

Chapter 4

A Test of the Rational Electoral-Cycle

Hypothesis*

1. Introduction

The electoral-cycle model focuses on the general idea of the incumbent government trying to manipulate fiscal policies before periodic elections to enhance its prospects for re-election.¹ Despite ample anecdotal evidence of political opportunism, little systematic evidence supports the electoral-cycle hypothesis. For example, Alt and Crystal (1983) conclude, “No one could read the political business literature without being struck by the lack of supporting evidence.” Although this book was written more than fifteen years ago, there are still very few supporting studies. More recently, Alesina, Roubini, and Cohen (1997) have conducted a multitude of tests of the electoral-cycle hypothesis with regard to both policy instruments (fiscal or monetary policy) and policy outcomes (inflation, output growth, and unemployment). They only find an electoral cycle in the fiscal deficit (but not its individual components: taxes or spending) for a panel of OECD countries; no cycle is found for a U.S. sample. Bizer and Durlauf (1990) (US data), Blais and Nadeau (1992) and Reid (1998) (data from Canadian Provisional governments), and Shi and Svensson (2000) (data from 123 developed and developing countries) are the only studies claiming to have found support for the rational budget cycle model.

In this paper, I will take a fresh look at the empirical relevance of the electoral-cycle hypothesis. To achieve this, I have constructed a new data set from Swedish local

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¹ In the literature, the electoral cycle model is also called the rational opportunistic or the budget cycle model.

governments. The empirical analysis can be viewed as a direct test of the rational budget-cycle model originally developed by Rogoff and Sibert (1988) and Rogoff (1990).

This new panel data set gives me several advantages to previous studies. First, I have nearly 6000 observations (N=274, T=21), about 2000 of which correspond to election periods. Second, elections are held at fixed intervals, thereby avoiding the problem of separating the additional incentive of opportunistic election timing from the electoral-cycle.² Third, I avoid many of the difficulties with cross-country comparisons since Swedish local governments operate under the same constitutional and institutional setting.

There have not been many tests of the rational electoral-cycle model for fiscal policy, let alone for the more specific predictions.³ According to this model, elections serve the purpose of selecting the most competent policymaker. Voters do not directly observe the competence of politicians, but they do know that competent policymakers are more likely to successfully manipulate spending or taxes than incompetent ones. In equilibrium, voters reward those incumbents who manage to increase spending or decrease taxes enough in election years since they increase the probability of having a more competent government tomorrow. If this theory is correct, re-elected incumbents should be associated with a more pronounced fiscal policy cycle than ousted incumbents.

The empirical findings of this paper are consistent with the rational opportunistic model. The whole sample displays electoral cycles for both spending and taxes. On average, spending is 1.5 percent higher and taxes 0.4 percent lower in election years than in off-election years. However, no cycle can be detected in spending or taxes when the incumbent has been ousted from office.

My analysis is also related to the empirical work on principal agent models by Besley and Case (1995a,b). Their works share the common assumptions of imperfect information about types of policymakers and a re-election mechanism that could affect

² Ensuring that the timing of elections is exogenous to policy choices is difficult. In particular, this is the case for cross-country studies, since in the years 1961 to 1988 in flexible-term OECD nations 44 percent of the elections were held at least one year before the current government's term was due to expire. Using data from developing countries, such as in Shi and Svensson (2000), might also be problematic, since many of these countries are nascent democracies where calling an election is a political decision.

³ To the best of my knowledge, only Bizer and Durlauf (1990) test the more specific prediction. Using US federal data, they find that taxes are reduced two years prior to successful presidential re-election bids, but otherwise not.

policy choices. However, Besley and Case focus on different issues: either on term limits as an obstacle to reputation building (1995a),⁴ or relative performance evaluation in voting decisions (1995b).

The remainder of the paper is organized as follows. The next section presents a simple rational election cycle model that will guide the empirical analysis. Section 3 discusses the identification strategy of the empirical analysis and the data to which it is applied. Section 4 presents the results and some tests of their robustness. Finally, section 5 concludes.

2. A basic model

In this section, I will sketch a basic rational electoral-cycle model that will guide the empirical analysis. The specific model is based on Persson and Tabellini (2000), but the general idea that the role of elections is to select the most competent politician is due to Rogoff and Sibert (1988) and Rogoff (1990). The main difference between the two models is the information assumption. Persson and Tabellini's formulation, which builds on Holmström's (1982) career-concern model, has no asymmetric information between voters and policymakers. This has the advantage of making the analysis easier, without changing the main implications of its predecessors.

The model has an infinite horizon. Elections are held every other period. The policy instrument is government spending; taxes $\underline{\tau}$ are fixed and there is no debt. To begin with, the government budget must be balanced in each period, t . We write the government budget constraint as

$$g_t = \eta_t(\underline{\tau}y - r_t), \quad (1)$$

where g_t is government spending, y is average income and r_t represents "rents" which benefit politicians but not the general citizen.⁵ We consider these rents as party finance or outright diversion of resources for private use in connection with the production of public goods. The parameter η_t reflects the competence of the government in providing public

⁴ Besley and Case (1995a) also find evidence of an electoral cycle. Their results indicate that spending is increased when an incumbent cannot stand for reelection. This finding is at odds with the results of this paper. To reconcile their findings with a political agency model it must be assumed that voters has a low marginal utility of government spending.

goods, with a higher value of η_t corresponding to a more competent policymaker. We assume that η_t is a first-order moving average process

$$\eta_t = \mu_t + \mu_{t-1}.$$

We let μ be distributed as a uniform distribution, i.e.,

$$F(m) = [m - (1-2/\varepsilon)]\varepsilon,$$

with the expected value 1 and density ε . If the policymaker is removed from office, the winning opponent is drawn from the same distribution.

The preferences of voters in period t are:

$$u_t = y(1-\underline{t}) + \alpha g_t, \quad (2)$$

where $\alpha \geq 1$ is an exogenous parameter. Since we assume taxes to be fixed, voters only care about having the highest possible quantity of public consumption in each period. The utility function of the incumbent policymaker in an election period t is:

$$v_t = r_t + P_t \delta (R + r_{t+1}), \quad (3)$$

where r_t is rents grabbed in period t , P_t is the perceived probability that the incumbent is re-elected, δ is the discount rate, and R is the exogenous gain from winning the election.

The timing of events is the following: (i) An incumbent policymaker is in office in period t and chooses rent for that period, r_t . He does not yet know his competence, η_t . (ii) The value of his competence is revealed and the public good is residually determined, so as to satisfy (1). Voters observe their own utility but neither the policymaker's competence, η_t , nor rents, r_t . (iii) Elections are held. If the incumbent wins, his competence is still η_t , otherwise an opponent is appointed whose competence is drawn at random from the same distribution. (iv) Period $t+1$ rents r_{t+1} are set, and public spending is once again residually determined from (1)

Under these assumptions, the policymaker has no incentive to behave well in off-election years. Thus, rents are maximal $r_{t+1} = \underline{t}$ and public spending is relatively low, $g_{t+1} = \eta_t(\underline{y} - \underline{t})$. Voters are clearly better off with a more competent policymaker (high η_t) since utility in period $t+1$ will be higher. In election years, however, there is an incentive for the policymaker to improve his performance because voters will only reappoint the

⁵ We make the following assumption $r_t \leq \underline{t} \leq \underline{y}$. This assumption serves the purpose of giving the voters a motive to maintain competent incumbents in office.

incumbent if his estimated competence is higher than the opponent's expected competence. In other words, the politician will maximize his utility (3), subject to the constraint of optimal voting behavior. More formally, via the budget constraint (1), voters form an estimate of the incumbent's competence $g_t / (\underline{y} - x_t)$ where x_t denotes the solution to the politician's optimization problem. If the expected estimate is above one, the politician is reelected. Equivalently, we can express this in terms of the perceived probability of winning the election from the incumbent's point of view as

$$1 - \xi(\underline{y} - x_t) / (\underline{y} - r_t) - (0.5 - \xi).^6$$

Thus, we can now solve the incumbent's maximization problem under this reelection constraint, which gives us the equilibrium rents in election years:

$$r_t = \underline{y} - \xi\beta(R + \underline{r}) \leq r_{t+1}.^7$$

Hence, in election years, each incumbent perceives a trade-off between rents and the probability of winning because voters only reappoint incumbents if they deliver sufficiently high welfare in the period preceding the election. Voters use this retrospective voting strategy because it will raise the probability of having a more competent government tomorrow. In other words, incumbents who can please voters are re-appointed, while incompetent ones are ousted from office.

This is, of course, a very simplified model. For example, taxes are exogenously fixed and there is a strong assumption that the politician does not know his own competence when setting his policy in period t . A more elaborate model, such as in Rogoff (1990), would relax those assumptions. With taxes as an additional policy instrument, these would also entail information about the politician's competence. Lower taxes would be associated with more competent politicians. With asymmetric information about types of policymakers, policy could be used as a deliberate signal of competence with competent policymakers being more likely to engineer a political budget cycle than incompetent ones.⁸ These extensions would, however, not change the main implications from the simplified model.

⁶ The derivation uses the budget constraint (1) and the fact that μ is uniform.

⁷ Here we have used fact that the incumbent's optimal choice must be consistent with the voters' conjectures about these choices in equilibrium, i.e., $x_t = r_t$

⁸ This is the case if there are only two types of policymakers, such as in Rogoff (1990). However, with a continuum of types, everyone would induce a cycle (Sibert and Rogoff 1988). This prediction also relies on

To summarize, the main implications from the rational electoral cycle model are that spending (taxes) would, on average, be higher (lower) in election years than in off-election years. Moreover, competent policymakers, i.e., those able enough to induce a budget cycle, are re-elected while those who fail are ousted.

3. Empirical identification strategy and data

3.1 Identification strategy

In this section, I present the empirical identification strategy for the test of the rational electoral-cycle hypothesis and the data to which this test is applied. According to the model in the previous section, we should expect spending to be higher in election years than in off-election years, and the reverse to apply for taxes. Thus, we can test this model by including an indicator variable that takes the value of one in election years and zero otherwise. Hence, we could estimate an equation of the form

$$p_{it} = \alpha + \lambda E_{it} + x_{it}\beta + \varepsilon_{it}, \quad i = 1, \dots, N; \quad t = 1, \dots, T, \quad (4)$$

where p_{it} is the policy instrument, i.e., spending or taxes, E_{it} is the indicator variable, x_{it} is a vector of other variables that might be considered to affect the particular policy instrument. Finally, ε_{it} is an error term yet to be specified. Since we have panel data, equation (4) is indexed with i and t , where i denotes local governments and t time. The main coefficient of interest from the point of view of theory is λ . The prediction from theory is that λ should be positive for spending and negative for taxes. Moreover, re-elected incumbents should, on average, have larger budget cycles than incumbents ousted from office. Thus, the λ coefficient should be larger (in absolute value) for governments having had a successful re-election bid than for those that were ousted from office.

In the empirical identification strategy, I will assume that the error term ε consists of a fixed municipality effect μ_i and a remainder disturbance v_{it} :

$$\varepsilon_{it} = \mu_i + v_{it}, \quad v_{it} \sim \text{IID}(0, \sigma_v^2). \quad (5)$$

By introducing fixed effects, I will primarily identify a prospective election cycle from the time-series variation in the data. More specifically, I am only using the within-municipality variation over time to identify the election parameter. Including time-

the existence of a separating equilibrium. In a pooling equilibrium, however, all types of policymakers

specific effects in the error term would not be informative, since Swedish local governments had a synchronized fixed election date every third year throughout the sample period.⁹ Nevertheless, I will still try to control for common shocks to the macroeconomy by including the percentage change in real GDP in regression (4).

I will also include other explanatory variables: lagged policy instruments, population size, the proportion of young (0-15), the proportion of elderly (65+), population density, central governmental grants and average municipality income.¹⁰

Lagged policy instruments are included because there are good reasons for believing there to be inertia in fiscal policy outcomes.¹¹ For example, several theoretical papers show that the options available to a newly elected government may be restricted because of the actions taken by the previous incumbent.¹² In Chapter 3, I find that the level of debt is used strategically by an incumbent government not likely to be re-elected, in order to affect the policies of its successor. Other reasons for inertia could be regulations imposed on the sub-national government by the central government, or incremental routines of budget making (e.g., see Wildavsky 1974).

Proportions of young and elderly are linked to the cost and benefits of government spending. These variables can also be seen as controlling for the mandatory part of municipal spending since education, childcare, and care of the elderly are mainly mandatory tasks.¹³ Population density and population size are included because they capture the possibility of congestion effects or scale economies in the provision of local

induce the same cycle.

⁹ The time-specific effects would be perfectly co-linear with the election year indicator.

¹⁰ There is still no consensus concerning the process that generates government fiscal decisions. For example, see Inman (1988) for a survey of various models of government expenditure determination.

¹¹ Inclusion of a lagged dependent in a panel data context creates some estimation problems. By now, there is a sizeable literature on different estimation techniques (e.g. see Baltagi (1995) and the references cited therein). For example, Judson and Owen (1999), using a Monte Carlo approach, compare the bias of different dynamic panel data estimators. Their conclusion is to use a GMM or Anderson-Hsiao estimator for large T panels, since the bias of the FE-estimator could be sizeable even when T=20. However, this bias concerns the parameter of the lagged dependent variable and it is not clear-cut from their study which of the compared estimators performs best concerning the bias of the parameters of the other regressors. Since the main interest of this paper is the electoral-cycle and not the lagged dependent variable per se, it is not obvious which estimator to use. However, I have also used the Anderson-Hsiao estimator with the level of dependent variable $p_{i,t-2}$ as an instrument, and all results from this estimator are very similar to the results presented in this paper.

¹² See Persson and Tabellini (1999) and the references cited therein.

¹³ A local government's freedom of action of running a mandatory operation depends on the constraints imposed by legislation, which may vary from one field to another.

government services. I also include intergovernmental grants. However, this variable is probably not exogenous with respect to fiscal decisions, since most intergovernmental grants in the sample period were matching grants.¹⁴ Nevertheless, certain parts of the governmental grants are block grants or grant-in aid and the estimate of the electoral-cycle, λ , could possibly be biased by not being included as an explanatory variable. Finally, I control for average municipality income.¹⁵ One reason is that income is related to the fiscal capacity of a municipality, as the bulk of revenues comes from a proportional local income tax and thus measures the ability to raise tax revenues. Income could also be seen as a control for local business cycle variations. Table 1 presents summary statistics for the explanatory variables.

3.2 Data

My objective is to test the rational electoral-cycle hypothesis by using data from Swedish local governments. One advantage of this data set is that there is a fixed election every third year, which avoids the endogeneity bias when incumbent governments have the discretion to call early elections. The full sample consists of 274 municipalities in 1974-1994. In this period there have been seven elections: 1976, 1979, 1982, 1985, 1988, 1991 and 1994. Thus, there is a total of 5754 (274×21) observations from local governments, 1918 (274×7) of which correspond to election periods. In this period, 1329 governments were re-elected and 301 ousted from office.¹⁶

I use total expenditures and the personal income tax rate as dependent variables. Expenditures are expressed in terms of per capita and in 1991 years price and the tax rate is expressed in percent.¹⁷ As a backdrop to the investigation, Table 2 presents summary statistics for the spending and the income tax rate in the sample period. These statistics provide a condensed history of municipality budgets. To facilitate the interpretation of

¹⁴ About 80 percent of the total grants were matching grants while 20 percent were grant-in-aid. Even the grant-in-aid program was determined by the fiscal behavior of the municipalities. For a description of the Swedish grant-in-aid system see Aronsson and Wikström (1996).

¹⁵ Due to centralization of tax collection, the tax receipts to the local governments in year t are based on the taxable personal income in year $t-2$. In the empirical analysis, I have tried to deal with this feature by including both the average municipality income in year t and $t-2$ as regressors.

¹⁶ I am obliged to exclude 288 observations due to the fact that these cannot be classified as having a clear defined majority, i.e., either a left- or right-wing government.

¹⁷ I have used the implicit GDP deflator. The deflator is constructed by taking the ratio of GDP at current market prices to GDP at fixed market prices.

Table 2, the mean of spending and taxes, 1 standard deviation bound, minimum and maximum are plotted in Figure 1 and 2.

A more and less steady upward trend in expenditure per capita and tax rates can be seen from Figures 1 and 2.¹⁸ Real spending has increased by a factor of 1.8, while tax rates have increased by a factor of 1.4. The standard deviation for spending starts at 18 percent of the mean and declines to roughly 14 percent. However, the standard deviation for taxes is roughly 7 percent of the mean during the whole period. The maximum spending is typically more than twice the minimum, while the maximum tax rate is a factor 1.8 larger than the minimum.

4. Results

4.1 Basic results

In this section, I present the basic empirical results from the tests of the rational electoral-cycle model. First, I test the principal prediction from theory, namely that there is an electoral cycle in both spending and taxes. Then, I test the more specific prediction of the election cycle being larger for reelected governments than for those ousted from office.

Table 3 shows the effect of election timing on spending and taxes, using the total sample. From this table, we can see that there is a highly statistically significant and non-negligible electoral cycle in both spending and taxes. Spending increases by 418 SEK per capita (1.5 percent of mean spending) in election years compared to off-election years. Similarly, taxes are decreased by 0.07 percentage points (0.4 percent of mean taxes). Thus, these results support the main prediction from the electoral budget cycle model.

Table 3 also reveals that the spending regression accounts for 85 percent and the tax regression for 95 percent of the variation in policy. Moreover, the lagged dependent variable, grants and average income all have positive effects on spending and taxes, while population density and the aggregate growth of the Swedish economy all have negative effects on spending and taxes. Population size has a negative effect on spending, but a

¹⁸ Some peculiarities in these trends need to be explained. In 1983, Statistics Sweden changed its definition of total spending. Before 1983, spending also included internal transactions. In 1992, there was a care of elderly reform, where the municipalities took over most of the responsibility for care of the elderly that had previously been handled by the county councils. This also entailed a switch in the tax rate. In the empirical

positive effect on taxes. Finally, the age structure seems not to matter for spending or taxes, except for the proportion of elderly in the tax regression.

Now, I turn to the evidence of the more conditional prediction from the theory, namely that re-elected governments should, on average, be associated with a larger politically induced budget cycle than those ousted from office. Columns 1 and 2 in Table 4 present the results on spending and taxes for the subsample of re-elected governments, while columns 3 and 4 show the results from a subsample of the ousted ones. As in Table 3, there is a significant and sizeable electoral-cycle in spending and taxes for the re-elected governments. In contrast, there is no electoral-cycle in spending for the ousted governments. Spending is 1.5 percent higher in election years for re-elected governments than for replaced ones. This finding supports the more specific prediction from the rational electoral-cycle model.

4.2 Extensions

In this section, I make two extensions. First, I investigate whether the accumulation of debt also displays an electoral cycle. The previous findings of higher spending and lower taxes in election years should, almost by definition, also have the implication of a positive association between the accumulation of debt and election periods. In fact, a more elaborate rational electoral-cycle model would also predict a positive association.¹⁹ Table 5 presents the results from the debt regression. There is a highly statistically significant electoral-cycle in debt. In election years, the accumulation of debt is 739 SEK per capita (7 percent of the mean) larger than in off-election years. A back of the envelope calculation shows that the magnitude is more or less consistent with the cycle in spending and taxes.

Second, I disaggregate the sample further to see whether different parties have different electoral-cycles. Even though the rational electoral-cycle model does distinguish between parties, there is some evidence of different parties pursuing different policies, once in office. In chapter 2, I show that left-wing parties in the government spend and tax

analysis, I have tried to deal with these trend shifts by including dummy variables. My results are, however, robust whether I include these dummies or not.

¹⁹ Shi and Svensson (2000) have constructed such a model.

more than right wing parties.²⁰ Moreover, Besley and Case (1995a), argue that different parties could exert different controls over their individual members, which could make the electoral-cycle conditional on the identity of the incumbent party. They find that only Democratic governors respond to binding term limits, which they interpret as incomplete party discipline on behalf of the Democratic Party.

Tables 6-8 present the results when I split the sample into left- or right-wing incumbents. Columns 1 and 2 in Table 6 constitute the subsample where either the left- or the right-wing government is re-elected, while columns 3 and 4 constitute the subsample where they are ousted from office. Table 6 reveals a high similarity between left- and right-wing incumbents. When they are re-elected, we observe a spending cycle, but when they are replaced from office, we observe no such cycle. Turning to tax regression, unlike the pooled regression in Table 4, Table 7 reveals that the electoral cycle in taxes is now also consistent with the rational electoral-cycle model. The cycle is statistically significant and of similar magnitude for both re-elected left- and right-wing incumbents. However, there is no significant cycle for ousted incumbents. Turning to the debt regression, Table 8 shows an electoral-cycle of roughly similar magnitude for both re-elected left- and right-wing incumbents. However, the behavior of the ousted incumbents differs sharply from each other. A right wing government, which is ousted from office, has a larger cycle than a re-elected right-wing government. In contrast, a left-wing government ousted from office has a smaller cycle than a re-elected left-wing government. This finding is consistent with the strategic debt model developed by Persson and Svensson (1989) and very similar to those in Chapter 3.²¹

In summary, it seems that both left- and right-wing governments induce an electoral-cycle in spending, taxes and debt when re-elected. However, when the incumbent government is ousted from office, there is no such cycle in spending or taxes for either type of government. These findings are consistent with the predictions from the rational

²⁰ There is nothing strange about the fact that both partisan and opportunistic motives can co-exist. For example, Drazen (2000) writes “ On the one hand, few office holders care simply about clinging to power with no concern about what policies are implemented. On the other hand, politicians must win elections in order to implement their preferred policies, so that even the most partisan policymaker will sometimes display opportunistic or office-motivated behavior.”

²¹ In Chapter 3, I use a different empirical identification strategy to test strategic debt behavior than the strategy used here.

electoral-cycle hypothesis. In addition, there is an electoral-cycle in public debt, consistent with strategic debt behavior.

5. Discussion

I have conducted a test of the rational budget cycle model drawing on a new panel data set from Swedish local governments with nearly 6000 observations ($N=274$, $T=21$), 2000 of which correspond to election periods. I find highly significant electoral cycles in both spending and taxes. On average, spending is 1.5 percent higher and taxes 0.4 percent lower in election years than in off-election years. Moreover, there is no cycle in spending or taxes when the incumbent has been ousted from office. These findings are consistent with models stressing elections as a means of selecting the most competent politician, such as Rogoff and Sibert (1988), Rogoff (1990) and Persson and Tabellini (2000).

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Table 1
Summary statistics

Variables	Mean	Standard d.	Min	Max
Proportion of young (0-15)	0.21	0.028	0.13	0.37
Proportion of elderly (65+)	0.17	0.045	0.016	0.41
Average income	73778	12488	15943	162960
Population size	27799	45523	3480	692954
Population density	107	360	0.28	3700
Grants	7572	2416	-276	24670
Growth of the Swedish Economy	1.50	1.76	-2.2	4

Notes: Average income and grants are expressed in terms of per capita and in 1991 prices.

Table 2
Municipality spending and tax rates

Year	Total expenditures				Taxes			
	Mean	Std d.	Min	Max	Mean	Std d.	Min	Max
1974	23042	4250	14391	41132	14.11	0.93	10.10	16.85
1975	23361	4110	14697	41780	14.57	1.01	10.10	17.00
1976	24586	4318	16951	42551	14.88	0.99	10.10	17.25
1977	26811	4376	17876	44114	15.12	1.01	10.60	17.50
1978	27416	4675	18030	45684	15.73	1.05	10.60	18.60
1979	28838	4809	19798	47560	15.86	1.09	10.60	18.60
1980	29240	5012	19481	47579	15.92	1.08	10.60	18.60
1981	29948	4864	20940	50418	16.06	1.09	10.60	18.60
1982	30610	5108	20917	51757	16.11	1.10	10.60	18.60
1983	25651	4032	18988	43366	16.21	1.09	10.60	19.60
1984	25779	4091	18314	41254	16.25	1.10	10.40	19.60
1985	26464	4444	19236	42720	16.27	1.12	9.70	19.60
1986	26967	4212	19441	40712	16.23	1.06	11.30	18.00
1987	27550	4205	20297	45602	16.26	1.09	10.90	18.00
1988	27158	3760	19966	39525	16.36	1.12	10.90	18.00
1989	27671	3908	19048	44434	16.41	1.09	11.40	18.00
1990	28377	3687	21441	40053	16.49	1.07	11.40	18.25
1991	29588	3914	22059	45130	16.52	1.05	11.40	18.00
1992	34741	4835	25219	55771	19.03	1.30	13.20	21.70
1993	32098	4828	20483	51258	19.13	1.39	13.15	21.93
1994	31834	4299	20014	46816	19.14	1.38	13.15	21.93
74-94	27987	5209	14391	55771	16.32	1.71	9.70	21.93

Notes: Each row reports summary statistics for 274 municipalities. Spending is expressed in 1991 SEK per capita and taxes in percent.

Table 3
Electoral cycles in spending and taxes

Dependent variable	Spending	Taxes
Sample	Total	Total
Election	418 (7.17)	-0.07 (-7.11)
Lagged dependent variable	0.42 (29.55)	0.52 (55.32)
Proportion young	-4322 (-1.40)	-0.83 (-1.14)
Proportion elderly	-1033 (-0.32)	3.12 (4.81)
Population size	-0.05 (-2.19)	0.00001 (3.17)
Income (t)	0.08 (13.79)	7.46e-06 (9.78)
Income (t-2)	0.09 (17.50)	2.24e-06 (2.84)
Population density	-9.06 (-4.97)	-0.001 (-4.95)
Aggregate growth	-175 (-9.63)	-0.01 (-4.40)
Grants	0.80 (19.72)	0.0001 (20.24)
Unit specific fixed effects	Yes	Yes
R ²	0.8477	0.9472
Number of obs.	5480	5480

Notes: The dependent variable is spending in the first column and taxes in the second column. Estimates are based on Swedish municipality data for 1974-1994. All regressions include fixed municipality effects. There are also two dummy variables included in the spending regression (D=1 for 1974 to 1982 and zero otherwise, D=1 for 1992 to 1994 and zero otherwise) and one dummy variable in the tax regression (D=1 for 1992 to 1994 and zero otherwise) because of trend shifts. For information about the causes for these trend shifts, see footnote 18 in the text. These dummy coefficients are not reported. *t*-statistics are in parentheses and white standard errors were used in calculating *t*-statistics.

Table 4
Electoral cycles in spending and taxes: Incumbent government re-elected or ousted

Dependent variable	Spending	Taxes	Spending	Taxes
Sample	Incumbent re-elected		Incumbent ousted	
Election	408 (5.98)	-0.06 (-5.00)	229 (1.38)	-0.07 (-2.42)
Same controls as in Table 3	Yes	Yes	Yes	Yes
Unit specific effects	Yes	Yes	Yes	Yes
R ²	0.8616	0.9503	0.8455	0.9621
Number of obs.	3755	3755	848	848

Notes: The dependent variable is spending in the first and third columns and taxes in the second and fourth columns. Estimates are based on Swedish municipality data for 1974-1994. The first two columns only include data from the term of office before a successful re-election bid, whereas the last two columns only include data from an unsuccessful re-election bid. All regressions include fixed municipality effects. There are also two dummy variables included in the spending regression (D=1 for 1974 to 1982 and zero otherwise, D=1 for 1992 to 1994 and zero otherwise) and one dummy variable in the tax regression (D=1 for 1992 to 1994 and zero otherwise) because of trend shifts. For information about the causes for these trend shifts, see footnote 18 in the text. These dummy coefficients are not reported. *t*-statistics are in parentheses and white standard errors were used in calculating *t*-statistics.

Table 5
Political cycles in debt

Dependent variable	Debt	Debt	Debt
Sample	Total	Incumbent re-elected	Incumbent ousted
Election	739 (11.03)	711 (9.44)	739 (3.37)
Same controls as in Table 3	Yes	Yes	Yes
Unit specific effects	Yes	Yes	Yes
R ²	0.7892	0.7972	0.8157
Number of obs.	5477	3752	848

Notes: The dependent variable is debt. Estimates are based on Swedish municipality data for 1974-1994. The first column consists of the whole sample. The second column only includes data from the term of office before a successful re-election bid, whereas the third column only includes data from an unsuccessful re-election bid. All regressions include fixed municipality effects. *t*-statistics are in parentheses and white standard errors were used in calculating *t*-statistics.

Table 6
Electoral cycles in spending for left-wing and right-wing governments

Dependent variable	Spending	Spending	Spending	Spending
Sample	Left-wing incumbent reelected	Right-wing incumbent reelected	Left-wing incumbent ousted	Right-wing incumbent ousted
Election	456 (4.72)	304 (3.22)	-19 (-0.06)	-189 (-0.60)
Same controls as in Table 3	Yes	Yes	Yes	Yes
Unit specific effects	Yes	Yes	Yes	Yes
R ²	0.8610	0.7856	0.9244	0.8507
Number of obs.	2021	1734	293	555

Notes: The dependent variable is spending. Estimates are based on Swedish municipality data for 1974-1994. The first column only includes data from the term of office before a successful re-election bid for left wing governments, whereas the second column only includes data before a successful re-election bid for right-wing governments. The third column only includes data from the term of office before an unsuccessful re-election bid for left-wing governments, whereas the fourth column only includes data from an unsuccessful re-election bid for left-wing governments. All regressions include fixed municipality effects. All regressions include fixed municipality effects. There are also two dummy variables included in the spending regression (D=1 for 1974 to 1982 and zero otherwise, D=1 for 1992 to 1994 and zero otherwise) because of trend shifts. For information about the causes for these trend shifts, see footnote 18 in the text. These dummy coefficients are not reported. *t*-statistics are in parentheses and white standard errors were used in calculating *t*-statistics.

Table 7
Electoral cycles in taxes for left-wing and right-wing governments

Dependent variable	Taxes	Taxes	Taxes	Taxes
Sample	Left-wing incumbent reelected	Right-wing incumbent reelected	Left-wing incumbent ousted	Right-wing incumbent ousted
Election	-0.07 (4.33)	-0.05 (-2.74)	-0.04 (-0.80)	-0.10 (-1.35)
Same controls as in Table 3	Yes	Yes	Yes	Yes
Unit specific effects	Yes	Yes	Yes	Yes
R ²	0.9429	0.9482	0.9638	0.9666
Number of obs.	2021	1734	293	555

Notes: The dependent variable is taxes. Estimates are based on Swedish municipality data for 1974-1994. The first column only includes data from the term of office before a successful re-election bid for left wing governments, whereas the second column only includes data before a successful re-election bid for right-wing governments. The third column only includes data from the term of office before an unsuccessful re-election bid for left-wing governments, whereas the fourth column only includes data from an unsuccessful re-election bid for left-wing governments. All regressions include fixed municipality effects. All regressions include fixed municipality effects. There is also one dummy variable in the tax regression (D=1 for 1992 to 1994 and zero otherwise) because of trend shifts. For information about the causes for these trend shifts, see footnote 18 in the text. These dummy coefficients are not reported. *t*-statistics are in parentheses and white standard errors were used in calculating *t*-statistics.

Table 8
Electoral cycles in debt for left-wing and right-wing governments

Dependent variable	Debt	Debt	Debt	Debt
Sample	Left-wing incumbent reelected	Right-wing incumbent reelected	Left-wing incumbent ousted	Right-wing incumbent ousted
Election	758 (6.88)	632 (6.35)	170 (0.51)	1276 (3.33)
Same controls as in Table 3	Yes	Yes	Yes	Yes
Unit specific effects	Yes	Yes	Yes	Yes
R ²	0.7969	0.7849	0.8781	0.8238
Number of obs.	2018	1734	293	555

Notes: The dependent variable is debt. Estimates are based on Swedish municipality data for 1974-1994. The first column only includes data from the term of office before a successful re-election bid for left wing governments, whereas the second column only includes data before a successful re-election bid for right-wing governments. The third column only includes data from the term of office before an unsuccessful re-election bid for left-wing governments, whereas the fourth column only includes data from an unsuccessful re-election bid for left-wing governments. All regressions include fixed municipality effects. *t*-statistics are in parentheses and white standard errors were used in calculating *t*-statistics.



