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Timing tariff increases over the electoral cycles : an empirical investigation of political decision making

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Timing tariff increases over the electoral cycles: An empirical investigation of political decision making[☆]

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Abstract

This paper uses data on municipal tariff decisions to analyze the effect of elections on policy choices. Like existing research, the results confirm the existence of a political cycle in fiscal policy. More importantly, this paper shows that standard approaches to the electoral cycle may miss an important point because they imperfectly model the underlying data generating process that is best characterized as a corner solution. As tax and tariff cuts are quite rare, the usual decision faced by a politician is to increase tariffs or not, and if yes, by how much. This paper uses appropriate corner solution models like the hurdle model to analyze this decision structure and finds that this leads to notably stronger effects. In addition, apart from average effects of the electoral cycle, this paper shows the predictive power of the electoral cycle is strongest for small and intermediate tariff increases but less so when analyzing large tariff changes, which are probably the result of other factors. Finally, sensitivity tests show that the size of the estimated effect depends strongly on the specific operationalization of the election cycle variable. Moreover, the paper confirms recent research emphasizing that the intensity of the political cycle is conditional on the institutional setting, notably political accountability and the fragmentation of the city council.

Keywords: Political cycle, Electoral cycle, Tariffs
JEL: D71, D78, H71

1. Introduction

The existence or non-existence of political or electoral cycles has occupied generations of researchers from various disciplines. While some claim that it can "be seen by the naked eye in the raw data" even in highly aggregate quantities like growth (see Grier (2008), p.337.) others are more skeptical (see e.g. Golden and Poterba (1980)) or at least make its existence conditional on the institutional setting (see e.g. Brender and Drazen (2007)). Some of the confusion surrounding the political cycle may be lifted if one considers how research developed. A rough sketch of the evolution of research on the topic could be given by stating that electoral cycles have moved from begin a macro phenomenon to a micro phenomenon. This could be said for both the theoretical as well as the empirical literature. On the theoretical side, the original ideas of Nordhaus (1975), where politicians try to use the phillips-curve relationship to make voters better off and hence gain in popularity, have

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been largely discarded in the favor of models with microfoundation that focus on the decision process of voters and politicians in, very often, a signaling game. Now the theoretical discussion centers around models of adverse selection like Rogoff (1990) or moral hazard/career concern models like Persson and Tabellini (2002). A similar movement can be seen on the empirical side, where the usual search for patterns in aggregate output measures has steadily shifted to analyzing actual policy variables and budget items. This is most clearly spelled out by Rogoff (1990):

"The Keynesian theory has generated a plethora of empirical studies aimed at testing for electoral cycles in national output, unemployment, and inflation. In light of the theoretical weaknesses of the underlying model, perhaps it should not be too surprising that the results have been mixed. The equilibrium political budget cycle theory suggests that it would be more promising to focus empirical research on testing for electoral cycles in taxes, transfers, and government consumption spending."[2]

This shift of focus may also be the explanation why several different definitions of political cycles exist, which are sometimes used synonymous, leading to some confusion. For instance the political business cycle typically refers to Nordhaus-style research, focusing on highly aggregate economic outcomes like growth or currency fluctuations (see e.g. Dreher and Vaubel (2009)). In contrast, the political budget cycle usually implies analysis of items in the government budget, such as tax revenues, public debt or spending (see Ohlsson and Vredin (1996), Persson and Tabellini (2003), Brender and Drazen (2007) or Streb et al. (2009) for papers analyzing all three of them). Since its shift away from aggregate output measures the analysis and search for political or bureaucratic cycles has flourished in various directions and ranges from cycles in awarded government contracts (see Mayer (1995)) to cycles in road construction (see Khemani (2004)) and even cycles in non-democratic and authoritarian regimes (see e.g. Guo (2009)). Thus in this paper, the general terms political cycle and electoral cycle are used (synonymously) to indicate any political intervention in policy instruments that are related to re-election seeking behavior on the side of the politician.

Unlike more exotic applications of the electoral cycle, this paper chooses local taxes and tariffs as object of analysis - an area already been prescribed above by Rogoff (1990). It is interesting to note that there is a wide research gap on the tax side although as argued by Mikesell (1978) taxes are a highly visible budget component and tax changes equally unpopular with the electorate as with the politicians who try to avoid them:

"Thus, political bodies would much prefer to budget out of the expanded revenues provided by an income elastic tax structure, relying on a 'fiscal illusion' to conceal the increased effective rates, but avoiding the necessity of increasing statutory rates."[3]

And although a number of papers analyzes tax revenues, the full potential from the tax side has not been uncovered yet because looking at tax and tariff changes directly strips the policy variable of all noise that is bothering contemporary empirical research. Very often the empirical procedures used to test for political cycles involve several steps. These approaches often include filtering techniques like the Hodrick-Prescott filter and eventually a full blown dynamic panel estimator like difference or

[2] See Rogoff (1990), p.33.
[3] See Mikesell (1978), p.99.

system GMM to take account of the bias introduced by fixed effects and a lagged dependent variable. Many of these measures are taken to deal with the manifold sources of noise in the data. Looking at tax rates or tariffs is therefore not only interesting because it has hardly been analyzed before, but also because they can offer a much deeper and clearer insight into the actual mechanisms through which the electoral cycle affects public policy.[4] In Tufte's murder-mystery analogy, analyzing tariff changes brings us closer to the policy instrument, i.e. the 'murder weapon'.

The dataset used in this study, changes in water tariffs by Austrian municipalities, is described in detail over the next sections. But one important empirical feature is that the tariffs just like taxes typically evolve in a manner that looks like a step-function that changes infrequently. As can be seen for instance in Nelson (2000), state taxes changed very infrequently during the postwar period in the U.S.[5] This empirical feature of the data is not only relevant because it raises some econometric issues but also because it is interesting to look at the two decisions the politician faces separately, e.g. using a hurdle model. Such models typically imagine a two-part process, where a politician first chooses whether to increase or not and if the answer is yes, the politician decides on the amount of the increase. For instance, if citizens only observe that tariffs have changed but have no clear knowledge about the size of the increase – as indicated by Eurobarometer survey (see Directorate General Health and Consumer Protection (2003)) – the electoral cycle should affect only the decision to increase tariffs but not the amount by which tariffs increase. We would therefore expect politicians to decrease the frequency of tariff increases and instead favor larger but fewer changes. Methods relating to corner solution problems allow these two decisions to be determined differently and are used later on in the paper. As we will see, this opens up the possibility for new and interesting insights on the mechanism that relates elections to policy instruments.

To increase comparability with the existing literature, this paper uses a wide range of operationalizations for the political cycle. The results indicate that the choice of indicator has a large impact on the size of the cycle – a point that did not receive enough attention in the past. Moreover, a series of political and institutional variables is interacted with the cycle to test for possible conditionalities of the electoral cycle. The recent literature has emphasized that specific institutional settings may increase or decrease the magnitude of the cycle (see e.g. Persson and Tabellini (2003),Akhmedov and Zhuravskaya (2004), Shi and Svensson (2006) or Brender and Drazen (2007)).

The contribution of this paper is twofold. Firstly, it is an in depth analysis of the electoral cycle in tariff setting in Austria. The wide range of robustness, sensitivity and tests for conditionalities should make the reader confident that there is some strong evidence of a political budget cycle. Secondly, the focus on actual political decisions in the form of tariff changes is an innovation of this paper. While this step solves some empirical problems and creates new challenges for statistical analysis, the main advantage is that new insights are possible by focusing on actual policy changes, which are

[4] The only paper known to the author that descriptively analyzes the effect of elections on tax rate changes is Nelson (2000).

[5] Implicitly, this data feature is also present in other policy variables because the underlying regulations don't change very often, but it is buried under the noise present in many aggregate budget items. E.g. social security benefits may change very rarely but this cannot easily be observed by looking at social spending which is affected by many other things like unemployment or growth.

affected by no-one else than the politician seeking re-election.

In the next section, the necessary institutional background is presented along with some insightful descriptive statistics motivating the empirical approach chosen in section 3. The empirical strategy in section 3 has three parts after an initial outline of the variables and methodology. Finally, section 4 concludes.

2. Institutional and empirical background

2.1. Water tariffs as a political instrument

The main source of finance for water distribution in Austria are tariffs. Since there is no competition on this market and connection to the municipal water system is mandatory, water tariffs can be considered a local tax. Austrian municipalities typically tax water consumption by charging a two-part fee, consisting of a fixed part and a variable part depending on the amount of water consumed. Water tariffs are determined by the municipal government in the city council by enacting a by-law. There is large heterogeneity between municipalities regarding the number of changes in water tariffs. While some municipalities change tariffs almost every year, others refrain from increasing tariffs for over a decade. There is no regulatory agency supervising water prices in Austria but only a federal law, which specifies that revenues from public services are allowed to be twice total cost (including operation, construction, interest and amortization).[6] This peculiar regulatory setting gives municipalities considerable leeway regarding price setting. In Tufte's murder-mystery analogy, local politicians in Austria have ample 'opportunity' for fiscal electioneering because they control a number of policy instrument (see Tufte (1980)). Because other sources of revenues are either not controllable or already exhausted, revenues from local services (17.4% of total revenues) are an alterable and important source of municipal finance.[7] Thus local politicians not only control public service tariffs but since the associated revenue component is indispensable, politicians cannot afford to keep tariffs low for an extended period. The finding that water prices increased much stronger than average inflation over the sample period analyzed below is an interesting point in this respect. The fiscal pressure for politicians to increase tariffs at some point is certainly helpful for identification of the electoral cycle.

Finally, Tufte's 'motive' for politicians manipulating public service tariffs relates to the visibility of water tariffs. Tariffs are interesting for a politician because compared to other (local) taxes they change more frequently. In the sample analyzed below, on average municipalities increased prices more than three times between 1993 and 2006. In addition, water tariff changes basically affect the whole local constituency since they have to be paid by every household, which are typically billed four times a year. The 'motive' for manipulating water tariffs may be especially strong compared to other services because water is often considered a basic right and not so much a good. Moreover, that political acceptability is a main driver behind tariff-setting decisions is appearantly not only true for Austria, but applies to other countries as well, as argued for instance by R. Martínez-Espineira

[6] See Finanzausgleichsgesetz 2008, Art. 1 § 15

[7] The most important sources of finance to an Austrian municipality are shares from the fiscal equalization scheme (33%), local taxes, e.g. on business and property (16.7%) and tariffs for public services (17.4%). See Statistik Austria (2008).

and González-Gómez (2009) for the case of Spain.

2.2. Data and descriptive analysis

The dataset from Statistik Austria (2007) comprises 74 municipalities from 1993 to 2006, all of which have a population over 10,000. There were 173 elections in the sample period, on average 2.34 per municipality. As provinces in Austria have different electoral schedules for municipal elections, it is possible to differentiate year effects from actual election effects because only a subsample of municipalities votes in a given year. The dependent variable in the following empirical analysis is $tariff$, which denotes the annual water tariff for a representative household.[8] This measure is used instead of sole cubic meter tariffs in order to account for the two-part tariffs, which are typically composed of a fixed annual part and a variable part depending on the individual amount of water consumed.[9] In the whole sample of 934 observations water tariffs increased 376 times.[10] The overall average water tariff was 147.6 euro and the average increase (i.e. including the zeros) was 5.25 euro. Interestingly, water tariffs increased much stronger than average inflation over the sample period, with roughly 54% compared to roughly 30% for consumer prices (See Statistik Austria (2008)). Summary statistics and pairwise correlations are presented in Table 1 and Table 2.

Table 1: Summary statistics

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**
tariff	147.557	44.609	56.18	273.84
tariffchange	0.403	0.491	0	1
dtariff	5.245	9.614	0	94.5
election_dummy	0.185	0.389	0	1
majority	0.625	0.484	0	1
winmargin	29.842	15.145	2.5	74.194
fraggov	4.316	0.6	3	5
directmayor	0.478	0.5	0	1
longcycle	0.409	0.492	0	1
partisan	40.303	18.515	4.762	83.871
debt	1.486	0.851	0.133	7.045
pop	0.472	1.823	0.017	15.501
area	0.658	0.689	0.045	4.147
N		934		

To get a first glance at the relationship between the election cycle and water tariffs, Figures 1 shows the number of price increases for each period of the electoral cycle.[11] The electoral cycle is defined as the number of years until the next election, with 0 indicating an election year. Depending upon the province, municipalities vote every five or six years according to a fixed schedule. Therefore, Figure 1 exhibits two panels, one for each of these two groups. The graph highlights a few interesting patterns regarding price changes over the electoral cycle. To start with, a large number of

[8] A representative household is presumed to consume 150m^3 per year on average.

[9] See for example Chong et al. (2006).

[10] The periods in which tariffs decreased (24 observations) were deleted in order to facilitate the interpretation. While only 3 of them were above 2 euros – all of them in or one period before an election – most of the changes resulted from rounding prices during the conversion from Schilling to Euros. The results are, however, insensitive toward inclusion of these variables.

[11] Looking at the amount of price increases instead of the sole number leads to an almost identical figure.

Table 2: Cross-correlation table

Variables Variables	tariff	tariff-change	dtariff	election-dummy	majority	win-margin	fraggov	direct-mayor	long-cycle	partisan	debt	pop	area
tariff	1.00												
tariffchange	0.01	1.00											
dtariff	0.07	0.66	1.00										
election_dummy	-0.01	-0.16	-0.18	1.00									
majority	-0.00	-0.05	-0.07	0.04	1.00								
winmargin	0.06	-0.07	-0.07	0.05	0.76	1.00							
fraggov	0.14	-0.01	0.01	0.08	-0.06	0.07	1.00						
directmayor	-0.21	0.02	-0.07	0.01	-0.19	-0.34	-0.32	1.00					
longcycle	-0.13	0.05	0.05	-0.09	-0.34	-0.35	-0.20	0.56	1.00				
partisan	0.10	0.00	0.06	-0.01	0.28	0.29	-0.10	-0.11	0.05	1.00			
debt	0.19	-0.06	-0.01	0.03	0.13	0.11	0.25	-0.36	-0.37	0.07	1.00		
pop	0.19	-0.06	-0.02	-0.01	-0.05	-0.08	0.06	-0.12	-0.08	0.05	0.04	1.00	
area	0.21	-0.09	-0.03	-0.01	0.07	0.02	0.00	-0.09	-0.00	0.03	0.04	0.63	1.00

price increases is bunched just after the election period, most clearly visible in the 5-year cycle. This quick adjustment of tariffs after elections may be interpreted as a sign that politicians try to take advantage of voter myopia by maximizing the distance between unpopular policy choices and elections. On the other hand, tariff increases which would have been due before the election may have simply been postponed and fiscal pressure does not allow further deference. The voter myopia interpretation is also somewhat weakened by the fact that the number of increases does not decline continuously as the election year approaches. Especially the panel with the 5-year cycle reveals that the idea of a single peaked electoral cycle is not supported by the descriptive analysis. Thus although the graph clearly indicates an electoral cycle in the sense that tariffs are changed less frequently in or immediately before election periods, the behavior in non election years is much more complicated. This is an important finding for the following regression analysis because it appears that the way we operationalize the electoral cycle may have a large impact on the estimated effects. What we would ideally like to measure in the sense of a causal effect of elections on tariffs is the difference between a politically sensitive period and a politically insensitive period. Using a simple dummy variable for the election year may not give us a very precise nor a very interesting estimate of that effect.

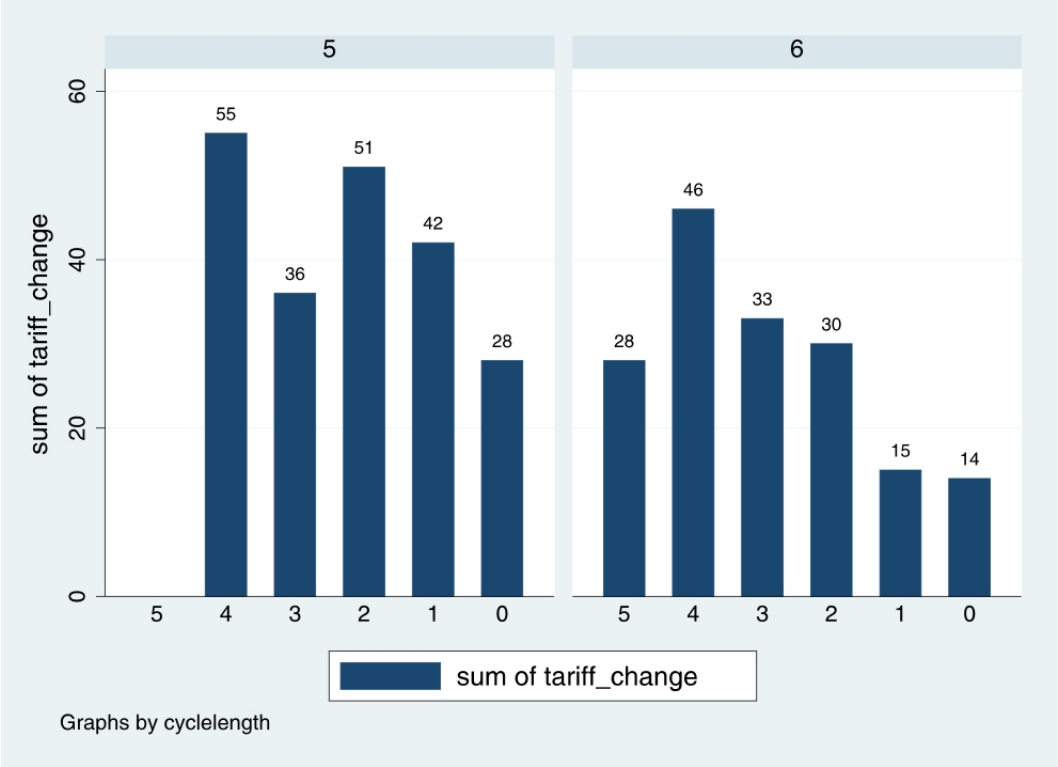

Figure 1: Tariff change over the electoral cycle

To elaborate a bit further on the patterns associated to tariff setting, it is helpful to look at disaggregated data for individual municipalities. Figure 2 shows the evolution of water tariffs for four municipalities from the sample and the vertical red lines indicate election periods. While the specific municipalities have been chosen deliberately for presentation purposes, there are many more

municipalities which exhibit a similar, tariff development.[12] The single most striking feature about Figure 2 is that the behavior of municipalities in tariff setting is best described by a step function. The municipalities exhibited in the four panels change tariffs quite seldom, usually once between two elections. If a municipality increases tariffs, typically shortly after the election, the increase is rather substantial and can easily surpass 10%. While these graphs are at best suggestive evidence of an electoral cycle, two points are worth noting irrespective of how one interprets Figure 2. First, some, arguably many, Austrian municipalities choose to increase tariffs substantially for a few times instead of a countinuous increase. While there may be practical and administrative reasons for such behavior, this pattern is also consistent with the idea that there is a fixed political cost to every tariff increase. If increasing public good tariffs is unpopular and always to some extent punished by voters, it may be optimal for politicians to increase tariffs less frequently but by a greater amount. Such fixed political costs would indicate that voters may be somewhat myopic, irrational or at least asymmetric in their judgement about a 10% increase compared to two increases of 5% each. An interesting example in this respect is the city of Vienna, which enacted a so called 'valorization-law' (Valorisierungsgesetz) in 2007.[13] This law links public service prices to official inflation statistics and would therefore lead to an automatic annual increase in tariffs. The law has, however, to pass the city council every year, which has chosen repeatedly not to apply the law since its introduction in 2007 until 2011 - just one year after the election in 2010. Now prices are increased for the first time since 1995 in Vienna, by 33%. This example highlights that politicians may prefer a one-off unpopular move compared to a series of tariff increases, even if they could try to shift blame to an automated mechanism. The tariff setting behavior in the Austrian water sector therefore seems to be a prime example for the 'stickyness' of user charges, as describes for public services in general by Bird and Tsiopoulos (1997).

Second and related, the figure shows many periods where tariffs do not change because the choice by the agent is zero. This is different from the notion in much of the empirical literature on the political budget cycle that uses a lagged dependent variable because policy instruments exhibit a great deal of inertia.[14] Unlike most other empirical papers on electoral cycles using aggregate data, the dataset employed in this work shows the actual political decisions. Here the politicians really choose not to increase tariffs and these decisions are an important part of the following analysis, where the empirical situation is analyzed as a corner solution problem.

3. Econometric Analysis

3.1. Variables and Methodology

The basic model to be estimated is

$$\Delta tariff_{it} = election_{it}\beta + X_{it}\gamma + \epsilon_{it} \tag{1}$$

[12] It should be noted, however, that there are municipalities in the sample which show a much more continuous evolution of tariffs. Some of them increase prices quite frequently and the relation of their tariff changes to elections is far from clear.

[13] Note that periods after 2006 are not in the sample anymore.

[14] E.g. aggregate tax revenues or spending components are highly autoregressive quantities, which evolve very slowly over time.

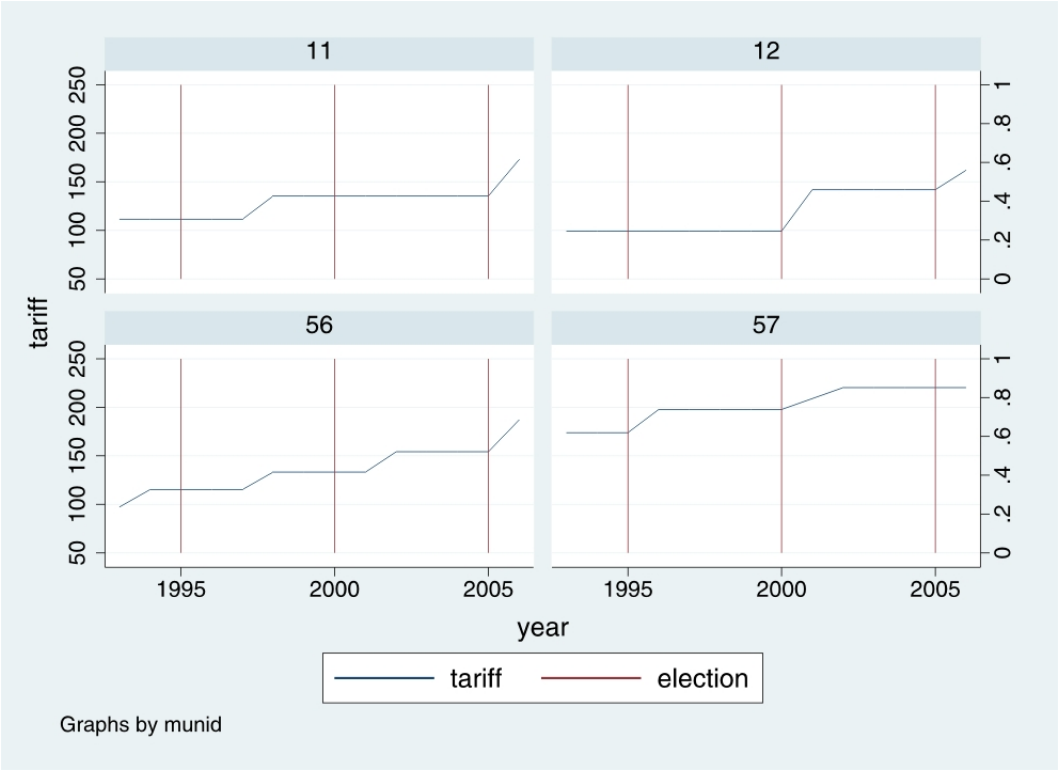

Figure 2: Examples of step functions

where $\Delta tariff$ is the change in water tariffs, $election$ is, for now, a dummy variable which is one in an election year, and X is a set of control variables. The control variables contain a number of political variables, municipal characteristics and year fixed effects. $Majority$ is a dummy variable that indicates if a party controls more than 50% of the council seats in a municipality, $winmargin$ refers to the difference (in %) of council seats between the strongest and the second strongest party. $Fraggov$ is an indicator variable for political fragmentation and counts the number of parties represented in the city council.[15] Depending on the province in which a municipality is situated, the mayor can be elected directly by the local constituency or is chosen by the elected members of the city council. Municipalities with directly elected mayors are identified by the dummy $directmayor$. To control for the different length of legislative terms, 5 or 6 years, a dummy $longcycle$ is used to identify municipalities with 6 year legislative terms. Ideological differences are covered by $partisan$, indicating if the strongest municipal party is a left wing party. $Debt$ is used to control for the fiscal stance of a municipality. In addition, pop and $area$ signify the population and area (in square kilometer) of a municipality. The estimation results for the electoral cycle are, however, highly insensitive to the included controls and remain almost unchanged when no control variables enter equation (1).

The empirical strategy will proceed in three steps. First, the next section will establish the baseline of this papers empirical analysis. The main goal is to provide evidence of an electoral cycle in the

[15]The variable is somewhat upward cencored because it accounts for the presence of the four established political parties in Austria - SPÖ, ÖVP, FPÖ, Grüne - and if at least one additional (local) party is present in the city council. Thus the variable can only vary from 1 to 5, even if more than 5 parties are actually present in the council.

sample of water tariffs at hand. A number of different models will be used to show the results from the simple model in (1) are robust to other, arguably more sophisticated econometric specifications. Most obviously, one may wish to incorporate the panel structure of the data into the model. This could be done by using a first difference (FD) or fixed effects estimator (FE) to purge unobserved time-invariant municipal characteristics

$$\Delta(\Delta tariff_{it}) = \Delta(election_{it})\beta + \Delta(X_{it})\gamma + \epsilon_{it} \tag{2}$$

$$\Delta tariff_{it} = election_{it}\beta + X_{it}\gamma + \alpha_i + \epsilon_{it} \tag{3}$$

In addition and to allow for a dynamic data generating process, the model could be enriched by a lagged dependent variable. Thus if the inertia in the dependent variable is considered problematic because tariffs are not changed every period, the following models could be estimated

$$\Delta tariff_{it} = \Delta tariff_{i,t-1}\rho + election_{it}\beta + X_{it}\gamma + \epsilon_{it} \tag{4}$$

$$\Delta tariff_{it} = \Delta tariff_{i,t-1}\rho + election_{it}\beta + X_{it}\gamma + \alpha_i + \epsilon_{it} \tag{5}$$

where the model in (4) is a pooled model with a lagged dependent variable (LDV) and (5) is a model with both a LDV and FE. As is well known, OLS estimation of the model in (5) leads to an inconsistent estimate due to the so-called Nickel-bias, especially in panels with a short time dimension. Consistent estimation is typically achieved by applying 'difference GMM' (see Arellano and Bond (1991)) or 'system GMM' (see Blundell and Bond (1998)), which use internal instruments to correct the bias. Since dynamic panel models like difference GMM are probably the most popular models for analyzing electoral cycles in recent years, it will be interesting to compare the basic model in (1) with the workhorse model of contemporary research on the topic.

The second part of the empirical analysis focuses more strongly on the underlying decision process when politicians determine tariffs. As noted at the end of the previous section, the dataset at hand gives us direct information on tariff decisions, i.e. we observe if a tariff was increased or stayed the same and if it was increased, by how much. Given this feature of the data, where a lot of observations are zero (when looking at changes of tariffs) the empirical situation can be interpreted as a corner solution problem. This class of models (see below and Wooldridge (2010) chapter 17 for an overview) tries to overcome problems associated to estimating a linear model like (1) for an inherently non-linear relationship. Apart from the econometric issues, such an approach is interesting because they allow to separately analyze the probability of a tariff increase and the amount decision if the agent has chosen to increase tariffs. The amount decision is often estimated conditional-on-positive, which means that only those observations are included for which $tariffchange == 1$. The logic behind this step is best explained in terms of the hurdle model, where it is assumed that agents basically have to make two choices. First, they have to choose whether to participate, i.e. increase tariffs or not. Second and only if the first hurdle has been passed, the participating agent has to choose about the quantity. For instance, in the underlying sample the mean change in tariffs is 5.2 euro when all observations are included. If we look only at those observations when tariffs actually

increased, the average increase is more than twice as large and amounts to 13.0 euro. Therefore both, the effect of the election cycle on the probability to increase tariffs (participation decision) as well as on the actual change in tariffs (amount decision), will be estimated separately. To start with, the participation decision is estimated by OLS or probit

$$tariffchange_{it} = election_{it}\beta + X_{it}\gamma + \epsilon_{it} \tag{6}$$

which is the same as the baseline equation (1) except that in this case the dependent variable ($tariffchange$) is a dummy variable indicating whether tariffs have changes or not. In the amount decision, it is important how a zero, i.e. observations where tariffs stay the same, are interpreted. While standard OLS as in equation (1) takes the zeros at face value, approaches such as the above described hurdle model try to take account of the large number of zeros by incorporating the participation decision in some way. Two very popular approaches in this respect are the Tobit model (in the classification of Wooldridge (2010) the 'type I Tobit model') and the two-part model (2PM). Both approaches can be described in a latent variable framework, where in contrast to actual censoring, the latent variable (y^*) has no interpretation. E.g. in the underlying case we cannot think of a change in tariff if the municipality chooses not to change tariffs. Since there is no meaningful counterfactual in this setting, it means that we are not confronted with a selection problem but simply a non-linear outcome variable. The 2PM relaxes some of the assumptions of the type I Tobit model by estimating the participation and the amount decision simply independent. To ensure that the outcome is nevertheless positive, assumptions like a lognormal distribution of the latent variable are used.

Especially the 2PM is interesting for the underlying situation because it allows the covariates to have a different impact on the participation and the amount decision. For instance, the decision to increase tariffs may be affected by other factors than the amount decision. This flexibility is possible in the 2PM, where e.g. the electoral cycle may affect the decision to change tariffs or not, while once this hurdle has been passed the presence of an election year does not matter for the actual amount the tariff changes. Such a result would be a strong indication of large fixed political costs for the politician when increasing tariffs. Thus, the basic idea here is to disentangle the decision process surrounding tariff changes.

Because some researchers (see Angrist and Pischke (2008)) have raised doubts about the causal interpretation of so-called conditional on positive estimations like Tobit or the 2PM, the paper follows Angrist (2001) and additionally analyzes the effect of the election cycle on the distribution of tariff increases. The basic motivation for approaches like the 2PM is that analyzing the effect on averages, in cases where it is observed that the distribution of the dependent variable has a mass point at zero, may miss important points. Thus, as recommended by Angrist and Pischke (2008), instead of modeling two processes, one can also simply look directly at effects on the distribution by estimating the probability that a tariff increase falls into some specified interval, like $\Delta tariff > 0, > 10, > 20$. This approach may help to understand how electoral cycles affect the probability of observing tariff changes of different sizes.

The third and final empirical part tries to assess the sensitivity of the empirical results with respect to the chosen operationalization for the electoral cycle and potential conditionalities. A number of

different variables to capture the electoral cycle have been proposed by the empirical literature and their performance in the underlying sample is evaluated. The most common indicator used in empirical studies of electoral cycles is a simple dummy indicating the election year, such as the variable $election$. In addition, some authors have added a post-election dummy, to indicate the period after an election (see e.g. Persson and Tabellini (2003) or Alt and Lassen (2006)). These two dummies together are thought to give a more complete picture of the cycle, with an opportunistic move before the election and an adjustment afterward.[16] Another criticism of the simple election year dummy approach argues that it is important at what time of the year an election takes place. An election year dummy does not differentiate between an election that takes place in January or December despite the fact that this may shift the opportunistic behavior and possibly also the adjustment process after an election. Therefore Franzese Jr (2000) suggested a variable that takes on $M/12$ in the election year and $1 - (M/12)$ in the pre-election year, where M is the month in which the election takes place. An election in February would therefore be much more important for the pre-election year (=0.83) than for the election year itself (=0.17). Since politicians have to time opportunistic policies as to gain popular support or at least avoid unpopular moves, this definition should more precisely be able to track the time periods affected by elections. In a similar vein, Brender and Drazen (2007) try to differentiate elections, which take place early in the year from those that take place late in the year and split the election dummy into two dummy variables. Finally, since not only the periods in or immediately around elections may be affected by the electoral cycle, some researchers have used complete parameterizations of the cycle, either using dummies for every period over the cycle (See e.g. Stein and Streb (2004)) or a single ordinal variable that indicates the distance from/since the last election (see e.g. Grier (2008)).

The second part of the sensitivity analysis deal with conditionalities of the political cycle. Because recent research forcefully argues that political cycles are conditional on (political) institutions, a series of potential determinants of cycle intensity are tested. Specifically, the election indicator is interacted with a range of political variables that may affect its intensity. For instance, Aidt et al. (2011) find that incumbents with small win-margins behave more opportunistically. I therefore test this possible conditionality by interacting the political cycle variable with $winmargin$. In addition, because politicians may have less leeway to use policy instruments in a coalition – the absence of a majority – the electoral cycle variable may change when it is interacted with $majority$. The study by Geys (2007), however, indicates that coalition governments lead to a larger cycle. A related point regarding the possibility for a politician to behave opportunistically is made by Streb et al. (2009) who find that checks and balances decrease the electoral cycle. Although the institutional checks and balances are very similar throughout Austria, the number of parties in the city council may increase government monitoring. For this reason the variable $fraggov$ is interacted with the cycle variable. As some literature (see e.g. Persson and Tabellini (2003)) has argued that higher accountability in majoritarian systems affects the cycle, directly elected mayors (indicated by the variable $directmayor$) may as well behave differently. Finally, $longcycle$ and $partisan$ is also interacted with the cycle variable because we cannot rule a change in the cycle intensity when the legislative term is longer or when a left wing government is in control.

[16] Interestingly Alt and Lassen (2006) add the estimated coefficients for the two variables to calculate the cycle effect.

Except when indicated otherwise, all regressions are estimated using heteroscedasticity and cluster-robust standard errors. Therefore, standard errors are fully robust with respect to arbitrary serial correlation within municipalities as well as general heteroscedasticity (see Stock and Watson (2008)).

3.2. Baseline results

Table 3: Baseline

	OLS	FD	FE	LDV	ABond
L.dprice				-0.055*	-0.116***
				0.030	0.036
election	-4.746***	-4.199***	-4.675***	-4.994***	-4.744***
	0.745	0.760	0.778	0.724	0.747
N	934	834	934	837	739

Cluster and heteroscedasticity robust standard errors in parentheses, except for column (5) with only robust standard errors.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Covariates election, majority, winmargin, fraggov, directmayor, longcycle, partisan, debt, pop and area included. Difference GMM estimator in column (5) is estimated using the first, second and third lags as instruments.

The results in Table 3 correspond to the models described in equations 1 to 5. For presentation purposes, the covariates X are omitted from the table here, but the full table can be found in the appendix. The estimated effect of the dummy variable $election$ ranges between -4 and -5 euros. With an average tariff of roughly 150 euros, the estimated effect of an election year ranges between 3 and 4 percent. It is remarkable how stable the estimated effect, not only statistically but also in magnitude, is for the various specifications displayed in Table 3. While the OLS estimate is somewhere in the middle, the FD estimator leads to the lowest and the LDV estimator to the highest election year effect. Turning to the difference GMM estimator in column (5) it appears that adding a LDV and FE to the simple OLS regression does not change results much.[17] A first indication that the underlying data is different from the aggregate quantities usually analyzed in the electoral cycle literature can be found in the coefficient of the LDV. The coefficient is not only much smaller than when analyzing revenue or spending data, but it is negative. This is very likely the result of the step function patterns in the evolution of tariffs. If there was an increase in the last period, it is very likely that there will be no increase in the next period, which is indicated by the negative coefficient of the LDV. Thus analyzing tariff decisions themselves rather than the outcome of these decisions may not only affect the way we model the DGP but could also be used to gain additional insights. And it is precisely these tariff decisions that are analyzed in more detail in the next part of the empirical analysis.

3.3. Participation decision, amount decision and distributional effects

The first two columns in Table 4 show the results of estimating the model in equation (6), the participation decision, by OLS and Probit.[18] Both models lead to rather similar results, indicating that the probability of a tariff increase is roughly 20% lower in an election year than in a non-election year. The more interesting part of Table 4 shows the effect of an election year on the amount tariffs

[17] We arrive at the same conclusion if we were to use the absolute value of $tariff$ instead of $\Delta tariff$.

[18] Again the full Table can be found in the appendix.

Table 4: Participation and amount

	Participation		Amount				
	OLS	Probit	OLS	Tobit	2PM Log-Lin	2PM Trunc-Normal	ET2TM
election	-0.193***	-0.204***	-4.746***	-3.862***	-9.719***	-11.587***	-8.938***
	(0.029)	(0.032)	(0.745)	(0.585)	(2.981)	(3.013)	(2.999)
N	934	934	934	934	376	376	934

Cluster and heteroscedasticity robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Covariates election, majority, winmargin, fraggov, directmayor, longcycle, partisan, debt, pop and area included. The coefficients in column (4) (Tobit) and (5) (2PM) are average marginal effects conditional on $\Delta tariff > 0$

are increase, exhibited in columns (3) to (7). For comparison, column (3) shows the OLS result, which were already obtained in Table 3. As shown in column (4) the marginal effect of $election$ is -3.9 euro in the Tobit model and therefore somewhat slightly smaller than OLS. That the average marginal effect from the Tobit model are typically close to OLS is not surprising. It relates to the fact that the Tobit model not only requires the coefficients of both the participation and the amount decision to be the same but also that explanatory variables have the same relative effects in both parts. This is not the case for the 2PM estimated in column (5) and (6), where a log-normal and a truncated normal specification were chosen. The reported effects are again average marginal effects, here also conditional on $\Delta tariff > 0$. Depending on the chosen distribution, the 2PM's on the sub-sample of observations with a tariff change larger than zero lead to an estimated effect of the election year of -9.7 and -11.6 euro, which is more than twice as large as the results from OLS. Thus, if we interpret tariff setting in terms of a hurdle model, we find that election years affect both the decision to increase tariffs but also the effect of how much to increase tariffs. The results from the 2PM's indicate that once the decision to increase tariffs was made, the average difference between election and non-election periods will be around 10 euros. Finally, the last column shows the so-called 'Exponential Type II Tobit Model', which is a further generalization because it allows for correlation between the participation and amount decision. Such a correlation could for instance be the result of unobserved factors that affect both decisions. The results are similar to the 2PM and indicate that if we observe an increase in price, the average increase is roughly 9 euros smaller during an election year.

Table 5: Distributional effects of election

Range		0	0<y≤5	5<y≤10	10<y≤20	20<y≤30	30<y
		(.597)	(.105)	(.089)	(.122)	(.054)	(.033)
$\Delta tariff$	OLS	0.193***	0.043*	-0.065***	-0.085***	-0.040***	-0.045***
		(0.029)	(0.024)	(0.022)	(0.020)	(0.015)	(0.013)
	Probit	0.204***	0.043**	-0.069***	-0.119***	-0.077***	-0.071***
		(0.032)	(0.021)	(0.025)	(0.038)	(0.030)	(0.027)
$\Delta tariffm3$	OLS	0.215***	-0.004	-0.050**	-0.087***	-0.038**	-0.035***
		(0.032)	(0.019)	(0.022)	(0.021)	(0.015)	(0.012)
	Probit	0.233***	-0.002	-0.051**	-0.126***	-0.069**	-0.052**
		(0.037)	(0.017)	(0.023)	(0.040)	(0.031)	(0.022)

Cluster and heteroscedasticity robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Covariates election, majority, winmargin, fraggov, directmayor, longcycle, partisan, debt, pop and area included but omitted from the table.

As noted above, some researchers have raised doubts about the causal interpretation of conditional-on-positive models like the 2PM. Angrist and Pischke (2008) suggest looking at the distribution of effects instead. Hence, Table 5 presents the results of OLS and probit estimations on the distribution of tariff increases. E.g. the first column of Table 5 exhibits the effect of the election year dummy on the probability that a tariff increase is zero while the second column shows the effect on the probability that a tariff increase is in the interval $5 \geq y > 0$. Regarding the first two rows in Table 5, it can be seen that qualitatively OLS and probit produce a similar pattern. The effect of an election year in the first interval is statistically significant and positive around 0.2 and simply means that the probability of observing a tariff increase of 0 increases in an election year. When moving up along the intervals the coefficient of the election year increases in magnitude (negatively) and indicates that election years decrease the probability of observing larger increases in tariffs. Interestingly, and this highlights the virtue of looking at the distribution of effects, the coefficient increases after the fourth interval (i.e. when $y > 20$). This finding states that the effect of the election cycle decreases when we look at very large tariff changes only. It is quite surprising that very large tariff increases are not timed as opportunistically as small or medium sized increases. Other factors such as equity considerations but also interest group pressure may be driving this result. A second surprising finding is that the interval $5 \geq y > 0$ has a positive coefficient, implying that the presence of election years increases the probability of observing not only zero increases but also small increases up until 5 euros. When looking at the raw data one can see, however, that a large part of the increases between 0 and 5 euros are due to rounding differences or changes in the fixed component of the water tariff. If the fixed component is excluded from our tariff measure and therefore only account for changes in the consumption related price (rows 3 and 4 of Table 5), the positive and significant effect in the second interval disappears.[19] The overall pattern in the distributional effects, however, remains and shows that the electoral cycle has its strongest (negative) impact in the interval between 10 and 20 euro, which corresponds to increases between 7 and 14%.

3.4. Sensitivity to election cycle definition and Conditionalities

The final section of the empirical analysis looks at the sensitivity of the previous results with respect to the various different variables that can be used to model the electoral cycle and potential conditionalities. To analyze the former, Table 6 and Table 7 show the effect of different indicators for the election cycle.[20] Given the findings from the previous section, i.e. that there may be differences between the decision whether to increase tariffs or not and the decision regarding the amount of the increase, Table 6 features the participation decision and Table 7 the amount decision. Both decisions are estimated with OLS and the only marked difference between the two tables is that the post election dummy in column (2) is significant only in the amount equation. Other than that, the results are qualitatively very similar. The first column in both tables exhibits the results using the simply election year dummy that was used in all estimations before. If a post election dummy is added, some signs of adjustment after an election are indicated by the positive coefficient. Calculating a compound cycle measure like Alt and Lassen (2006) by adding the absolute value of two coefficients would lead to a much larger cycle, which may correspond more closely to what people

[19] Just as in the original measure of water tariffs, a fictive household that consumes 150 cubic meters, it is necessary to multiply the cubic meter tariff by 150 to be comparable to the original intervals.

[20] The full tables including control variables can be found in the appendix.

Table 6: Sensitivity to election cycle definition: Participation OLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
election	-0.193***	-0.185***					
	(0.029)	(0.029)					
post		0.042					
		(0.044)					
election_month			-0.279***				
			(0.047)				
early				-0.185***			
				(0.036)			
late				-0.216***			
				(0.072)			
years_to_election					0.049***		
					(0.009)		
min_distance						0.074***	
						(0.016)	
T-1							0.079*
							(0.045)
T-2							0.242***
							(0.041)
T-3							0.167***
							(0.049)
T-4							0.248***
							(0.048)
T-5							0.196***
							(0.073)
N	934	934	931	934	934	934	934

Cluster and heteroscedasticity robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Covariates election, majority, winmargin, fraggov, directmayor, longcycle, partisan, debt, pop and area included.

Table 7: Sensitivity to election cycle definition: Amount OLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
election	-4.746***	-4.372***					
	(0.745)	(0.794)					
post		2.100**					
		(0.867)					
election_month			-6.375***				
			(0.944)				
early				-4.842***			
				(0.913)			
late				-4.453***			
				(0.836)			
years_to_election					1.332***		
					(0.198)		
min_distance						1.617***	
						(0.342)	
T-1							2.057**
							(0.887)
T-2							5.405***
							(0.990)
T-3							4.095***
							(1.030)
T-4							6.405***
							(1.190)
T-5							6.001***
							(1.281)
N	934	934	931	934	934	934	934

Cluster and heteroscedasticity robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Covariates election, majority, winmargin, fraggov, directmayor, longcycle, partisan, debt, pop and area included.

interpret as the electoral cycle, the total distance between the opportunistic behavior before elections and the adjustment afterward. Columns (3) and (4) try to tackle the problem that election not always occur at the end of the year. The $election_month$ indicator by Franzese Jr (2000) seems to capture this feature quite well and increases the magnitude of the estimated cycle effect by roughly 50%. Separating the election year dummy into two indicators for $early$ and $late$ elections does not seem to be able to tackle to problem as both coefficients in column (4) are quite similar to each other and also the ordinary election year dummy estimate. Finally, incorporating more periods than those in or around the election year into the electoral cycle variable(s) seems to support the idea, that elections affect the tariff setting behavior over the whole legislative term. For instance the variable $years\_to\_election$ in column (5) indicates that a tariff increase is 5% more likely for every additional year until the next election (Table (6)). At the same time, the relationship may not be linear as indicated by the variable $min\_distance$ in column (6), which is defined as the minimum distance in years until the next or the previous election. To allow even more flexibility in this attempt to model the effect of the electoral cycle on tariff setting, column (7) allows for a dummy variable for each year until the election. E.g. the variable $T-2$ indicates the period two years before an election and all these indicators are contrasts relative to the election year $T-0$. The finding is very similar for both the participation and the amount decision and shows that it may be very difficult to disentangle periods which are affected by the cycle and those which are not. What can be said, however, is that there is a strong pre-election drop on tariff increases beginning in $T-2$. The periods immediately after an election are more difficult to interpret because although there is some indication of an adjustment (i.e. tariff increases) after an election, it is not the period with the most increases or the strongest increases. The problem of how to operationalize the political business cycle is therefore still ambigious even in the presence of very detailed data on policy decisions.

The last part here is devoted to explore potential conditionalities of the electoral cycle in the present context. Even though there is no cross-country variation in the overall institutional setting in the present data, politicians may have a stronger motive or different opportunities to manipulate tariffs in some situations than in others. Tables 8 and 9 show the results of these exercises. The tables present the coefficient on the election dummy and depending on the column, the effect from interacting the election indicator with the respective institutional or political variable.[21] There appears to be no conditinality of the electoral cycle regarding $majority$, $winmargin$, $longcycle$ and $partisan$. The general finding that the results for the electoral cycle are not sensitive to more of the employed conditionality variables may be due to the overall similar institutional background for all municipalities in Austria or simply indicate that cycle conditionalities are highly sensitive to the application and the effort devoted to uncover them. Moreover, the finding of little or no conditionalities when analyzing sub-national governments is also consistent with the existing literature such as Khemani (2004).

On the other hand, the interaction effect is significant in the case of $directmayor$ and $fraggov$. The coefficient on the interaction effect between $election$ and $directmayor$ (column (4)) is positive and statistically significant in both the participation and the amount equation. This means that the cycle

[21] If the conditional term is no dummy variable (e.g. winmargin), the interaction effect is demeaned to allow an at-means marginal effects interpretation.

Table 8: Conditionalities of the electoral cycle: Participation

	(1) majority	(2) win-margin	(3) frag-gov	(4) direct-mayor	(5) long cycle	(6) partisan
election	-0.225***	-0.193***	-0.192***	-0.312***	-0.215***	-0.180***
	(0.050)	(0.028)	(0.029)	(0.055)	(0.042)	(0.042)
inter	0.051	-0.003	-0.001	0.221***	0.059	-0.025
	(0.071)	(0.002)	(0.060)	(0.082)	(0.088)	(0.067)
majority	0.020	0.026	0.029	0.027	0.028	0.029
	(0.067)	(0.066)	(0.066)	(0.065)	(0.066)	(0.066)
winmargin	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
fraggov	0.048	0.047	0.048	0.050	0.048	0.047
	(0.036)	(0.036)	(0.040)	(0.035)	(0.036)	(0.036)
directmayor	0.031	0.029	0.030	-0.007	0.029	0.030
	(0.056)	(0.056)	(0.055)	(0.053)	(0.055)	(0.055)
longcycle	0.007	0.010	0.008	0.005	-0.002	0.008
	(0.056)	(0.056)	(0.056)	(0.056)	(0.059)	(0.056)
partisan	-0.005	-0.004	-0.005	-0.004	-0.005	-0.000
	(0.052)	(0.052)	(0.052)	(0.051)	(0.052)	(0.052)
debt	-0.018	-0.018	-0.018	-0.018	-0.018	-0.017
	(0.022)	(0.022)	(0.022)	(0.021)	(0.022)	(0.022)
pop	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002
	(0.011)	(0.012)	(0.012)	(0.011)	(0.012)	(0.012)
area	-0.061	-0.062	-0.062	-0.059	-0.062	-0.062
	(0.049)	(0.049)	(0.049)	(0.049)	(0.049)	(0.049)
N	934	934	934	934	934	934

Cluster and heteroscedasticity robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Covariates election, majority, winmargin, fraggov, directmayor, longcycle, partisan, debt, pop and area included.

Table 9: Conditionalities of the electoral cycle: Amount

	(1) majority	(2) win-margin	(3) frag-gov	(4) direct-mayor	(5) long cycle	(6) partisan
election	-5.450***	-4.748***	-4.624***	-6.748***	-5.263***	-4.907***
	(0.936)	(0.740)	(0.738)	(1.277)	(1.147)	(0.807)
inter	1.104	-0.026	-1.762**	3.694**	1.370	0.310
	(1.186)	(0.042)	(0.860)	(1.424)	(1.579)	(0.992)
majority	-0.588	-0.423	-0.405	-0.421	-0.404	-0.401
	(1.001)	(0.929)	(0.931)	(0.920)	(0.931)	(0.931)
winmargin	-0.041	-0.035	-0.040	-0.039	-0.041	-0.041
	(0.031)	(0.034)	(0.031)	(0.031)	(0.031)	(0.031)
fraggov	0.675	0.670	0.984*	0.721*	0.675	0.675
	(0.420)	(0.421)	(0.521)	(0.417)	(0.420)	(0.422)
directmayor	-1.716**	-1.758**	-1.750**	-2.374***	-1.764**	-1.744**
	(0.810)	(0.812)	(0.819)	(0.877)	(0.818)	(0.820)
longcycle	1.127	1.168	1.170	1.099	0.924	1.146
	(0.758)	(0.752)	(0.748)	(0.743)	(0.848)	(0.757)
partisan	0.984*	0.999*	0.979*	1.005*	0.991*	0.937
	(0.574)	(0.577)	(0.575)	(0.570)	(0.575)	(0.610)
debt	0.023	0.021	0.033	0.021	0.025	0.019
	(0.393)	(0.393)	(0.390)	(0.387)	(0.393)	(0.395)
pop	-0.070	-0.071	-0.072	-0.083	-0.075	-0.072
	(0.128)	(0.129)	(0.129)	(0.128)	(0.129)	(0.129)
area	-0.581	-0.585	-0.583	-0.548	-0.582	-0.583
	(0.463)	(0.462)	(0.465)	(0.458)	(0.462)	(0.463)
N	934	934	934	934	934	934

Cluster and heteroscedasticity robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Covariates election, majority, winmargin, fraggov, directmayor, longcycle, partisan, debt, pop and area included.

is less intense in those municipalities where mayors are elected directly. If direct election of mayors indeed increases accountability, the finding here is at odds with those in Persson and Tabellini (2003), who find more pronounced cycles in majoritarian systems. This does not seem to be the case in Austrian municipalities. It appears possible, however, that the result is driven by the presence of a parliamentary type of democracy, where the findings in Persson and Tabellini (2003) were less clear. Another interpretation arises, if the coefficient on $directmayor$ itself is considered. As indicated by the negative and statistically significant effect in both participation and amount equation, municipalities with directly elected mayors anyway have lower tariffs. This could be seen as evidence that directly elected mayors are already at the opportunistic maximum and we therefore see a less pronounced cycle. Apart from $directmayor$, the interaction term from $fraggov$ is negative and statistically significant but only in the amount equation. This result is quite interesting because it implies that the number of political parties does not affect the cycle when it comes to the decision to increase or not. But once the hurdle has been passed, meaning a municipality has chosen to increase tariffs, the cycle is intensified in the presence of more political parties. Thus, once the resistance by the opposition or coalition parties has been overcome, municipalities with a more fragmented political system tends to behave more opportunistically.

4. Conclusion

This paper analyzes the effect of elections on tariff setting by Austrian municipalities. The findings support the general notion that there is an electoral cycle in economic policy instruments. All the evidence in this paper suggests that an electoral cycle is present in Austrian water tariffs and the results are robust to a range of alternative models. The paper goes, however, one step further in its analysis of the underlying mechanisms. The tariff decision is analyzed as a corner solution problem that consists of two parts, a participation decision that determines whether a municipality increases tariffs or not. And secondly, if tariffs are changed, the politician faces the amount decision, determining the size of the increase. This approach delivers some new insights and appears to better accommodate the step-wise movements in tariffs. Related to this, the paper also looks at the distributional effects of the electoral cycle and finds that it has the strongest effect in determining no or intermediate tariff changes. For larger tariff changes, the effect of the election cycle decreases and thus factors beyond the model seem to be driving very large tariff increases.

In addition, the paper tests the sensitivity of the election cycle effect with respect to the operationalization of the political cycle as well as its sensitivity regarding potential institutional differences. The results suggest that the operationalization of the cycle, e.g. a simple dummy compared to more sophisticated indicators, seems to matter greatly for the size of the obtained results. This point has been largely ignored in previous exmpirical tests of political cycles. Among the tested conditionalities of the cycle to political institutions, the number of parties in the city council and the direct election of mayors were found to affect the political cycle in a statistically significant size. Again, these tests show that distinguishing between the participation and amoung decision is important. For instance, the fractionalization of the city council is relevant for the decision to increase or not, but does not seem to affect the decision about the size of the increase.

The paper tries to take a new look at an old research question. By looking at tariff changes directly, the paper generated some new insights but also has to tackle new empirical problems. Since data on statutory tax rates or tariffs for public services is easily available, the advantages of analyzing political decisions directly represents a promising avenue for future research on the topic. This should allow the researcher to use more simple modeling strategies and concentrate on the sensitivities and conditionalities that have been stressed by recent research.

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Appendix A. Estimation Tables

Table A.10: Baseline

	OLS	FD	FE	LDV	ABond
L.dprice				-0.055*	-0.116***
				0.030	0.036
election	-4.746***	-4.199***	-4.675***	-4.994***	-4.744***
	0.745	0.760	0.778	0.724	0.747
majority	-0.401	1.607	0.156	0.115	2.831
	0.930	1.636	1.352	1.032	2.197
winmargin	-0.040	0.022	-0.054	-0.064*	-0.015
	0.031	0.057	0.051	0.034	0.120
fraggov	0.673	0.364	0.631	0.656	2.593*
	0.421	1.085	0.731	0.440	1.424
bmdirekt	-1.744**	0.177	-1.887	-1.914*	-5.080
	0.820	2.612	1.602	0.964	3.367
longcycle	1.150	8.037	-0.102	1.748*	-5.527
	0.753	8.351	2.755	0.877	6.214
partisan_spo	0.993*	8.570	5.022	0.874	6.031
	0.574	6.704	3.714	0.601	6.440
debt	0.022	-0.012	0.533	0.169	-0.051
	0.393	1.900	0.725	0.443	1.431
ew2001	-0.071			-0.327**	
	0.128			0.155	
area2001km2	-0.586			-0.256	
	0.461			0.517	
N	934	834	934	837	739

Cluster and heteroscedasticity robust standard errors in parentheses, except for column (5) with only robust standard errors.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Covariates election, majority, winmargin, fraggov, directmayor, longcycle, partisan, debt, pop and area included. Difference GMM estimator in column (5) is estimated using the first, second and third lags as instruments.

Table A.11: Participation and amount

	Participation		Amount				
	OLS	Probit	OLS	Tobit	2PM Log-Lin	2PM Trunc-Normal	ET2TM
election	-0.193***	-0.204***	-4.746***	-3.862***	-9.719***	-11.587***	-8.938***
	(0.029)	(0.032)	(0.745)	(0.585)	(2.981)	(3.013)	(2.999)
majority	0.029	0.031	-0.401	0.113	-1.435	-1.389	-1.290
	(0.066)	(0.065)	(0.930)	(0.788)	(2.031)	(1.944)	(2.062)
winmargin	-0.002	-0.002	-0.040	-0.035	0.029	-0.031	0.025
	(0.002)	(0.002)	(0.031)	(0.027)	(0.079)	(0.063)	(0.068)
fraggov	0.047	0.049	0.673	0.714*	1.903	1.033	1.755
	(0.036)	(0.035)	(0.421)	(0.395)	(1.180)	(1.060)	(1.121)
directmayor	0.030	0.034	-1.744**	-0.351	-2.578	-4.919***	-2.328
	(0.055)	(0.053)	(0.820)	(0.611)	(1.888)	(1.570)	(1.925)
longcycle	0.008	0.005	1.150	0.436	2.714	1.478	2.470*
	(0.056)	(0.053)	(0.753)	(0.583)	(1.697)	(1.427)	(1.715)
partisan	-0.005	-0.003	0.993*	0.399	2.439	2.694**	2.216*
	(0.052)	(0.051)	(0.574)	(0.564)	(1.650)	(1.225)	(1.498)
debt	-0.018	-0.018	0.022	-0.109	1.507**	0.789	1.362
	(0.022)	(0.023)	(0.393)	(0.318)	(0.617)	(0.790)	(1.077)
pop	-0.002	-0.005	-0.071	-0.079	-0.004	-0.080	-0.006
	(0.011)	(0.013)	(0.128)	(0.144)	(0.343)	(0.461)	(0.569)
area	-0.062	-0.064	-0.586	-0.755	0.663	0.651	0.572
	(0.049)	(0.053)	(0.461)	(0.579)	(1.538)	(1.192)	(1.375)
N	934	934	934	934	376	376	934

Cluster and heteroscedasticity robust standard errors in parentheses, except for column (5) with only robust standard errors.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Covariates election, majority, winmargin, fraggov, directmayor, longcycle, partisan, debt, pop and area included. The coefficients in column (4) (Tobit) and (5) (2PM) are average marginal effects conditional on $\Delta tariff > 0$

Table A.12: Sensitivity to election cycle definition: Participation OLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
election	-0.193***	-0.185***					
	(0.029)	(0.029)					
post		0.042					
		(0.044)					
election_month			-0.279***				
			(0.047)				
early				-0.185***			
				(0.036)			
late				-0.216***			
				(0.072)			
years_to_election					0.049***		
					(0.009)		
min_distance						0.074***	
						(0.016)	
T-1							0.079*
							(0.045)
T-2							0.242***
							(0.041)
T-3							0.167***
							(0.049)
T-4							0.248***
							(0.048)
T-5							0.196***
							(0.073)
majority	0.029	0.029	0.036	0.029	0.029	0.028	0.029
	(0.066)	(0.066)	(0.065)	(0.066)	(0.066)	(0.066)	(0.066)
winmargin	-0.002	-0.002	-0.003	-0.002	-0.003	-0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
fraggov	0.047	0.046	0.045	0.047	0.038	0.045	0.044
	(0.036)	(0.036)	(0.035)	(0.036)	(0.036)	(0.036)	(0.036)
directmayor	0.030	0.026	0.025	0.031	0.006	0.029	0.020
	(0.055)	(0.056)	(0.055)	(0.055)	(0.056)	(0.055)	(0.058)
longcycle	0.008	0.012	0.005	0.009	-0.008	-0.012	0.005
	(0.056)	(0.055)	(0.056)	(0.057)	(0.058)	(0.055)	(0.058)
partisan	-0.005	-0.004	-0.004	-0.004	-0.001	-0.007	-0.003
	(0.052)	(0.052)	(0.051)	(0.051)	(0.052)	(0.052)	(0.052)
debt	-0.018	-0.018	-0.019	-0.018	-0.021	-0.018	-0.021
	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
pop	-0.002	-0.002	-0.002	-0.001	-0.002	-0.001	-0.002
	(0.011)	(0.011)	(0.011)	(0.012)	(0.011)	(0.011)	(0.011)
area	-0.062	-0.061	-0.060	-0.062	-0.061	-0.063	-0.061
	(0.049)	(0.049)	(0.048)	(0.049)	(0.049)	(0.049)	(0.049)
N	934	934	931	934	934	934	934

Cluster and heteroscedasticity robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Covariates election, majority, winmargin, fraggov, directmayor, longcycle, partisan, debt, pop and area included.

Table A.13: Sensitivity to election cycle definition: Amount OLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
election	-4.746***	-4.372***					
	(0.745)	(0.794)					
post		2.100**					
		(0.867)					
election_month			-6.375***				
			(0.944)				
early				-4.842***			
				(0.913)			
late				-4.453***			
				(0.836)			
years_to_election					1.332***		
					(0.198)		
min_distance						1.617***	
						(0.342)	
T-1							2.057**
							(0.887)
T-2							5.405***
							(0.990)
T-3							4.095***
							(1.030)
T-4							6.405***
							(1.190)
T-5							6.001***
							(1.281)
majority	-0.401	-0.376	-0.326	-0.404	-0.394	-0.410	-0.378
	(0.930)	(0.937)	(0.934)	(0.932)	(0.940)	(0.935)	(0.937)
winmargin	-0.040	-0.043	-0.046	-0.040	-0.046	-0.041	-0.045
	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)
fraggov	0.673	0.608	0.528	0.676	0.432	0.595	0.546
	(0.421)	(0.422)	(0.426)	(0.424)	(0.417)	(0.423)	(0.425)
directmayor	-1.744**	-1.976**	-1.946**	-1.760**	-2.376***	-1.807**	-2.117**
	(0.820)	(0.837)	(0.807)	(0.813)	(0.798)	(0.818)	(0.830)
longcycle	1.150	1.358*	1.100	1.133	0.678	0.766	0.902
	(0.753)	(0.755)	(0.750)	(0.756)	(0.755)	(0.747)	(0.752)
partisan	0.993*	1.045*	1.069*	0.985*	1.089*	0.934	1.072*
	(0.574)	(0.579)	(0.581)	(0.575)	(0.581)	(0.582)	(0.582)
debt	0.022	-0.009	-0.006	0.020	-0.064	0.027	-0.066
	(0.393)	(0.400)	(0.398)	(0.392)	(0.392)	(0.390)	(0.394)
pop	-0.071	-0.086	-0.068	-0.077	-0.080	-0.051	-0.077
	(0.128)	(0.129)	(0.130)	(0.133)	(0.130)	(0.129)	(0.129)
area	-0.586	-0.565	-0.600	-0.577	-0.565	-0.623	-0.569
	(0.461)	(0.464)	(0.459)	(0.470)	(0.468)	(0.462)	(0.464)
N	934	934	931	934	934	934	934

Cluster and heteroscedasticity robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Covariates election, majority, winmargin, fraggov, directmayor, longcycle, partisan, debt, pop and area included.

Table A.14: Conditionalities of the electoral cycle: Participation

	(1) majority	(2) win-margin	(3) frag-gov	(4) direct-mayor	(5) long cycle	(6) partisan
election	-0.225***	-0.193***	-0.192***	-0.312***	-0.215***	-0.180***
	(0.050)	(0.028)	(0.029)	(0.055)	(0.042)	(0.042)
inter	0.051	-0.003	-0.001	0.221***	0.059	-0.025
	(0.071)	(0.002)	(0.060)	(0.082)	(0.088)	(0.067)
majority	0.020	0.026	0.029	0.027	0.028	0.029
	(0.067)	(0.066)	(0.066)	(0.065)	(0.066)	(0.066)
winmargin	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
fraggov	0.048	0.047	0.048	0.050	0.048	0.047
	(0.036)	(0.036)	(0.040)	(0.035)	(0.036)	(0.036)
directmayor	0.031	0.029	0.030	-0.007	0.029	0.030
	(0.056)	(0.056)	(0.055)	(0.053)	(0.055)	(0.055)
longcycle	0.007	0.010	0.008	0.005	-0.002	0.008
	(0.056)	(0.056)	(0.056)	(0.056)	(0.059)	(0.056)
partisan	-0.005	-0.004	-0.005	-0.004	-0.005	-0.000
	(0.052)	(0.052)	(0.052)	(0.051)	(0.052)	(0.052)
debt	-0.018	-0.018	-0.018	-0.018	-0.018	-0.017
	(0.022)	(0.022)	(0.022)	(0.021)	(0.022)	(0.022)
pop	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002
	(0.011)	(0.012)	(0.012)	(0.011)	(0.012)	(0.012)
area	-0.061	-0.062	-0.062	-0.059	-0.062	-0.062
	(0.049)	(0.049)	(0.049)	(0.049)	(0.049)	(0.049)
N	934	934	934	934	934	934

Cluster and heteroscedasticity robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Covariates election, majority, winmargin, fraggov, directmayor, longcycle, partisan, debt, pop and area included.

Table A.15: Conditionalities of the electoral cycle: Amount

	(1) majority	(2) win-margin	(3) frag-gov	(4) direct-mayor	(5) long cycle	(6) partisan
election	-5.450***	-4.748***	-4.624***	-6.748***	-5.263***	-4.907***
	(0.936)	(0.740)	(0.738)	(1.277)	(1.147)	(0.807)
inter	1.104	-0.026	-1.762**	3.694**	1.370	0.310
	(1.186)	(0.042)	(0.860)	(1.424)	(1.579)	(0.992)
majority	-0.588	-0.423	-0.405	-0.421	-0.404	-0.401
	(1.001)	(0.929)	(0.931)	(0.920)	(0.931)	(0.931)
winmargin	-0.041	-0.035	-0.040	-0.039	-0.041	-0.041
	(0.031)	(0.034)	(0.031)	(0.031)	(0.031)	(0.031)
fraggov	0.675	0.670	0.984*	0.721*	0.675	0.675
	(0.420)	(0.421)	(0.521)	(0.417)	(0.420)	(0.422)
directmayor	-1.716**	-1.758**	-1.750**	-2.374***	-1.764**	-1.744**
	(0.810)	(0.812)	(0.819)	(0.877)	(0.818)	(0.820)
longcycle	1.127	1.168	1.170	1.099	0.924	1.146
	(0.758)	(0.752)	(0.748)	(0.743)	(0.848)	(0.757)
partisan	0.984*	0.999*	0.979*	1.005*	0.991*	0.937
	(0.574)	(0.577)	(0.575)	(0.570)	(0.575)	(0.610)
debt	0.023	0.021	0.033	0.021	0.025	0.019
	(0.393)	(0.393)	(0.390)	(0.387)	(0.393)	(0.395)
pop	-0.070	-0.071	-0.072	-0.083	-0.075	-0.072
	(0.128)	(0.129)	(0.129)	(0.128)	(0.129)	(0.129)
area	-0.581	-0.585	-0.583	-0.548	-0.582	-0.583
	(0.463)	(0.462)	(0.465)	(0.458)	(0.462)	(0.463)
N	934	934	934	934	934	934

Cluster and heteroscedasticity robust standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Covariates election, majority, winmargin, fraggov, directmayor, longcycle, partisan, debt, pop and area included.