Document de treball de l'IEB 2011/8

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

Matz Dahlberg, Eva Mörk

ABSTRACT: Do governments increase public employment in election years? This paper answers 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. These facts make it possible for us to 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).

JEL Codes: D72, H72, P16

Keywords: election cycle, public employment, exogenous elections

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* This paper has benefited from comments from Per Pettersson-Lidbom, Patrik Hesselius, Jon Fiva, Gissur Erlingsson, one anonymous referee and from seminar participants at IFN, 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.

1 Introduction

Following the theoretical contributions of Nordhaus (1975), Lindbeck (1976), Rogoff and Sibert (1988), Rogoff (1990), Besley and Case (1995), Persson and Tabellini (2000), and Drazen and Eslava (2005), there is a large and growing empirical literature studying the effects of elections on public policy (see, e.g., Shi and Svensson, 2006, and Drazen and Eslava, 2010, for two recent studies and for references to earlier work). The present paper adds to the empirical political business cycle literature by investigating, on data from Finland and Sweden, whether there is an election cycle in local (municipal) employment.

From an economic point of view, the paper adds to the literature by looking for election effects in municipal employment. This is a variable that is highly visible for the voters, especially in welfare states of the Nordic type where the local governments are 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. 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 and Eslava, 2006, 2010).

Employment as an outcome variable is also interesting more generally since it is, from a policy perspective, important that we know its determinants. Taking Sweden as an example, approximately 25 percent of all employed are employed by the municipalities.[1] The implication is that to understand the determinants of overall employment, it is imperative that we understand the determinants of local government employment. From this perspective it is remarkably few studies that examine potential election effects in government employment.[2]

On the methodological side we aim at improving on the earlier literature in two dimensions. By using data from two countries that are very similar in many

[1] For a further discussion on this issue, see Section 2.

[2] The only study we know of is Coelho *et al.* (2006). For studies examining local government employment more generally, see Bergtsröm *et al*. (2004) and the references cited therein.

respects, and that have fixed election dates but elections in different years, we are in a good position to both deal with the endogeneity issue and to be able to separately identify time effects from election year effects. [3]

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.[4] 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 and Nadeau, 1992, and Pettersson-Lidbom, 2003) and tax rates (see, e.g., Andrikopoulos *et al.*, 2004, Kneebone and McKenzie, 2001, and Pettersson-Lidbom, 2003), we thought it could be interesting to use our empirical approach also on these outcomes. We find a significant election effect also in

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[3] The nature of the endogeneity problem when estimating potential election effects is discussed in Shi and Svensson (2006). The best approach so far in the literature is the one adopted by Akhmedov and 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 and Svensson (2006) and Brender and Drazen (2004). 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 predetermined.

[4] Both Shi and Svensson (2006) and Brender and 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 and Drazen). Brender and Drazen 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.

the local tax rate and in local total consumption. This is interesting since it contrasts with the results in Brender and Drazen (2004) and Shi and Svensson (2006) who do not get any election effects for their sub-samples of developed countries (in which Sweden and Finland are included).[5]

2 Institutional background

This paper builds on the assumption that Sweden and Finland are 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 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 (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 where the regions with lowest population densities are found in the northern part 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

[5] Brender and Drazen (2004) and Shi and Svensson (2006) separate between new and mature democracies and find that the election cycle is present in new democracies only.

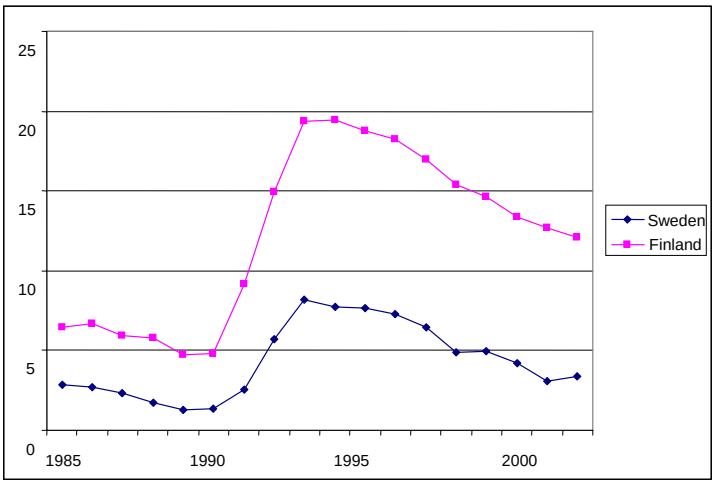
2002) and four in Finland (on the fourth Sunday in October 1988, 1992, 1996, and 2000).

The main source of municipal income comes from the local income tax (approximately 50–60 percent of municipal revenue).[6] 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 others), 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.[7]

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.

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

[6] In Finland the municipalities also raise property taxes whereas this is not the case in Sweden.
[7] 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).



Source: Statistics Sweden and VATT

3 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.[8] *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 population aged 1–15 and older than 64 respectively.[9] The reason for including these population shares is that many of the services supplied by the

[8] We also follow earlier studies on Finnish data and exclude 16 Finnish municipalities on the island Åland.

[9] Employment is expressed as employees per 1,000 capita, wages in euros per month, and tax base and intergovernmental grants in Euros per capita. The exchange rate used for Finland was 1 Euro = 5.94573 FIM for the period before the Euro was introduced in Finland, while the exchange rate for Sweden varied a bit over the period studied, from a minimum of 8.445858 SEK to a maximum of 9.35 SEK for 1 Euro.

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[10], which is somewhat higher in Sweden.[11] 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 2 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.

[10] 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.

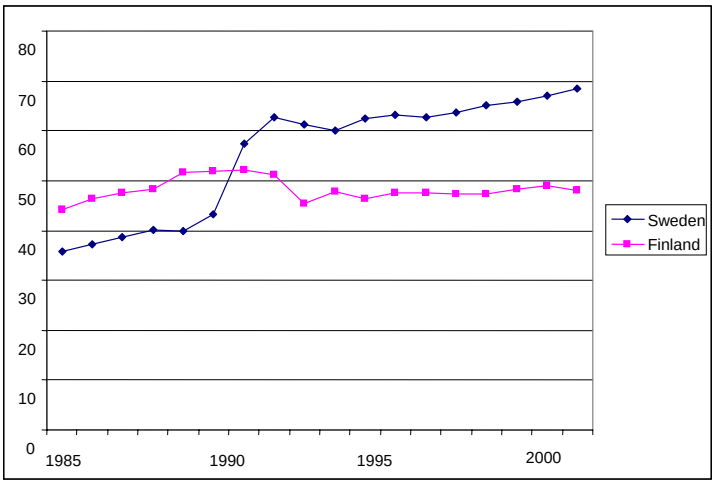
[11] One explanation for this might be that the Finnish data does not include part time employees whereas the Swedish data does.

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

4 Econometric model

When estimating the effects of elections on different policy variables one typically aims at estimating the following equation:

$$y_{jt} = \phi ELECTION_{jt} + \theta' x_{jt} + f_j + \tau_t + \varepsilon_{jt} \qquad (1)$$

where y_{jt} is the policy variable of interested in country j, in year t, $ELECTION_{jt}$ is a dummy variable taking on the value 1 if there is an election in country j in year t, zero otherwise, x_{jt} are observable characteristics of the country, f_j are unobserved, country-specific fixed effects, and τ_t are time-specific fixed effects. The major problem with estimating (1) using cross-country data is that the timing of the elections may be determined by other factors that are potentially affected by public policy, implying that the estimated election effect is quite likely to be biased. This is discussed in e.g. Shi and Svensson (2006). One solution to the endogeneity problem, that also have been used in empirical work analyzing election year effects, is 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 elec-

tions[12] or by using panel data on local government elections[13]. The problem with the first approach is that one gets rather few observations, which makes it hard to find statistically significant effects. The latter approach has a major advantage over national time series data in that it generates more observations on election periods. However, in most countries local elections are held on the same day across the country, implying that it is not possible to control for time-specific fixed effects, i.e. τ_t in equation (1). Hence, it is hard to separately identify an election effect from other time effects.

Our solution to this problem is to use data from two countries (Sweden and Finland) with fixed election dates, 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. We will hence estimate the following baseline model:

$$y_{itc} = \phi ELECTION_{tc} + \theta' x_{itc} + f_{ic} + \tau_t + \varepsilon_{itc} \qquad (2)$$

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, $ELECTION_{tc}$ is a dummy variable taking on the value 1 if there is an election in country c in year t, zero otherwise, x_{ict} is observable char-

[12] 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 and Poterba, 1980, Beck, 1987 and Alesina, 1989).

[13] This approach has e.g. been taken by Drazen and Eslava (2005), investigating Columbian local governments, Pettersson-Lidbom (2003), investigating Swedish municipalities, Galli and Rossi (2002), investigating Western German Länder, and Baleiras and da Silva Costa (2004), Coelho *et al.* (2006), and Veiga and Veiga (2007), investigating Portuguese local governments.

acteristics of the municipalities[14], f_i are unobserved, municipality-specific fixed effects, and τ_t are time-specific fixed effects.[15]

In order to examine how important it is to actually control for time effects we will however start by estimating the model in equation (1) for Sweden and Finland separately. In these estimations we are not able to control for time-fixed effects. Also, we will estimate the model in equation (2) without time-fixed effects.

5 Results

5.1 Estimating election year effects for Sweden and Finland separately

We start by treating Finland and Sweden separately and estimate the model given by equation (1) for each country, but ignoring time-effects. The resulting election year effects are given in column (i) in Table 2. We find a positive and significant effect for Finland but not for Sweden. Earlier work following this method has however not ignored time-effects completely but tried different ways to take them into account. The most straightforward way would be to include linear time trends, and this is done in column (ii). However, the parameter estimates are not much affected by doing this. Looking at Figure 2, we note that there are some things going on in municipal employment in the two countries that will not be taken into account by linear time trends.

[14] 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.

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

Therefore, 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 instead 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. These results, presented in column (iii), show that doing this we actually find negative election year effects both for Sweden and Finland, even thought the latter is statistically insignificant. Including the linear time trend in column (iv) does not change these findings.

Table 2 Election effects on municipal employment: results for Sweden and Finland separately

	i)	ii)	iii)	iv)
Sweden				
Election year	-0.0627	-0.116	-0.282***	-0.356***
	(0.0763)	(0.0763)	(0.0882)	(0.0827)
Finland				
Election year	0.678***	0.747***	-0.191	-0.0854
	(0.135)	(0.148)	(0.142)	(0.140)
Trend	No	Yes	No	Yes
Sweden_91/Finland_93	No	No	Yes	Yes

Notes: Robust standard errors clustered at the municipal level within parentheses. *** denotes significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level. The estimations control for intergovernmental grants, tax base, wage for municipal employees, population, share of population 0–15, share of population older than 64, as well as municipality specific fixed effects. 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.

5.2 Estimating election year effects controlling for time effects

As is clear from Table 2, we find rather puzzling and non-robust results when estimating election year effects for the two countries separately. We will now turn to our main analysis where we are able to separate election year effects from time effects. The results are given in Table 3.

As a baseline, column (i) presents the same model as column (i) in Table 2, but where we pool data from Sweden and Finland and let the election year effect be the same for the two countries, but still not take time effects into

account. Doing this, we find a positive election year effect of 0.48. In column (ii) we instead estimate the model from equation (2) including time year effects. 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).

The specification in equation (2) is then tested against some more flexible specifications. In column (iii) we include the 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. 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 iv) we instead allow for different linear time trends in municipal employment in the two countries. It turns out that this does not alter our findings; the parameter estimates are almost identical in columns ii) and iv). The same is true if we include time trends in the model in column iii), see column (v).

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 allow the parameter vector θ to vary between the two countries. 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 vi) as our preferred specification.

Table 3 Baseline results

	i)	ii)	iii)	iv)	v)	vi)
Election year	0.484***	1.929***	1.562***	1.885***	1.592***	0.577**
	(0.0878)	(0.189)	(0.259)	(0.193)	(0.259)	(0.257)
Other covariates	Yes	Yes	Yes	Yes	Yes	Yes
Munic. fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Time fixed effects	No	Yes	Yes	Yes	Yes	Yes

Sweden91/ Finland93	No	No	Yes	No	Yes	Yes
Country-specific trends	No	No	No	Yes	Yes	Yes
Country-specific θ	No	No	No	No	No	Yes
Number of obs.	12,284	12,284	12,284	12,284	12,284	12,284
Number of munic.	687	687	687	687	687	687
R-squared	0.59	0.65	0.65	0.65	0.65	0.66

Notes: Robust standard errors clustered at the municipal level 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.

So far we have adjusted the standard errors for correlation over time for a single municipality. Since the elections to the central government are held on the same day as the municipal elections in Sweden, we may worry that there also exists 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. In the working paper version of this paper (Dahlberg and Mörk, 2008) we investigate this more carefully by calculating the residuals from our preferred model specification and then plot them to check graphically if there seems to be any correlation over time and/or space. When doing this we observe a positive relationship over time; municipalities that had large residuals yesterday also seem to have large residuals today, but that 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. Hence, our conclusion is that clustering the standard errors on cross-sectional unit will be enough to ensure that the standard errors are not underestimated.

5.3 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 and Nadeau, 1992, and Pettersson-Lidbom, 2003), spending on different categories (see, e.g., Andrikopoulos *et al.*, 2004, Blais and Nadeau, 1992, and Kneebone and McKenzie, 2001), tax rates (see, e.g., Andrikopoulos *et al*,. 2004, Kneebone and McKenzie, 2001, and Pettersson-Lidbom, 2003), and deficits (see, e.g., Brender and Drazen, 2004, Galli and Rossi, 2002, and Shi and Svensson, 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 gives the results for local tax rates and local total consumption.[16] 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).

Table 4 Results for local tax rates and local consumption

	Local tax rate	Local total consumption
Election year effect	-0.046***	68.06***
	(2.67)	(7.53)
Other observable covariates	Yes	Yes
Municipality fixed effects	Yes	Yes
Time fixed effects	Yes	Yes

[16] 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.

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.

6 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.

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 and Drazen (2004) and Shi and 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 effect. 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 intergovernmental 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 incum-

bent 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***	1.562***	1.592***	0.577***
	(10.69)	(7.38)	(7.33)	(2.74)
Grants	0.005***	0.005***	0.005***	0.013***
	(12.95)	(12.40)	(11.18)	(14.56)
Tax base	0.000***	0.000***	0.000***	-0.000
	(3.12)	(3.79)	(3.77)	(0.91)
Wage	-0.026***	-0.026***	-0.026***	-0.015***
	(8.97)	(8.43)	(8.44)	(5.39)
Population	-0.000***	-0.000***	-0.000***	-0.000***
	(7.42)	(7.25)	(7.26)	(8.97)
Share 0–15	-21.217**	-18.570**	-18.243*	-2.074
	(2.29)	(1.99)	(1.95)	(0.18)
Share 65+	-51.826***	-50.401***	-49.181***	-1.693
	(6.97)	(6.77)	(6.40)	(0.14)
Share 0–15 *Sweden 91–02	57.052***	-2.666	-1.351	-4.671
	(27.01)	(0.25)	(0.13)	(0.47)
Share 65+ * Sweden 92–02	76.039***	64.675***	64.997***	38.636***
	(35.90)	(19.91)	(19.71)	(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***	12.805***	12.405***
		(5.74)	(5.43)	(5.61)
Finland 93–02		-1.152**	-0.812	-6.414***
		(2.34)	(1.30)	(8.90)
Trend Sweden			1.676***	1.581***
			(10.20)	(11.59)
Trend Finland			1.628***	1.876***
			(10.00)	(8.66)
Constant	75.131***	73.617***	71.819***	62.814***
	(21.44)	(20.39)	(20.48)	(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***	90.689***
	(2.67)	(9.98)
Grants	-0.001***	0.653***
	(5.71)	(10.07)
Tax base	-0.000**	0.082***
	(2.19)	(6.84)
Wage	-0.003***	0.052
	(9.62)	(0.38)
Population	0.000	-0.015***
	(0.57)	(3.23)
Share 0–15	-4.000	-1,055.772
	(1.54)	(1.03)
Share 65+	0.678	746.441
	(0.29)	(0.71)
Share 0–15 * Sweden 91–02	10.335***	2,224.240***
	(6.68)	(3.60)
Share 65+ * Sweden 92–02	10.511***	2,222.581***
	(26.47)	(13.19)
Grants * Finland	0.001***	0.021
	(7.25)	(0.31)
Wage * Finland	0.004***	-0.040
	(10.27)	(0.27)
Tax base * Finland	0.000	-0.012
	(1.52)	(0.70)
Population * Finland	0.000**	0.009
	(2.45)	(1.54)
Share 0–15 * Finland	2.623	1,532.816
	(0.92)	(1.23)
Share 65+ * Finland	-2.918	2,521.152**
	(1.10)	(1.99)
Sweden 91–02	-1.812***	-561.213***
	(5.72)	(4.42)

	Local tax rate	Local total consumption
Finland 93–02	0.132	-33.815
	(1.61)	(0.85)
Sweden 95–02		-610.877***
		(24.06)
Trend Sweden	0.397***	95.306***
	(26.16)	(15.09)
Trend Finland	0.053***	85.566***
	(5.07)	(15.65)
Constant	17.744***	755.233***
	(43.54)	(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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