still of economic as well as statistical significance; municipalities employ 0.6 more full-time employees per 1,000 capita in election years than in other years. This corresponds to an increase, on average, by approximately 1 percent. Since an F-test rejects the null of equal parameter estimates for the two countries, we consider the model in column iv) as our preferred specification.

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<sup>17</sup> Full results from the estimations in this section as well as from the results in the following sections are given in the Appendix.

**Table 2** Baseline results

	i)	ii)	iii)	iv)
Election year effect	1.929*** (10.69)	1.562*** (7.38)	1.592*** (7.33)	0.577*** (2.74)
Other observable covariates	Yes	Yes	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes
Sweden_91-dummy	No	Yes	Yes	Yes
Finland_93- dummy	No	Yes	Yes	Yes
Country-specific time trends	No	No	Yes	Yes
Country-specific parameters for covariates	No	No	No	Yes
Number of observations	12,284	12,284	12,284	12,284
Number of municipalities	687	687	687	687
R-squared	0.65	0.65	0.65	0.66

**Notes:** Robust standard errors within parentheses. \*\*\* denotes significance at the 1 percent level, \*\* at the 5 percent level, and \* at the 10 percent level. Other observable covariates include intergovernmental grants, tax base, wage for municipal employees, population, share of population 0–15, share of population older than 64. In order to control for the decentralization of schooling (1991) and elderly care (1992) in Sweden, the parameters for share of population 0–15, share of population older than 64 are allowed to differ for Sweden after the decentralization.

## 6.2 Are the residuals correlated?

So far we have adjusted the standard errors to be heteroscedasticity consistent. We have however assumed that there is no correlation across observations, neither in time nor in space. As discussed in section 5.2, there might however be good reasons to suspect that the residuals are correlated, either over time for a single municipality or over municipalities in a given year (or both). If there is some correlation in the standard errors, not taking this into account in the estimations implies that the standard errors estimated in the baseline model might be underestimated (leading to wrong conclusions regarding statistical significance of the parameters).

In order to learn whether we need to worry about correlations in the residuals we estimate our preferred model specification:

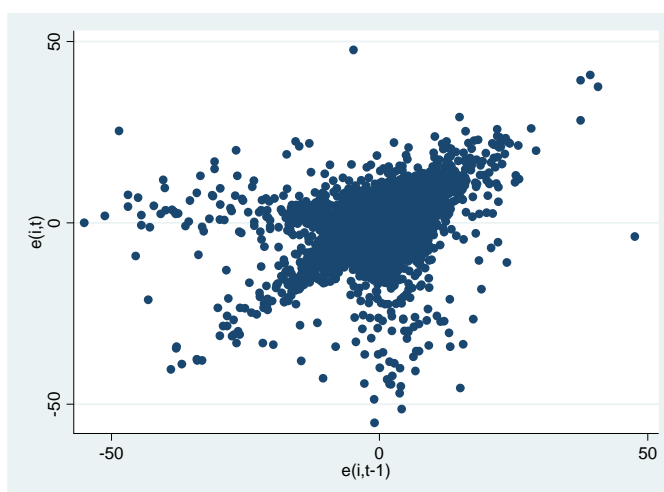
$$y_{itc} = \phi ELECTION_{itc} + \theta'_c x_{itc} + f_{itc} + \tau_t + trend_c + SWE91 + FIN\_93 + \varepsilon_{itc},$$

which corresponds to the model in equation (2). Then we calculate the estimated residuals ( $\hat{\varepsilon}_{itc}$ ) and check graphically if there seems to be any correlation over time and/or space.

### 6.2.1 Correlation over time

First, in order to investigate whether the residuals are correlated within municipalities over time we look at the correlation between the estimated current residual for the municipality ( $\hat{\varepsilon}_{itc}$ ) and the estimated residual lagged one year ( $\hat{\varepsilon}_{i,t-1,c}$ ). From the correlation, shown in *Figure 3*, we observe a positive relationship over time; municipalities that had large residuals yesterday also seem to have large residuals today.<sup>18</sup> As noted earlier, this is in line with earlier Swedish evidence on municipal behavior (see, e.g., Dahlberg & Johansson, 2000, and Bergström *et al*, 2004). A straightforward way to take this into account in the estimations is to cluster the standard errors on cross-sectional unit (i.e. on municipalities).

**Figure 3** Are the residuals correlated over time within municipalities?



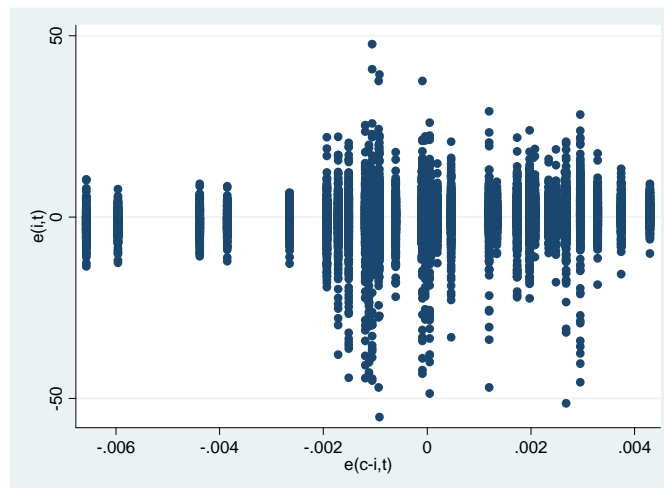
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<sup>18</sup> The correlation between the two variables is 0.46 (calculating the correlations by country, we get a correlation of 0.71 for Sweden and 0.41 for Finland).

### 6.2.2 Correlation over municipalities

Second, in order to investigate whether the residuals in one country are correlated over municipalities we look at the correlation for the estimated residual for each municipality ( $\hat{\varepsilon}_{itc}$ ) and the average residual of all other municipalities in that country in the same year ( $\bar{\hat{\varepsilon}}_{c-i,t,c}$ ). From the correlation, shown in *Figure 4*, it is difficult to decipher any pattern in the relationship over municipalities in a given year; it does not seem to be the case that the residual in one specific municipality is correlated with all other municipalities in the same country.<sup>19</sup> This indicates that we need not worry that the residuals within one country are correlated over municipalities (i.e., we need not cluster on year).

**Figure 4** Are the residuals correlated over municipalities within time?



To sum up the graphical analysis in this section, it seems like clustering the standard errors on cross-sectional unit will be enough to ensure that the

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<sup>19</sup> The correlation between the two variables is 0.13 (calculating the correlations by country, we get a correlation of 0.25 for Sweden and 0.09 for Finland). These correlations are considerably lower than the correlations over time.

standard errors are not underestimated. Next we will therefore re-estimate our preferred specification from *Table 2* but with the standard errors clustered on cross-sectional unit.

### 6.3 Results when clustering on cross-sectional unit

*Table 3* shows the estimated election year effect in the specification given in equation (2) (i.e., with country specific time trends, country specific parameter estimates for the covariates as well as a Sweden-specific time-effect for the years 1991–2002 and a Finland-specific time-effect for the years 1993–2002), and with the standard errors clustered on cross-sectional unit.<sup>20</sup> As is clear from the table, the election year effect is still statistically significant, but now at a 2.5 percent level instead of a one percent level. The economic significance of the estimate is of course the same as above; municipalities' employment is 1 percent higher in election years than in other years.

**Table 3** Results with clustered standard errors

Election year effect	0.577** (2.24)
Other observable covariates	Yes
Municipality fixed effects	Yes
Time fixed effects	Yes
Sweden_91-dummy	Yes
Finland_93- dummy	Yes
Country-specific time trends	Yes
Country-specific parameters for covariates	Yes
Number of observations	12,284
Number of municipalities	687
R-squared	0.66

**Notes:** Robust standard errors within parentheses. \*\*\* denotes significance at the 1 percent level, \*\* at the 5 percent level, and \* at the 10 percent level. Other observable covariates include intergovernmental grants, tax base, wage for municipal employees, population, share of population 0–15, share of population older than 64. In order to control for the decentralization of schooling (1991) and elderly care (1992) in Sweden,

<sup>20</sup> Technically, this is conducted by using the cluster-command in STATA.

the parameters for share of population 0–15, share of population older than 64 are allowed to differ for Sweden after the decentralization.

#### 6.4 Alternative outcome-variables

So far, we have focused on the number of employees in the municipal sector. The reason for this is that we believe that number of employees is highly visible for the voters and has a direct impact on the quality of the services provided by the municipalities. The earlier work cited in the introduction has typically not investigated the effects on municipal employment (the exception is Coelho *et al*, 2006); instead, the earlier work has focused on total spending (see, e.g., Andrikopoulos *et al*, 2004, Blais & Nadeau, 1992, and Pettersson-Lidbom, 2003), spending on different categories (see, e.g., Andrikopoulos *et al*, 2004, Blais & Nadeau, 1992, and Kneebone & McKenzie, 2001), tax rates (see, e.g., Andrikopoulos *et al*, 2004, Kneebone & McKenzie, 2001, and Pettersson-Lidbom, 2003), and deficits (see, e.g., Brender and Drazen, 2005, Galli & Rossi, 2002, and Shi & Svensson, 2002 and 2006). These studies have typically found election cycles in those outcome-variables. It is therefore interesting to investigate whether we reach the same conclusion when separating time effects from election year effects.

*Table 4* gives the results for local tax rates and local total consumption.<sup>21</sup> We find statistically significant election effects in both taxes and consumption. Starting with the local tax rates, we find an election effect of -0.05, i.e. tax rates are 0.05 percentage points smaller in election years than in other years. This corresponds to 0.25 percent lower taxes (the average tax rate is 18 percent). Looking at local consumption, the corresponding election effect is an increase with 68 euros per capita, which corresponds to 2.3 percent of the average value (average consumption is 2,900 euros per capita).

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<sup>21</sup> The tax rate and consumption variables are quite similar in the two countries in terms of descriptive statistics. While the local tax rate in Sweden has an average of 18.81 (with a standard deviation of 2.39), the corresponding figures for Finland are 17.66 (0.86). The corresponding figures for the consumption variable, which is only available for the years 1985–2001, is, for Sweden, 2,712 (704), and, for Finland 3,062 (833). All variables are expressed in Euros. It would be interesting to also investigate whether there are election year effects in deficits and different types of spending. However, we do not have data on these outcome measures for the Finnish municipalities.

**Table 4** Results for local tax rates and local consumption

	Local tax rate	Local total consumption
Election year effect	-0.046***	68.058***
	(2.67)	(7.53)
Other observable covariates	Yes	Yes
Municipality fixed effects	Yes	Yes
Time fixed effects	Yes	Yes
Sweden 91_02-dummy	Yes	Yes
Finland 93_02-dummy	Yes	Yes
Country-specific time trends	Yes	Yes
Country-specific parameters for covariates	Yes	Yes
Number of observations	12,284	12,001
Number of municipalities	687	687
R-squared	0.92	0.91

**Notes:** Robust standard errors clustered on cross-sectional units within parentheses. \*\*\* denotes significance at the 1 percent level, \*\* at the 5 percent level, and \* at the 10 percent level. Local total consumption is only available for the period 1995–2001. Other observable covariates include intergovernmental grants, tax base, wage for municipal employees, population, share of population 0–15, share of population older than 64. In order to control for the decentralization of schooling (1991) and elderly care (1992) in Sweden, the parameters for share of population 0–15, share of population older than 64 are allowed to differ for Sweden after the decentralization. Also, the definition of consumption changes slightly between 1995 and 1996 in Sweden. The model for local consumption therefore also include a Sweden specific dummy, taking the value 1 for years 1996–2001 in Sweden, and zero otherwise.

## 7 Conclusions

In this paper, we have examined whether local governments increase public employment in election years. By using data from Sweden and Finland, two countries that are similar in many respects when it comes to municipal decision making but in which local elections are held at different points in time, we can separately identify an election year effect from other time effects. This is a methodological problem that has not been dealt with in the earlier literature investigating election cycles in public policy.

Our results indicate that there is an election year effect in local public employment; municipalities employ 0.6 more full-time employees per 1,000

capita in election years than in other years. This corresponds to an increase by approximately 1 percent. The results also show that there are election year effects in both local tax rates and local consumption; tax rates are 0.25 percent lower and consumption is 2.3 percent higher in election years. Our results thus contrasts with the results in Brender & Drazen (2004) and Shi & Svensson (2006) who do not get any election effects for their sub-samples of developed countries (in which Sweden and Finland are included). The results are however in line with Pettersson-Lidbom (2003) who finds election year effects corresponding to a 0.6 percent decrease in tax rates and a 3 percent increase in spending.

The next question to ask is why there exists an election year effects. Having the literature on political business cycles in mind, we argue that the most likely explanation is that local governments increase employment in election years in order to increase their vote share and hence their re-election probability. By increasing the number of persons employed by the local government the local government accomplishes two things, where both are likely to be valued by the voters. First, since the services provided by local governments are labor intensive, the quality of the provided services increases. The amount of personnel at child care centers, schools and elderly care centers are highly visible among the users and most local voters are users of local services. Second, unemployment in the municipality will quite likely fall. Even though labor market policy is the responsibility of the central government, local governments have taken an increasing active part in tackling unemployment, and it is likely that voters are not perfectly aware of where the local responsibilities end and where the central responsibilities begin. Hence, there is a possibility that local politicians are rewarded for low local unemployment rates.

Given that voters might be unaware of the exact division of responsibilities between the central and the local government, one potential alternative explanation is that it's in fact the *central* government that is behind the increase in municipal employment. This is perhaps most likely for Sweden where local and central elections are held the same day. How would a central government go about if it wants to increase municipal employment? The best instrument that the central government has at its disposal is the use of intergovernmental grants. By conditioning grants on increases in municipal employment it is possible for the central government to affect the number of employees in the municipalities. In Sweden there have actually been several examples where the central government has tried to increase the personnel-density in municipal services. However, given that we in our estimations control for intergovern-



mental grants, these kinds of actions would not result in an election year effect. If some of the central government grants aimed at increasing local public employment in election years are not part of the intergovernmental grants that we control for in the econometric specification, this will be picked up by the election dummy. The interpretation is then that we have an election year effect, but that we cannot be sure whether it has been instigated by the local or by the central government. We do however think that there are few such grants that we do not control for. There are of course other, more direct ways, in which the central government may try to influence local governmental behavior (e.g. laws and prescriptions). We are however not aware of any such matters occurring. We are therefore inclined to believe that the election year effect we have found in this paper is a result of local governmental rather than central governmental behavior.

The next interesting question to investigate is whether increases in the number of municipal employees actually increase the vote share of the incumbent government. Although this is a highly relevant issue to investigate, it is not a trivial issue, since there are many other things that also affect individuals' voting decisions. We hope that future research will manage to find a way to answer this question in a credible way.

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## Appendix

**Table A.1. Baseline results: Municipal Employment**

	i)	ii)	iii)	iv)
Election year	1.929*** (10.69)	1.562*** (7.38)	1.592*** (7.33)	0.577*** (2.74)
Grants	0.005*** (12.95)	0.005*** (12.40)	0.005*** (11.18)	0.013*** (14.56)
Tax base	0.000*** (3.12)	0.000*** (3.79)	0.000*** (3.77)	-0.000 (0.91)
Wage	-0.026*** (8.97)	-0.026*** (8.43)	-0.026*** (8.44)	-0.015*** (5.39)
Population	-0.000*** (7.42)	-0.000*** (7.25)	-0.000*** (7.26)	-0.000*** (8.97)
Share 0–15	-21.217** (2.29)	-18.570** (1.99)	-18.243* (1.95)	-2.074 (0.18)
Share 65+	-51.826*** (6.97)	-50.401*** (6.77)	-49.181*** (6.40)	-1.693 (0.14)
Share 0–15 *Sweden 91–02	57.052*** (27.01)	-2.666 (0.25)	-1.351 (0.13)	-4.671 (0.47)
Share 65+ * Sweden 92–02	76.039*** (35.90)	64.675*** (19.91)	64.997*** (19.71)	38.636*** (10.65)
Grants * Finland				-0.008*** (8.67)
Wage * Finland				-0.012*** (3.10)
Tax base * Finland				0.001*** (3.87)
Population * Finland				0.001*** (8.35)
Share 0–15 * Finland				-8.560 (0.53)
Share 65+ * Finland				-41.695*** (2.82)
Sweden 91–02		13.281*** (5.74)	12.805*** (5.43)	12.405*** (5.61)
Finland 93–02		-1.152** (2.34)	-0.812 (1.30)	-6.414*** (8.90)
Trend Sweden			1.676*** (10.20)	1.581*** (11.59)
Trend Finland			1.628*** (10.00)	1.876*** (8.66)
Constant	75.131*** (21.44)	73.617*** (20.39)	71.819*** (20.48)	62.814*** (18.85)

	i)	ii)	iii)	iv)
Year effects	Yes	Yes	Yes	Yes
No of observations	12,284	12,284	12,284	12,284
No of municipalities	687	687	687	687
R-squared	0.65	0.65	0.65	0.66

**Notes:** Robust standard errors within parentheses. \*\*\* denotes significance at the 1 percent level, \*\* at the 5 percent level, and \* at the 10 percent level.

**Table A.2. Results: Other outcomes**

	Local tax rate	Local total consumption
Election year	-0.046*** (2.67)	90.689*** (9.98)
Grants	-0.001*** (5.71)	0.653*** (10.07)
Tax base	-0.000** (2.19)	0.082*** (6.84)
Wage	-0.003*** (9.62)	0.052 (0.38)
Population	0.000 (0.57)	-0.015*** (3.23)
Share 0–15	-4.000 (1.54)	-1,055.772 (1.03)
Share 65+	0.678 (0.29)	746.441 (0.71)
Share 0–15 * Sweden 91–02	10.335*** (6.68)	2,224.240*** (3.60)
Share 65+ * Sweden 92–02	10.511*** (26.47)	2,222.581*** (13.19)
Grants * Finland	0.001*** (7.25)	0.021 (0.31)
Wage * Finland	0.004*** (10.27)	-0.040 (0.27)
Tax base * Finland	0.000 (1.52)	-0.012 (0.70)
Population * Finland	0.000** (2.45)	0.009 (1.54)
Share 0–15 * Finland	2.623 (0.92)	1,532.816 (1.23)
Share 65+ * Finland	-2.918 (1.10)	2,521.152** (1.99)
Sweden 91–02	-1.812*** (5.72)	-561.213*** (4.42)

	Local tax rate	Local total consumption
Finland 93–02	0.132 (1.61)	-33.815 (0.85)
Sweden 95–02		-610.877*** (24.06)
Trend Sweden	0.397*** (26.16)	95.306*** (15.09)
Trend Finland	0.053*** (5.07)	85.566*** (15.65)
Constant	17.744*** (43.54)	755.233*** (3.88)
Year effects	Yes	Yes
No of observations	12,284	12,001
No of municipalities	687	687
R-squared	0.92	0.92

**Notes:** Robust standard errors clustered on cross-sectional units within parentheses. \*\*\* denotes significance at the 1 percent level, \*\* at the 5 percent level, and \* at the 10 percent level.



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