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Do elections affect the composition of fiscal policy in developed, established democracies?

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Abstract This paper investigates the impact of elections on the level and composition of fiscal instruments using a sample of 19 high-income OECD democracies during the period 1972–1999. We find that elections shift public spending towards current expenditures at the cost of public investment. Although we find no evidence for an electoral cycle for government deficit and overall expenditures, we find a negative effect of elections on revenue attributed to a fall in direct taxation. Our results apply for predetermined electoral periods while endogenous elections seem to increase deficit and leave the composition of fiscal policy unaffected.

Keywords Political budget cycles · Elections · Composition of fiscal policy · Quality of public expenditure

JEL Classification D72 · E62

1 Introduction

A growing literature suggests that elections create distortions in economic policy. An important part of this literature focuses on the incentives of office-motivated politicians to manipulate economic variables in order to get re-elected. This theoretical argument, firstly formulated in the traditional opportunistic model of 'political business cycles' of Nordhaus

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(1975) (see also Lindbeck 1976; Tufte 1978) has been later addressed in a rational expectations framework by emphasizing the presence of uncertainty regarding the electoral result as well as the competence level of policymakers.[1] Rosenberg (1992) shows that in election periods the incumbent, who is uncertain about the electoral outcome, may increase expenditure targeted to activities that will raise his employment prospects in case he is not re-elected. An important feature of rational opportunistic 'political budget cycles' (PBC) models is the presence of uncertainty regarding the policymakers' competence. In this environment, the incumbent has an incentive to manipulate fiscal instruments, if voters' expectations regarding her competence depends on the value of this instrument (see, e.g., Cukierman and Meltzer 1986; Rogoff and Sibert 1988; Rogoff 1990; Persson and Tabellini 1990; Shi and Svensson 2006).[2]

In general, rational PBC models predict a negative electoral impact on taxation. However, aggregate public spending may rise in the election period, as the incumbent will have an incentive to increase expenditures financed by a deficit observed by voters in the post-election period, but it may also fall, as a rise in the incumbent's level of effort will limit 'wasteful' public spending (see Besley and Case 1995). The main empirical implications of PBC models have been widely tested.[3] A general conclusion of existing studies is that budget cycles exist in developing countries and in 'new' democracies, whereas established democracies with more competitive electoral systems seem to experience a fiscal revenue cycle (Persson and Tabellini 2003: Chap. 8; Brender and Drazen 2005; Shi and Svensson 2006).[4]

Rogoff (1990) was the first to provide a firm theoretical foundation for the possibility of electorally timed shifts in the *composition* rather than the *level* of public spending. Rogoff shows that electoral incentives may induce the incumbent to signal her competence by shifting public spending towards more 'visible' government consumption and away from public investment goods. A similar prediction regarding the composition of public spending is derived by Saporiti and Streb (2008), who also investigate the institutional framework that may limit this type of electoral cycle. Moreover, Drazen and Eslava (2010) show that shifts in the composition of public spending in favor of targeted goods in the election periods signal the preferences rather than the competence level of the incumbent.

Empirical studies that attempt to assess the electoral impact on the composition of public spending using multi-country data are limited and their results are mixed (see Schuknecht 2000; Block 2002; Vergne 2009).[5] Surprisingly, although the theoretical models of PBC were originally conceived for developed countries, the electoral impact on the composition of fiscal policy has so far been investigated only for developing countries. Several arguments support this approach. Schuknecht (1996) suggests that fiscal manipulation in developing

[1] As emphasized by McCallum (1978) and Paldam (1989), the traditional opportunistic model of 'political business cycles' is based on the implicit assumption of voters' myopia and cannot survive in a rational expectations framework where economic policy effects are anticipated by voters.

[2] Maley et al. (2007) investigate the impact of electoral incentives on capital accumulation and macroeconomic fluctuations in a Dynamic Stochastic General Equilibrium framework.

[3] For a discussion of the empirical implications of PBC models see Alt and Chrystal (1983), Persson and Tabellini (1990), Blais and Nadeau (1992), Drazen (2000), Franzese (2002), Mink and de Haan (2006), and Vergne (2009).

[4] Studies that find strong support for opportunistic cycles in young and imperfect democracies include Schuknecht (2000), Block (2002), Gonzalez (2002), Akhmedov and Zhuravskaya (2004), Brender and Drazen (2005), and Shi and Svensson (2006). Weak support for opportunistic cycles in developed countries is found in Alesina and Roubini (1992), Alesina et al. (1997) and Reid (1998).

[5] A brief survey is attempted in the following section.

countries is more likely because checks and balances are weaker. Moreover, the informational asymmetries regarding competence that are a crucial assumption of PBC models may be more plausible in developing countries (see Block 2002).

On the other hand, it should be stressed that PBC models are all based on the assumption of competitive elections, which is more applicable to developed established democracies, rather than to developing countries many of which are 'new' democracies. Indeed, multiparty electoral competition that characterizes developed countries is critical in motivating incumbents to engage in pre-electoral economic policy distortions in order to retain their office. Similarly, the underlying voting behavior of the PBC models is closer to the voting behavior of more experienced voters in established democracies rather than to the inexperienced voting behavior in 'new' democracies. Moreover, it is not clear that the assumption of 'lower visibility' of capital expenditures conforms to the experience of developing countries. Schuknecht (2000) argues that in developing countries public works projects can be easily started and stopped around elections, whereas current expenditures may reflect longer-term commitments. Clearly, this argument is less applicable to developed economies.[6]

Our paper deals with an important question that has not been properly addressed by the relevant literature. Do elections in developed, established democracies affect the composition of fiscal policy? In other words, when conditions that facilitate electoral manipulation such as uninformed voters, weak institutional structures and corruption are not apparent, is there still room for electoral manipulation? We try to answer this question by looking at the impact of the elections on different types of fiscal expenditure and revenue for a sample of 19 'old' democracies over the period 1972–1999. While we provide some new evidence on the electoral cycle of aggregate fiscal variables, we further classify public spending as capital and current expenditures whereas we decompose tax variables as direct (distortionary) taxation and indirect (non-distortionary) taxation (see Kneller et al. 1999).

To the best of our knowledge, our paper is novel in the following two aspects: First, we examine the existence of pre-electoral fiscal policy distortions in the composition of public spending, using for the first time a sample of developed countries that can be considered as established democracies during the entire sample period. Second, apart from looking at the composition of public spending, we also look at the electoral impact on the composition of tax instruments. The electoral impact on different tax policy instruments has not been empirically tested before for any sample of developed and/or developing countries.[7]

Our main results can be summarized as follows: firstly, using alternative electoral indicators, we find that elections shift the composition of public expenditures towards current expenditures and away from capital expenditures. Moreover, the level and the change in capital expenditures as a percentage of GDP fall. Secondly, regarding aggregate fiscal variables, we find no evidence for an electoral cycle for government deficit and expenditures, but we do find a negative effect of election on revenue. These results are consistent with existing stylized facts presented by Brender and Drazen (2005) and Shi and Svensson (2006). Thirdly, we find that the revenue cycle is attributed to an electoral cycle in direct taxation. The choice of policymakers to decrease direct rather than indirect taxation could be explained both in

[6] In fact Block (2002) shows that the hypothesis that elections give rise to a substitution of current for capital expenditures is best applied to the relatively richer countries in his sample of developing and middle-income countries.

[7] A related empirical study is by Ashworth and Heyndels (2002), who focus on the impact of elections on tax structure turbulence. Their results for 18 OECD countries for the 1965–1995 period suggest a change in the composition of tax revenue occurs in the election year but they are not informative as to the exact categories of tax revenue (e.g., direct taxation, consumption taxes) that are affected and to the direction of the effect.

terms of 'electoral efficiency' and economic efficiency. One could argue that a fall in direct taxation produces a more 'visible' effect on voters' income compared to a fall in indirect taxation. In fact, Alesina et al. (1993) suggest that direct taxes might be more easily manipulated before elections than other revenue categories. Moreover, a large literature suggests that decreasing direct taxation compared to indirect taxation will have a positive growth effect resulting from its impact on household's savings and labor supply decisions (see, e.g., Kneller et al. 1999).[8] In fact, in recent years governments have become increasingly interested in using indirect taxation to finance a larger share of public spending. Finally, if we distinguish between predetermined and endogenous elections—in line with the assumptions of our theoretical model—the above results apply only for predetermined electoral periods.

The rest of the paper is organized as follows. Section 2 reviews briefly the existing literature on the impact of elections on the composition of fiscal policy. Section 3 outlines our theoretical framework. Section 4 describes the empirical setup, presents the empirical results and discusses various sensitivity tests. Finally, Sect. 5 offers some concluding remarks.

2 Elections and the composition of fiscal policy: a brief literature review

The aim of our paper is to look at the impact of electoral incentives of an opportunistic policymaker on the composition of fiscal policy. In the model developed in the following section the incumbent's optimal behavior does not depend on her ideology. However, a related literature focuses on the influence of government ideology on the composition of fiscal policy. In some models (see, e.g., Alesina and Tabellini 1990; Alt and Lassen 2006) excessive spending and deficit creation crucially rely on the assumption that policymakers differ in their ideologies regarding the desired composition of public spending, since partisan differences increase the incumbent's utility loss from not being re-elected. Different preferences about the composition of fiscal policy have always been an implicit or explicit assumption of partisan models.[9] Parties of the left are expected to favor a larger government and have less aversion to public deficits than parties of the right (see Tufte 1978). The greater preference for redistribution of left-wing parties implies more spending on items promoting redistribution, such as social transfers, and the provision of specific public goods, such as health and education.

Empirical findings on the effect of partisanship on the composition of fiscal policy are mixed. The impact of ideology on social transfers seems to be the most robust finding, while, in some cases, social and welfare policies as well as the tax-structure may also be affected.[10] A formal demonstration of the impact of partisanship on the composition of public spending is given by Bräuninger (2005) in the framework of a salience model of budget politics. Using data for 19 OECD countries from 1971 to 1999 the author finds that it is not parties' ideology but rather parties' programmatic preferences that affect the composition of

[8] Angelopoulos et al. (2007), using data on various measures of effective tax rates find that moving from labor taxation to consumption taxation will have a positive impact on growth.

[9] 'Partisan' models deal with the behavior of ideologically motivated politicians. The first formulation of partisan electoral cycles theory is attributed to Hibbs (1977). Alesina et al. (1997) and Drazen (2000) provide a comprehensive review of partisan models.

[10] For an analytical survey of the existence of partisan cycles in spending and tax policies and on impact of partisanship in specific categories of public spending, such as social and welfare policies, see Cusack (1997) and Franzese (2002). Regarding the impact of the ideology on the composition of fiscal revenue, Hallerberg and Basinger (1998) find that left governments prefer income over consumption taxation while Belke et al. (2007) find political partisan cycles in privatization initiatives.

public expenditure. In Potrafke (2006), empirical evidence for 15 OECD countries for the 1994–2000 period suggests that left governments care more about spending for 'environmental protection', 'education' and 'recreation, culture and religion', while Potrafke (2011) attempts a further decomposition of budgetary spending for West German states and looks at the impact of government ideology on the allocation of spending within budget categories, such as education and cultural affairs.

In the framework of rational opportunistic models, Rogoff (1990) was the first to predict that electoral cycles may take the form of changes in the composition of fiscal policy rather than its level. Using an adverse selection PBC model, he demonstrates that electoral incentives may induce the incumbent to signal her competence by shifting public spending towards more 'visible' government consumption and away from public investment goods. Government consumption expenditures are more 'visible' in the sense that they are observed before elections, while capital expenditures (e.g., infrastructure) are mostly long-term projects that will increase voter's utility upon completion. Assuming that the cost of fiscal distortion is greater for the more competent incumbent, a rise in current expenditures in the election period at the cost of lower public investment will signal the high competence of the incumbent if the cost of mimicking this policy is sufficiently large for the less competent policymaker. In the separating equilibrium of the resulting signaling game, the competent incumbent will distort fiscal policy without being 'punished' by rational voters who perceive this distortion as the cost of acquiring information regarding policymakers' competence.

More recently, Saporiti and Streb (2008) focus on the incumbent's budgetary discretion and show that with a single fiscal authority the incumbent has an incentive to change the composition of public spending in favor of the more 'visible' public good in order to appear competent to the voters and to increase the probability of being re-elected. However, separation of powers can work as a commitment device and may moderate or even eliminate the PBC under certain conditions regarding the institutional framework. Moreover, in Drazen and Eslava (2010), voters have specific preferences over the composition of public spending but cannot observe the preferences of politicians. In this framework the incumbent will have an incentive to shift the composition of public spending in order to signal that his preferences are close to those of voters. Thus, in equilibrium, targeted expenditures that benefit specific groups of voters will increase in the election period at the expense of other expenditures that voters consider to be 'wasteful' in the sense that they fail to recognize the positive externalities produced by these services.

A limited empirical literature investigates the impact of elections on the composition of public spending using multi-country panel data for developing countries. Schuknecht (2000), using data on 24 developing countries from 1973 to 1992, finds that PBCs can be attributed mainly to more rather than less capital expenditures as a fraction of GDP. Additionally, Block (2002), using data on 69 developing countries from 1975 to 1990, investigates whether elections affect the share of capital expenditures in total expenditures. Indeed, his results reveal a deterioration of public investment during the election year. Along the same lines, a recent study of Vergne (2009), using data on 42 developing countries from 1975 to 2001, finds that elections shift the composition of spending towards current expenditures and away from capital expenditures, both measured as percentages of GDP.

A richer literature investigates the electoral impact on specific categories of public spending using data for *local* elections, but with few exceptions these studies also refer to developing countries and/or 'new' democracies. In studies of developed countries that can be considered as established democracies, Blais and Nadeau (1992) test for a political budget cycle in ten Canadian provinces from 1951 to 1984 and find that elections have a positive impact on social services and road expenditures. A later study of Kneebone and McKenzie (2001)

for Canadian provinces over the period 1966–1997 is consistent with Blais and Nadeau's (1992) result as far as road expenditures are concerned, but finds an opposite electoral impact on social services expenditures. Finally, Schneider (2010) argues that the high degree of fiscal transparency in Germany restricts the possibility of deficit creation, inducing governments to manipulate elections by increasing 'visible' spending. The author investigates the existence of PBCs in West German states from 1970 to 2003 and finds electoral cycles in the form of larger social security transfers payments and some evidence for a fall in education expenditures in specific states. In studies of developing countries and/or 'new' democracies, Veiga and Veiga (2007) use a data set for Portuguese mainland municipalities for the period 1979–2001 and find that elections lead to a rise in 'visible' investment expenditures (e.g., *other buildings*) and to a decline in less 'visible' investment expenditures (e.g., *machinery and equipment)*. Drazen and Eslava (2010) base their empirical analysis on annual data for all municipalities in Colombia over the period 1987–2002 and find that the composition of local government expenditures shifts before elections towards 'targeted' expenditures, such as development projects, and away from current transfers.

Concluding, empirical results for either developing countries or at the level of local government seem to support the prediction of Rogoff (1990) of a pre-election decline in less 'visible' spending, but they differ in their definition of the budget categories that are 'visible'. As a result, empirical evidence on the effect of opportunistic behavior on public investment is rather mixed. This is because—as partly discussed in the Introduction- the prediction of Rogoff (1990) for an electoral fall in capital spending is mainly applicable for 'old' democracies and at the central government level.[11] Therefore, testing the impact of opportunistic incentives on the composition of fiscal policy using multi-country panel data for developed countries that can be considered as 'old' democracies is important for the following reasons: Firstly, PBC models assume competitive elections, which is more relevant to 'old' democracies. Secondly, Rogoff's prediction of shifts in public spending towards more 'visible' government consumption and away from public investment goods may not hold for developing countries and local governments where public investment (or parts of it) may not be characterized by 'low' visibility. In that respect, it should be noted that studies at the local level are not easily comparable with multi-country studies since some expenditure categories that are 'visible' at a local level may not be 'visible' at a country level (e.g., construction). The same holds true for the comparison of multi-country studies for developed and developing countries since expenditure that may be 'visible' in a developing country (e.g., public investment) may not be 'visible' in a developed economy. Thirdly, in local elections one would expect the incumbent to manipulate expenditures that are on the one hand 'visible' and on the other hand, clearly identifiable as provincial government responsibilities. Given that fiscal responsibilities of local governments differ across countries, it is difficult to compare results applying to local elections in different countries and to derive policy conclusions that can be generalized.

3 The theoretical framework

Our theoretical framework assumes the presence of information asymmetries between voters and politicians regarding the competence level of the latter. In the rational opportunistic PBC

[11] Still, it should be noted that empirical research using local government data has its merits, since it is usually based on more disaggregated data, local elections in these countries occur in the same exogenously determined date and local governments operate under the same institutional environment (see Schneider 2010).

models of Cukierman and Meltzer (1986), Rogoff and Sibert (1988), and Rogoff (1990) a competent incumbent has an incentive to signal her level of competence to voters through fiscal policy in order to increase the probability of being re-elected. A drawback of the adverse selection type models is the difficulty of testing their empirical implications since they predict budget cycles that will depend on the policymakers' level of competence that is not directly observable. This difficulty is overcome by the moral hazard type models that are, however, also based on the presence of uncertainty regarding the policymakers' competence (see, in particular, Persson and Tabellini 1990; Shi and Svensson 2006). In these models the policymaker, irrespective of her level of competence, has an incentive to manipulate some fiscal instrument thereby affecting voters' expectations regarding her competence. For example, in this environment a policymaker will have an incentive to increase expenditures in the election period by increasing the deficit, which will be observed by voters in the post-election period.

This section develops a simple theoretical model that borrows its main features from Shi and Svensson (2006). These authors focus on the impact of elections on government deficit. In our model we neglect debt issuing and introduce public investment as the fiscal policy instrument with relatively lower visibility to voters.

The economy consists of a large number of individuals each of whom derives utility from public goods and private consumption. As in Rogoff (1990) the government produces a 'consumption' good (per capita) g and an 'investment' good (per capita) k. The utility function of voter i in period t is,

$$U_t^i = \sum_{s=t}^{T} \beta^{s-t}(c_s + u(g_s) + v(k_s) + \delta^i x_s) \tag{1}$$

where $v' > 0$, $v'' < 0$ and $u' > 0$, $u'' < 0$, c denotes the private consumption good and x takes the value $-(\frac{1}{2})$ if the incumbent is re-elected and $(\frac{1}{2})$ otherwise. As in Shi and Svensson (2006) we assume that voters differ in their preferences on issues other than consumption. This voter heterogeneity is captured by δ that is uniformly distributed on $[-1/2, 1/2]$ and reflects the voter's preference on other issues so that if voter i prefers the incumbent (opponent) then $\delta^i < 0$ (> 0).

Regarding the political environment, there are two parties competing for office, the one of the incumbent denoted by *in* and the opposition party denoted by *op*. Politicians are office-motivated in the sense that they derive utility Z from being in office. Thus, the utility function of political candidate j is given by,

$$W_t^j = \sum_{s=t}^{T} \beta^{s-t}(c_s + u(g_s) + v(k_s) + Z_s) \tag{2}$$

Every period each individual is endowed with income y and pays a lump-sum tax τ so that consumption in period t is,

$$c_t = y - \tau_t \tag{3}$$

The budget constraint of the government is given by,

$$g_t + k_{t+1} = \tau_t + \omega_t^j \tag{4}$$

where ω_t denotes the government's level of competence that captures its administrative ability. Taking the level of tax revenue as given, a government with a high level of competence

is able to produce more public goods than a government with low competence. We assume that competence is random since the problems the policymaker faces may change over time. Moreover, we assume that competence is persistent in the sense that competence in the post-election period will depend on competence in the pre-election period. Thus, in line with existing literature (see Rogoff 1990; Saporiti and Streb 2008), we assume that competence follows a first-order moving average process,

$$\omega_t^j = \varepsilon_t^j + \varepsilon_{t-1}^j \tag{5}$$

where $E(\varepsilon) = 0$, $Var(\varepsilon) = \sigma^2$. $F(\varepsilon)$ and $f(\varepsilon)$ are the distribution function and the density function of ε respectively with $f(0) > 0$. Note that the competence shock represents a source of uncertainty for the government since it is realized after policies are set.

3.1 Equilibrium without political competition

In the absence of political competition our problem can be broken down into a sequence of static maximization problems where the policymaker in office maximizes (2) with respect to g_t and k_{t+1} subject to (3), (4) and (5). The first order condition is given by,

$$u_g(g_t) = v_k(k_{t+1}) = 1 \tag{6}$$

3.2 Elections

Let us now assume that elections take place every other period. In that case our assumptions regarding preferences and the stochastic environment allow us to break our problem into a sequence of two-period maximization problems. Our methodology is borrowed from Shi and Svensson (2006). The timing of events is the following. At the beginning of the pre-election period t, the policymaker decides on the level of the public consumption good, g, and the level of the public investment good, k, that will be consumed in the next period. In the absence of any fiscal rule, policies are set with discretion. After policies are decided the competence shock, ε_t, is realized and elections take place at the end of this period. We assume that voters are to some extent imperfectly informed in the sense that before they vote they observe variables that represent consumption only in the current period such as $c_t(g_t)$ and τ_t, while they observe the public investment good, k_{t+1}, only in the next period.

In the post-election period, $t + 1$, the timing of events is similar but no elections take place. In addition, the incumbent has no incentive to influence voters' perception about his competence since competence in the next post-election period, $t + 3$, is uncorrelated with competence in period $t + 1$. Thus, the optimal tax rate and the optimal public investment will satisfy the first order condition (6).

In the election period, t, voters vote for the candidate who offers them higher utility in period $t + 1$. This will depend on their relative preference for the two parties, reflected by δ, and on the expected level of the policymaker's competence. Voter's i expected utility if the opposition party is re-elected is given by,

$$EU_{t+1}^i = E_t(c_{t+1}^{op}) + u(g^*) + E_t v(k_{t+1}^{op}) + \delta^i(1/2) \tag{7}$$

where

$$E_t(c_{t+1}^{op}) = y - g^* - k^* \tag{8}$$

Similarly, if the incumbent is re-elected expected utility in period $t+1$ is

$$EU^i_{t+1} = E_t(c^{in}_{t+1}) + u(g^*) + E_t v(k^{in}_{t+1}) - \delta^i(1/2) \tag{9}$$

where

$$E_t(c^{in}_{t+1}) = y - g^* - k^* + E_t(\varepsilon^{in}_t) \tag{10}$$

Note that g^* and k^* are the levels of the public 'consumption' good and public 'investment' good satisfying the first-order condition in (6), which equalizes the marginal utility of spending on public investment with the marginal utility of spending on public consumption and both with the marginal disutility of taxation.[12] $k^{in}_{t+1} = k^*_E$ is the optimal public investment set by the policymaker in the election period. Following the Shi and Svensson (2006) framework, subtracting (7) from (9) implies that voter i will vote for the incumbent if $E_t(\varepsilon^{in}_t) - \delta^i \geq 0$ and given the distribution of δ,

$$\Pr(E_t(\varepsilon^{in}_t) - \delta^i \geq 0) = E_t(\varepsilon^{in}_t) + \frac{1}{2} \tag{11}$$

we can re-write (4) as

$$\varepsilon^j_t = g_t + k_{t+1} - \tau_t - \varepsilon^j_{t-1} \tag{12}$$

Since voters can observe g_t and τ_t before the election, their estimate of the incumbent's current competence shock, $\hat{\varepsilon}_t$ will depend on their estimate of public investment, $\hat{k}_{t+1}$

$$\hat{\varepsilon}^{in}_t = g_t + \hat{k}_{t+1} - \tau_t - \varepsilon^{in}_{t-1} = \varepsilon^{in}_t + \hat{k}_{t+1} - \hat{k}_{t+1} \tag{13}$$

Thus, the probability that the incumbent will receive at least 50% of the votes is,

$$P_t = \Pr\left(\varepsilon^{in}_t + \hat{k}_{t+1} - \hat{k}_{t+1} + \frac{1}{2} \geq \frac{1}{2}\right) = \Pr(\varepsilon^{in}_t \geq \hat{k}_{t+1} - \hat{k}_{t+1}) = 1 - F(\hat{k}_{t+1} - \hat{k}_{t+1}) \tag{14}$$

The next step is to maximize the two-period utility function of the incumbent with respect to $\hat{k}_{t+1}$ taking into account (14),

$$\begin{aligned} \max_{k_{t+1}} & E_t[y - g^* - k_{t+1} + v(k^*) + u(g^*) + X] \\ & + E_t[1 - F(\hat{k}_{t+1} - \hat{k}_{t+1})][y - g^* - k^* + \varepsilon^{in}_{t+1} + v(k_{t+1}) + u(g^*) + X] \\ & + E_t F(\hat{k}_{t+1} - \hat{k}_{t+1})[y - g^* - k^* + \varepsilon^{op}_{t+1} + v(k_{t+1}) + u(g^*)] \end{aligned} \tag{15}$$

The first order condition is

$$v'(\hat{k}_{t+1}) = 1 + F'(\hat{k}_{t+1} - \hat{k}_{t+1})X \tag{16}$$

In equilibrium $\hat{k}_{t+1} = \hat{k}_{t+1} = k^*_E$ so we can write (16) as

$$v'(k^*_E) = 1 + f(0)X \tag{17}$$

[12] Note that from (2), (3) and (4) one can see that the marginal utility of taxation in this simple model equals -1.

Given that $f(0) > 0$ and $v'' < 0$, we can conclude from comparing (6) with (17) that $k_E^* < k^*$. This implies that electoral competition decreases capital spending in the election period. One can easily see from (4) that taxation will also fall.[13]

To sum up, the model's main implication is that the electoral motives of the incumbent when fiscal policy is discretionary will decrease the part of public spending that is 'less visible' in the election period allowing a fall in taxation, which is a 'more visible' fiscal policy instrument. As in Shi and Svensson (2006), in equilibrium this policy is fully expected and has no impact on the incumbent's re-election probability.

4 Econometric analysis

4.1 Data set and variables

Following previous studies in this area our empirical analysis is based on central government data. This applies both for papers studying aggregate fiscal variables (see, e.g., Brender and Drazen 2005; Shi and Svensson 2006) as well as papers looking at the composition of fiscal spending (see, e.g., Schuknecht 2000; Block 2002; Bräuninger 2005; Vergne 2009).[14] Our fiscal data are from the *IMF*, '*Government Financial Statistics*' (*GFS*) obtained from the '*Global Development Network Growth Database*', whereas GDP data come from '*Global Development Finance* and *World Development Indicators*'. *GFS* is the only multi-country source for disaggregated central government data and, therefore, is the standard database used in empirical research (see Schuknecht 2000; Block 2002; Bräuninger 2005; Vergne 2009). Unfortunately, although until 1999 financial information was calculated according to *Government Finance Statistics Manual 1986* (*GFSM 1986*) classifications, since then the *Government Finance Statistics Manual 2001* (*GFSM 2001*) framework has been used. The GFSM 2001 provides observations until 2008, but has only been back-dated until 1990. Given that, as it will be explained in a later section, merging the two databases involves a number of inaccuracies of unknown magnitude, we initially restrict our data set to the period from 1972 to 1999. In Sect. 4.4 we reproduce our results with the expanded sample following the approach suggested by Gemmell et al. (2007), which is similar to the one followed by Brender and Drazen (2009).

Our sample includes 19 OECD countries with competitive electoral systems.[15] Specifically, we focus on countries that satisfy our theoretical assumptions regarding the political structure (e.g., strongly competitive political systems, informed and experienced voters) and the 'lower visibility' of public investment' (e.g., institutional structures that do not allow public investment commitments to be reversed). Therefore, we test our model for OECD

[13] The result that taxes fall while production of the government consumption good, g, remains constant stems from the way we formulate preferences. We could as easily set up voters' utility function so that taxation remains constant and g increases.

[14] Apart from comparison purposes with previous literature, there are two other important reasons for using central government data: first, given that general government data include all levels of government (state, local, central), results based on such data would be more difficult to interpret. As noted by Schuknecht (2000) the central government controls directly only its own budget while changes in public spending of the general government may be affected by both state and local elections. Second, data from general government accounts are less consistent across countries and time periods.

[15] The countries of our sample are Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Iceland, Ireland, Italy, Japan, Luxemburg, the Netherlands, Norway, Sweden, Switzerland, the United Kingdom and the United States. New Zealand is excluded from the sample due to data unavailability.

countries that can at the same time be considered as established democracies. Greece, Portugal and Spain have been dropped from our sample since they cannot be considered as established democracies during the entire sample period. These 'new' democracies are more prone to fiscal manipulation, since incumbents might be rewarded at the polls if they can 'mislead' inexperienced voters by attributing the good economic conditions to their competency. Alternatively, we could have included these countries in our sample, dropping only those elections that occur in the period that the democracy was in fact 'new'. However, since, as argued by Brender and Drazen (2005), there are doubts about how long the 'new democracy' effect persists, we opt for excluding the countries.

In this study we want to check the existence of political cycles in aggregate fiscal data, as well as whether or not the composition of public spending and taxation is affected by elections. For the aggregate fiscal data, we use central governmental expenditures, revenues and budget surplus/deficit (denoted as *expenditures, revenues* and *balance*, respectively). As a second step, we use the economic classification provided by the *GFS* database and separate expenditures into public investment and public consumption. In particular, we disaggregate expenditures into capital expenditures (*capital*) and current expenditures (*current*). Both aggregated and disaggregated fiscal data are scaled by GDP and expressed as percentages. Finally, in order to test the electoral impact on the composition of total expenditures, we use the same measures expressed as percentages of total expenditures and denoted as *capital_exp* and *current_exp*, respectively.

Regarding the tax variables, we apply the approach of Kneller et al. (1999) and classify them as direct (distortionary) taxation and indirect (non-distortionary) taxation. Direct taxation includes taxation of income and profits, social security contributions, taxation of payroll and manpower and taxation of property, while indirect taxation includes taxation of domestic goods and services. Again, both direct and indirect taxation are expressed as percentages of GDP (*direct, indirect*) and as percentages of total revenues (*direct_rev*, *indirect_rev*).

Apart from the fiscal variables, in our estimated model we include a number of socioeconomic variables, proposed by Shi and Svensson (2006). In particular, we use the log of real per capita GDP (*lgdppc*) and real GDP growth (*growth*), since these variables should capture the possibility that the timing of elections depends on the state of the economy. All of the observations on the macroeconomic control variables are obtained from World Bank's '*World Development Indicators*' (*WDI*). A complete list of all variables used in our estimations is provided in the Data Appendix.

We measure electoral uncertainty by constructing two alternative electoral indicators. First of all, following the majority of the empirical literature, we construct an election dummy (*elec*) that receives the value one in an election year and zero otherwise. It is important to note that this indicator is not affected by the specific time (month) that the elections took place. We use this measure so that our results can be more easily comparable to the PBC literature. However, many researchers have noticed that if elections take place early in the year, then the dummy variable may be capturing primarily post-electoral effects. One way to deal with this problem when using annual data is to construct a pre-election indicator (*elec_2*) that takes the value of one at the year preceding election and zero otherwise (see, e.g., Franzese 2000; Potrafke 2006; Angelopoulos and Economides 2008). More precisely, in election year t, $elec_t\_2 = x/12$ with x denoting the month the election is held, and $[elec_{t-1}\_2 = (12 - x)/12]$ is allocated to the year before the election (if pre-election years overlap, *elec_2* can exceed one). Hence, this indicator allows us to directly control for differences in election dates across as well as within countries. It is worth noting that we restrict our attention to legislative elections for countries with parliamentary political systems and presidential elections for countries with presidential systems. Election dates are

taken from '*Comparative Political Data Set I*' (see Armingeon et al. 2008). These data were complemented, when needed, by the '*Inter-Parliamentary Union*' database.

4.2 Empirical specification

Many previous studies have explored pre-electoral effects on fiscal policy in a dynamic Fixed Effects model specification (see, among others, Schuknecht 2000; Persson and Tabellini 2003; Brender and Drazen 2005). Hence, these studies estimate an equation of the following form:

$$Y_{it} = a_0 Y_{it-1} + a_1 elec_{it} + \beta Z_{it} + \mu_i + \lambda_t + \varepsilon_{it} \tag{18}$$

where Y_{it} is a fiscal indicator in country i in year t, *elec* is the indicator we use to capture the influence of elections and Z is the vector of country-specific and time-varying socio-economic control variables. Finally, μ_i and λ_t are country and time-specific fixed effects and ε_{it} is the error term.

In line with these studies we include the lagged dependent variable Y_{it-1} on the right-hand side of our estimated equation, since fiscal instruments display a great deal of persistence. The lagged dependent variable is utilized as a means of capturing the dynamics of politics. There are theories in which an attitude at time t is a function of that same attitude at time $t-1$ as modified by new information. Before estimating (18), we need to test for the existence of unit roots in our data, given the presence of the lagged dependent variable. If our dependent variable is not stationary, we are faced with spurious relationships when that variable is entered on the right-hand side of the equation. Different tests for unit roots have been proposed; however, only a few of them are directly applicable to unbalanced data (see Breitung and Pesaran 2008). Here we rely on the Maddala and Wu (1999) test to check for the presence of a unit root.[16] This is a non-parametric Fisher-type test that combines the p-values of standard augmented Dickey Fuller (ADF) unit root tests for each country. The test assumes that all series are non-stationary under the null hypothesis against the alternative that at least one series in the panel is stationary. Given that OECD governments grew in size over the past decades, a linear time trend is included in the panel unit root test regressions in levels. As can be seen in Table 1, when a constant and a trend are included we cannot reject the null of non-stationarity of the variables *expenditures*, *capital* and *current*. For this reason, we apply the same panel unit root test to the first-differenced data. The trend drops out in that case and therefore is not included in the panel unit-root-test regressions in first differences. The results indicate that we can reject the null of non-stationarity at the 1% significance level.

A common approach for dealing with non-stationary data is to take first differences in order to proceed with a dynamic specification in differences. Hence, we end up estimating the following equation:

$$\Delta Y_{it} = a_0 \Delta Y_{it-1} + a_1 elec_{it} + \beta \Delta Z_{it} + \lambda_t + \varepsilon_{it} \tag{19}$$

where we first difference our dependent variable and the covariates of our model—all measured initially in levels—with the exception of the election-indicator variables. This implies

[16] Alternatively, a widely used panel unit root test is the Im et al. (2003) test, which is based on the average of augmented Dickey Fuller (ADF) t-statistics for each cross section in the panel. However, the Maddala and Wu (1999) test has the advantage over the Im et al. (2003) test insofar as its value does not depend on different lag lengths in the individual ADF regressions and varying time dimensions for each cross-section unit.

Table 1 Maddala and Wu (1999) panel unit root test (H_0: unit root)

Variables	Level		First difference
	Constant	Constant and Trend	Constant
balance	83.684***	77.452***	221.684***
	(0.000)	(0.000)	(0.000)
expenditures	73.884***	34.992	158.692***
	(0.000)	(0.609)	(0.000)
revenues	57.369**	51.695*	244.206***
	(0.022)	(0.068)	(0.000)
capital	36.526	57.881**	280.242***
	(0.537)	(0.020)	(0.000)
current	93.047***	37.781	153.175***
	(0.000)	(0.479)	(0.000)
direct	77.618***	79.248**	226.655***
	(0.000)	(0.000)	(0.000)
indirect	60.405**	60.405**	266.309***
	(0.012)	(0.002)	(0.000)
capital_exp	77.610***	70.583***	259.223***
	(0.000)	(0.001)	(0.000)
current_exp	79.766***	69.032***	250.855***
	(0.000)	(0.001)	(0.000)
direct_rev	83.211***	60.958**	205.784***
	(0.000)	(0.011)	(0.000)
indirect_rev	76.049***	68.756***	266.104***
	(0.000)	(0.001)	(0.000)

Notes: Figures without parenthesis are test statistics and those inside parentheses are respective probabilities. The lag lengths in panel unit root tests were selected using Akaike information criterion (AIC)
***Denotes significance at 1% level. **Denotes significance at 5% level. *Denotes significance at 10% level

that we put more structure on the data for the identification of the election effect. This specification was applied by Levitt (1997) in his empirical analysis of electoral cycles in police hiring. Also, by taking first differences we eliminate time-invariant country effects, but not time fixed effects. It is worth noting that the F-test results presented in our tables indicate that time fixed effects are in general significant and therefore they are included in regressions.[17]

To continue the discussion, the inclusion of a lagged dependent variable often eliminates serial correlation of errors. However, including a lagged dependent variable introduces a potential bias by not satisfying the strict exogeneity assumption of the error term ε_{it}. In order to correct for the bias, Arellano and Bond (1991) and Arellano and Bover (1995)/Blundell and Bond (1998) have developed GMM estimators that yield consistent estimates in '*small T large N panels*'. Hence, in our panel with a long time dimension and a smaller country dimension, these specifications could lead to biased and imprecise estimates.[18] Therefore,

[17]The qualitative results in all regressions do not significantly change when we exclude year effects.

[18]Note that applying Arellano and Bond (1991) or Arellano and Bover (1995)/Blundell and Bond (1998) GMM estimators does not alter our results. However, the relatively small country dimension of our sample

although dynamic panel GMM methods can be extremely useful, in our case it will not resolve the bias issue. Still, as it is analyzed in the literature, the estimated bias of this formulation is of order $1/T$, where T is the time length of the panel, even as the number of countries becomes large (see, among others, Nickell 1981; Kiviet 1995). The average time series length of our panel depends on the fiscal indicator we use, but in general is above 23 years and the bias is probably negligible.

Finally, our model is tested by computing a modified Wald statistic for groupwise heteroskedasticity, as proposed by Greene (2003). The null hypothesis of homoscedasticity is strongly rejected. Therefore, in our estimation procedure we use Huber-White standard errors as a corrective.

4.3 Results

In this section we first present evidence from estimating (19) for various fiscal variables using *elec* as the pre-electoral indicator. We then address the potential endogeneity of electoral procedures by separating out those elections whose timing is predetermined. Finally, we test whether our results are affected by using *elec_2* as an alternative pre-electoral indicator.

4.3.1 Basic results

Table 2 reports the results when we include the binary indicator *elec* in our regressions.[19] The change of GDP per capita growth has a positive impact on the change in surplus and a negative impact on the change in total and current expenditure as a share of GDP. This can be attributed to the countercyclical behavior of public expenditure, while the overall surplus rises with economic activity.[20] For similar reasons the negative effect of a change in growth on the change in government spending is also expected and it is in line with most empirical findings (see Mueller 2003; Kittel and Winner 2005). The negative effect of a change in economic growth on the change in revenues is less expected, but one possible explanation is that in periods of high growth, governments may implement tax-reducing reforms. Regarding aggregate fiscal data, we find no evidence for an electoral cycle for government deficit and expenditures as suggested by the statistical insignificance of *elec* in columns (1) and (2). These findings are corroborated by the stylized facts presented in the studies of Brender and Drazen (2005) and Shi and Svensson (2006), in which fiscal cycles are driven by the experience of 'new' democracies and less developed countries. Moreover, in accordance with the results of Persson and Tabellini (2003, Chap. 8) and Brender and Drazen (2005) in a context of developed/established democracies, we find a revenue cycle. More specifically, in column (3) the coefficient of variable *elec* is negative and statistically

does not allow us to keep the number of instruments less than or equal to the number of cross sections. As pointed out by Roodman (2009a, 2009b), an excessive number of instruments can result in biasing the results towards those of the OLS.

[19] As can be seen in the tables, the lagged first difference of the dependent variable does not always appear statistically significant in the estimated equations, but qualitative results do not change if we exclude this variable from the specification.

[20] It is worth noting that the right-hand-side variables do not seem to display problematic correlations. In addition, given that $\Delta growth$ and $\Delta lgdppc$ depict some correlation, it should be noted that our results remain unaffected when variable $\Delta growth$ is dropped from the specification. Moreover, in the next section we include in our regressions alternative socio-economic variables, as proposed by Persson and Tabellini (2003), in order to show that our results are not driven by the inclusion of specific control variables.

Table 2 Elections and fiscal policy: basic findings

Dependent variable	Fiscal variables scaled to GDP							Spending variables scaled to total expenditures		Tax variables scaled to total revenues	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Δ *balance*	Δ *expenditures*	Δ *revenues*	Δ *capital*	Δ *current*	Δ *direct*	Δ *indirect*	Δ *capital_exp*	Δ *current_exp*	Δ *direct_rev*	Δ *indirect_rev*
elec	−0.197	0.042	−0.270**	−0.086**	0.132	−0.294***	−0.015	−0.250**	0.232**	−0.327*	0.182
	(−1.09)	(0.30)	(−2.19)	(−2.23)	(1.07)	(−2.76)	(−0.30)	(−2.58)	(2.24)	(−1.71)	(1.11)
ΔY_{t-1}	−0.080	0.105*	0.020	−0.173***	0.163***	0.067	0.021	−0.191***	−0.136**	0.007	0.072
	(−0.74)	(1.83)	(0.39)	(−2.60)	(2.77)	(1.29)	(0.34)	(−3.36)	(−2.25)	(0.10)	(1.02)
Δ *lgdppc*	0.242**	−0.236***	−0.015	−0.020	−0.215***	−0.001	0.010	0.015	−0.021	0.008	0.030
	(2.52)	(−4.40)	(−0.32)	(−1.60)	(−4.50)	(−0.02)	(0.55)	(0.47)	(−0.57)	(0.12)	(0.54)
Δ *growth*	−0.145*	−0.080*	−0.121***	−0.002	−0.081**	−0.105***	−0.029**	0.007	0.002	−0.116**	0.047
	(−1.83)	(−1.95)	(−3.51)	(−0.21)	(−2.22)	(−3.99)	(−2.08)	(0.27)	(0.06)	(−2.30)	(1.07)
R^2	0.239	0.441	0.149	0.143	0.496	0.133	0.093	0.133	0.091	0.079	0.063
N	448	453	453	442	442	452	461	442	442	452	452
Avg. time series length	23.6	23.8	23.8	23.3	23.3	23.8	24.3	23.3	23.3	23.8	23.8
Specification tests											
N_1	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
N_2	0.000	0.000	0.003	0.136	0.000	0.272	0.000	0.027	0.127	0.366	0.648

Notes: The 'Δ' prefix of a variable indicates that the first differences were taken. *t*-statistics, calculated using White's heteroskedasticity robust standard errors, are reported in parentheses. N_1 = Likelihood ratio test for groupwise heteroskedasticity. N_2 = F-test time. *P*-values are reported for the respective tests

***Denotes significance at 1% level. **Denotes significance at 5% level. All regressions include time intercepts. *Denotes significance at 10% level

significant at the 5% level, indicating that the change in government revenues decreases by 0.27% of GDP during an election year.[21]

By looking at the decomposed fiscal data in column (4), we observe that elections have a negative impact on public investment, since *elec* is negatively related to $\Delta capital$ at the 5% level. The change in public investment decelerates by 0.09% of GDP during election years. By way of contrast, in column (5) $\Delta current$ does not seem to be affected during election years. Turning now to the effect of tax variables, in column (6) the change in direct taxation deteriorates by 0.29% of GDP, while in column (7) indirect taxation does not seem to be affected by the election period. These results indicate that direct taxation seems to be the driving force of the government revenue cycle.

Next, in columns 8–11 of Table 2 we test whether elections affect the composition of total expenditures and total revenues. The results are consistent with our previous finding of a fall in government investment before elections. In column (8) we observe that *elec* is negatively related to $\Delta capital\_exp$ at the 5% level, indicating that the change in capital expenditures as a share of total expenditures decreases by 0.25% during an election year. This implies that the change in current expenditures as a percentage of total spending should rise. Indeed, in column (9) the coefficient of *elec* is positively related to $\Delta current\_exp$ at the 5% level, which implies that the change in current expenditures as a share of total expenditures increases by 0.23% during an election year. This result is similar to those obtained by Block (2002) and Vergne (2009), but for a sample of 'new' democracies and developing countries rather than a sample of established/developed democracies as in our case. It is worth noting that the magnitude of fiscal manipulation seems more pronounced in these former studies. In addition, regarding pre-electoral tax composition, *elec* is negatively related to $\Delta direct\_rev$ at the 10% level revealing that only direct taxation seems to be affected in the election year.

These results are consistent with Rogoff's (1990) notion that ego rents from staying in office and information asymmetry induce incumbents to manipulate fiscal policy towards more 'visible' public goods. Our results indicate that the incumbent tries to 'signal' her competence by decreasing 'visible' government revenues, and more particularly direct taxation, in order to provide immediate economic benefit to voters. On the other hand, capital expenditures, which may only be observed by voters with a lag, seem to decrease in the election year. As far as the composition of expenditures is concerned, we observe that public spending shifts towards more 'visible' current expenditures and away from capital expenditures. The fact that we do not observe a PBC seems to confirm the argument that these cycles are driven by the experience of 'new' democracies and less developed countries, where information asymmetries are more pronounced. The results derived with the simple electoral dummy indicate that incumbents in these advanced economies are more reluctant to increase deficits when manipulating fiscal policy, because hi-tech media and advanced accounting practices allow well-informed voters to evaluate more precisely a government's performance (competence). Furthermore, some studies support the view that voters seem to be fiscal conservatives and punish rather than reward loose fiscal policies during election years (see, e.g., Peltzman 1992; Brender and Drazen 2008). This may explain why incumbents on the one hand decrease taxation, while on the other hand restrict the budgetary impact of the fall in revenue by

[21] In the '*Global Development Network Growth Database*' variable overall budget surplus/deficit is calculated as total revenues and official grants received, less total expenditure and lending minus repayments. If we recalculate the overall budget surplus/deficit as total revenues less total expenditures, the results obtained are essentially the same. Moreover, the difference in the coefficients on total revenues and total expenditures is much closer to the estimated coefficient of the budget surplus/deficit.

decreasing public investment. This behavior is consistent with what Schneider (2010) describes as 'fighting with one hand tied behind the back' as a metaphor for incumbents trying to increase political support by manipulating expenditure while being restricted in terms of rising deficits (e.g., due to institutional restrictions or voters' punishment).

4.3.2 Predetermined vs. endogenous election dates

Another interesting issue concerning this literature is that election dates may not be exogenous. As argued by Rogoff (1990), when elections are held at the end of the term, distortions on the shape of fiscal policy can be more severe. The main reason is that incompetent incumbents may prefer to wait until the end of the term in order to receive additional ego rents from staying in office. At the same time, when the election date is known in advance, an opportunistic incumbent has time to use fiscal policy in order to increase re-election probabilities, far greater, compared to the case of elections being called earlier.

One way to address the issue of early elections, based on the approach of Brender and Drazen (2005), is to look at the constitutionally determined election interval and take as predetermined those elections which are held during the expected year of the constitutionally fixed term. We use the '*Inter-Parliamentary Union*' database that allows us to draw an inference as to whether elections were held in the expected year or not. Hence, we separate binary indicator *elec* into variables *elec_pred* and *elec_end*, for predetermined and endogenous election dates, respectively. In our case, among the 125 elections in the sample, 64 elections are classified as predetermined.

As can be seen in Table 3, the impact of *elec_pred* on fiscal variables supports our previous results of a pre-electoral decrease in total revenue attributed to a fall in direct taxation as well as a decrease in capital expenditures. The latter result is reflected in a shift of the composition of public spending towards more 'visible' current expenditures and away from capital expenditures. Additionally, the significance levels for the estimates of *elec_pred* remain the same as those obtained in Table 2 for variable *elec*. Regarding the endogenously-chosen electoral procedures, the coefficient of variable *elec_end* is insignificantly related to all fiscal variables, while the coefficient of variable $\Delta balance$ ceases to be significant at the 10% level. The contrast between the results for predetermined and endogenous elections confirms that we need to make this distinction, in order to properly identify how electoral procedures shape fiscal policy.

4.3.3 Weighted electoral indicator

Moving one step forward, in Table 4 we use the alternative electoral indicators $elec_t\_2$ and ele_ct-1_2 that take into account the exact timing of elections. Thus, we proceed into a four-way split. More precisely, we split indicator *elec_2* into $elec_t\_pred\_2$ ($elec_t\_end\_2$) and $elec_{t-1}\_pred\_2$ ($elec_{t-1}\_end\_2$), for predetermined (endogenous) election and pre-election years, respectively.

Regarding the case of predetermined elections, one can draw two main conclusions from Table 4. Firstly, all electoral effects are captured by $elec_t\_pred\_2$, while in all cases the coefficients of variable $elec_{t-1}\_pred\_2$ are insignificantly related to fiscal variables. This finding indicates that electoral manipulation of fiscal policy occurs close to the election date. Secondly, the impact of $elec_t\_pred\_2$ on the fiscal variables supports our findings in Table 3. More specifically, according to the election-year dummy (indicator) in Table 3 (4), the change in revenues/direct taxation decreases by 0.36%/0.37% (0.24%/0.23%) of

Table 3 Predetermined vs. endogenous elections

Dependent variable	Fiscal variables scaled to GDP							Spending variables scaled to total expenditures		Tax variables scaled to total revenues	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Δ *balance*	Δ *expenditures*	Δ *revenues*	Δ *capital*	Δ *current*	Δ *direct*	Δ *indirect*	Δ *capital_exp*	Δ *current_exp*	Δ *direct_rev*	Δ *indirect_rev*
elec_pred	−0.092	−0.105	−0.356**	−0.112**	0.032	−0.372***	−0.021	−0.294**	0.245*	−0.390*	0.231
	(−0.40)	(−0.72)	(−2.56)	(−2.40)	(0.26)	(−3.06)	(−0.38)	(−2.55)	(1.91)	(−1.84)	(1.28)
elec_end	−0.351	0.259	−0.143	−0.045	0.287	−0.178	−0.006	−0.181	0.212	−0.233	0.108
	(−1.58)	(1.14)	(−0.77)	(−0.80)	(1.37)	(−1.15)	(−0.08)	(−1.30)	(1.54)	(−0.75)	(0.42)
$\Delta\ Y_{t-1}$	−0.077	0.101*	0.016	−0.176***	0.161***	0.064	0.021	−0.192***	−0.136**	0.006	0.071
	(−0.71)	(1.75)	(0.31)	(−2.62)	(2.73)	(1.24)	(0.34)	(−3.36)	(−2.23)	(0.09)	(0.99)
Δ *lgdppc*	0.242**	−0.237***	−0.015	−0.020	−0.215***	−0.001	0.010	0.015	−0.021	0.008	0.030
	(2.51)	(−4.41)	(−0.33)	(−1.60)	(−4.49)	(−0.02)	(0.55)	(0.48)	(−0.57)	(0.13)	(0.53)
Δ *growth*	−0.143*	−0.081**	−0.122***	−0.003	−0.082**	−0.107***	−0.029**	0.006	0.002	−0.117**	0.048
	(−1.79)	(−1.98)	(−3.54)	(−0.26)	(−2.25)	(−4.03)	(−2.08)	(0.24)	(0.07)	(−2.31)	(1.08)
R^2	0.240	0.444	0.151	0.144	0.498	0.135	0.093	0.134	0.091	0.079	0.064
N	448	453	453	442	442	452	461	442	442	452	452
Avg. time series length	23.6	23.8	23.8	23.3	23.3	23.8	24.3	23.3	23.3	23.8	23.8
Specification tests											
N_1	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
N_2	0.000	0.000	0.004	0.126	0.000	0.301	0.000	0.026	0.147	0.371	0.666

Notes: see Table 2

Table 4 Alternative timing of elections and predetermined vs. endogenous elections

Dependent variable	Fiscal variables scaled to GDP							Spending variables scaled to total expenditures		Tax variables scaled to total revenues	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Δ *balance*	Δ *expenditures*	Δ *revenues*	Δ *capital*	Δ *current*	Δ *direct*	Δ *indirect*	Δ *capital_exp*	Δ *current_exp*	Δ *direct_rev*	Δ *indirect_rev*
$elec_t$_*pred*_2	−0.053	−0.223	−0.413*	−0.182**	−0.024	−0.394**	−0.009	−0.527***	0.362*	−0.256	0.163
	(−0.16)	(−0.94)	(−1.84)	(−2.51)	(−0.12)	(−2.01)	(−0.11)	(−2.82)	(1.71)	(−0.78)	(0.64)
$elec_{t-1}$_*pred*_2	−0.404	−0.109	−0.221	−0.059	0.065	0.070	−0.057	−0.334	0.301	0.533	−0.198
	(−1.03)	(−0.32)	(−0.79)	(−0.61)	(0.21)	(0.30)	(−0.41)	(−1.21)	(1.05)	(1.09)	(−0.44)
$elec_t$_*end*_2	−0.704**	0.643**	−0.016	−0.032	0.675**	−0.030	−0.008	−0.272	0.296	−0.024	−0.111
	(−2.25)	(2.15)	(−0.06)	(−0.44)	(2.54)	(−0.16)	(−0.08)	(−1.35)	(1.50)	(−0.05)	(−0.38)
$elec_{t-1}$_*end*_2	0.618	0.250	0.866***	0.034	0.023	0.694**	−0.132	0.000	−0.290	0.521	−1.378***
	(1.17)	(0.57)	(2.60)	(0.21)	(0.06)	(2.19)	(−0.80)	(0.00)	(−0.88)	(0.76)	(−3.04)
Δ Y_{t-1}	−0.074	0.099*	0.013	−0.173***	0.158***	0.055	0.021	−0.186***	−0.132**	0.003	0.063
	(−0.67)	(1.70)	(0.24)	(−2.61)	(2.67)	(1.04)	(0.33)	(−3.32)	(−2.22)	(0.04)	(0.89)
Δ *lgdppc*	0.240**	−0.238***	−0.017	−0.020	−0.216***	−0.003	0.010	0.015	−0.021	0.006	0.033
	(2.49)	(−4.42)	(−0.37)	(−1.59)	(−4.50)	(−0.09)	(0.57)	(0.48)	(−0.56)	(0.09)	(0.59)
Δ *growth*	−0.140*	−0.080**	−0.119***	−0.003	−0.083**	−0.105***	−0.029**	0.007	0.001	−0.117**	0.045
	(−1.75)	(−1.97)	(−3.45)	(−0.26)	(−2.26)	(−3.94)	(−2.09)	(0.26)	(0.04)	(−2.30)	(1.02)
R^2	0.246	0.449	0.157	0.145	0.504	0.136	0.094	0.139	0.094	0.079	0.079
N	448	453	453	442	442	452	461	442	442	452	452
Avg. time series length	23.6	23.8	23.8	23.3	23.3	23.8	24.3	23.3	23.3	23.8	23.8
Specification tests											
N_1	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
N_2	0.000	0.000	0.007	0.176	0.000	0.380	0.000	0.035	0.177	0.399	0.642

Notes: see Table 2

GDP during predetermined elections.[22] At the same time, both electoral indicators suggest a decrease in the change in capital expenditures as a percentage of GDP by 0.11% during predetermined elections. Given that the mean value of capital expenditures in the sample is 2.53% of GDP, the estimate implies that, on average, capital expenditures change decreases by 4.4% during predetermined elections. Moreover, the election year dummy (indicator) in Table 3 (4) implies that the change in capital expenditures as a percentage of total expenditures falls by 0.29% (0.31%), whereas the change in current expenditures as a percentage of total expenditures increases by 0.25% (0.21%) during predetermined elections.

For endogenous elections dates, the results reveal a significant fiscal balance cycle that seems to be driven by an increase in current expenditures immediately before elections, as shown by the coefficient of $elec_t\_end\_2$. Hence, according to the election year indicator in Table 4, the change in deficit increases by 0.46% of GDP during endogenous elections. Moreover, according to the estimated coefficient of the variable $elec_t\_end\_2$, the change in expenditures (current expenditures) increases by 0.42% (0.44%) of GDP. It is worth noting that the simple electoral dummy in Table 3 did not reveal a cycle in expenditures. Most surprisingly, the additional results show that in the 'early' pre-election period, captured by $elec_{t-1}\_end\_2$, the change in revenues rises on average by 0.43% of GDP. This rise in revenues seems to be driven by an increase in the change in direct taxation by 0.34% of GDP. One possible explanation is that incumbents may call for elections prematurely when fiscal conditions are favorable, because the rise in revenue provides them a 'leeway' for pre-electoral spending, while incumbents without this option can only rely on fiscal manipulation (see Heckelman and Berument 1998). Our results indicate that this favorable fiscal condition occurs in the 'early' pre-election period, captured by $elec_{t-1}\_end\_2$, and is followed by a rise in current expenditures and deficit in the period immediately before elections, captured by $elec_t\_end\_2$.

These findings suggest a different behavior on behalf of the incumbent, which depends on the timing of elections. In particular, for predetermined elections an incumbent has the opportunity to shape fiscal policy far greater than if early elections were held. Hence, opportunistic incumbents provide immediate benefits to voters by decreasing direct taxation, while public investment seems to deteriorate so that the fiscal balance remains unaffected. On the other hand, although early and unexpected elections may follow a period of favorable fiscal conditions, short campaign periods may induce incumbent to react abruptly and engage in expansionary fiscal policy close to the election date despite the fact that this behavior may have counter effects if it is perceived by voters as electoral manipulation.

To sum up, our results for competitive elections in high-income OECD countries and 'old' democracies reveal that incumbents manipulate fiscal policy in order to stay in office. When elections are exogenous, this manipulation affects the composition of fiscal policy while deficits remain unaffected. In fact, our results support the prediction of our theoretical model and Rogoff (1990) for a decrease in public investment in the election period. This fall in capital spending seems to allow for lower direct taxation. Moreover, our findings indicate that incumbents in high-income 'old' democracies manipulate fiscal policy instruments more cautiously than incumbents in 'new' democracies and developing countries. This inference is supported by the magnitude of fiscal policy manipulation in our sample, which seems much smaller in comparison with similar studies for 'new' democracies and developing countries according to Block (2002) and Vergne (2009).

[22] Regarding the calculations for the weighted electoral indicator, we followed the approach of Mink and de Haan (2006) and multiplied the estimated coefficients for the election-year indicator, presented in Table 4, by the average value of the weighted indicator over all predetermined (or endogenous) election years.

4.4 Sensitivity analysis

In this section we examine the robustness of our results by re-estimating the regressions under various modifications. *First*, we estimate (18) with all variables measured in levels. *Second*, we attempt to expand the time span of our panel by merging the *Government Finance Statistics Manual 1986* (GFSM 1986) classification of fiscal variables and the *Government Finance Statistics Manual 2001* (GFSM 2001) framework. *Third*, we re-estimate our baseline specification using alternative control variables as proposed by Persson and Tabellini (2003). *Fourth*, we apply the method developed by Hadi (1992) in order to identify and drop outlier observations from our regressions. *Finally*, we conduct some additional empirical checks in order to assess the robustness of our empirical findings. In order to save some space, we present analytical results only for our basic election dummy *elec*, while for specifications in Tables 3 and 4 we present results only for the electoral indicators.[23]

4.4.1 Regressions in levels

So far we have presented our main results from estimating (19), where we first differenced our dependent variable and all covariates of our model with the exception of the election-indicator variables. However, given the low power of panel unit root tests it is difficult to argue for the statistical properties of our data series with certainty. Hence, in this section, we estimate (18) where variables are expressed in levels. This specification is closely related to the theory developed in Sect. 3 and is the usual approach followed in the prior literature. According to the F-test results presented in Tables 5a and 5b, we can clearly reject the hypothesis that all country fixed effects are jointly zero and we observe that time fixed effects are in general significant and therefore they are included in the regressions.

As shown in Tables 5a and 5b, our results regarding the electoral impact on fiscal policy are very similar to those from our baseline specification in first differences. More specifically, for predetermined elections we obtain a significant government revenue cycle, a fall in public investment and a shift in spending composition towards more 'visible' public goods. Again, the revenue cycle takes the form of a fall in direct taxation financed by a fall in capital expenditure. Our previous finding that these results apply to predetermined elections continues to hold. Furthermore, we find evidence indicating that in endogenous elections incumbents behave quite differently by adopting expansionary policies that deteriorate the fiscal balance.

4.4.2 Expanded sample

As already mentioned, the dataset used in this paper is obtained from the '*Global Development Network Growth Database*', whose primary source is the *GFS* database. It covers consolidated central government accounts and spans from 1972 through 1999. In this subsection we attempt to update our dataset by bridging the GFSM 1986 classification of fiscal variables and the GFSM 2001 framework. The oldest accounting practice provides observations for each country until 1999 or 2000, while the later has only been back-dated until 1990. It is, however, difficult to bridge the two classifications since fiscal variables are measured on a 'cash' basis in GFSM 1986 and on an 'accrual' basis in GFSM 2001 classification. This implies that expanding our data set beyond 1999 might be related to a number of inaccuracies of unknown magnitude.

[23] The full set of results is available upon request.

Table 5a Robustness: regressions in levels

Dependent variable	Fiscal variables scaled to GDP							Spending variables scaled to total expenditures		Tax variables scaled to total revenues	
	(1) *balance*	(2) *expenditures*	(3) *revenues*	(4) *capital*	(5) *current*	(6) *direct*	(7) *indirect*	(8) *capital_exp*	(9) *current_exp*	(10) *direct_rev*	(11) *indirect_rev*
elec	−0.183	0.005	−0.267**	−0.077**	0.097	−0.270***	−0.021	−0.227**	0.212**	−0.327*	0.173
	(−1.08)	(0.03)	(−2.21)	(−1.97)	(0.78)	(−2.69)	(−0.43)	(−2.32)	(2.07)	(−1.73)	(1.13)
Y_{t-1}	0.719***	0.832***	0.805***	0.748***	0.848***	0.780***	0.775***	0.760***	0.754***	0.808***	0.788***
	(12.44)	(29.66)	(23.20)	(18.31)	(32.07)	(19.48)	(18.17)	(22.17)	(21.98)	(17.32)	(20.07)
lgdppc	3.958	−1.390	0.854	−0.187	−1.309	0.374	0.170	0.170	−0.252	−0.271	−0.834
	(1.38)	(−0.96)	(0.66)	(−0.47)	(−1.00)	(0.36)	(0.38)	(0.16)	(−0.23)	(−0.15)	(−0.62)
growth	0.093	−0.334***	−0.170***	−0.018	−0.316***	−0.130***	−0.018	0.035	−0.034	−0.079*	0.108**
	(1.38)	(−7.91)	(−4.07)	(−1.64)	(−8.08)	(−4.56)	(−1.14)	(1.22)	(−1.09)	(−1.70)	(2.29)
R^2	0.642	0.897	0.830	0.689	0.924	0.790	0.747	0.789	0.793	0.696	0.695
N	469	474	474	464	464	473	481	464	464	473	473
Avg. time series length	24.7	24.9	24.9	24.4	24.4	24.9	25.3	24.4	24.4	24.9	24.9
Specification tests											
N_1	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
N_2	0.004	0.000	0.000	0.001	0.000	0.000	0.008	0.001	0.001	0.001	0.000
N_3	0.000	0.002	0.112	0.141	0.000	0.343	0.000	0.100	0.097	0.028	0.558

Notes: t-statistics, calculated using White's heteroskedasticity robust standard errors, are reported in parentheses. N_1 = Likelihood ratio test for groupwise heteroskedasticity. N_2 = F-test country. N_3 = F-test time. P-values are reported for the respective tests. All regressions include country and time intercepts

***Denotes significance at 1% level. **Denotes significance at 5% level. *Denotes significance at 10% level

Table 5b Robustness: regressions in levels and predetermined vs. endogenous elections

Dependent variable	Fiscal variables scaled to GDP							Spending variables scaled to total expenditures		Tax variables scaled to total revenues	
	(1) *balance*	(2) *expenditures*	(3) *revenues*	(4) *capital*	(5) *current*	(6) *direct*	(7) *indirect*	(8) *capital_exp*	(9) *current_exp*	(10) *direct_rev*	(11) *indirect_rev*
elec_pred	−0.027	−0.153	−0.345**	−0.112**	−0.024	−0.346***	−0.010	−0.288**	0.247**	−0.388*	0.264
	(−0.12)	(−1.06)	(−2.58)	(−2.38)	(−0.19)	(−2.94)	(−0.17)	(−2.48)	(1.98)	(−1.80)	(1.44)
elec_end	−0.426**	0.248	−0.146	−0.021	0.292	−0.153	−0.039	−0.129	0.157	−0.232	0.032
	(−1.96)	(1.01)	(−0.79)	(−0.36)	(1.31)	(−1.07)	(−0.53)	(−0.92)	(1.12)	(−0.75)	(0.13)
$elec_t$_pred_2	−0.015	−0.244	−0.424**	−0.186***	−0.062	−0.405**	0.024	−0.508***	0.362*	−0.358	0.310
	(−0.04)	(−1.05)	(−2.03)	(−2.73)	(−0.31)	(−2.09)	(0.27)	(−2.86)	(1.89)	(−1.03)	(1.12)
$elec_{t-1}$_pred_2	−0.258	−0.189	−0.193	−0.064	−0.075	0.105	−0.045	−0.298	0.259	0.571	−0.193
	(−0.68)	(−0.63)	(−0.71)	(−0.66)	(−0.27)	(0.47)	(−0.31)	(−1.12)	(0.93)	(1.24)	(−0.42)
$elec_t$_end_2	−0.790***	0.660*	0.004	0.012	0.670**	0.024	−0.057	−0.139	0.149	0.017	−0.218
	(−2.64)	(1.94)	(0.02)	(0.17)	(2.22)	(0.12)	(−0.61)	(−0.77)	(0.82)	(0.04)	(−0.73)
$elec_{t-1}$_end_2	0.156	0.446	0.611*	0.134	0.196	0.527*	−0.204	0.221	−0.398	0.426	−1.435***
	(0.31)	(1.14)	(1.85)	(0.81)	(0.60)	(1.79)	(−1.36)	(0.48)	(−0.94)	(0.69)	(−3.35)

Notes: see Table 5a

Taking into account the problems that may arise, and applying the approach of Gemmell et al. (2007) we use the following rules in order to proxy for the evolution of fiscal variables beyond 1999[24]: (i) if a country's series overlaps, we project the annual rate of change of the new data for central government to the old data and we update the series until 2008, (ii) if a country's series does not overlap, we unite data streams only if the difference between the last observation of the old dataset and the first observation of the new dataset is below one percent of GDP. In particular, for Luxemburg and Norway, where we observe from one year to the other differences above 5% of GDP, we use only the old data. For Japan and Sweden, where the new data do not provide observations beyond 1999, we likewise use only the old data. This procedure allows us to expand our sample until 2008 for seven out of the eleven fiscal indicators we use in this paper and include in our specification 46 new electoral procedures (37 of which are classified as predetermined). Unfortunately, it is not possible to update variables $\Delta capital$, $\Delta current$, $\Delta capital\_exp$, $\Delta current\_exp$.[25] For a detailed description of the series used under both classification systems see the Appendix.

Regarding the effect of elections on fiscal policy, apart from some negligible changes in the significance levels of a few fiscal indicators, the qualitative results presented in Tables 6a and 6b remain essentially the same as those depicted in Tables 2–4.

4.4.3 Alternative control variables

Until now we have included in our baseline regressions the time varying socio-economic variables proposed by Shi and Svensson (2006). In order to test if our results are driven by this specification, we introduce in our estimated equation some alternative control variables as proposed by Persson and Tabellini (2003). In particular, since our specification is in first differences, we use the change in the log of real per capita GDP ($\Delta lgdppc$) as we expect, according to Wagner's law, that richer countries should have larger public sectors. Moreover, we use the change in output gap ($\Delta gdphp$) to control for fluctuations in fiscal policy, induced by the business cycle and defined as the change in the log difference between real GDP and its country specific trend. Furthermore, we use two demographic variables representing the change in the percentage of population aged 15–64 ($\Delta pop1564$), and above 65 years of age ($\Delta pop65+$), as demographic evolution may put pressure on the public budget. Finally, we use the change in the degree of a country's openness to trade ($\Delta trade$), since according to Rodrik's (1998) proposition, more open economies are expected to have larger public sectors as a safety net against the exposure to the terms of trade risk. All macroeconomic data for control variables are expressed as percentages and are obtained from World Bank's '*World Development Indicators*' (*WDI*).

Regarding the socio-economic variables, we observe that the coefficient of $\Delta lgdppc$ is negative when statistically significant, while the coefficient on $\Delta gdphp$ is positive and significant when included with variable $\Delta revenues$. Moreover, for the two demographic variables, $\Delta pop1564$ and $\Delta pop65+$, we get mixed results, although in most of the cases they

[24] It is worth noting that Brender and Drazen (2009) also update their dataset by bridging the GFSM 1986 classification of fiscal variables and the GFSM 2001 framework following a similar method.

[25] The new classification does not longer provide data for the capital expenditures and current expenditures series included in the GFSM 1986 classification. This occurs for two reasons. First, *capital transfers*, one of the four components of capital expenditures are classified as variable *expense* that also includes current expenditure in GFSM 2001. Second, the other three components of capital expenditures, i.e., *the acquisition of fixed capital assets, purchases of stocks and land and intangible assets*, are classified on a net basis (acquisition minus disposals) in contrast to the GFSM 1986 classification where they are classified as simply acquisitions. For more details see: www.imf.org/external/pubs/ft/gfs/manual/pdf/class.pdf.

Table 6a Robustness: expanded sample

Dependent variable	Fiscal variables scaled to GDP					Tax variables scaled to total revenues	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Δ *balance*	Δ *expenditures*	Δ *revenues*	Δ *direct*	Δ *indirect*	Δ *direct_rev*	Δ *indirect_rev*
elec	−0.193	0.129	−0.167	−0.210**	0.011	−0.234	0.135
	(−1.35)	(1.09)	(−1.57)	(−2.30)	(0.24)	(−1.45)	(0.92)
Δ Y_{t-1}	−0.073	0.073	−0.011	0.029	0.040	−0.003	0.036
	(−0.74)	(1.47)	(−0.25)	(0.61)	(0.73)	(−0.05)	(0.57)
Δ *lgdppc*	0.223***	−0.237***	−0.019	−0.008	0.011	0.004	0.026
	(2.72)	(−5.10)	(−0.46)	(−0.28)	(0.66)	(0.07)	(0.47)
Δ *growth*	−0.133*	−0.071∗	−0.118***	−0.102***	−0.028**	−0.118**	0.052
	(−1.86)	(−1.90)	(−3.68)	(−4.19)	(−2.09)	(−2.57)	(1.23)
R^2	0.256	0.418	0.157	0.145	0.096	0.085	0.076
N	584	578	584	584	581	584	573
Avg. time series length	30.7	30.4	30.7	30.7	30.6	30.7	30.2
Specification tests							
N_1	0.000	0.000	0.000	0.000	0.000	0.000	0.000
N_2	0.000	0.000	0.000	0.000	0.000	0.029	0.132

Notes: see Table 2

appear to be statistically insignificant. Finally, Δ*trade* does not seem to verify Rodrik (1998) proposition, as in most of the cases there is a negative relation between fiscal variables and countries' openness to trade.

Regarding the impact of elections on fiscal variables, the qualitative results in Tables 7a and 7b are in line with those depicted in Tables 2–4.

4.4.4 Testing for outliers

As a next step, in order to further increase the precision of our results and ensure that they are not driven by extreme values, we use the Hadi (1992) method that identifies multiple outliers in multivariate data. This method measures the distance of data points from the main body of data and then iteratively reduces the sample to exclude distant data points.[26]

Hadi's (1992) approach identifies three to ten outlier observations for the fiscal variables used in the estimated equations. In Tables 8a and 8b we re-estimate our regressions after dropping the identified outlier observations. As can be seen, the only notable change is that Δ*current_exp* loses its significance. At the same time, for the case of endogenous elections the impact of the simple electoral indicator, *elec_end,* is now very similar to the impact of the weighted electoral indicator, $elec_t$*_end*_2.

[26] We set the significance level for outlier cutoff at $p = 0.1$.

Table 6b Robustness: expanded sample and predetermined vs. endogenous elections

Dependent variable	Fiscal variables scaled to GDP					Tax variables scaled to total revenues	
	(1) Δ *balance*	(2) Δ *expenditures*	(3) Δ *revenues*	(4) Δ *direct*	(5) Δ *indirect*	(6) Δ *direct_rev*	(7) Δ *indirect_rev*
elec_pred	−0.108	−0.000	−0.267**	−0.310***	0.017	−0.352**	0.258
	(−0.61)	(−0.00)	(−2.29)	(−3.13)	(0.33)	(−2.01)	(1.60)
elec_end	−0.335*	0.355*	0.002	−0.041	−0.000	−0.037	−0.066
	(−1.79)	(1.67)	(0.01)	(−0.27)	(−0.00)	(−0.13)	(−0.28)
$elec_t$_pred_2	−0.103	−0.054	−0.338*	−0.354**	0.049	−0.257	0.316
	(−0.40)	(−0.27)	(−1.73)	(−2.16)	(0.57)	(−0.90)	(1.22)
$elec_{t-1}$_pred_2	−0.320	−0.025	−0.173	0.076	−0.047	0.634	−0.241
	(−0.97)	(−0.08)	(−0.70)	(0.38)	(−0.41)	(1.58)	(−0.63)
$elec_t$_end_2	−0.603**	0.754***	0.178	0.129	0.024	0.189	−0.243
	(−2.35)	(2.72)	(0.73)	(0.66)	(0.27)	(0.46)	(−0.85)
$elec_{t-1}$_end_2	0.396	0.280	0.664**	0.625**	−0.121	0.613	−1.108***
	(0.91)	(0.78)	(2.27)	(2.35)	(−0.92)	(1.10)	(−2.93)

Notes: see Table 2

Table 7a Robustness: alternative control variables

Dependent variable	Fiscal variables scaled to GDP							Spending variables scaled to total expenditures		Tax variables scaled to total revenues	
	(1) Δ *balance*	(2) Δ *expenditures*	(3) Δ *revenues*	(4) Δ *capital*	(5) Δ *current*	(6) Δ *direct*	(7) Δ *indirect*	(8) Δ *capital_exp*	(9) Δ *current_exp*	(10) Δ *direct_rev*	(11) Δ *indirect_rev*
elec	−0.216	0.045	−0.285**	−0.086**	0.137	−0.301***	−0.014	−0.252***	0.234**	−0.327	0.199
	(−1.21)	(0.32)	(−2.24)	(−2.23)	(1.09)	(−2.68)	(−0.27)	(−2.60)	(2.26)	(−1.64)	(1.22)
Δ Y_{t-1}	−0.074	0.070	−0.014	−0.179***	0.114**	0.041	0.028	−0.196***	−0.143**	−0.003	0.072
	(−0.65)	(1.28)	(−0.28)	(−2.64)	(2.11)	(0.81)	(0.40)	(−3.40)	(−2.33)	(−0.05)	(1.01)
Δ *lgdppc*	0.085	−0.293***	−0.233***	−0.030	−0.254***	−0.147**	−0.018	−0.004	−0.008	−0.088	0.113
	(0.54)	(−2.88)	(−2.98)	(−1.10)	(−2.93)	(−2.39)	(−0.48)	(−0.07)	(−0.10)	(−0.73)	(1.13)
Δ *gdphp*	0.064	−0.005	0.166**	0.010	−0.030	0.086	0.011	0.029	−0.014	0.008	−0.062
	(0.40)	(−0.04)	(1.97)	(0.32)	(−0.32)	(1.24)	(0.25)	(0.41)	(−0.16)	(0.05)	(−0.48)
Δ *pop65+*	1.767**	−0.425	0.674	−0.272*	−0.756	0.128	0.022	−0.638*	0.722*	−0.934	−0.473
	(2.29)	(−0.62)	(1.04)	(−1.78)	(−1.42)	(0.21)	(0.09)	(−1.89)	(1.68)	(−0.80)	(−0.63)
Δ *pop1564*	0.998*	−0.277	0.517	−0.068	−0.532	0.466	0.020	0.055	0.087	0.287	−0.141
	(1.66)	(−0.64)	(1.28)	(−0.59)	(−1.53)	(1.35)	(0.14)	(0.19)	(0.27)	(0.40)	(−0.30)
Δ *trade*	−0.039	−0.014	−0.037*	−0.001	−0.014	−0.026*	−0.024**	0.009	−0.017	−0.003	−0.046*
	(−0.95)	(−0.53)	(−1.91)	(−0.07)	(−0.63)	(−1.72)	(−2.44)	(0.63)	(−1.07)	(−0.10)	(−1.77)
R^2	0.239	0.437	0.145	0.148	0.494	0.117	0.108	0.140	0.100	0.072	0.072
N	448	453	453	442	442	452	461	442	442	452	452
Avg. time series length	23.6	23.8	23.8	23.3	23.3	23.8	24.3	23.3	23.3	23.8	23.8
Specification tests											
N_1	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
N_2	0.000	0.000	0.003	0.122	0.000	0.082	0.002	0.049	0.214	0.143	0.301

Notes: see Table 2

Table 7b Robustness: alternative control variables and predetermined vs. endogenous elections

Dependent variable	Fiscal variables scaled to GDP							Spending variables scaled to total expenditures		Tax variables scaled to total revenues	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Δ *balance*	Δ *expenditures*	Δ *revenues*	Δ *capital*	Δ *current*	Δ *direct*	Δ *indirect*	Δ *capital_exp*	Δ *current_exp*	Δ *direct_rev*	Δ *indirect_rev*
elec_pred	−0.062	−0.102	−0.351**	−0.113**	0.040	−0.361***	−0.014	−0.301**	0.251*	−0.379*	0.241
	(−0.27)	(−0.69)	(−2.46)	(−2.42)	(0.32)	(−2.83)	(−0.26)	(−2.57)	(1.94)	(−1.76)	(1.33)
elec_end	−0.444**	0.262	−0.187	−0.042	0.288	−0.211	−0.012	−0.177	0.208	−0.251	0.137
	(−2.04)	(1.13)	(−0.99)	(−0.76)	(1.36)	(−1.31)	(−0.16)	(−1.29)	(1.53)	(−0.78)	(0.54)
$elec_t$_pred_2	−0.002	−0.232	−0.415*	−0.187**	−0.029	−0.385*	−0.005	−0.539***	0.373∗	−0.246	0.169
	(−0.01)	(−0.97)	(−1.78)	(−2.58)	(−0.15)	(−1.86)	(−0.06)	(−2.85)	(1.75)	(−0.73)	(0.66)
$elec_{t-1}$_pred_2	−0.478	−0.116	−0.291	−0.056	0.071	0.023	−0.074	−0.325	0.282	0.518	−0.193
	(−1.26)	(−0.34)	(−1.01)	(−0.57)	(0.22)	(0.09)	(−0.53)	(−1.19)	(1.00)	(1.03)	(−0.43)
$elec_t$_end_2	−0.786**	0.651**	−0.048	−0.029	0.680**	−0.055	−0.008	−0.269	0.296	−0.041	−0.073
	(−2.54)	(2.14)	(−0.19)	(−0.39)	(2.56)	(−0.27)	(−0.08)	(−1.36)	(1.53)	(−0.09)	(−0.25)
$elec_{t-1}$_end_2	0.601	0.314	0.905***	0.043	0.071	0.747**	−0.112	0.013	−0.304	0.611	−1.361∗∗∗
	(1.14)	(0.76)	(2.74)	(0.27)	(0.22)	(2.30)	(−0.68)	(0.03)	(−0.92)	(0.86)	(−2.94)

Notes: see Table 2

Table 8a Robustness: testing for outliers

Dependent variable	Fiscal variables scaled to GDP							Spending variables scaled to total expenditures		Tax variables scaled to total revenues	
	(1) Δ *balance*	(2) Δ *expenditures*	(3) Δ *revenues*	(4) Δ *capital*	(5) Δ *current*	(6) Δ *direct*	(7) Δ *indirect*	(8) Δ *capital_exp*	(9) Δ *current_exp*	(10) Δ *direct_rev*	(11) Δ *indirect_rev*
elec	−0.249	0.092	−0.247**	−0.070**	0.165	−0.272***	−0.026	−0.167**	0.148	−0.287	0.174
	(−1.59)	(0.70)	(−2.25)	(−1.98)	(1.45)	(−2.94)	(−0.57)	(−2.00)	(1.59)	(−1.56)	(1.18)
Δ Y_{t-1}	0.027	0.096*	0.002	−0.191***	0.164***	0.075	0.070	−0.175***	−0.108**	0.088*	0.016
	(0.54)	(1.92)	(0.05)	(−3.38)	(3.14)	(1.64)	(1.23)	(−3.78)	(−2.01)	(1.84)	(0.27)
Δ *lgdppc*	0.149***	−0.202***	−0.010	−0.011	−0.171***	−0.023	0.020	0.038	−0.007	0.016	0.022
	(2.81)	(−4.14)	(−0.29)	(−1.08)	(−4.16)	(−0.83)	(1.25)	(1.54)	(−0.22)	(0.27)	(0.40)
Δ *growth*	−0.030	−0.082**	−0.100***	−0.000	−0.075**	−0.079***	−0.029**	0.012	−0.021	−0.128***	0.055
	(−0.64)	(−2.08)	(−3.61)	(−0.03)	(−2.13)	(−3.43)	(−2.22)	(0.61)	(−0.77)	(−2.63)	(1.26)
R^2	0.269	0.430	0.154	0.141	0.471	0.158	0.138	0.149	0.090	0.108	0.096
N	441	450	445	434	439	442	455	432	436	448	447
Avg. time series length	23.2	23.7	23.4	22.8	23.1	23.3	23.9	22.7	22.9	23.6	23.5
Specification tests											
N_1	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
N_2	0.000	0.000	0.003	0.192	0.000	0.186	0.000	0.027	0.108	0.221	0.022

Notes: *t*-statistics, calculated using White's heteroskedasticity robust standard errors, are reported in parentheses. Regressions are estimated after using Hadi (1992) method to identify and eliminate outlier observations. N_1 = Likelihood ratio test for groupwise heteroskedasticity. N_2 = F-test time. *P*-values are reported for the respective tests. All regressions include time intercepts

***Denotes significance at 1% level **Denotes significance at 5% level and *Denotes significance at 10% level

Table 8b Robustness: testing for outliers and predetermined vs. endogenous elections

Dependent variable	Fiscal variables scaled to GDP							Spending variables scaled to total expenditures		Tax variables scaled to total revenues	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Δ *balance*	Δ *expenditures*	Δ *revenues*	Δ *capital*	Δ *current*	Δ *direct*	Δ *indirect*	Δ *capital_exp*	Δ *current_exp*	Δ *direct_rev*	Δ *indirect_rev*
elec_pred	−0.150	−0.105	−0.385***	−0.091**	0.012	−0.349***	−0.020	−0.173*	0.117	−0.398**	0.273
	(−0.79)	(−0.73)	(−3.21)	(−2.19)	(0.10)	(−3.60)	(−0.37)	(−1.85)	(1.08)	(−2.01)	(1.55)
elec_end	−0.391*	0.389*	−0.041	−0.037	0.405**	−0.161	−0.035	−0.158	0.195	−0.121	0.023
	(−1.82)	(1.89)	(−0.24)	(−0.68)	(2.18)	(−1.04)	(−0.54)	(−1.18)	(1.44)	(−0.41)	(0.11)
$elec_t$_pred_2	−0.188	−0.245	−0.472**	−0.147**	−0.012	−0.354**	−0.015	−0.255*	0.117	−0.238	0.207
	(−0.65)	(−1.08)	(−2.48)	(−2.30)	(−0.06)	(−2.48)	(−0.20)	(−1.92)	(0.72)	(−0.78)	(0.83)
$elec_{t-1}$_pred_2	−0.451	−0.156	−0.199	−0.009	0.068	0.155	−0.174	−0.136	0.175	0.735	−0.431
	(−1.36)	(−0.47)	(−0.75)	(−0.10)	(0.23)	(0.71)	(−1.60)	(−0.59)	(0.66)	(1.64)	(−1.22)
$elec_t$_end_2	−0.737**	0.764***	0.079	−0.020	0.793***	−0.001	−0.042	−0.234	0.267	0.237	−0.183
	(−2.43)	(2.66)	(0.33)	(−0.28)	(3.14)	(−0.00)	(−0.45)	(−1.19)	(1.35)	(0.59)	(−0.73)
$elec_{t-1}$_end_2	0.639	0.245	0.888***	0.039	0.053	0.576**	−0.111	0.129	−0.100	0.739	−1.221***
	(1.22)	(0.56)	(2.85)	(0.40)	(0.15)	(2.32)	(−0.71)	(0.53)	(−0.42)	(1.32)	(−2.80)

Notes: see Table 8a

4.4.5 Some additional robustness checks

Next, we conduct some additional checks to ensure that our findings are not driven by the choice of a particular sample or specification.

First, as mentioned above, we excluded Greece, Portugal and Spain from our sample, since there are doubts about the persistence of the 'new democracy' effect. Alternatively, following Brender and Drazen (2005), we re-estimate (19) including all developed countries in the sample, but we exclude the first four competitive elections that took place in Greece, Portugal and Spain. The results derived are similar to the initial sample of established democracies. If we further estimate our equations including all elections of 'new' democracies in our sample, most of our results remain unaffected. What differs is that in endogenous elections[27] we find support for a stronger deficit cycle, accompanied by a rise in current expenditure while our evidence for an electoral impact on capital spending is now somewhat weaker (the coefficient of $\Delta capital\_exp$ is still significant but the coefficient of $\Delta capital$ is insignificant but not far from the 10% significance level).

Second, as outlined by Persson and Tabellini (2003), pre-electoral manipulation of fiscal policy may depend significantly on the nature of the political system. More specifically, electoral cycles may differ between proportional and majoritarian systems or presidential and parliamentary governments. Regarding the first classification, 14 countries in our sample use proportional voting rules (while five countries use majoritarian rules). Our findings for the sample of 19 'old' democracies are very much akin to those depicted for countries that use proportional rules. This result seems to verify the notion that the fall in government investment is indeed a more expected result in a proportional system. Politicians in this system are more prone to cut geographically targeted public spending, such as investment, rather than transfers which are easier to target across social groups (see Persson and Tabellini 2002; Milesi-Ferretti et al. 2002). Concerning the second part of constitutional rules, we have only two presidential democracies, Switzerland and the United States. It should be noted that the qualitative results remain unaffected after dropping the two countries from our sample. The same picture remains if we also exclude France, which appears to be the sample's only semi-presidential democracy. In presidential countries the elections of the legislature and the executive do not necessarily coincide. In some cases, mid-term legislative elections take place in the interval between years of simultaneous presidential and legislative elections. Following the relevant literature (see, among others, Persson and Tabellini 2003; Shi and Svensson 2006; Vergne 2009) in our research we do not include these mid-term elections, since as argued by Persson and Tabellini (2003) the incentives created by these mid-term elections are weaker relative to the election years in which both the president and the legislature are elected. Nevertheless, we further test the robustness of our results by including the seven U.S. mid-term elections, but our results remain essentially the same.

Third, we use the polity score of the *'Polity IV dataset'* in order to take into account political regime changes for the countries of our sample. We define a policy regime change as a one-unit change in the polity score. The polity score indicates regime changes for France

[27] Note that the majority of 'new' electoral periods added in our sample are endogenous. Our results are not directly comparable with Brender and Drazen (2005), who find that in 'new' democracies a deficit cycle exists both for predetermined and endogenous elections since Brender and Drazen (2005) in that part of their analysis do not distinguish between developed and developing countries. However, if we don't split our electoral indicator into predetermined and endogenous electoral periods, we do get a deficit cycle when all elections in Greece, France and Portugal are included in our sample. This deficit cycle disappears when we exclude only the first four competitive elections, in line with the results presented in Table 4 of Brender and Drazen (2005).

for the years 1968 and 1986. Hence, we created a dummy variable for France that takes the value one in the first five years of theses regime changes. Other than that, we include a dummy variable in our equation that receives the value one for Germany for 1990 onwards and zero otherwise, in order to control for German re-unification. It is worth noting that we checked our results including a dummy variable that receives the value one after 1990 and zero before 1990 for all countries but the results basically reproduce those reported in earlier sections.

Finally, important international incidents or changes in statistical conventions may often cause structural breaks. In order to test for the existence of such breaks, we performed the Chow test in our basic specification. We assume two exogenously imposed break points which are the second energy crisis in 1979 and year 1992, when the Maastricht treaty was signed, the ERM crisis was in progress and the cold war came to an end after the dissolution of the Soviet Union. In both cases, we fail to reject the null hypothesis of no structural break, which indicates that our results are quite stable over time.[28]

5 Conclusions

Electoral incentives can affect both the level and the composition of fiscal policy. Office motivated incumbent policymakers may shift public expenditure towards more 'visible' current expenditure and away from less 'visible' capital expenditure in order to improve the voters' perception of their ability, thereby increasing the probability of being re-elected. This paper presented empirical results for a sample of 19 developed, established democracies over the period 1972–1999 that support this theoretical prediction.

Regarding aggregate fiscal variables, we find no evidence of an electoral cycle for government deficits and expenditures but we do find a significant revenue cycle. Going one step further, we look at the electoral impact on the composition of fiscal revenue and find that lower revenues in election periods can be attributed to reductions in direct taxation. This suggests that policymakers prefer to cut taxes that are more 'visible' to voters because they have direct impacts on their disposable income. Moreover, a cut in direct taxation is likely to have a positive impact on economic growth by affecting individuals' savings and labor supply decisions. Finally, attempting to distinguish between pre-determined and endogenous elections reveals that—in line with the assumptions of the theoretical model—both the revenue cycle and the shift in the composition of public expenditure occur when elections are exogenous.

Existing empirical studies on the budgetary impact of elections suggest that budget cycles exist in developing countries and in 'new' democracies, whereas established democracies with more competitive electoral systems seem to experience a fiscal revenue cycle (see Persson and Tabellini 2003; Brender and Drazen 2005; Shi and Svensson 2006). Our results confirm these findings and shed more light on the nature of these revenue cycles. It seems that, in developed economies where democratic systems are 'old', elections tend to decrease the more 'visible' part of taxation while the resulting fall in revenue is financed by reducing

[28] However, we do find a structural break in 1986 (the middle of our sample) for revenues and indirect revenues equations. Given that both breaks clearly are attributed to endogenous elections and our core theoretical and empirical results concern predetermined elections, in this paper we do not investigate this issue any further. However, splitting our sample into two sub-samples (pre- and after- 1986) for the two equations reveals that, interestingly enough, an (indirect) revenue cycle exists also for endogenous elections after 1986. Future work could attempt explaining this result by shed more light on the relatively under-investigated issue of endogenous elections.

the less 'visible' component of public spending, which is public investment expenditure. Given that both the fall in public investment and the fall in distortionary taxation may affect growth, but in an opposite direction, further work could look at the growth impact of this 'opportunistic' behavior and relate our result to the existing literature on political economy and growth. More than that, our results suggest that, even if voters may punish 'irresponsible' fiscal policy in the form of deficit creation, opportunistic behavior can still lead to electoral cycles that affect mainly the composition of fiscal policy rather than its level. This implies that balanced budget rules or expenditure ceilings may be ineffective in eliminating this type of political cycle. Future research in this area could reveal whether voters punish or reward the fiscal manipulation identified herein.

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Appendix: Variable descriptions, descriptive statistics and data sources

Variable	Description	Mean	Std. dev.	Data source
balance	Overall deficit/surplus as a share of GDP (%)	−3.290	3.899	Global Development Network Growth Database (*GDNGD*)
Δ*balance*	Change in overall deficit/surplus as a share of GDP (%)	0.274	2.037	*GDNGD*
Δ*balance (expanded sample)*	Change in overall deficit/surplus (GFSM 1986) and cash surplus/deficit (GFSM 2001) as a share of GDP (%)	0.046	1.899	*GDNGD* and Government Finance statistics (*GFS*) online.
expenditures	Total expenditure as a share of GDP (%)	35.228	10.241	*GDNGD*
Δ*expenditures*	Change in total expenditure as a share of GDP (%)	0.273	1.788	*GDNGD*
Δ*expenditures* (*expanded sample*)	Change in total expenditure (GFSM 1986) and total outlays (GFSM 2001) as share of GDP (%)	0.168	1.709	*GDNGD* and *GFS* online.
revenues	Total revenue as a share of GDP (%)	32.692	9.556	*GDNGD*
Δ*revenues*	Change in total revenue as a share of GDP (%)	0.236	1.299	*GDNGD*
Δ*revenues (expanded sample)*	Change in total revenue (GFSM 1986) and total revenue (GFSM 2001) as share of GDP (%)	0.172	1.271	*GDNGD* and *GFS* online.
capital	Capital expenditures as a share of GDP (%)	2.534	1.298	*GDNGD*
Δ*capital*	Change in capital expenditures as a share of GDP (%)	−0.048	0.426	*GDNGD*

Variable	Description	Mean	Std. dev.	Data source
capital_exp	Capital expenditures as a share of total expenditures (%)	7.503	4.138	Own calculations, data taken from *GDNGD*.
Δ*capital_exp*	Change in capital expenditures as a share of total expenditures (%)	−0.226	1.104	Own calculations, data taken from *GDNGD*.
current	Current expenditures as a share of GDP (%)	32.716	9.822	*GDNGD*
Δ*current*	Change in current expenditures as a share of GDP (%)	0.288	1.610	*GDNGD*
current_exp	Current expenditures as a share of total expenditures (%)	92.527	4.121	Own calculations, data taken from *GDNGD*.
Δ*current_exp*	Change in current expenditures as a share of total expenditures (%)	0.217	1.112	Own calculations, data taken from *GDNGD*.
direct	Sum of taxation on income and profits, social security contributions, taxation on payroll and manpower and taxation on property as a share of GDP (%)	19.528	7.131	Own calculations based on Kneller's et al. (1999) methodology, data taken from *GDNGD*.
Δ*direct*	Change in the sum of taxation on income and profits, social security contributions, taxation on payroll and manpower and taxation on property as a share of GDP (%)	0.202	1.081	Own calculations based on Kneller's et al. (1999) methodology, data taken from *GDNGD*.
Δ*direct (expanded sample)*	Change in the sum of taxation on income, profits and capital gains, social security contributions, taxation on payroll and workforce and taxation on property (GFSM 1986), sum of taxation on income, profits and capital gains, social contributions, taxation on payroll and workforce and taxation on property (GFSM 2001) as a share of GDP (%)	0.151	1.051	Own calculations based on Kneller's et al. (1999) methodology, data taken from *GDNGD* and *GFS* online.
direct_rev	Sum of direct taxation as a share of revenues (expanded sample) (%)	60.275	13.739	Own calculations based on Kneller's et al. (1999) methodology, data taken from *GDNGD*.
Δ*direct_rev*	Change in the sum of direct taxation as a share of revenues (expanded sample) (%)	0.202	1.981	Own calculations based on Kneller's et al. (1999) methodology, data taken from *GDNGD*.
Δ*direct_rev (expanded sample)*	Change in the sum of direct taxation (expanded sample) as a share of total revenues (%)	0.164	1.860	Own calculations based on Kneller's et al. (1999) methodology, data taken from *GDNGD* and *GFS* online.
indirect	Domestic taxes on goods and services as a share of GDP (%)	9.287	4.284	*GDNGD*
Δ*indirect*	Change in domestic taxes on goods and services as a share of GDP (%)	0.045	0.509	*GDNGD*
Δ*indirect (expanded sample)*	Change in domestic taxes on goods and services (GFSM 1986) and taxes on goods and services (GFSM 2001) as a share of GDP (%)	0.035	0.487	*GDNGD* and *GFS* online.

Variable	Description	Mean	Std. dev.	Data source
indirect_rev	Taxation on domestic goods and services as a share of total revenues (%)	27.775	10.684	Own calculations, data taken from *GDNGD*
Δ*indirect_rev*	Change in taxation on domestic goods and services as a share of total revenues (%)	−0.070	1.534	Own calculations, data taken from *GDNGD*
Δ*indirect_rev (expanded sample)*	Change in indirect taxation (expanded sample) as a share of total revenues (%)	−0.051	1.533	Own calculations, data taken from *GDNGD* and *GFS* online
lgdppc	The log of real per capita income.	9.963	0.330	World Bank Development indicators (*WDI*) online.
Δ*lgdppc*	Change in the log of real per capita income multiplied by 100.	2.163	2.133	*WDI* online.
growth	Per capita growth rate of output (%)	2.881	2.271	*WDI* online
Δ*growth*	Change in per capita growth rate of output (%)	−0.058	2.431	*WDI* online
Δ*gdphp*	Change in the difference between the natural log of real GDP in the country and its country-specific trend (obtained, using the Hodrick-Prescott filter).	0.008	1.843	*WDI* online
Δ*pop1564*	Change in population between 15 and 64 years old as a share of total population (%).	0.097	0.225	*WDI* online
Δ*pop65+*	Change in population over the age of 65 as a share of total population (%).	0.119	0.148	*WDI* online
Δ*trade*	Change in the sum of imports plus exports as a share of GDP (%)	1.214	4.881	*WDI* online
elec	Dummy variable that receives the value 1 in the election year and 0 otherwise.	.287	0.453	Armingeon et al. (2008). Comparative Political Data Set I
elec_pred	Dummy variable that receives the value 1 when elections held in the predetermined date and 0 otherwise.	0.179	.383	Armingeon et al. (2008). Comparative Political Data Set I
elec_end	Dummy variable that receives the value 1 when elections not held in the expected year and 0 otherwise.	0.107	0.310	Armingeon et al. (2008). Comparative Political Data Set I
$elect_t\_pred\_2$	Indicator variable that receives value $(x/12)$ in the election year, with x the months before election, when elections held in the predetermined date and 0 otherwise.	0.104	0.248	Own calculations, data taken from Armingeon et al. (2008). Comparative Political Data Set I.
$elect_{t-1}\_pred\_2$	Indicator variable that receives value $(1 - elect_t\_pred\_2)$ in the election year, with x the months before election, when elections held in the predetermined date and 0 otherwise.	.073	0.189	Own calculations, data taken from Armingeon et al. (2008). Comparative Political Data Set I.

Variable	Description	Mean	Std. dev.	Data source
$elect_t\_end\_2$	Indicator variable that receives value $(x/12)$ in the election year, with x the months before election, when elections not held in the predetermined date and 0 otherwise.	0.070	0.224	Own calculations, data taken from Armingeon et al. (2008). Comparative Political Data Set I.
$elect_{t-1}\_end\_2$	Indicator variable that receives value $(1 - elect_t\_end\_2)$ in the election year, with x the months before election, when elections not held in the predetermined date and 0 otherwise.	0.040	0.154	Own calculations, data taken from Armingeon et al. (2008). Comparative Political Data Set I.

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