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Political budget cycles: Do they differ across countries and why?

Min Shi [a,*], Jakob Svensson [b,c,1]

[a] *University of Wisconsin-Madison, United States*
[b] *Institute for International Economic Studies, Stockholm University, Sweden*
[c] *CEPR, United Kingdom*

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Abstract

This paper uses a large panel data set to examine the relation between elections and fiscal policy. We find evidence of political budget cycles: on average, government fiscal deficit increases by almost 1% of GDP in election years. Moreover, these political budget cycles are significantly larger, and statistically more robust, in developing than in developed countries. We propose a moral hazard model of electoral competition to explain this difference. In the model, the size of the electoral budget cycles depends on politicians' rents of remaining in power and the share of informed voters in the electorate. Using suitable proxies, we show that these institutional features explain a large part of the difference in electoral budget cycles between developed and developing countries.

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* Corresponding author.
E-mail addresses: mshi@bus.wisc.edu (M. Shi), jakob.svensson@iies.su.se (J. Svensson).
[1] Development Research Group, The World Bank, United States.

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1. Introduction

The empirical cross-country literature on political budget cycles has three common features.[2] It is based on data sets from a relatively small number of countries; it focuses on identifying whether or not there exist any electoral effects on fiscal policy; and it treats the timing of elections as exogenous.

While the literature has provided important insights, it also has its drawbacks. First, the lack of systematic studies based on data from both developing and developed countries makes it difficult to conclude whether political budget cycles are a universal phenomenon. Second, the literature tells us little about the cross-sectional variation in the size of these electoral effects (e.g., between developed and developing countries). Finally, since both the timing of elections and the fiscal policies could be influenced by (unobserved) variables that are not included in the standard regressions, we do not know if the positive association between the incidence of elections and the greater election-year fiscal deficit constitutes a causal relationship.[3]

This paper avoids these problems by assembling a large panel data set consisting of 85 countries over a 21-year period (1975–1995). We find evidence of political budget cycles. On average, government fiscal deficit increases by one percent of GDP in elections. We also show that there are systematic differences between developed and developing countries. Specifically, political budget cycles are large in developing countries but small or nonexistent in developed countries.[4] When we restrict the analysis to elections whose timings are determined by the constitution or announced a year in advance, we find very similar results.

What may explain the difference in the size of political budget cycles across countries? We develop a simple moral hazard model of electoral competition to guide the empirical work.[5] The underlying feature of the model is the incumbent's ability to manipulate policy instruments (which are observable to voters only with a lag) in order to bias the voters' inference process before elections to his favor. To increase his chances of re-election, the incumbent has an incentive to boost the supply of public goods prior to the election, hoping that voters would attribute the boost to his competence. In the model, all politicians, independent of their competence levels, face the same incentives, implying that the empirical predictions are not conditional on their type or ability.[6] More importantly, the politicians'

[2] There is a large empirical literature on political business cycles, dating back to Nordhaus (1975), MaRae (1977), Hibbs (1977), and Tufte (1978). Most of this literature uses U.S. data. Alesina and Roubini (1992) and Alesina et al. (1997) study electoral cycles across OECD countries. Studies using data from developing countries include Block (2002), Gonzalez (2002b), Magloire (1997), Khemani (2000), Kraemer (1997), Schuknecht (1996). None of the above papers combine data from developed and developing countries. Brender and Drazen (2004) and Persson and Tabellini (2002) are exceptions. Drazen (2000a,b) review the theoretical and empirical political business cycles literature.

[3] Studies on the U.S. (and other countries with constitutionally fixed election dates, e.g., Pettersson Lidbom, 2002) do not suffer from this potential endogeneity problem.

[4] This finding is consistent with the results in Brender and Drazen (2004). In a sample of established democracies, they do not find any robust evidence of political budget cycles.

[5] The model builds on the career concern model of Holmstrom (1982). Lohmann (1998) and Persson and Tabellini (2000) develop models with similar underlying features, but address different policy problem. Lohmann (1998) focuses on the inflationary consequences of pre-electoral monetary policy expansion in a Neo-Keynesian macro-model. Persson and Tabellini (2000) study cycles in wasteful spending and rent-extraction.

[6] This result contrasts those of the signaling model of Rogoff (1990) and other similar models where only the more competent politicians distort the economy prior to an election in a separating equilibrium. Since only the competent type signals by creating a preelection spending boom, the prediction of political budget cycles cannot be easily tested without information on politicians' type.

incentives depend on the politico-institutional environment.[7] Specifically, the more private benefits politicians gain when in power (i.e., higher rents of remaining in power), the stronger are their incentives to influence the voters' perceptions prior to an election. Moreover, the more voters that (ex ante) fail to distinguish pre-electoral manipulations from incumbent competence, the higher is the return for the incumbent to boost spending prior to an election. We proxy for these institutional variables using cross-country data on government corruption and rent-seeking activities, and data on access to free media. Our results show that the institutional indicators can explain a large part of the difference in the size of policy cycles between developed and developing countries.

The rest of the paper is structured as follows. The next section presents some stylized facts of the magnitude and variation in political budget cycles across countries. Section 3 sets out a model to account for these empirical facts. Section 4 discusses the data we use to proxy for the institutional factors that our model identifies. In Section 5 we test whether these institutional indicators can explain the difference in the size of political budget cycles between developed and developing countries. Finally, Section 6 concludes.

2. Evidence of political budget cycles in a large sample of countries

In this section, we present some stylized facts on the relation between elections and government fiscal policy outcomes. To this end, we employ an empirical specification that takes the following generic form

$$\mathrm{BUDGET}_{i,t} = \sum_{j=1}^{2} \gamma_j \mathrm{BUDGET}_{i,t-j} + \chi' w_{i,t} + \beta \mathrm{ELE}_{i,t} + \xi_i + \varepsilon_{i,t}, \quad (1)$$

where $\mathrm{BUDGET}_{i,t}$ is government fiscal budget balance as a share of GDP in country i and year t, $w_{i,t}$ a vector of control variables, $\mathrm{ELE}_{i,t}$ an election dummy variable, ξ_i an unobserved country-specific effect, and $\varepsilon_{i,t}$ an i.i.d. error term. In all the regressions, we include the logarithm of real GDP per capita and GDP growth rate as control variables.

Eq. (1) is a standard dynamic panel data specification. The presence of lagged dependent variables and the country-specific effects render the Ordinary Least Squares estimator biased. Fixed-Effects (FE) estimators can eliminate the country-specific effect. However, the bias caused by the inclusion of lagged dependent variables remains. The bias of the FE estimator, which influences all variables, depends on the length of the time series and only when it goes to infinity will the FE estimator be consistent (see Nickell, 1981; Kiviet, 1995). Since the average number of observations across countries in our sample is 20, the bias of the FE estimator may be non-negligible.

In order to avoid these problems, we adopt the GMM estimator developed for dynamic panel data by Arellano and Bond (1991), Arellano and Bover (1995), and Blundell and Bond (1998).[8] GMM estimator is our preferred method because it controls for the unobserved country-specific effects as well as the bias caused by the lagged dependent variables.

[7] Gonzalez (2002a), building on Rogoff (1990), also points to the variation in institutional environment. See also Brender (2003), who argues that whether election-year deficits are rewarded or not at the polls depend on the availability of information.

[8] See Appendix 1 for a detailed discussion on the moment conditions of the GMM regressions. See Hahn et al. (2001), and references therein, on the pros and cons of GMM.

Table 1A
Number of elections by country, 1975–95

Argentina	3	El Salvador	4	Luxembourg*	4	Sierra Leone	2
Australia*	7	Fiji	4	Malawi	1	Singapore*	5
Austria*	7	Finland*	5	Malaysia	5	Solomon Islands	4
Bahamas, The*	4	France*	5	Maldives	4	Spain*	6
Bangladesh	3	Ghana	2	Mali	3	Sri Lanka	3
Barbados	5	Greece*	5	Malta*	4	Suriname	4
Belgium*	6	Guatemala	5	Mauritius	5	Sweden*	7
Bolivia	4	Guyana	3	Mexico	4	Switzerland*	6
Botswana	4	Honduras	3	Nepal	4	Syrian Arab Rep.	3
Brazil	2	Hungary	1	Netherlands*	7	Thailand	6
Burkina Faso	2	Iceland*	5	New Zealand*	7	Togo	2
Burundi	2	India	5	Nicaragua	2	Trinidad and Tobago	5
Canada*	5	Indonesia	4	Nigeria	2	Tunisia	2
Chad	1	Iran	2	Norway*	5	United Kingdom*	4
Chile	2	Ireland*	6	Pakistan	3	United States*	5
Colombia	5	Israel*	5	Panama	3	Uruguay	3
Costa Rica	5	Italy*	6	Papua New Guinea	4	Venezuela	4
Cyprus*	4	Jamaica	5	Paraguay	5	Zambia	4
Denmark*	9	Japan*	7	Peru	4	Zimbabwe	4
Dominican Rep.	5	Kenya	4	Philippines	4		
Ecuador	5	Korea, Rep.	2	Portugal*	8		
Egypt, Arab Rep.	3	Liberia	2	Romania	2		

*Indicates a country belongs to the group of developed countries.

The Database on Political Institutions from the World Bank (Beck et al., 2001) provides a wide coverage of countries' political systems and elections between 1975 and 1995. The binary election indicator, ELE, takes the value 1 in election years and 0 otherwise. We only include legislative elections for countries with parliamentary political systems and executive elections for countries with presidential systems. Table 1A, B provides an overview of the countries in the sample and the numbers of elections that took place during the sample period. On average, developed countries in the sample had 5.7 elections per country (roughly one every fourth year), while developing countries had 3.4 elections (roughly one every sixth year) during the sample period.[9]

Real GDP per capita data are from the Penn World Tables. Government fiscal balance data, expressed as shares of GDP, are obtained from International Financial Statistics (IFS), published by the International Monetary Fund. It is well known that fiscal data from the IFS are noisy (see the discussion in Brender and Drazen, 2004). In Appendix 2, we discuss the adjustments we made to the data in relation to the extensive data cleaning effort in Brender and Drazen (2004). They clean the data by filling in missing observations from other sources and by dropping or replacing what they argue are unreliable data points. We follow their data adjustment strategy with one exception: we do not delete observations a priori as it has no theoretical justification.[10]

[9] Some countries are classified as having a third political system where the president is elected by the assembly but cannot be easily recalled by the assembly. In this case, we include either the executive or legislative elections depending on where the executive power rests, using information from Political Handbook. In the sample, 8 countries and 17 elections took place under this political system.

[10] However, to ensure that our results were not completely driven by a few observations, we re-estimated all the regressions with the outliers identified by Brender and Drazen (2004) dropped. We discuss below if and how our results are affected by dropping these outliers.

Table 1B
Descriptive statistics of key variables

Variable	Sample	Mean	Std. dev.	No. obs.	No. countries
Fiscal balance/GDP (BUDGET)	All	−4.17	6.11	1683	85
	Developed	−3.83	4.35	561	27
	Developing	−4.34	6.81	1122	58
Corruption index (TI)	All	0	2.58	1211	60
	Developed	−2.61	1.36	499	24
	Developing	1.83	1.38	712	36
Institutional index (ICRG)	All	0	11.9	1511	76
	Developed	−12.1	7.42	561	27
	Developing	7.16	7.46	950	49
Informed Voters (INFO)	All	0.23	0.38	1655	85
	Developed	0.53	0.47	561	27
	Developing	0.08	0.18	1094	58
Composite measure 1: TI and INFO (COMP1)	All	0.16	1.84	1211	60
	Developed	1.93	1.41	499	24
	Developing	−1.08	0.77	712	36
Composite measure 2: ICRG and INFO (COMP2)	All	0.04	1.83	1501	76
	Developed	1.80	1.62	561	27
	Developing	−1.01	0.90	940	49

Our final sample consists of 85 countries and 1683 observations that all have data on government fiscal budget balance and elections, but may have missing values for other variables.[11]

Because we are interested in studying cross country variations in political budget cycles, we partition the sample into sub-samples of developed and developing countries. We create a variable DEV, which takes the value 1 for developed countries and 0 otherwise. Developed countries include the high income countries defined by the IFS as having a per capita GNP greater than USD 9656 in 1997. 27 countries in our sample belong to this group and the other 58 are classified as developing countries.

2.1. Baseline findings

We initially treat the election indicator, ELE, as an exogenous variable. The main reason for this is that there are no good instrumental variables for elections. Moreover, this makes our results comparable to other work as this is the standard approach in the literature. In Subsection 2.2, we relax this assumption. Table 2 reports the baseline findings. All regressions include two lagged values of government budget balance, two control variables (logarithm of real GDP per capita and GDP growth rates), a country dummy, and an election indicator.[12]

Column 1 reports result of the FE estimation. The coefficient estimate on ELE suggests a negative relation between elections and fiscal balance. This is confirmed in the GMM specification, reported in column 2.[13] The GMM estimate implies a somewhat greater electoral effect on fiscal balance. The GMM estimate suggests that fiscal deficit as a share of GDP is 0.9

[11] Another data filtering rule is that we require countries to have fiscal budget data for at least 10 consecutive years to be included in our sample.

[12] We test and confirm that the time-series of government fiscal variables are stationary. That is, the roots of the polynomial equation based on the coefficient estimates are greater than 1, in absolute value.

[13] We use the same observations in the FE specification as in the GMM specification.

Table 2
Elections and government fiscal balance: basic findings

Dep. variable	Fiscal budget balance				
Regression	(1)	(2)	(3)[b]	(4)	(5)
Method	FE	GMM	GMM	GMM	GMM
Sample	Full	Full	Full	Developing	Developed
ELE	−0.69***	−0.91***	−0.96***	−1.27**	−0.08
	(.27)	(.31)	(.31)	(.50)	(.48)
BUDGET(−1)	0.47***	0.43***	0.39***	0.46***	0.78***
	(.03)	(.09)	(.10)	(.10)	(.14)
BUDGET(−2)	0.09***	0.28***	0.33***	0.25***	0.13
	(.03)	(.08)	(.09)	(.08)	(.15)
Growth	8.48***	14.8	4.00	12.0	33.8***
	(2.43)	(12.8)	(13.6)	(13.2)	(8.96)
LnGDP	0.23	0.59	0.44	0.34	1.15
	(.98)	(.78)	(.60)	(.87)	(1.56)
Hansen test[c]		81.1	76.0	55.0	21.6
		[.99]	[.99]	[.99]	[.99]
Serial corr.[d]		−0.48	−0.88	−0.14	0.73
		[.63]	[.38]	[.89]	[.46]
z-test[e]					1.72
					[.04]
No. countries	85	85	85	58	27
No. obs.	1204	1204	1204	793	411
Adj. R^2	.64				

Notes: (a) Full regression: $\text{BUDGET}_{it}=\gamma_1\text{BUDGET}_{i,t-1}+\gamma_2\text{BUDGET}_{i,t-2}+\chi_1\text{GDP}_{i,t}+\chi_2\text{GROWTH}_{i,t}+\beta\text{ELE}_{i,t}+\xi_i+\varepsilon_{i,t}$. Robust standard errors, with finite-sample correction for the two-step covariance matrix developed by Windmeijer (2005) are reported in parenthesis in the GMM specifications. *** (**) [*] denote significance at the 1 (5) [10] percent level. The instruments used in the GMM regressions are lagged levels (two periods) of the dependent variable, GDP, and GROWTH for the differenced equation, and lagged difference (one period) for the level equation. ELE is instrumented by itself in the differences equation. (b) Time-specific fixed effects are included as regressors. (c) Hansen test is a test of the over-identifying restrictions where the null is that the instruments are not correlated with the error term. P-values are shown in parentheses. (d) Serial corr. is a test of the hypothesis that the error term in the regression is not serially correlated, with p-values reported in parentheses. (e) z-test is a test of the hypothesis that the coefficients on ELE in the sub-samples of developing and developed countries are equal, which is distributed as $N(0,1)$, with p-values shown in parentheses.

percentage point higher in election years. Given the average fiscal deficit in the sample (4.17% of GDP), the estimate implies that, on average, fiscal deficit increases by 22% in election years. In column 3, we allow the error term to include a time-specific unobserved component. The estimated coefficient on the election indicator, though slightly larger in absolute value, is qualitatively the same.

We perform two tests of the GMM model. First, we test the validity of the assumption that the error term in Eq. (1) is not serially correlated. This is important because the moment conditions ((A3) and (A4)) rely on this assumption. The test is implemented as a test of second-order serial correlation in the difference Eq. (A2). The second test is a Hansen test of over-identifying restrictions, where the null hypothesis is that the instruments are uncorrelated with the residuals. Both the serial correlation test of the error term and the Hansen test confirm that the moment conditions cannot be rejected.

Are there any systematic differences in the size of political budget cycles between developed and developing countries? To investigate this issue, we run the baseline GMM regression

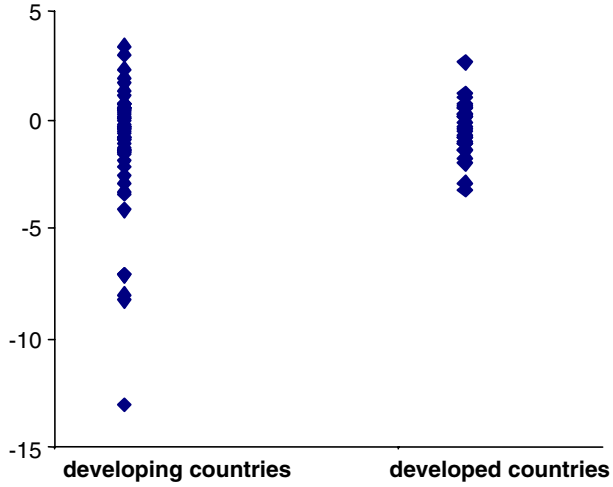

Fig. 1. Coefficient estimates on ELE in country-by-country OLS regressions.

separately for the two groups of countries.[14] We then test the hypothesis that the coefficients on the election dummies are the same in the two groups of countries. We report the ratio of the difference of the coefficient estimates on ELE to the standard error of this difference (z statistic). Assuming that the coefficients on the election dummies for the two groups are independent, the z statistic is asymptotically normal. The results are reported in columns 4 and 5.

There is a substantial difference in the size of the electoral budget cycles between the two sub-samples. The coefficient estimates on ELE imply that, in election years, the fiscal balance worsens by 1.3 percentage points in the average developing country. In contrast, the same point estimate in the sample of developed countries is -0.1, and is not significantly different from zero.[15]

Fig. 1 provides additional evidence of the large variation in the electoral effect on fiscal balance across countries. The figure plots the coefficient estimates on ELE in country-by-country OLS regressions, where fiscal balance is regressed on its two lagged values, logarithm of real GDP per capita, GDP growth rates, and the election dummy. The average coefficients on ELE for the two sub-samples are close to those reported in Table 2. However, there are large differences in the coefficient estimates, especially, among developing countries. In Section 3, we set up a moral hazard model of electoral competition to explain this difference.

2.2. Endogeneity of election timing

A potential critique of the results presented above is that we treat the election variable as exogenous relative to fiscal policy, which may not be the case in reality. For example, both timing of elections and fiscal policies could be influenced by a number of (unobserved)

[14] We also carried out the regressions on the full sample with ELE*DEV as additional explanatory variable. The result of this regression reinforces the findings presented in Table 2. Estimating the model in the full sample with an interaction term imposes the restriction that the coefficients on all other explanatory variables are the same between the two sub-samples.

[15] These results remain qualitatively unchanged if we delete the "outlier" observations identified by Brender and Drazen (2004) or allow for a more general error process.

Table 3
Elections and government fiscal balance: endogeneity of election timing

Dep. variable	Fiscal budget balance		
Regression	(1)	(2)	(3)
Method	GMM	GMM	GMM
Sample	Full	Developing	Developed
ELE_PRE	−0.55**	−1.07**	−0.41
	(.27)	(.41)	(.96)
ELE_END	−1.54**	−1.86	−0.08
	(.74)	(1.27)	(1.19)
BUDGET(−1)	0.44***	0.47***	0.83***
	(.09)	(.10)	(.15)
BUDGET(−2)	0.28***	0.24***	0.09
	(.08)	(.09)	(.16)
Growth	15.1	12.4	36.5***
	(12.3)	(13.4)	(9.58)
LnGDP	0.57	0.39	0.91
	(.72)	(.92)	(1.67)
Hansen test	80.4	55.0	21.6
	[.99]	[.99]	[.99]
Serial corr.	−0.44	−0.14	0.73
	[.66]	[.89]	[.46]
No. countries	85	58	27
No. obs.	1204	793	411

Notes: See notes for Table 2.

variables, such as crises or social unrest, which are not included in our regression. In this case, our coefficient estimate on ELE will be biased. In particular, if the omitted variables correlate positively with election timing and negatively with fiscal policy outcomes (e.g., crisis), or vice versa, it could result in a negative estimated coefficient on ELE in the fiscal balance regression.[16]

One way to mitigate the potential omitted variable bias is to focus on elections whose timing is predetermined relative to current fiscal policies. To achieve this, we analyzed all the elections in the sample, using information from the Political Handbook (various years). We classify an election to be predetermined if either (i) the election is held on the fixed date (year) specified by the constitution;[17] or (ii) the election occurs in the last year of a constitutionally fixed term for the legislature; or (iii) the election is announced at least a year in advance.

We create two new election indicators, ELE_PRE and ELE_END to replace ELE. ELE_PRE equals 1 in country i and year t when there was a predetermined election, 0 otherwise; while ELE_END equals 1 in country i and year t if an election that was not predetermined took place, and 0 otherwise. Among the 352 elections in the sample, 64% are classified as predetermined. The shares of elections classified as predetermined in the sub-samples of developed and developing countries are 66 and 62%, respectively. Interestingly, the shares of predetermined

[16] Another potential problem is that the incumbent politicians may strategically choose the timing of elections conditional on fiscal policy outcomes, causing a reverse causality problem. This problem is attenuated with our alternative election indicator ELE_PRE since strategically timed elections are less likely to be coded as predetermined.
[17] Some countries have constitutionally fixed intervals for elections, but the incumbent disregarded the constitution and either advanced or delayed the elections. In these cases, elections are NOT coded as predetermined.

elections under presidential and parliamentary electoral systems, 70 and 60%, respectively, are not dramatically different from each other either.

In Table 3, we re-estimate the original fiscal balance equation with ELE replaced by ELE_PRE and ELE_END. In the full sample, both ELE_PRE and ELE_END enter the regression negatively and highly significantly. The coefficient on ELE_PRE implies that, on average, the fiscal deficit as a share of GDP is 0.6 percentage point higher in years when predetermined elections took place.

In columns 2 and 3, we report the results from separate regressions for developed and developing countries with these new election indicators. The coefficient estimates on ELE_PRE suggest that predetermined elections are associated with a large (1.1 percentage point) and significant increase in government fiscal deficit in developing countries. In the sub-sample of developed countries, however, the negative effect of elections on fiscal balance is small and not statistically significant.[18] These findings confirm the previous results that political budget cycles are mostly a developing country phenomenon.

3. What explains the variation in the size of political budget cycles

Why are political budget cycles different across countries, and more specifically, large in developing countries but not so in developed countries? Clearly, countries differ in many dimensions that may affect politicians' incentives and ability to manipulate fiscal policy prior to elections. Below, we set up a model that points to two possible factors: the politicians' rents of remaining in power and the share of informed voters.

3.1. The model

The economy is composed of a large number of citizens, each of them derives utility from a private consumption good (c) and a public good (g). There are two politicians (parties), denoted with superscripts a and b. All agents are expected utility maximizers. The utility function of voter i in period t is

$$U_t^i = \sum_{s=t}^{T} \beta^{s-t}\left[g_s + u(c_s) + \theta^i z_s\right], \tag{2}$$

where z is a binary variable taking the value $-1/2$ if a is elected and $1/2$ if b is elected, and u is a standard concave utility function. All voters are alike in their preferences over consumption, but they differ in the parameter θ^i, which captures the effect of candidates' other policies (rather than fiscal ones) or personal characteristics on voters' utility. Voters with $\theta^i<0$ are biased in favor of party a and voters with $\theta^i>0$ prefer party b all else equal. We assume that θ^i is uniformly distributed on $\left[-\frac{1}{2},\frac{1}{2}\right]$. β is the discount factor and is assumed to equal to 1 for simplification.

At the beginning of each period, all citizens receive an exogenous income y. Public good provision is financed with a lump-sum tax τ. Thus

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

[18] Excluding the outliers identified by Brender and Drazen (2004) has no effect on the coefficient estimate on ELE_PRE, but the coefficient estimate on ELE_END becomes smaller in absolute values (−0.7). It remains insignificantly different from zero.

The politicians (parties) are assumed to derive utility from consumption goods in the same way as other citizens. However, as in Rogoff (1990) and others, we also assume that being in power provides the politician with additional "ego rents" of $X_t = X > 0$ per period in office. One can conceptualize these rents in a variety of ways, from non-monetary benefits due to the great honor of being the chief executive, to misuse of public office for private gains.[19] Thus, political candidate j's utility function is,

$$V_t^j = \sum_{s=t}^{T} \beta^{s-t}[g_s + u(c_s) + X_s] \tag{4}$$

for $j = \{a, b\}$. Elections take place at the end of every other period.

At a given period t, the incumbent determines taxes (τ_t) and borrowing (d_t). In addition to these inputs, the provision of public goods also requires administrative competence (e.g., ability to limit waste in the budget process, ability to deal with exogenous shocks) indexed by η_t^j. Public output (g_t) is then residually determined by,

$$g_t = \tau_t + d_t - R(d_{t-1}) + \eta_t^j \tag{5}$$

where $R(d)$ is a continuous cost function of public borrowing with $R(0)=0$, $R'(0)=1$, and $R''(d)>0$ for all $d>0$.[20]

We assume that leadership competence follows a first-order moving average process,

$$\eta_t^j = \mu_t^j + \mu_{t-1}^j \tag{6}$$

where each μ^j is an i.i.d. random variable with zero mean, finite variance, distribution function $F(\mu)$ and density function $f(u)$ with $f(0)>0$. That is, competence is persistent, although it may change over time. This is a plausible assumption since circumstances change over time and a policy-maker that is competent in some tasks in one period need not to be competent on other tasks in other periods. We also assume that the past competence shock is common knowledge to all agents.

The timing of event is as follows. At the beginning of period t, the incumbent politician decides on taxes (τ_t) and borrowing (d_t). The shock η_t occurs during the period and the election takes place at the end of period t. This timing implies that the incumbent, facing a large set of possible policy problems, is ex ante uncertain about how well he will be able to transform resources to public output. An alternative interpretation is that while the government knows the tax code, it is uncertain about the tax revenues it will generate.

The voters' ability to assess the incumbent's policy choices differ. Specifically, a share σ of the electorate is assumed to be "informed", i.e., they have access to a free flow of information. Therefore, they not only observe election year spending (g_t) and taxes (τ_t), but also the amount of borrowing (d_t) before casting their votes. A share $1-\sigma$ of the electorate is "uninformed" in that they do not have access to a free flow of information and only observe the policy instruments that directly influence their utility, i.e., g_t and τ_t. This is a reasonable assumption

[19] Implicitly we assume that in the latter scenario, the rents per capita are negligible as the population is sufficiently large. All results, however, continue to hold if we add a constant rent variable to voters' budget constraint in Eq. (3).

[20] $R'(0)=1$ implies the "interest rate" on the first infinitesimal unit of debt is zero (to be consistent, we also assume a zero discount rate in the model). *Our set-up also presumes* that the government internalizes the total cost of running a politically induced deficit (public borrowing), which includes potential effects such as higher real interest rates, and lower savings and private investment. For countries that are restricted to borrow on a small domestic market (many developing countries), the assumption that the government can borrow at an exogenous interest rate may not be particularly realistic.

since the government can, through clever accounting techniques, obstruct voters' ability to assess its borrowing needs. Access to free media may help voters to overcome this problem and provide them with a good estimate of d_t. However, this requires both resources (ownership of radios and television sets, newspapers, etc) and skills to process information. Neither of these is equally distributed/available across countries.[21]

3.2. Equilibrium without elections

As a reference point, we first solve the model without elections. In this case, a randomly drawn candidate remains in power for ever. The equilibrium is easy to characterize. Since the marginal utility of public consumption is constant (equal to one) and borrowing is costly, there will be no borrowing in equilibrium. Therefore, $d_t=0$ for $t=1, 2,\ldots T$. Given the simple production technology and quasi-linear preferences, the incumbent's problem can be broken down into a sequence of static maximization problems,

$$\max_{\{\tau_t\}} E_t[g_t + u(c_t) + X] \tag{7}$$

$$\text{s.t.} \qquad g_t = \tau_t + \eta_t, \tag{8}$$

and (3). E_t is the expectation operator conditional on information at time t. The first-order condition equates the marginal disutility of taxes with the marginal utility of spending. Solving for τ_t yields,

$$\tau_t = \tau^* = y - u_c^{-1}(1) \;\forall\; t \tag{9}$$

The competence shock η will only affect spending. Realized spending is $g_t=\tau^*+\eta_t$ for $t=1, 2,\ldots T$.

3.3. Political budget cycles

With elections taking place every other period, the problem becomes somewhat more complex. However, under the assumptions of quasi-linear preferences and the MA(1) process for competence, the problem can again be broken down into a sequence of simple two-period maximization problems.

Working backwards, consider first the elected politician's problem in the post-election period $t+1$. Given the process for competence and that past competence is observable to voters, the incumbent has no incentives to manipulate the voters' perception of his competence in the post-election period $t+1$. This is so because the expected competence level of politicians in period $t+3$, which determines the outcome in the next election at the end of period $t+2$, is uncorrelated with the competence shock in period $t+1$. That is, $E_{t+1}[\eta_{t+3}|\eta_{t+1}]=E_{t+1}[\eta_{t+3}]=0$. Moreover, since borrowing is costly and the marginal utility of public consumption is constant, the government will not borrow in period $t+1$; it will run a primary surplus to pay down its debt. Thus,

$$g_{t+1} = \tau^* - R(d_t) + \eta_{t+1}, \tag{10}$$

where τ^* is the same optimal tax rate as in Eq. (9).[22]

[21] González (2002a) studies a related issue in a signaling model: how the degree of transparency (defined as the likelihood with which the voters learn the politicians' competence) influences the incumbent's incentive to signal his type. See also Brender (2003).

[22] With less restrictive assumptions on preferences, the amortization would be spread over a longer horizon. This would likely generate a rising trend in debt.

Similarly, there is no borrowing in period $t-1$ (since it is also a post-election period). Therefore, the budget constraint in period t is,

$$g_t = \tau^* + d_t + \eta_t. \tag{4'}$$

In period t, an election period, the voters will vote for the candidate that will deliver the best expected outcome in period $t+1$, conditional on their party (or candidate)-specific preferences. Assume party a is in power in period t and let d_t^* denote the solution to the incumbent's optimization problem (yet to be determined). Since the electorate has no information about the challenger's competence (and no way to make an inference), the expected outcome if electing the challenger is

$$\tau^b = \tau^* \tag{11}$$

$$E_t\left[g_{t+1}^b\right] = \tau^* - E_t[R(d_t^*)] \tag{12}$$

since $E_t[\eta_{t+1}^b]=E_t[\mu_{t+1}^b]+E_t[\mu_t^b]=0$.

The expected outcome if re-electing the incumbent is

$$\tau^a = \tau^* \tag{13}$$

$$E_t\left[g_{t+1}^a\right] = \tau^* - E_t[R(d_t^*)] + E_t\left[\mu_t^a\right] \tag{14}$$

since $E_t[\mu_{t+1}^a]=0$. Comparing (11)–(12) and (13)–(14), we see that voter i would vote for the incumbent if and only if

$$E_t\left[\mu_t^a\right] - \theta^i \geq 0. \tag{15}$$

Thus, the incumbent's expected share of votes is

$$\Pr\left(E_t\left[\mu_t^a\right] - \theta^i \geq 0\right) = E_t\left[\mu_t^a\right] + \frac{1}{2}. \tag{16}$$

Voters differ in their ability to assess the incumbent's current competence shock. For the σ share of informed voters who observe both election year spending (g_t), taxes (τ^*), and the amount of borrowing (d_t) before casting their votes, Eq. (4′) implies that they can determine the incumbent's current competence shock prior to the election as,

$$\mu_t^a = g_t - \tau^* - d_t - \mu_{t-1}^a. \tag{17}$$

On the other hand, the $1-\sigma$ share of the uninformed electorate must form an estimate about the incumbent's competence, say $\hat{\mu}_t^a$, by forming an estimate of d_t, say $\hat{d}_t$, based on the observable variables g_t, τ^*, and knowing the equilibrium strategy of the incumbent. Thus,

$$\hat{\mu}_t^a = g_t - \tau^* - \hat{d}_t - \mu_{t-1}^a = \mu_t^a + d_t - \hat{d}_t. \tag{18}$$

Incorporating the knowledge of the two types of voters on the competence shock of the incumbent, we can compute the probability that the incumbent remains in power, i.e., the probability that he receives at least 50% of the votes, as

$$\begin{aligned} P_t &= \Pr\left(\sigma\left[\mu_t^a + \frac{1}{2}\right] + (1-\sigma)\left[\mu_t^a + d_t - \hat{d}_t + \frac{1}{2}\right] \geq \frac{1}{2}\right) = \Pr\left(\mu_t^a \geq (1-\sigma)\left(\hat{d}_t - d_t\right)\right) \\ &= 1 - F\left((1-\sigma)\left(\hat{d}_t - d_t\right)\right) \end{aligned} \tag{19}$$

At the beginning of period t, the incumbent sets τ_t and d_t to maximize his total expected utility over the next two periods. Since the incumbent cannot commit to follow a policy (budget) rule, he acts under discretion and takes d_t as given when calculating the probability of re-election. Exploiting the solution for the optimal tax rate, the incumbent's maximization problem can be stated as,

$$\begin{aligned}\max_{d_t} & E_t\left[\tau^* + d_t + \eta_t^a + u(y-\tau^*) + X\right] \\ & + E_t\left[1 - F\left((1-\sigma)\left(\hat{d}_t - d_t\right)\right)\right]\left[\tau^* - R(d_t) + \eta_{t+1}^a + u(y-\tau^*) + X\right] \\ & + E_t F\left((1-\sigma)\left(\hat{d}_t - d_t\right)\right)\left[\tau^* - R(d_t) + \eta_{t+1}^b + u(y-\tau^*)\right]. \end{aligned} \tag{20}$$

The first-order condition of the maximization problem (20) is,[23]

$$1 + (1-\sigma)F'\left((1-\sigma)\left(\hat{d}_t - d_t\right)\right)X - R'(d_t) \leq 0. \tag{21}$$

Eq. (21) compares the marginal gain of higher pre-electoral spending, which includes the marginal utility of the increased public consumption in the election period (equal to one) and the enhanced (ex ante) likelihood of re-election times the value of getting re-elected (the second term), with the marginal cost of borrowing, $R'(d_t)$. In equilibrium, the incumbent's optimal choice (d_t^*) must be consistent with the voters' expectations, so $d_t^* = d_t = d_t$. Given our assumptions on $f(0)$ and R, the first-order condition (22) holds as equality. Thus in equilibrium,

$$1 + (1-\sigma)f(0)X - R'\left(d_t^*\right) = 0. \tag{22}$$

Condition (22) defines the equilibrium deficit d_t^*, which is positive. Note that even though voters are rational and forward looking, in equilibrium, the incumbent will overstimulate the economy before an election by borrowing. Note also that since the chosen debt level is fully expected by voters, it has no effect on the incumbent's re-election probability in equilibrium.

Moreover, it follows from (22) that the magnitude of the pre-electoral deficit depends on two variables, X and σ. Differentiating the first-order condition yields the following comparative statics results,

$$\frac{\partial d^*}{\partial X} > 0; \quad \frac{\partial d^*}{\partial \sigma} < 0 \tag{23}$$

Intuitively, the higher the politicians' rents of remaining in power (X), the stronger are their incentives to increase spending in order to enhance the chance of re-election. As a result, the equilibrium level of pre-electoral borrowing (d^*) increases. On the other hand, a greater share of informed voters has the opposite effect since the voting decision of fewer voters can ex ante be influenced by a pre-electoral spending boom. Thus, the expected gain of boosting spending falls, which results in a lower level of pre-electoral equilibrium borrowing.

Combining the first-order condition (22), the budget constraints (10) and (4′), and the comparative statics results (23) yields the central results of the model: the government's budget balance is influenced by the timing of elections. Prior to elections, the incumbent engages in expansionary policy manipulations to increase his chance of re-election. As a result, a deficit is created. The magnitude of the deficit depends on two institutional features of the economy: the politicians' rents of remaining in power and the share of informed voters in the electorate.

[23] The second-order condition holds strictly given the assumptions on $R(d)$.

4. Conditional findings with rents and informed voters

Our model highlights two institutional features that may explain the variation in the size of political budget cycles across countries. In this section, we first describe how we proxy for these factors and discuss how they vary across countries as well as over time. We will then test whether these institutional factors can explain the difference in the size of electoral budget cycles between developed and developing countries.

4.1. Measurement and variation in rents and information ability

We construct two proxies for politicians' rents of being in power, X. The first proxy, denoted as TI, is based on the corruption index published by Transparency International, an international non-governmental organization devoted to combating corruption. This index measures each country's "degree of corruption as seen by business people and risk analysts".[24] We re-scale the original index by taking the difference between each country's score and the average score across all countries. Thus, a country with an average level of corruption has a TI value of 0, and countries with more than average level of corruption get positive values for TI. For our purpose, the drawback with this index is that time-series data are not available.[25]

Our second proxy for "rents" is constructed from the five institutional indicators provided by International Country Risk Guide, a private international risk service company.[26] These institutional indicators are designed to provide private investors with measures of governmental rent-seeking activities. To mitigate concerns with measurement error in the individual indicators, we create an aggregate index by summing up the five indicators. We then use the same re-scaling procedure as for the TI proxy and denote the new variable ICRG. Thus, ICRG takes the value 0 in a country-year observation with the average rent-seeking activity by the government. Country-year observations with higher levels of "rents" have larger values of ICRG. The advantage with the ICRG variable is that time-series information is available since 1982. Therefore, we can study how variations in the degrees of governmental rental-seeking activities across countries as well as over time affect electoral budget cycles.[27,28]

To proxy for the share of informed voters (σ), we combine data on access to media with information on whether a country has free media. Access to media is measured by "radios per capita" from the Global Development Network Growth Database (World Bank). A country-year observation is classified as having free media if the country had "freedom of broadcasting" in that year using information from Freedom House.[29] Finally, we multiply "radios per capita" with

[24] The TI index is a summary measure of corruption, based on information from various sources; it is a survey of surveys.

[25] The first corruption indices were produced in the mid 1990s, but only had 40 countries included. We use data from 2001 (100 countries included).

[26] The indicators are: "rule of law", "corruption in government", "quality of the bureaucracy", "risk of expropriation of private investment", and "risk of repudiation of contracts". See Knack and Keefer (1995) and the discussion in Barro and Sala-I-Martin (1995) for more information.

[27] We use the 1982 score for years prior to 1982. A drawback of using ICRG is that it may suffer more from potential perception biases compared to the TI index, since it only draws information from one source.

[28] Both TI and ICRG are subjective measures of corruption in general and not specifically a measure of politicians' rents of being in power. However, to the extent that corruption reflects an underlying institutional framework, different forms of corruption are likely to be correlated. For a discussion on the shortcomings with these type of data, see Svensson (2003, 2005).

[29] Freedom House started to provide this data from 1980. For years before 1980, we use the freedom indicator of 1980.

the country-year specific indicator of "freedom of broadcasting", and use the product to proxy for σ. The variable is denoted as INFO.

We also create two composite variables, COMP1 and COMP2, where COMP1 (COMP2) is the standardized sum of − TI and INFO (− ICRG and INFO).[30] A higher value on the composite variables implies a stronger institutional constraint on politicians' incentive and ability to manipulate policies before an election, and thus should be associated with smaller political budget cycles.

Summary statistics of these variables are provided in Table 1B. As is evident, both institutional variables differ substantially across countries and between the two sub-samples. For example, the differences in the average TI corruption scores and in the ICRG scores between developed and developing countries are almost two standard deviations of the pooled sample. For INFO, the difference is greater than one standard deviation.

4.2. Conditional effects

To test whether the institutional factors identified in the model can explain the difference in the size of the political budget cycles across countries, we augment the original fiscal balance regression (reported in column 2 of Table 2) with interaction terms of ELE * INST and INST, where INST refers to any of the proxy variables described above. The conditional findings are reported in Table 4.

In column 1, we add the interaction term ELE * TI.[31] The coefficient on the interaction term measures how the electoral effect on fiscal balance varies among countries with different corruption levels. As shown in column 1, both the election dummy and the interaction term enter the regression highly significantly and with the predicted signs. The effect is qualitatively important. For example, the election-induced increase in fiscal deficit in a country that has the average corruption level of developed countries (i.e., TI = − 2.69) is only 0.1% of GDP. But the electoral effect in a country with the average corruption level of developing countries (i.e., TI = 1.75) is as large as 1.9% of GDP.

Column 2 reports the coefficient estimates with ELE * ICRG and ICRG added to the baseline specification. The results are similar with this alternative measure of "rents". On average, countries with more governmental rents-seeking activities (i.e., higher values of ICRG) have larger electoral budget cycles. Note that while institutional features are typically persistent and the estimated effect of ICRG is consequently driven mainly by the cross-country differences, some countries (such as Bolivia, Ghana, Malta) have seen their ICRG score fall by more than one standard deviation of the pooled sample (12.4). For these countries, the coefficient estimates suggest a reduction in the increase of fiscal deficit in election years by almost 1% of GDP over the sample period.

The effect of a better informed electorate is shown in column 3. Consistent with the model, a greater share of informed voters leads to smaller political budget cycles. Specifically, if INFO

[30] Specifically, $\text{COMP1} = [\text{TI}/\text{StD}(\text{TI})] + \left[(\text{INFO} - \overline{\text{INFO}})/\text{StD}(\text{INFO})\right]$, where StD(x) is the standard deviation of x and $\overline{\text{INFO}}$ is the mean of INFO. COMP2 is defined similarly but with TI replaced by ICRG. Note that the means of TI and ICRG are zero by construction.

[31] In this specification, we do not include TI as a separate regressor because TI does not vary over time for a given country. The direct effect of TI on fiscal budget balance is captured in the country-specific dummies.

Table 4
Elections and government fiscal balance: conditional findings

Dep. variable	Fiscal budget balance				
Regression	(1)	(2)	(3)	(4)	(5)
Method	GMM	GMM	GMM	GMM	GMM
ELE	−1.14**	−1.30***	−1.16***	−1.22***	−1.16***
	(.48)	(.38)	(.42)	(.44)	(.36)
ELE * TI	−0.38**				
	(.18)				
ELE * ICRG		−.07**			
		(.03)			
ELE * INFO			0.81*		
			(.48)		
ELE * COMP1				0.49**	
				(.25)	
ELE * COMP2					0.35**
					(.17)
ICRG		−0.09			
		(.06)			
INFO			1.67		
			(1.49)		
COMP1				−0.02	
				(.44)	
COMP2					0.69
					(.47)
BUDGET(−1)	0.39***	0.39***	0.43***	0.39***	0.40***
	(.11)	(.10)	(.09)	(.11)	(.10)
BUDGET(−2)	0.24**	0.30***	0.29***	0.24**	0.30**
	(.10)	(.08)	(.08)	(.11)	(.08)
Growth	28.2***	13.7	16.2	28.6***	13.7
	(4.96)	(12.7)	(12.6)	(5.11)	(12.7)
LnGDP	−0.20	−0.44	0.11	−0.25	−0.57
	(.50)	(.82)	(.80)	(1.19)	(.91)
Hansen test	54.3	70.9	80.5	56.6	70.1
	[.99]	[.99]	[.99]	[.99]	[.99]
Serial corr.	−0.66	−0.93	−0.59	−0.62	−0.95
	[.51]	[.35]	[.56]	[.53]	[.34]
No. countries	60	76	84	60	76
No. obs.	890	1082	1190	890	1078

Notes: See notes for Table 2.

increases by a one standard deviation of the pooled sample (0.38), the size of political budget cycle reduces by 0.3% of GDP.[32]

To examine the effects of the two types of institutional features simultaneously, we augment the baseline regression with the composite variables (COMP1 and COMP2) and their interaction terms with the election dummy. The results of these specifications are reported in columns 4 and 5. Countries/years with greater values of the composite variables (i.e., stronger institutional constraints on the politicians ability to expropriate public resources and/or a larger share of

[32] Decomposing INFO into the sub-components "radios per capita" and "freedom of broadcasting" and estimating the model with these sub-components entered separately, does not produce significant results. This suggests that it is the joint effect of these two measures that matters.

informed voters) are associated with smaller political budget cycles. The effect again is quantitatively important. For example, the election-induced increase in fiscal deficit in a country/year that has the average COMP1 score of developing countries (i.e., COMP1 = − 1.05) is 1.5% of GDP higher than in a country/year with the average score of developed countries (i.e., COMP1 = 1.96).[33]

One potential problem with the findings reported in columns 1–5 is that since the average scores of the institutional indicators differ substantially between the two sub-samples, they could potentially pick up the effect of income in the above regressions. To investigate this possibility, we augment these specifications with an additional interaction term, ELE × DEV and re-estimate all specifications. The results are reported in Table 5. Although the standard errors on the interaction terms ELE × INST increase, the coefficient estimates on ELE and ELE × INST are similar to the ones reported in Table 4 and remain jointly statistically significant. ELE × DEV does not enter any regression significantly. Thus, after controlling for variations in institutional features across countries, there is no difference in the size of political budget cycles between developed and developing countries.

We also experimented with replacing ELE × INST with ELE_PRE * INST and ELE_END * INST. In all cases, we find that predetermined elections have a greater effect on government fiscal balance in countries/years with weaker institutional constraint on politicians and larger share of informed voters. The results for other elections are inconclusive.[34] Dropping the outliers identified by Brender and Drazen (2004) or allowing for a more general error process do not qualitatively change the main finding that political budget cycles are significantly larger in countries with weak institutional features.

5. Robustness tests

We ran a number of robustness tests on the results reported above. First, we experimented with alternative election indicators in order to allow the electoral effect on fiscal policies to differ depending on whether the election took place earlier or later in the year. A priori, it is not clear what is the best way to capture this timing effect. Therefore, we tried several election dummies: ELE10m, ELE8m, and ELE6m. ELE10m takes the value 1 in year t if an election takes place during the last 10 months in year t and the first 2 months of year $t+1$, 0 otherwise. ELE8m and ELE6m are defined similarly. The key findings reported above remain basically intact using these alternative election indicators. For example, in the baseline regression of fiscal balance, all the three alternative election indicators enter the regression significantly with coefficient estimates − 0.67 (on ELE10m), − 0.95 (on ELE8m), and − 1.93 (on ELE6m).

We also added additional controls, including terms-of-trade shocks, share of population above 65, and share of population under 15. The coefficient estimates on the election dummy and on

[33] An alternative empirical strategy is to augment Eq. (1) with both institutional variables (TI (or ICRG) and INFO) and the interaction terms of elections and the institutional proxies simultaneously. The result of such a specification is that both interaction terms enter the regression with the predicted signs, although the coefficient estimates are not significant at conventional levels. This is likely the result of a multicollinearity problem caused by the high correlation between the institutional variables. For example, the variance inflation factors for the institutional variables and the interaction terms range from 3.2 to 11.9 in the specification reported in column 2, Table 4 (using FE). The multicollinearity problem will mask the individual effect of the two variables, but not their joint effect. The hypothesis that the coefficients on both the interaction terms are zero can be soundly rejected (results available upon request).

[34] Results available upon request.

Table 5
Robustness tests: income

Dep. variable	Fiscal budget balance				
Regression	(1)	(2)	(3)	(4)	(5)
Method	GMM	GMM	GMM	GMM	GMM
ELE	−0.81	−1.01***	−1.29***	−1.27*	−1.22***
	(.71a)	(.48)	(.53)	(.70)	(.54)
ELE * TI	−0.54*				
	(.30)				
ELE * ICRG		−0.10**			
		(.04)			
ELE * INFO			0.38		
			(.48)		
ELE * COMP1				0.38	
				(.25)	
ELE * COMP2					0.32*
					(.19)
ICRG		−0.08			
		(.07)			
INFO			1.87		
			(1.48)		
COMP1				0.03	
				(.45)	
COMP2					0.71
					(.50)
ELE * DEV	−0.87	−0.83	0.64	0.41	0.13
	(1.23)	(.74)	(.69)	(.97)	(.69)
Growth	28.0***	13.8	16.1	28.0***	13.8
	(5.05)	(12.7)	(12.9)	(5.03)	(12.5)
LnGDP	−0.19	−0.40	0.06	−0.22	−0.60
	(.52)	(.81)	(.82)	(1.15)	(.90)
F-test (ELE, ELE * INST)	6.18**	10.6**	6.23**	6.51**	7.72**
Hansen test	55.5	70.2	80.7	52.0	70.0
	[.99]	[.99]	[.99]	[.99]	[.99]
Serial corr.	−0.68	−0.94	−0.60	−0.59	−0.93
	[.50]	[.35]	[.55]	[.56]	[.35]
No. countries	60	76	84	60	76
No. obs.	890	1082	1190	890	1078

Notes: See notes for Table 2. All regressions include lagged values, BUDGET($t-1$) and BUDGET($t-1$), of government budget balance. *F*-test is an *F* test of the null hypothesis that the coefficients on ELE and ELE * INST are zero. *P*-values are shown in parentheses.

the interaction terms between the election dummy and the institutional variables in Tables 2–4 remain essentially the same. The additional controls had no robust significant relation with government fiscal balance and are uncorrelated with the timing of elections. Since including them reduces the sample size, we leave them out of the baseline specification. We also experimented with adding the oil price and an international interest rate, but this did not change our basic findings either.

Another concern with the results reported above is that not all elections in our sample are democratic. One may argue that in situations where political rights are restricted and voting outcomes can be manipulated, elections need not trigger a change in fiscal policy. To check that our results are not driven by the inclusion of the less democratic countries, we use information

Table 6
Robustness tests: democracy

Dep. variable	Fiscal budget balance					
Regression	(1)	(2)	(3)	(4)	(5)	(5)
Method	GMM	GMM	GMM	GMM	GMM	GMM
ELE	−0.84***		−1.07***	−1.18***	−1.15	−1.07**
	(.29)		(.40)	(.38)	(.73)	(.53)
ELE_PRE		−0.47*				
		(.29)				
ELE_END		−1.46**				
		(.64)				
ELE*COMP1			0.42**		0.39	
			(.21)		(.32)	
ELE*COMP2				0.34*		0.39**
				(.20)		(.19)
COMP1			0.55		0.56	
			(.51)		(.54)	
COMP2				1.00*		1.00*
				(.56)		(.59)
ELE*DEV					0.14	−0.24
					(1.30)	(.75)
Growth	8.59	8.95	21.7***	6.53	21.6***	6.34
	(10.0)	(9.96)	(3.88)	(9.65)	(4.32)	(10.2)
LnGDP	1.24	1.21	−1.25	−0.46	−1.31	−0.48
	(.98)	(.97)	(1.18)	(1.17)	(1.24)	(1.15)
F-test (ELE, ELE*INST)			7.21**	9.82***	6.55**	7.44**
Hansen test	72.0	69.8	52.5	67.7	52.5	68.2
	[.99]	[.99]	[.99]	[.99]	[.99]	[.99]
Serial corr.	−0.71	−0.66	−1.07	−1.21	−1.08	−1.20
	[.48]	[.51]	[.28]	[.23]	[.28]	[.23]
No. countries	79	79	59	73	59	73
No. obs.	1013	1013	793	929	793	929

Notes: See notes for Tables 2 and 5.

on "political rights" from Freedom House to classify whether a country in a particular year is democratic or not. Specifically, we define a country/year as democratic if the country "enjoys some elements of political rights, including the freedom to organize quasi-political groups, reasonably free referenda, or other significant means of popular influence on government" (Freedom House) in that year.[35] We then re-estimate all the regressions using the sub-sample of country-year observations that are democratic. We report a summary of the re-estimated findings in Table 6. As evident, the results for ELE in Table 2, ELE_PRE in Table 3, and ELE × INST in Table 4 remain qualitatively the same.[36]

[35] The "political rights" index that Freedom House assigns to each country/year ranges between 1 and 7, with 1 indicating the most extensive rights. A country/year with ratings between 1 and 5, which indicate the presence of basic political rights, are classified as democratic. Ratings 6–7 pertain to countries/years characterized as having minimal or no political rights. In the sample, 313 (out of 352) elections are classified as democratic elections.

[36] At first glance, these results might appear surprising. However, one should note that elections in countries with limited political rights are often a focal point for unsatisfied citizens to get organized and make threatening actions towards the ruling regime. Elections also provide an opportunity for the regime to signal that it has broad public support. Therefore, incumbents in these political systems also have strong incentives to appease the electorate before an election.

6. Discussion

This paper contributes to the political budget cycles literature in four aspects. First, we provide an empirical analysis of political budget cycles based on a large panel of countries. We find that, on average, government deficit as a share of GDP increases by almost one percentage point in election years. This effect is large; it implies that, on average, fiscal deficit increases by 22% in election years.

Second, we show that the size of political budget cycles is much larger in developing countries than in developed countries.

Third, we attempt to identify the causal effect from the incidence of elections to fiscal policy choices by distinguishing between elections that are predetermined and those that are not. The results suggest that the difference in the magnitude of political budget cycles between developed and developing countries still exists for predetermined elections.

Finally, we believe that we have pointed out an important area for future research, namely, the size of political budget cycles depends on institutional features of the country. In this paper, we provide some evidence regarding what institutional features matter, but more work along these lines is likely to be fruitful. The main finding in paper is that the strong institutional constraints on politicians in developed countries leave little room for public officials to expropriate public resources for private gains, and the large share of informed voters in these countries renders fiscal policy manipulations less effective. These institutional differences can, to a large extent, account for the difference in the size of the political budget cycles between developed and developing countries.

Appendix A. System GMM estimator

In this Appendix, we show the moment conditions of a system Generalized Method of Moments (GMM) estimator for Eq. (1),

$$y_{i,t} = \sum_{j=1}^{2} \gamma_j y_{i,t-j} + \chi' w_{i,t} + \beta \mathrm{ELE}_{i,t} + \xi_i + \varepsilon_{i,t}, \tag{A1}$$

where $y_{i,t}$ is the government fiscal budget balance in country i and year t. The key idea is to find instrumental variables which correlate with the explanatory variables, but not with the error term.

To eliminate the country-specific effects, we can take first-differences of (A1) to get

$$\Delta y_{i,t} = \sum_{j=1}^{2} \gamma_j \Delta y_{i,t-j} + \chi' \Delta w_{i,t} + \beta_e \Delta \mathrm{ELE}_{i,t} + \Delta \varepsilon_{i,t}, \tag{A2}$$

where $\Delta y_{i,t} = y_{i,t} - y_{i,t-1}$. Arellano and Bond (1991) note that under the assumption that the error term $\varepsilon_{i,t}$ is not serially correlated, values of y lagged two periods or more are valid instruments for the transformed lagged dependent variables $\Delta y_{i,t-1}$. For the control variables, we assume that w_{it} is weakly exogenous; that is, w_{it} is uncorrelated with future realizations of the error term. Thus, the GMM dynamic first-difference estimator uses the following linear moment conditions,

$$E\left[y_{i,t-s} \Delta \varepsilon_{i,t}\right] = 0 \qquad \text{for } s \geq 2, t = 3, \ldots . T \tag{A3}$$

$$E\left[w_{i,t-s}\Delta\varepsilon_{i,t}\right] = 0 \qquad \text{for } s \geq 2, t = 3, \ldots .T \tag{A4}$$

The election indicator ELE is assumed to be strictly exogenous and we therefore use $\Delta \text{ELE}_{i,t}$ as its own instrument in (A2).

While the moment conditions above are sufficient to estimate the parameters of the model, GMM estimators obtained after first differencing have been found to have large finite sample bias and poor precision in simulation studies. The intuition for this is simply that when the explanatory variables are persistent over time, lagged levels of these variables are weak instruments for the regression equation in differences. In order to increase the precision of the estimates, Arellano and Bover (1995), and Blundell and Bond (1998) propose to combine the above differenced regression with original regression in levels. The instruments for the regression in differences are those described above, while the instruments for the regression in levels (Eq. (1)) are the lagged differences of the dependent variables. Formally, the additional moment condition is the following:

$$E\left[\Delta y_{i,t-s}\left(\xi_i + \varepsilon_{i,t}\right)\right] = 0 \qquad \text{for } s \geq 1 \tag{A5}$$

$$E\left[\Delta w_{i,t-s}\left(\xi_i + \varepsilon_{i,t}\right)\right] = 0 \qquad \text{for } s \geqslant 1 \tag{A6}$$

Combining the moment conditions for the difference and level equations yields the system GMM estimator. Note that consistency of the system GMM estimator depends on the validity of the instruments. We consider two tests. The first is a Hansen test of over-identifying restrictions, where the null hypothesis is that the instruments are uncorrelated with the residuals. The second one is a test of the assumption of no serial correlation (in levels), which the moment conditions ((A3) and (A4)) rely on. This test is implemented as a test of second-order serially correlation in the difference Eq. (A2).

Although the system GMM estimator is asymptotically more efficient, the standard error estimates from the two-step covariance estimation tend to be severely downward biased (Arellano and Bond, 1991; Blundell and Bond, 1998). We correct the bias using the finite-sample correction of the two-step covariance matrix derived by Windmeijer (2005). Windmeijer (2005) shows that this correction leads to more accurate inference in Monte Carlo simulations.

Appendix B. Data

Data on government fiscal balance is from the 2004 International Financial Statistics (IFS) published by the IMF. Brender and Drazen (2004), BD, adjust these data in various ways. We revisit the changes they made. For the countries and time periods that our sample coincides with theirs, we make the following adjustments. (The data are available at http://www.iies.su.se/~svenssoj/#data):

Argentina: BD use the GFS data because the IFS data were missing before 1995. However, we obtain and use the IFS data since, in the 2004 version, data exist from 1976 and onward.

Austria: BD bridge the gaps in the IFS data using OECD data. We use IFS data because there are no breaks in the 2004 version of the IFS data.

Greece: we use the adjusted data from BD.

Israel: we use the adjusted data from BD.
Japan: we use the adjusted data from BD.
Ghana: it is not included in BD because IFS data did not exist. However, data from 1975 and onward exist in the 2004 version of the IFS, so we include it.
Guyana: it is not included in BD because IFS data did not exist. However, data from 1975 and onward exist in the 2004 version of the IFS, so we include it.
Jamaica: it is not included in BD because IFS data did not exist. However, data from 1975 to 1985 exist in the 2004 version of the IFS, so we include it.

Unlike BD, we do not drop country-year observations for which either no POLITY scores are available or the POLITY score, which ranges from −10 to 10, is below 0.

BD also exclude the following country-year observations (due to extraordinary changes in the series or shocks): Bolivia (1982–1985), Israel (1985), Botswana (1975–1995), Zambia (1975–1995). We find no evidence of reporting errors in the fiscal balance data and therefore include them in our baseline sample. We discuss in Sections 2 and 4 if and how our results are affected by dropping these observations.

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