The Dynamics of the Political Budget Cycle: An empirical examination of municipal spending in Austria☆

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Abstract

Using a fairly large data set covering more than 10,000 elections, this paper analyzes the effect of elections on municipal spending in Austria. The paper not only produces further evidence that there is an electoral cycle in fiscal policy but emphasizes the role of dynamics in government spending. In contrast to recent suggestions to look for the cycle in monthly data immediately before an election, this paper tests the hypothesis that pre-electoral spending increases may begin well before the election year. The paper shows that getting the dynamic specification right is crucial to identify the effect of elections on government spending decisions. Separating elections early and late during the year and allowing for differential dynamic processes may not only explain insignificant results in previous studies but also confirms the idea of pre-electoral expansions that will commence well before the election year.

Keywords: Political budget cycle, Local government, Government expenditure
JEL: D71, D78, H71

1. Introduction

Once every few years political agents in democratic systems face elections. For the incumbent government these elections determine whether it remains in office or not. Politicians in power have therefore an incentive to use policies and budgets at their disposal to increase their probability of reelection. As suggested by theories like Rogoff (1990) this will result in a pre-electoral manipulation of fiscal policy tools. The dynamics of such vote-seeking behavior over the electoral cycle are at the heart of this paper.

A huge number of studies has corroborated the existence of electoral cycles in various different areas of public policy. While potentially depending on the particular situation and institutional environment, a tendency for higher expenditures, lower taxes and larger deficits before elections is considered by many researchers a standard feature of democratic systems. What is less clear, however, is the behavior of political agents over the whole electoral cycle.

☆The author is grateful to Eshien Chong for helpful comments.
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The usual way to test theories of political budget cycles is to use a (dummy) indicator for the pre-election period, typically the election year. In the typical panel fixed effects context, this amounts to comparing average fiscal policy in the pre-election period to average fiscal policy in all other periods. The question arises, however, if the effect of approaching elections on the behavior of a government is not more complicated than e.g. assuming a single expenditure burst immediately before an election.

In a recent theoretical contribution Martinez (2009) derives that manipulation of fiscal policy will begin already before the election year. As a result of effort smoothing behavior, the politician will not only use the last (i.e. most recent) period before the election to signal performance to voters, but commence already before. Although more recent performance in terms of spending may still provide more information about future performance for voters, the emerging political cycle is not only characterized by a spike before elections. Therefore conversely to the models in Rogoff (1990), Persson and Tabellini (2000) or Shi and Svensson (2006), which predict additional effort only for the period immediately before the election, the theoretical framework by Martinez (2009) implies a different dynamic pattern of fiscal policy.

Using a fairly large data set on municipal elections and expenditure decisions in Austria, this paper tries to analyze the dynamic dimension of the political budget cycle. The current application is particularly suited for this because the Austrian electoral system has two features that are very advantageous when modeling the dynamics of the political cycle. Firstly, elections on the municipal level are pre-determined, not alterable and therefore not subject to strategic changes in election dates, which plague analyses on the national level. Secondly, and in contrast to many other countries, election dates of municipalities vary by state and it is therefore possible to control for aggregate time effects and still identify the effect of the electoral cycle.

The contribution of this paper is to show that the effect of elections on municipal spending is not restricted to the period immediately before the election. This is important as recent research has argued that political cycles are a very short term phenomenon that may be overlooked when using low-frequency data such as annual observations (see e.g. Akhmedov and Zhuravskaya (2004) or Streb et al. (2012)). In contrast, this paper finds that the political cycle is identifiable and of considerable size, also in annual data, as long as the dynamics are correctly specified. As a consequence, this paper corroborates the idea that governments increase their effort (e.g. through increased spending) well before the election year.

This paper proceeds by reviewing the related literature before presenting the used data set and the empirical strategy. The empirical results and findings are followed by a concluding section.

2. Related literature

A large number of empirical studies confirms that fiscal policy is different before elections. On the national level Persson and Tabellini (2003), Shi and Svensson (2006), Brender and Drazen (2007) and Streb et al. (2009) present evidence of pre-electoral changes in all major components of government budgets: tax revenues, public debt and spending. The same

hypotheses have been tested for sub-national governments. E.g. Akhmedov and Zhuravskaya (2004) for Russian regions, Geys (2007) for Flemish municipalities or Aidt et al. (2011) for Portuguese municipalities. The findings are largely in analogy to the results from cross-country studies in that differences in fiscal policy can be observed before elections.

An important conclusion of the research on the political budget cycle is that the intensity of these pre-electoral shifts may critically depend on the institutional setting. The obtained evidence is, however, far from conclusive and frequently conflicting. For instance the result of Brender and Drazen (2007) that pre-electoral changes are present only in developing and new democracies is contested by the recent findings in Klomp and Haan (2012).

While not directly related to the problem of (institutional) heterogeneity, sampling frequency has been suggested as another explanation why political cycles may be overlooked in too aggregate data. As proposed by Akhmedov and Zhuravskaya (2004) or Streb et al. (2012), temporally disaggregated time series information should be more easily able to identify pre-electoral variation in fiscal policy variables.

The argument for using higher frequency data is, however, not fully convincing. On the one hand, research on policy discretion by governments suggests that the political process in most countries does not allow for swift changes in discretionary spending (see Fatás and Mihov (2003)). If the electoral cycle is indeed just a singular spiked increase in the month(s) before the election, as suggested by the data and graphs in Akhmedov and Zhuravskaya (2004) for Russian regions, one may wonder about the type of spending or projects that is so perfectly timed but does not affect periods before or after the election.

Moreover, even if there is some expenditure spike just before the election, it should be 'visible' in more aggregate data too if the magnitude of such a spending increase is beyond mere statistical significance and to be economically meaningful. If the increased pre-electoral effort in the election year is not distinguishable from other years, then maybe the measured electoral cycle is not a relevant empirical phenomenon.

Finally, given the theoretical advances by Martinez (2009) along with the empirical evidence on retrospective and economic voting (see e.g. Eisenberg and Ketcham (2004)), it appears unlikely that voters take into consideration only the last months before the election. On the contrary, there is reason to believe that the electoral cycle is not restricted to the year preceding an election. Despite the fact that more recent performance is more informative for the voter, the signaling strategy of the politician as outlined in Martinez (2009) will lead to increased effort already before the immediate pre-election period.

Since this prediction is at odds with both the arguments for using high frequency data as well as the theoretical contentions of Rogoff (1990), Persson and Tabellini (2000) or Shi and Svensson (2006), the empirical analysis in this paper tries to test the prediction of rather long-term cyclical behavior in government spending. As will become clear shortly, modeling the dynamics of the political budget cycle is paramount to this task.

Table 1: Number of municipalities holding elections per year and state

	State								
year	1	2	3	4	5	6	7	8	Total
1990	0	0	589	0	0	551	0	96	1,236
1991	0	132	0	446	0	0	0	0	578
1992	166	0	0	0	0	0	283	0	449
1993	0	0	0	0	0	0	0	0	0
1994	0	0	0	0	120	0	0	0	120
1995	0	0	589	0	0	551	0	96	1,236
1996	0	0	0	0	0	0	0	0	0
1997	166	132	0	446	0	0	0	0	744
1998	0	0	0	0	0	0	283	0	283
1999	0	0	0	0	120	0	0	0	120
2000	0	0	589	0	0	551	0	96	1,236
2001	0	0	0	0	0	0	0	0	0
2002	166	0	0	0	0	0	0	0	166
2003	0	132	0	446	0	0	0	0	578
2004	0	0	0	0	120	0	283	0	403
2005	0	0	589	0	0	551	0	96	1,236
2006	0	0	0	0	0	0	0	0	0
2007	166	0	0	0	0	0	0	0	166
2008	0	0	0	0	0	0	0	0	0
2009	0	132	0	446	120	0	0	0	698
2010	0	0	589	0	0	551	283	96	1,519
Total	664	528	2,945	1,784	480	2,755	1,132	480	10,768

3. The data

Table 1 presents the number of municipalities holding elections over years and states in Austria from 1990 to 2010.[2] As can be seen from the table, depending on the state local governments are elected every 4 to 5 years, which results in 4 to 5 elections over the sample period and a total of 10,768 elections. As already stated in the previous chapter election dates are fixed by the state government and therefore clearly exogenous. Term length has also remained unchanged over the analyzed time period.

Important for the model specification is the fact that some states vote in the first half of the year while others vote in the second half of the year. Indeed in the present case only two states (Number 1 and 4 in Table 1) hold municipal elections in the second half of the year, thus leaving a majority of municipalities with elections already at the beginning of the respective election year. As it is possible that this will change the dynamics in that it shifts the increase in (fiscal) effort back and forth, the econometric specification will take account of this by separating the sample into early election, taking place within the first 6 months and late elections, taking place withing the second 6 months of the year.

Regarding the data on expenditure, Figure 1 highlights the development of average and per capita expenditure from 1990 to 2010. Although both expenditure variables more than

[2] Actually Austria has 9 states but Vienna as the only city state has been excluded because it is not only hardly comparable because of its size but also because the local government in Vienna can itself decide on election dates, and has done so in the past.

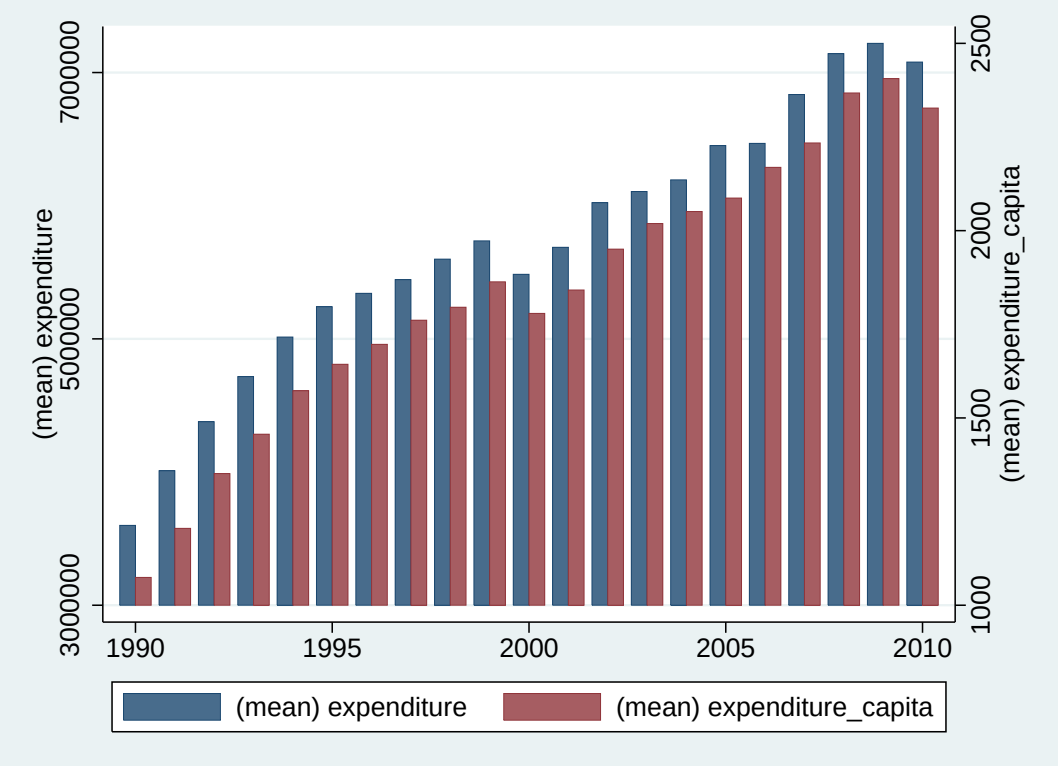

Figure 1: Expenditure over time: Average total and per capita expenditure from 1990 to 2010

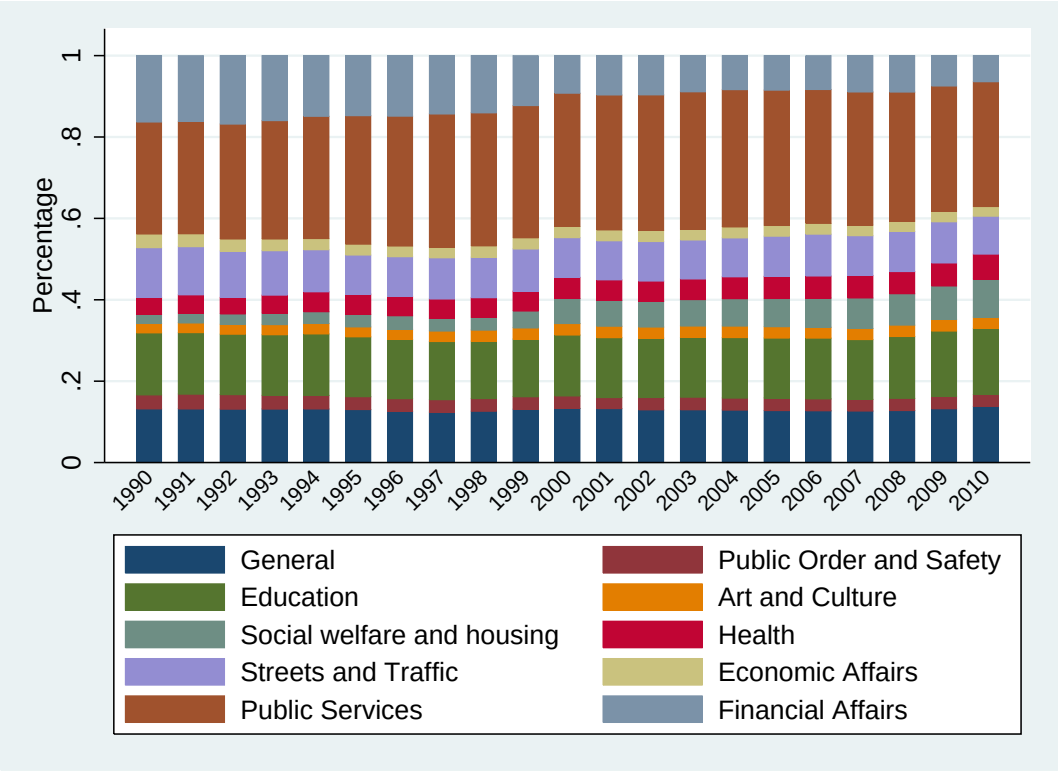

Figure 2: Expenditure shares by year

double over the period under study, two small corrections appear to have occurred in 2000 and 2010. The latter is due to the economic downturn following the financial crisis, whereas the decrease around the year 2000 is largely a result of a transfer of tasks from local governments to states or external authorities. This shift in 2000 can also be seen in Figure 2, presenting the share of different expenditure categories in total expenditure. As these changes in municipal tasks and responsibilities are somewhat state specific, both in terms of implementation period, extent as well as the specific type of tasks affected, the periods until 2000 may potentially contain some structural shifts. The subsample for the years after 2000 can therefore be considered as more consistent with respect to its time series properties.

Despite the fact that such structural breaks and one-off transactions are also frequently present in national and central government statistics (see e.g. Eurostat (2011)), research on the political budget cycle on the national level has usually ignored such events, also because a correction is often infeasible. To account for the potential effect of breaks in the time series and associated bias, the following empirical analysis will also show the results from using a limited sample, including only years after 2000.

For the estimations in the ensuing section, the dependent variable will not be total expenditure itself but total expenditure less expenditures on financial affairs. The category financial affairs is deducted because these expenditures almost exclusively involve financial transfers to other government levels, debt service or appropriations. This is done because one would expect the opposite trend for this type of spending compared to expenditures that can be used for vote-seeking purposes.[3]

4. Empirical analysis

To analyse the relationship between elections and expenditures the following model suggested by Wooldridge (2005):

$$y_{it} = w_t \alpha_i + x_{it} \beta + u_{it} \tag{1}$$

where w_t is a $1 \times J$ vector of aggregate time variables, α_i is a $J \times 1$ vector of individual-specific slopes on the aggregate time variables, x_{it} is a $1 \times K$ vector of covariates that change across time, β is vector of K slopes and u_{it} is an idiosyncratic error.

The model is fairly general in that it comprises not only the standard unobserved effects model, which can be estimated by fixed effects (FE) or first differences (FD), but also more general random trend models (RT). Setting $w_t = 1$ in the above equation generates the unobserved effects model while the random linear trend model is obtained by $w_t = (1, t)$. Given the sufficient number of time periods in the underlying sample, even more flexible trend formulations are possible, for example by choosing $w_t = (1, t, t^2)$.[4]

[3] The results are not particularly sensitive to this decision and qualitatively the same.

[4] To estimate the RT model, equation 1 is differenced according to the number of random trends that are included in the model. E.g. a model with $w_t = (1, t, t^2)$ is differenced twice before applying a fixed effect to absorb the remaining time constant effect.

While the above model in equation (1) is static, the model actually estimated will be dynamic in the sense that it contains leads of the election year indicator. These additional variables are used to model the dynamics of the political budget cycle and indicate if expenditure increases start already before the immediate election period. This leads to the following model:

$$y_{it} = w_t\alpha_i + x_{it}\beta + election_{it}\beta_{e0} + election_{it+1}\beta_{e1} + election_{it+2}\beta_{e2} + u_{it} \qquad (2)$$

with β_{e0}, β_{e1} β_{e2} being the coefficient estimates of the election year and their leads. The variable $election_{it+1}$ for instance is 1 if the following year is an election year and 0 otherwise. Its coefficient β_{e1} measures the effect of a pre-election year on municipal expenditures. While the choice of two leads is somewhat arbitrary it is important to note why too few and too many leads may be troublesome. Firstly, if only an election year indicator is used, the estimated coefficient compares the mean expenditure in an election year with all periods without an election. If, however, governments start earlier with increased spending, the contrast between these two periods is blurred.

Such cases are illustrated in Figure 3. Each of the four panels represent an Austrian municpality and depicts the expenditure dynamics over the electoral cycle. In the upper two panels the elections takes place in October, i.e. late during the election year whereas in the lower two panels the election took place early during the year (March or April). To illustrate the potential problem of pooling all non-election years, the left side shows expenditures differentiated by the number of years until the next election (0 indicating the election year) while on the right side there is just a comparision between the election year and non-election years. Although all four left hand side graphs reveal a sizeable increase in spending before the election, the simple comparisons of election and non-election periods on the right hand side shows the potential flaws of pooling all non-election years. In the case of early elections (the bottom two panels), election years appear to be accompagnied by a decrease in spending, seemingly contradicting the idea of pre-electoral expansion. This is, however, only the result of incorrectly interpreting the election year as a pre-election period when it actually rather represents the post-election period.

Compared to early elections, the two upper panels for late elections are not exhibiting the problem of turning the results upside down when comparing election and non-election periods, but are nevertheless problematic. The obvious heterogeneity among non-election years shows that the choice of a comparision year for the election year will have a large effect on the measured effect of elections. For instance using the average of all non-election periods as a comparison may dwarf the true effect of elections on spending because the spending increases in the period before the election are not recognized as being driven by the upcoming elections. As a consequence, pooling all non-election years - as done in typical electoral dummy approaches and as depicted on the right side - may not only miss the more complicated dynamics but is also poised to bury the difference between the years affected by election and those not.

The problem can also be framed as a question of choosing the right counter-factual. The average of all non-election periods may not be a very good counterfactual if for instance incumbent governments increase spending not only in the election year but already in the

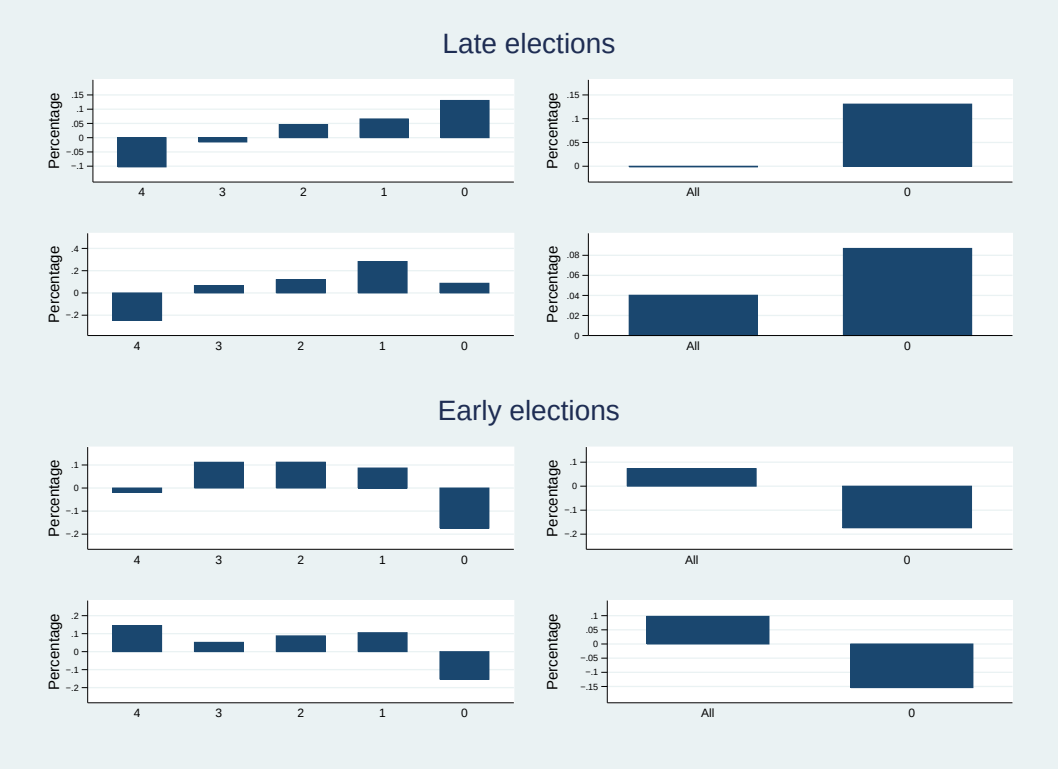

Figure 3: Potential patterns of dynamics

year before an election. In contrast to the famous Ashenfelter's dip, where the causal effect of job-training on wages may be overestimated because of a typical pre-training dip in wages, here the effect of elections on spending may be underestimated if the wrong comparison years are chosen. Apart from the technical side of this problem in the sense that one may end up with a dynamically misspecified model, the dummy variable appraoch simply misrepresents the actual behavior by governments over the electoral cycle. Finding little or no difference between election and non-election years may therefore be an artefact of a poorly specified model that treats all non-election years the same, and incorrectly so.

Conversely to the case of pooling all non-election years, using a full parametrization of the electoral cycle will make interpretation more difficult because it is unclear which period should serve as baseline and comparison year. The chosen specification with the election year and two leads appears to allow for enough flexibilty and still allows for a straightforward interpretation of the coefficients. The bottom line is that the number of leads is important because it determines the counter-factual for the election year indicators.

Apart from the static model in equation (1) and the dynamic cycle specification in (2), a lagged dependent variable (LDV) would make the model potentially even more dynamic. And although many empirical studies analyzing political budget cycles use such a dynamic model, using a LDV in the present context is problematic. In addition the so-called Nickel bias, which is introduced by estimating a LDV together with a unit specific unobserved effect, it is doubtful what a LDV adds in the present context. As the election year as well as its leads are not only predetermined but truly strictly exogenous - e.g. no feedback effects

are possible - there is no gain, except from potentially reducing error variance. However, if the errors are still serially correlated after introducing the LDV, and despite using the suggested difference or system GMM estimators, the results can be seriously biased. As it also possible if not likely that the expenditure data is non-stationary - the coefficient of the lagged dependent variable in a pooled cross-section is 0.96 - first differencing the data is not only a more direct way to tackle to problem of persistency in the time series but also deals more convincingly with the unit-root problem.

From a more theoretical point of view, the popular practice of using LDVs in models trying to test the existence of electoral cycles is in itself surprising. The typical argument for introducing it is 'persistency' in government spending. However, a more straightforward way to simply deal with a trending time series is to take first differences or use year fixed effects, as is applied here. More importantly, using a LDV changes the assumed data generating process in that it assumes that the effect of election years is not immediate but that it lasts for many periods and declines at a geometric rate. So the implicit assumption to this way of modelling is that there is a sustained effect of the pre-electoral expansion that will lead to a new long term equilibrium. While such a process is not unrealistic, it is certainly at odds with the idea of pre-electoral spending increases that not only occur singularly and very close before elections but are reversed in the post election period. Hence, using a LDV specification and at the same time arguing that the pre-electoral policy manipulation is reversed after the election is inconsistent as the LDV model assumes a lagged and cumulative impact of the pre-electoral expansion.

A similar logic applies to the use of covariates. With the election indicators being exogenous, adding covariates can at best bring efficiency gains. At worst, because of a number of potential endogeneity problems associated to the usual control variables, the resulting estimator will be inconsistent. Despite this reasons against more rich models in terms of dynamics and covariates, they will nevertheless be estimated to compare the results.

As already noted in the previous section, the dynamics may strongly depend on elections taking place in the first or second 6 months of a year. Accordingly, apart from pooling all municipalities the sample will be split into early and late elections, indicating if elections took place between January and June or July and December. The dependent variable, expenditure, is logged and therefore a percentage change interpretation arises.

5. Results

Table 2 presents the first results obtained from various different estimators (FE, FD, RT and the difference GMM estimator as suggested by Arellano and Bond (1991)).[5] As discussed before, municipalities can be distinguished according to whether they hold elections in the first or the second half of a calendar year. Consequently, three different specifications are presented for each estimator: all elections pooled (ALL), the sub-sample of those municipalities who vote in the first half of the year (forth on called 'early') and those who vote in

[5]The differenced lag of the dependent variable is instrumented by all its available lags in levels.

Table 2: Pooled, early and late elections

	(1) FE	(2) FD	(3) RT1	(4) RT2	(5) AB
			All elections pooled		
electionyear	0.002	0.005	0.005*	0.009**	0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.019)
electionyear-1	0.005	0.006*	0.006*	0.014***	0.074***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.020)
electionyear-2	-0.002	0.002	0.002	0.009**	0.043*
	(0.003)	(0.003)	(0.003)	(0.003)	(0.019)
L.exp					0.523***
					(0.011)
N	49297	46940	46940	44583	44583
			Early elections		
electionyear	-0.010*	-0.009*	-0.009*	-0.008*	0.001
	(0.004)	(0.004)	(0.004)	(0.004)	(0.023)
electionyear-1	0.012**	0.009*	0.009*	0.013**	0.063*
	(0.004)	(0.004)	(0.004)	(0.005)	(0.027)
electionyear-2	-0.001	0.000	0.000	0.005	0.009
	(0.004)	(0.004)	(0.004)	(0.005)	(0.024)
L.exp					0.504***
					(0.012)
N	36530	34785	34785	33040	33040
			Late elections		
electionyear	0.045***	0.044***	0.045***	0.059***	0.045*
	(0.007)	(0.007)	(0.007)	(0.008)	(0.019)
electionyear-1	0.015	0.023**	0.025**	0.053***	0.052**
	(0.008)	(0.008)	(0.008)	(0.010)	(0.020)
electionyear-2	0.007	0.019*	0.020*	0.040***	0.026
	(0.008)	(0.008)	(0.008)	(0.010)	(0.019)
L.exp					0.582***
					(0.018)
N	12767	12155	12155	11543	11543

Cluster robust standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

the second half of the year (subsequently called 'late').[6]

The first important results from Table 2 is that it is critical to distinguish the between early and late elections. The coefficient for the election year itself is negative for municipalities that vote in the first few months of a year whereas it is positive for elections taking place late during the year. This is not surprising in that for early elections, which take place in March or April, the election year has to be considered already as a post-election year in term of the political budget cycle. When voting early, the election year is characterized by a reversal of the fiscal expansion. Relevant effort by the government to increase its election probability falls to a large part into the years before the elections, not the election year itself. Conversely, in the case of late elections, taking place in October, the significant amounts of vote-seeking spending is effected in the election year itself.

In the pooled version that restricts the dynamic pattern of the cycle to be the same between municipalities, regardless of when they vote, the two conflicting spending trends basically cancel out each other. The estimated coefficient reflects just some averaged quantity that is dominated by the group with higher variation.[7] After splitting the sample both groups exhibit the expected behavior, but with somewhat varying size.

That the measured effect is considerably larger for late election is almost certainly a result of the more clear distinction between pre- and post-election periods. For municipalities with early elections, the fiscal year in which the election took place represents a mix of pre- and post-election regime and is therefore hard to categorize. As a result, a clear comparison between years affected by pre-electoral expansions and years affected by post-electoral contractions is not easily possible. As a lower bound estimate, one could either compare the coefficient in the election year with the coefficient in the pre-election year or relegate the election year into the control group by reestimating the early elections without an indicator for the election year. In the former case, the pre-electoral expansion amounts to roughly 2% in the year before the election but is insignificant two years before. The AB estimate is significantly larger than that but vanishes completely as soon as control variables are included (see Appendix).

In contrast, municipalities holding elections late during the year exhibit quite clear pre-electoral expansions of considerable magnitude. Spending increases between 4 to 5 % in the election year in all specifications. Moreover, all models except FE find a significant expenditure increases already in the years before the elections. While including covariates (see Appendix) would change the results for FE and align them with the FD or RT models, a number of different unit root tests casts doubt on the consistency of non differenced models like FE and AB. Moreover, as for the early elections the AB model with the lagged dependent variable exhibits a very strong sensitivity to the included covariates, both in magnitude as well as significance and overidentification tests (both Sargan and Hansen, which either robust many instruments or to heteroskedastic errors) reject in most cases. Therefore, the models

[6] Results with a number of covariates can be found in the appendix. The number of observations reduces, however, significantly because covariates are not always available for the whole sample period. If anything, adding covariates enforces the previous results.

[7] In a specification without leads but an election year indicator only, the coefficient on the election year dummy is also insignificant when pooling early and late elections.

using differenced data, FD and RT1, or even twice differenced data like RT2 are the preferred specifications.

To sum the findings, it is important to account for dynamics in several different respects. First, in the presence of annual fiscal data it is crucial to distinguish elections that take place early or late over the year. Failing to do so may critically affect the results and produce misleading findings. Second, even if election dates would perfectly coincide with the end of a fiscal year, there is more to the cycle than just increased spending in the year before the election. Choosing an adequate model and a dynamic specification involving sufficient leads of the election year indicator is paramount to identify actual underlying dynamics. The caricature of the cycle through an election year indicator is most certainly insufficient to this task.

6. Robustness and Sensitivity Analysis

This section will try to dig deeper into the previous findings regarding the electoral cycle and its dynamics. Three main issues are considered: heterogeneity among states, current versus investment expenditure and finally the question of a structural break in the time series before 2000.

Heterogeneity among States

The previous section argued that the difference between early and late elections is driven by the different dynamics over the cycle. Potentially, the differenes between the two groups of municipalities may also be a result of peculiar spending trends in the states of these groups. State governments may impose different fiscal constraints on municipalities and the observed behavior could therefore also reflect differences among states.

To narrow down the potential sources of the difference between the two groups, Figure 4 shows the evolution of expenditures over the electoral cycle for each of the states. The bars represent expenditure changes for each year preceding an election. As before, zero (0) indicates election years, 1 the pre-election year and so forth. While the spending dynamics for municipalities in states 1 and 4 (group late) are in line with the expectations of a political budget cycle, the other panels exhibit a more varied picture. As in the case of state 3, 6 and 8, the dynamics are actually the very opposite to what would be considered a pre-electoral spending increase.

To graph also shows, however, that the differences between the two groups is not simply the result of the one group holding elections early and the other one later during the year. Instead, the dynamics in terms of the underlying spending trends are markedly different. Looking at election dates in states 3, 6 and 8 (see Table 1), one could reach the conclusion that municipalities in these states were just unlucky with their election dates coinciding with economic downturns and expenditure reductions. Those three states held elections in 2000 as well as in 2010, the only two periods identified by Figure 1 to have an aggregate spending decrease on the municipal level. In contrast, municipalities in states 1 and 4 not only happened to have elections in growth periods but the expenditure reductions also tend to fall into periods just after an election. This increases the contrast between election/pre-election periods to post-election periods. In all cases, the true spending dynamics with

Figure 4: Expenditure cycle by state: Change in municipal expenditure over years until the election

respect to the election dates are blurred by the conflicting spending trends.

Instead of attributing the different patterns between states and groups of states (early vs late) to some kind of institutional heterogeneity - as is quite popular in current research on electoral cycles - this paper will argue that the seeming differences are caused by different expenditure trends. Although it is typically assumed that aggregate time fixed effects should take care of most of the exogenous spending trends, the following tables will show that this is not generally the case. More flexible specifications regarding expenditure trends may be required when analyzing an inherently dynamic phenomenon such as the political budget cycle.

Finally, if really spending trends differ mostly on the state level, it may be interesting to look at the municipal sub-samples for each state. Analyzing states separately is also a test if, even after controlling for individual trends, the results are driven by a single state or we observe similar dynamic patterns. Since year fixed effects are no longer possible because they are collinear with the election cycle indicators, 4th order polynomial of time trends are used to control for aggregate spending effects. Given the findings of the previous estimations, the first difference (FD) and random trend (RT1 and RT2) models will be used.

In line with the previous results, Table 3 confirms that municipalities in states that vote late in the year have significant higher spending in election years. Moreover, these states also tend to increase spending in year 1 and 2 before an election. Similar results are found for 'early' states. Although more different in significance and size of the effect, none of the obtained results (when significant) contrasts with the hypothesis of an accelerated spending

Table 3: Estimations per state

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Early Elections						Late Elections	
FD Models								
electionyear	0.008	-0.003	0.024*	-0.007	-0.012	-0.003	0.039***	0.034***
	(0.008)	(0.005)	(0.011)	(0.006)	(0.008)	(0.013)	(0.008)	(0.005)
electionyear-1	0.023*	0.029***	0.043***	0.020**	0.015*	0.022	0.018	0.008
	(0.009)	(0.005)	(0.012)	(0.006)	(0.007)	(0.015)	(0.010)	(0.006)
electionyear-2	-0.001	0.008	0.030*	0.006	0.012	0.017	0.017	-0.009
	(0.010)	(0.004)	(0.012)	(0.005)	(0.008)	(0.013)	(0.010)	(0.005)
N	2620	11433	2380	10854	5578	1920	3260	8895
RT1 Models								
electionyear	0.008	-0.003	0.024*	-0.007	-0.012	-0.003	0.039***	0.034***
	(0.008)	(0.005)	(0.011)	(0.006)	(0.008)	(0.013)	(0.008)	(0.005)
electionyear-1	0.023*	0.029***	0.043***	0.020**	0.015*	0.022	0.018	0.008
	(0.009)	(0.005)	(0.012)	(0.006)	(0.007)	(0.015)	(0.010)	(0.006)
electionyear-2	-0.001	0.008	0.030*	0.006	0.012	0.017	0.017	-0.009
	(0.010)	(0.004)	(0.012)	(0.005)	(0.008)	(0.013)	(0.010)	(0.005)
N	2620	11433	2380	10854	5578	1920	3260	8895
RT2 Models								
electionyear	0.007	0.003	0.022	-0.008	-0.002	-0.017	0.047***	0.046***
	(0.010)	(0.006)	(0.012)	(0.006)	(0.009)	(0.015)	(0.009)	(0.006)
electionyear-1	0.019	0.028***	0.045***	0.019**	0.030**	0.019	0.021*	0.032***
	(0.011)	(0.005)	(0.012)	(0.006)	(0.010)	(0.015)	(0.010)	(0.008)
electionyear-2	-0.006	0.004	0.036**	0.006	0.023*	0.022	0.008	0.015*
	(0.012)	(0.005)	(0.013)	(0.006)	(0.009)	(0.013)	(0.011)	(0.006)
N	2488	10859	2261	10309	5299	1824	3094	8449

Cluster robust standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

trend in the years before an election. Table 3 suggests that the overall results are not driven by a single group or individual state but are supported by the collective evidence found in all states.

Current vs Investment expenditure

Another question that sourrounds the previous results of this paper is the questions what kind of expenditure is actually increased by governments before elections. While the celebrated theoretical model by Rogoff (1990) postulates an increase in current expenditure at the cost of investment expenditures because the latter materialize only with a lag, some recent emirical studies have found that on the municipal level investment may actually rather increase before elections than decrease. For instance, analyzing the determinants of investment in Flemish municipalities Goeminne and Smolders (2011) find an expenditure cycle in investment, peaking in the year before the election. Similarly but in the context of Indian states Khemani (2004) shows an increase in investment expenditure before elections, notably driven by road construction.

To identify the source of the expenditure increase before elections for Austria municpalities, total expenditure is divided into current expenditure and investment expenditure. Although investment expenditure is not directly available, the cameralistic system of municpal accounts distinguishes ordinary and extraordinary expenditures. Ordinary expenditures are conceptually closely related to current expenditures and represent expenditures that occur regularly and are roughly predictable in size and ocurrance. In contrast, extraordinary expenditures represent unusual, non-recurrent types of expenditures. This means that a new road, buildings as well as restoration measures are considered an extraordinary expenditures, while typical and frequently repeated expenditures like wages are ordinary expenditures. For ease of interpretation the denotations remain current (ordinary) and investment (extraordinary) expenditures.

Tables 4 and 5 show the resuls of the associated estimations, distinguishing current and investment expenditure.

The results suggest that the main cause of the spending cycle are investment expenditures. The effect in Table 4 is substantially stronger than in previous models pooling investment and current expenditure. The result holds not only for late elections, where again the effect is much clearer, but also for early elections. In the latter case the difference between the contraction in the election year and the year before the election is much more pronounced than previously. In addition, Table 5 shows that the increase in investment is at least partly offset by current expenditures. While the effect is rather weak and mostly insignificant for early elections, the estimations involving late elections provide some evidence in this respect. This means that in addition to the previous evidence that the level of expenditures is increased before elections, there is also a compositional effect that leads to a shift from current to investment expenditure before elections. While consistent with other empirical studies like Khemani (2004), the result is at odds with the theoretical prediction by Rogoff (1990).

Reduced sample after 2000

The final sensitiviy test is related to the observation that the expenditure time series may exhibit a structural break in the years before 2000. As a number of changes in the functional

Table 4: Investment expenditure

	(1) FD	(2) RT1	(3) RT2
Early elections			
electionyear	-0.041*	-0.039*	-0.037
	(0.016)	(0.016)	(0.020)
electionyear-1	0.036	0.035	0.039
	(0.018)	(0.018)	(0.022)
electionyear-2	0.016	0.013	0.018
	(0.017)	(0.017)	(0.019)
N	31832	31832	30029
Late elections			
electionyear	0.230***	0.239***	0.344***
	(0.054)	(0.054)	(0.061)
electionyear-1	0.159*	0.175*	0.340***
	(0.067)	(0.068)	(0.084)
electionyear-2	0.102	0.109	0.220**
	(0.059)	(0.060)	(0.067)
N	11335	11335	10601

Cluster robust standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Current expenditure

	(1) FD	(2) RT1	(3) RT2
Early elections			
electionyear	-0.001	-0.001	-0.004
	(0.002)	(0.002)	(0.003)
electionyear-1	-0.002	-0.002	-0.001
	(0.002)	(0.002)	(0.003)
electionyear-2	-0.005	-0.005*	0.001
	(0.003)	(0.003)	(0.003)
N	34785	34785	33040
Late elections			
electionyear	0.006	0.007	0.009
	(0.004)	(0.004)	(0.005)
electionyear-1	-0.014**	-0.012**	-0.004
	(0.005)	(0.005)	(0.006)
electionyear-2	-0.015***	-0.013**	-0.005
	(0.004)	(0.004)	(0.006)
N	12155	12155	11543

Cluster robust standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: Reduced sample - 2000 until 2010

	(1) FD	(2) RT1	(3) RT2
	Early elections		
electionyear	-0.011*	-0.011*	-0.012
	(0.005)	(0.005)	(0.007)
electionyear-1	0.023***	0.023***	0.026**
	(0.007)	(0.007)	(0.008)
electionyear-2	0.014*	0.014*	0.019**
	(0.006)	(0.006)	(0.007)
N	17413	17413	15670
	Late elections		
electionyear	0.046***	0.046***	0.040***
	(0.008)	(0.008)	(0.008)
electionyear-1	0.029***	0.029***	0.023*
	(0.008)	(0.008)	(0.009)
electionyear-2	0.006	0.006	0.001
	(0.008)	(0.008)	(0.012)
N	6105	6105	5493

Cluster robust standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

responsibilities as well as incentives for debt shifting through the maastricht regulations on public debt took place between 1995 and 2000, the orignal model is reestimated using a reduced sample with the years 2000 until 2010. The results are found in Table 6 and basically confirm the previously obtained results. It is, however, interesting to see that the effects with the reduced sample produce much clearer and stronger estimates for the early elections. Particularly, the coefficient of $electionyear-2$ is less noisy and significantly different from zero. This provides additional evidence that the electoral cycle may start well before the election year.

7. Conclusion

This paper analyzes the effect of elections on municipal spending in Austria. The findings support the idea that there is an electoral cycle in fiscal policy. Beyond this, the paper concentrates on the dynamics of spending over the legislative term. In contrast to recent suggestions to look for the cycle in monthly data immediately before an election, this paper tests the hypothesis that pre-electoral spending increases may begin well before the election year. This conjecture is backed by the empirical results in this paper.

To get the dynamic specification right, two things appear crucial after reviewing the evidence found in this paper. Firstly, elections taking place early and late during the electoral year need to be treated differently. Not surprisingly, while in the latter case the increased spending coincides with the election year, early elections shift the main spending effort into the pre-electoral year. Secondly, the rich data set from 1990 to 2010, covering more than 10,000

municipal elections allows for very flexible dynamic specifications with respect to spending trends. Taking account of the differences in spending trends relative to the election dates leads to results that collectively confirm the idea of increased spending in the years before an election.

The paper tries to take a new look at an old research question. By looking at a large sample of institutionally rather homogenous municipalities, the paper emphasizes that dynamics are highly important when analyzing a temporal phenomenon like elections. While remaining agnostic about the often suggested conditionality of the political budget cycle on the political and institutional environment, the paper shows that seeming differences between jurisdictions disappear when dynamics are properly accounted for. In light of the new theoretical and empirical insights, replicating existing empirical studies and testing their robustness with respect to more sensible dynamic specifications appears paramount.

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8. Appendix

Table 7: All elections pooled

	(1) FE	(2) FD	(3) RT1	(4) RT2	(5) AB
electionyear	0.010**	0.007	0.008*	0.003	-0.004
	(0.004)	(0.004)	(0.003)	(0.004)	(0.026)
electionyear-1	0.006	0.011**	0.010*	0.013**	0.010
	(0.004)	(0.004)	(0.004)	(0.004)	(0.026)
electionyear-2	-0.006	0.002	0.002	0.012**	0.036
	(0.003)	(0.003)	(0.003)	(0.004)	(0.022)
grants	0.223***	0.074**	0.191***	0.167***	0.313
	(0.049)	(0.024)	(0.046)	(0.042)	(0.246)
taxes	0.259***	0.031***	0.181***	0.085***	0.231**
	(0.008)	(0.002)	(0.008)	(0.011)	(0.088)
debt	0.137***	-0.017***	-0.034***	-0.039***	0.087**
	(0.008)	(0.001)	(0.003)	(0.004)	(0.029)
growth	0.076	0.101	0.141	0.185	-0.291
	(0.068)	(0.083)	(0.087)	(0.150)	(0.677)
majority	-0.004	0.000	-0.009	-0.005	0.135
	(0.008)	(0.002)	(0.005)	(0.005)	(0.141)
partisan	-0.006	-0.005*	0.003	0.003	0.046
	(0.012)	(0.002)	(0.007)	(0.009)	(0.144)
turnout	0.325*	0.118**	0.098	-0.197*	1.324
	(0.145)	(0.040)	(0.097)	(0.094)	(1.806)
mayordirect	0.019	-0.008**	-0.004	-0.033	0.020
	(0.018)	(0.003)	(0.016)	(0.018)	(0.390)
partynumber	-0.003	-0.007***	0.003	0.006*	0.246**
	(0.004)	(0.001)	(0.003)	(0.003)	(0.083)
L.exp					0.350***
					(0.028)
N	28065	28065	28065	28059	25694

Cluster robust standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8: Early elections

	(1) FE	(2) FD	(3) RT1	(4) RT2	(5) AB
electionyear	-0.009	-0.010	-0.008	-0.010	-0.024
	(0.005)	(0.005)	(0.005)	(0.006)	(0.041)
electionyear-1	0.012	0.012*	0.016**	0.019**	-0.009
	(0.007)	(0.006)	(0.006)	(0.006)	(0.037)
electionyear-2	0.000	0.005	0.007	0.013*	-0.003
	(0.006)	(0.006)	(0.005)	(0.006)	(0.027)
grants	0.210***	0.092**	0.181***	0.160***	0.663*
	(0.054)	(0.029)	(0.051)	(0.045)	(0.325)
taxes	0.260***	0.033***	0.193***	0.114***	0.287**
	(0.010)	(0.003)	(0.010)	(0.014)	(0.098)
debt	0.143***	-0.018***	-0.032***	-0.040***	0.118**
	(0.010)	(0.001)	(0.004)	(0.005)	(0.043)
growth	0.046	0.146	0.209	0.241	-0.339
	(0.082)	(0.102)	(0.108)	(0.187)	(0.717)
majority	-0.006	-0.009**	-0.011	-0.005	0.192
	(0.010)	(0.003)	(0.007)	(0.007)	(0.226)
partisan	-0.010	-0.005*	0.012	0.013	0.194
	(0.016)	(0.002)	(0.010)	(0.013)	(0.282)
turnout	0.253	0.103	0.085	-0.150	-0.430
	(0.177)	(0.055)	(0.121)	(0.114)	(2.561)
mayordirect	0.013	-0.027***	-0.010	-0.036*	0.019
	(0.019)	(0.005)	(0.016)	(0.018)	(0.412)
partynumber	-0.004	-0.006***	0.002	0.004	0.258**
	(0.005)	(0.001)	(0.003)	(0.003)	(0.095)
L.exp					0.332***
					(0.026)
N	20874	20874	20874	20868	19119

Cluster robust standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 9: Late elections

	(1) FE	(2) FD	(3) RT1	(4) RT2	(5) AB
electionyear	0.061***	0.053***	0.057***	0.070***	0.045*
	(0.009)	(0.010)	(0.009)	(0.012)	(0.020)
electionyear-1	0.030***	0.044***	0.046***	0.073***	0.025
	(0.009)	(0.009)	(0.009)	(0.015)	(0.022)
electionyear-2	0.012	0.024*	0.028**	0.047***	0.010
	(0.009)	(0.009)	(0.009)	(0.011)	(0.023)
grants	0.316***	0.165*	0.271**	0.238*	0.170
	(0.079)	(0.068)	(0.093)	(0.117)	(0.298)
taxes	0.268***	0.031***	0.155***	0.011	0.262**
	(0.013)	(0.003)	(0.012)	(0.018)	(0.101)
debt	0.120***	-0.015***	-0.037***	-0.031***	0.084***
	(0.010)	(0.002)	(0.006)	(0.007)	(0.025)
growth	-0.004	-0.360*	-0.352*	-0.374	-0.609
	(0.149)	(0.146)	(0.156)	(0.264)	(0.635)
majority	-0.003	0.006	-0.005	-0.003	-0.011
	(0.013)	(0.004)	(0.008)	(0.009)	(0.096)
partisan	-0.004	-0.007*	-0.011	-0.008	0.044
	(0.016)	(0.003)	(0.009)	(0.011)	(0.086)
turnout	0.474*	0.012	0.077	-0.207	-0.007
	(0.205)	(0.080)	(0.134)	(0.164)	(1.068)
partynumber	-0.004	-0.012***	0.008	0.009	0.102
	(0.009)	(0.003)	(0.006)	(0.007)	(0.073)
L.exp					0.347***
					(0.053)
N	7191	7191	7191	7191	6575

Cluster robust standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$