Political Budget Cycles In Developed and Developing Countries*

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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 in both developed and developing countries: government spending increases before elections while revenues fall, leading to a larger deficit in election years. We demonstrate that political budget cycles are much larger in developing countries than in developed countries. Comparing predetermined elections, we find that the difference in the size of political budget cycles between developing and developed countries is magnified. Finally, we show that these electoral effects are particularly strong in countries with weak institutional constraints on incumbents' rent-seeking ability.

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1 Introduction

It is often claimed that fiscal policy is more expansionary in election years. Is this a universal phenomenon? Are there any systematic differences in the size and composition of such electoral budget cycles between developed and developing countries? If so, why? Despite a large literature on political budget cycles, these questions remain to a large extent unanswered. In this paper, we attempt to fill these gaps.

The empirical cross-country literature on political budget cycles has three common features.[1] It is based on data sets from a relatively small number of countries; it focuses on identifying whether or not there exists 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 renders it difficult to conclude if political budget cycles are a universal phenomenon. Second, the literature tells us little about the cross-sectional variation in the size and composition of these electoral effects (e.g., between developed and developing countries). Finally, since both the timing of elections and the fiscal policies could be affected by a common set of (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 relation.

This paper avoids these problems by assembling a large panel data set consisting of 91 countries over a 21-year period. The data allows us to study whether electoral effects on fiscal policy variables are common across countries, and whether they are more or less pronounced in developing countries. We also analyze all elections in the sample and identify whether or not the timing is predetermined. This enables us to distinguish between outcomes due to deliberate policy choices and unobserved events that are confounded with both the timing of elections and fiscal policies.[2]

We find political budget cycles to be a universal phenomenon. In election years, government spending increases while revenues fall, leading to a larger fiscal deficit. The electoral effect is considerable. On average, the fiscal deficit

[1] There is a large empirical literature on political business cycles, dating back to Nordhaus (1975), McRae (1977), Hibbs, (1977), and Tufte (1978). Most of this literature is on 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 (1999), Gonzalez(2002b), Magloire (1997), Khemani (1999), Kraemer (1997), Schuknecht (1996). None of the above papers combine data from developed and developing countries. Drazen (2000a,b) review the theoretical and empirical literature.

[2] Studies on U.S. data (and other countries with constitutionally fixed election dates, see e.g., Pettersson Lidbom [2002]) do not suffer from these potential endogeneity problems.

increases by 1 percent of GDP.

We also demonstrate that there are systematic differences between developed and developing countries. Specifically, political budget cycles are large in developing countries but not so in developed countries.

Comparing predetermined elections, we find that the difference in political budget cycles between developing and developed countries is magnified.

What may explain these differences? Shi and Svensson (2002) and Gonzales (2002a), building on Rogoff (1990), point to variation in institutional environment. We study one such institutional variable – the incumbents' access to rents. Intuitively, the more private benefits politicians gain while in power, the stronger their incentives to influence the voters' perceptions prior to the election to enhance the chances of re-election. Correlating standard cross-country corruption indicators with the magnitude of the political budget cycle, we show that countries with stricter institutional constraints on politicians' ability to use public resources and policies for private gains have smaller political budget cycles.

The rest of this paper is structured as follows. Section 2 briefly discusses the data used in the analysis, and provides initial evidence of political budget cycles. Section 3 estimates political budget cycles using dynamic panel data techniques. In Section 3, we also compare the cycles between developed and developing countries and discuss what may be the reason for the difference. In Section 4, we relax the assumption that the timing of elections is exogenous and show that the findings of Section 3 prevail. Various robustness tests are discussed in Section 5. Finally, Section 6 concludes.

2 Data sources and some initial evidence

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. We create a binary election indicator, ELE, which takes the value 1 in election years and 0 otherwise. For countries with parliamentary political systems, we (only) include legislative elections, while for countries with presidential systems, we include executive elections.[3]

Fiscal information is obtained from International Financial Statistics (IFS), published by the IMF. The government fiscal balance (BA), revenue (RE), and expenditures (EX) are expressed as shares of GDP. Real GDP per capita data

[3] About 20 countries have a third political system with assembly-elected president where the president is elected by the assembly but the assembly can not easily recall him. In this case, decisions on election dates are made based on where the executive powers rest (i.e. executive elections), based on information from the Political Handbook of various years.

are taken from the Penn World Tables. Our final sample consists of 91 countries with about 1,700 country-year observations.[4]

Because we are interested in studying cross country variations in political budget cycles, we partition our sample into subsamples of developed and developing countries. High income countries as defined by the IFS; i.e., countries with per capita GNP greater than USD 9,656 in 1997, are classified as developed countries. In our sample, there are 27 countries belonging to this group and the rest, 64 countries, are classified as developing countries. We define a variable DEV, which takes the value of 1 for developed countries and 0 otherwise. Table 1 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. Summary statistics of the key variables are provided in Table 2.

In Table 3, we report a simple measure of the average size of election-induced budget cycles in the sample, $APBC$. $APBC$ is the average of the country-specific measures of the political budget cycle (PBC), calculated as follows. For a given country, we compute the average of all election-year fiscal deviations, which is defined as the difference between the fiscal balance in an election year and the mean of the fiscal balance in the two adjacent years.[5] In the full sample, $APBC$ is -0.67, meaning that on average, the fiscal deficit as a share of GDP is 0.67 percent larger in election years than in the two adjacent non-election years. This difference is significant at the 5 percent level. We get a similar result if we compare the average fiscal deficit in election years ($BA \mid ELE = 1$) with that in non-election years ($BA \mid ELE = 0$). The difference is roughly 0.5 percent of GDP (Table 3, rows 2 and 3).

In columns 3 and 4 (Table 3), we report the results for the subsamples of developing and developed countries, respectively. In the former group, the average political budget cycle ($APBC$) is -0.81, which is more than twice as large as that in the developed countries (-0.36). Both measures are statistically significantly different from zero. The picture is also similar if we instead compare average deficit in election and non-election years.

We also did the similar calculations using government revenue and expen-

[4] In the panel time-series analysis, we include countries that have at least a decade of consecutive observations. Of the 91 countries, a handful did not have fiscal balance data broken down to revenues and expenditures, and some countries had financial data on revenues or expenditures, but not on fiscal balance. Thus, the samples for the three fiscal variables differ slightly.

[5] Formally, suppose there is an election in country i and year t, and $BA_{i,t}$ denotes its government fiscal balance, then the fiscal deviation in this election year is calculated as $BA_{i,t} - \frac{1}{2}(BA_{i,t-1} + BA_{i,t+1})$.

diture data. It appears that the political budget cycle is driven both by lower revenues (rows 4-6) and higher spending (rows 7-9) in election years. The main difference between the developed and developing countries seems to lie in the election-year expansion in government spending.

3 Estimating political budget cycles

3.1 Specification

The results reported in Table 3 suggest that political budget cycles are a universal phenomenon, and they are larger in developing countries. In this section, we present a regression analysis regarding the relation between elections and government fiscal policies. The basic empirical specification is

$$y_{i,t} = \sum_{j=1}^{k} \gamma_j y_{i,t-j} + \boldsymbol{\chi}' \mathbf{w}_{i,t} + \beta_e ELE_{i,t} + \xi_i + \varepsilon_{i,t}, \tag{1}$$

where $y_{i,t}$ is the fiscal policy outcome in country i and year t, $w_{i,t}$ a vector of control variables, $ELE_{i,t}$ an election dummy variable, ξ_i an unobserved country-specific effect, and $\varepsilon_{i,t}$ an i.i.d. error term. In the baseline specification, we include the logarithm of real GDP per capita and GDP growth rate as control variables.

Equation (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 to be biased. Fixed-effects estimators can eliminate the country-specific effect. However, the bias caused by the inclusion of lagged dependent variables remains. The bias of the Fixed Effects (FE) estimator, which influences all variables, is a function of T, and only when $T \to \infty$ will the FE estimator be consistent (see Nickell, 1981; and Kiviet, 1995). Since the average number of observations across countries in our sample is 19, 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).[6] 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. For comparison, we will also report the results of the basic specifications using FE.

[6] See Appendix 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.

We follow the literature and initially treat ELE as an exogenous variable in estimating equation (1). The assumption will be relaxed in Section 4. Although this assumption is mainly adopted in the literature to simplify the empirical analysis (there are no good instrumental variables for elections), the assumption is less restrictive than what it might appear. For example, the assumption is not that the timing of elections is exogenous, but that it is predetermined relative to the fiscal variables. Thus, if elections are held earlier than expected for reasons unrelated to fiscal policy outcomes, the identifying assumption is valid. Moreover, in cases when economic factors are driving the timing of elections, it is typically the state of the economy that matters (not necessarily directly the government's fiscal stance), which we (partly) control for in all of the regressions by including real GDP per capita and GDP growth rates.

3.2 Evidence of political budget cycles in a large sample of countries

Table 4 reports the results with the government budget balance as dependent variable. The FE estimates are shown in column 1. The coefficient estimate on ELE suggests a negative relation between elections and fiscal balance, and it is significant at the 5 percent confidence level.

The GMM results are reported in column 2. The estimated coefficient implies that the fiscal deficit as a share of GDP is one percentage point higher in election years. Given that the average fiscal balance is -4.4 percent of GDP in the sample, the estimate suggests considerable electoral effects on governments' fiscal policies. On average, the fiscal deficit increases by 23 percent in election years. Note that both the Sargan test and the serial correlation test of the error term confirm that the moment conditions used for GMM estimations cannot be rejected.[7]

It should be noted that each regression includes a up to three-period lag structure, as well as two control variables: real GDP per capita and GDP growth rates. Moreover, we have tested and confirmed that the time-series of government budget balance is stationary.[8]

The increases in fiscal deficit during election years could potentially result from either lower government revenues, and/or a higher spending. As stressed by Alesina et al. (1997), the choice between increased spending and reduced

[7] See appendix for discussions on these tests.

[8] That is, the roots of the polynomial equation based on the coefficient estimates are greater than 1, in absolute value. In determining the appropriate lag length, we first estimate (1) with a three-year lag structure. If a particular lag does not enter the regression significantly, it is dropped and (1) is reestimated.

taxes is ambiguous and may vary both over time and across countries. Therefore, it is interesting to investigate this issue empirically.

Regressions 3-4 report the relation between elections and government revenues. There is a strong negative relationship between these two variables. The coefficient estimate of the GMM regression suggests that the ratio of tax revenues to GDP is lower by 0.4 percentage points in election years. Thus, tax revenues are on average 2 percent lower (0.4 divided by the sample mean of 23.8) in election years.

The results on government expenditure are shown in regressions 5-6. The FE results are inconclusive, but the GMM estimates suggest that elections have a significantly positive effect on government expenditures. On average, government spending as a share of GDP increases by 0.5 percentage point in election years.

We turn next to the second main question of the paper: Are there any systematic differences in the size and composition of political budget cycles between developed and developing countries?

To investigate this question, we run the benchmark regressions separately for the two subsamples. This allows for different estimates of all the explanatory variables between the two subsamples. We then test the hypothesis that the coefficients on the election dummies are the same in the two regressions. We report the z statistic, which is defined as the ratio of the difference of the coefficient estimates to the standard error of this difference. GMM estimators are asymptotically normally distributed (Hansen, 1982). Assuming that the coefficients on the election dummies for the two subsamples are independent, the z statistic is also asymptotically normal.

Table 5 reports the results separately for the two subsamples of developed and developing countries. Regressions 1 and 2 show that the difference in the magnitude of the electoral budget cycle between the two subsamples is substantial. The coefficient on ELE implies that in election years, the fiscal balance worsens by 1.4 percentage points in the average developing country and roughly 0.6 percent in the average developed country. The difference is highly significant (z-test).[9]

Table 5 also reveals systematic differences in the composition of the political budget cycles between the two groups of countries. Regressions 3 and 4 present clear evidence of election-induced cycles in revenues, both in developed and developing countries. The point estimates are similar, implying that the difference in election-induced deficit cycles is not driven from the revenues side.

[9] We also estimated a pooled regression with $ELE * DEV$ as an additional control. The results of the pooled regressions (see Shi and Svensson, 2002) reinforce the findings presented in Table 3.

This conjecture is confirmed in regressions 5 and 6. There is only weak evidence of election-induced expenditure cycles in developed countries; ELE enters the regression with appropriate sign but the coefficient estimate is small and not statistically significant. However, for developing countries, the point estimates imply a 0.9 percentage point increase in government spending as a share of GDP in election years. That is, on average, developing countries increase spending by 3.5 percent in election years.

Why are political budget cycles larger in developing countries than in developed countries? Clearly, the two groups differ in many dimensions that may affect politicians' incentives and ability to manipulate fiscal policy prior to election (see Shi and Svensson, 2002). One such feature is the politicians' rents of being in power. Intuitively, the more private benefits politicians gain when in power, the higher the return of reelection, and the stronger the incentives to influence the voters' perceptions by expansionary policies prior to the election.

Measuring rents is difficult. We will proxy for it using data from Transparency International, an international non-governmental organization devoted to combat corruption, as well as data from International Country Risk Guide (ICRG), a private international risk service company.

The first proxy we use, denoted as TI, is based on the corruption index published by Transparency International. This index measures each country's "degree of corruption as seen by business people and risk analysts". We rescale the original index by taking the difference of that country's score from the average across all countries. So, a country with an average level of corruption has a TI value of 0, and countries with more than average levels of corruption get positive scores. For our purpose, the drawback with this index is that time-series data are not available.[10]

Looking at the raw cross-country data (Table 2), the average TI corruption score differs dramatically between the two subsamples. The difference is almost two standard deviations of the pooled sample. We regress our simple country-specific measure of political budget cycles, PBC as defined in Section 2, on TI. We find a negative (coeff =-0.22) and significant (t-value 2.6) relationship. Thus, high corruption countries tend to have larger political budget cycles (i.e., in absolute terms large PBC values). The group of countries with positive TI scores and large (in absolute values) political budget cycles are mostly low income countries, but again there are exceptions (e.g., Greece).[11] The group of countries with negative TI scores (i.e., countries with lower than average

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

[11] The group of countries with positive TI score and lower than average PBC, include Argentina, Bangladesh, Bolivia, Brazil, Ghana, Greece, Hungary, Malaysia, Mexico, Nicaragua, Nigeria, Panama, Philippines, Romania, Uruguay, Venezuela.

corruption) and small political budget cycles consists primarily of high-income countries, but there are exceptions (e.g., Chile, Tunisia).

We also perform regression analysis to support this finding; the results are reported in Table 6. We add an interaction term of ELE and TI to our basic econometric specification. The coefficient on the interaction term measures how the electoral effect on the fiscal budget varies among countries with different corruption levels. As shown in column 1, both the election term and the interactive term enter the regression highly significantly and with the predicted signs. The impact of corruption index on political budget cycles is also large. The election-induced increases in fiscal deficit in a country with an average TI score (=0) is roughly 0.9 percent of GDP. The magnitude of the cycle increases to 1.3 percent of GDP in a country with a TI score one-standard deviation above the mean.

Our second proxy for "rents" is constructed from the five institutional indicators provided by ICRG.[12] These institutional indicators are designed to provide private investors with measures of governmental rent-seeking activities, and have been used previously in the cross-country literature. We aggregate the five indicators, partly to minimize the effect of measurement errors in each individual indicator. Then we use the same rescaling procedure as for the TI proxy and denote the new variable $ICRG$. A country-year observation with an average rent-seeking activity by the government has a value of 0 for $ICRG$, and countries with higher levels of "rents" have larger values of $ICRG$. The advantage with the $ICRG$ variable is that time-series information is available since the early 1980s. We can therefore study how both cross-country and time variation in institutions affect the magnitude of political budget cycles.[13]

Column 2 of Table 6 reports the estimates in the augmented regression with $ELE \times ICRG$ (and $ICRG$) added to our basic specification. The results are similar with this alternative measure of "rents". On average, countries with higher values of $ICRG$ (higher governmental rents-seeking activities) 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. For these countries, the coefficient estimates suggest a reduction in election-year

[12] 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.

[13] The drawback of using $ICRG$ is that it may suffer more from potential perception bias compared to the TI index, since it only draws information from one source. For a discussion on the shortcomings with these type of data, see Svensson (2003)

increases in fiscal deficit by more than 0.85 percent of GDP over the sample period.

One potential problem is that since the average scores of TI and $ICRG$ differ substantially between the two subsamples, they could potentially pick up the effects of income in the above regressions. To investigate this possibility, we augment the above specifications with an additional interaction term, $ELE \times DEV$. As shown in columns 3 and 4, this modification does not change the qualitative results. $ELE \times TI$ and $ELE \times ICRG$ still enter the regressions significantly and with similar coefficient estimates, while $ELE \times DEV$ has no explanatory power.

4 Endogeneity of election timing

A potential critique of the results presented in Section 3 is that we treat the election variable as exogenous, although this may not be the case in reality. For example, both the timing of elections and the fiscal policies could be affected by a common set of (unobserved) variables, such as crises or social unrest, which are not included in our regression. In this case, our coefficient estimate of ELE will be biased. In particular, if the omitted variables correlate positively with election timing and negatively with fiscal policy outcomes, or vice versa, there will be a downward bias, and possibly a false negative coefficient estimate on ELE.[14]

One way to isolate the bias caused by the omitted variable is to focus on elections whose timing is predetermined relative to current fiscal policies. To achieve this, we analyzed all elections in the sample, using information from the Political Handbook (various years). We classify the election timing as predetermined if either (i) the election is held on the fixed date (year) specified by the constitution;[15] or (ii) the election occurs in the last year of a constitutionally fixed term for the legislature, or (iii) the election has been announced at least one year in advance. The predetermined elections will not, by definition, be correlated with the error term (i.e., omitted variables) in equation (1).

We create two additional election indicators $ELEPRE$, which takes the value 1 in an election year if the timing of the election is coded as predetermined, 0 otherwise, and $ELEEND$, which equals 1 in an election year if $ELEPRE$ equals to 0, and 0 otherwise. Among the 348 elections that enter the fiscal

[14] For example, if crises, in general, lead to a lower government fiscal balance, as well as early elections, omitting this variable will result in a downward bias in the coefficient estimate on ELE.

[15] Some countries have constitutionally fixed intervals, but the incumbent disregarded the constitution and either advanced or delayed the elections. We treat the election timing in this handful of countries as endogenous.

balance regression, 63 percent (220 elections) are classified to be predetermined. The share of elections classified as predetermined in the samples of developed and developing countries is 67 and 61 percent, respectively.[16]

Another potential problem is that the timing of elections may be chosen strategically by the incumbent politicians. This may pose a (reverse causality) problem if politicians condition the timing of elections on fiscal policy outcomes. If this is the case, our coefficient estimate on ELE does not correspond to the notion of political budget cycles that we set out to investigate. However, we believe this is less of a problem. Presumably, if the timing of elections was chosen as a function of fiscal policy, elections would be called in times when the fiscal deficit was small; i.e., in times of relative prosperity, suggesting a positive association between ELE and the fiscal balance. If the strategic effect is present in the data, it would attenuate any measured negative relation between the timing of elections and fiscal balance, and thus work against finding higher deficits in election years.[17]

In column 5 of Table 6, we replace ELE with $ELEPRE$ and $ELEEND$. In the full sample, $ELEPRE$ enters with a coefficient close to one, as in the baseline regression reported in column 2, Table 4. Thus, in the full sample, there is no evidence that unobservable events confounded with both the timing of elections and fiscal policy outcomes are driving the results.

In columns 6 and 7, we run separate regressions for developed and developing countries. In the subsample of developed countries, the coefficient on $ELEPRE$ (-0.38) is significantly smaller than the coefficient on $ELEEND$ (-1.06). That is, the election effect is smaller (in absolute terms) when the election timing is predetermined than when it is endogenous. This result implies that the inclusion of the elections with endogenous timing biases downward the estimate of ELE in regression 1 of Table 3. In the subsample of developing countries, the coefficient on $ELEPRE$ is larger (in absolute value) than the coefficient on $ELEEND$, but the estimates are not significantly different at the 10 percent significance level.

Comparing predetermined elections, the estimated size of the election effect on fiscal balance in the subsample of developing countries is now 4 times as large as that in the developed countries, and the difference is highly statistically significant (z-test). Thus, while a predetermined election, on average, is

[16] The share of elections coded as predetermined is roughly the same under presidential and parliamental electoral systems.

[17] The ideal way to deal with this reverse causality problem is to find an instrumental variable that is correlated with the timing of elections, but not with the error term in the baseline regression. However, it is unlikely that such an instrument exists for a cross-country study. Note that even though using $ELEPRE$ does not eliminate the potential bias, it reduces it. This is because all unexpected early elections, which are more likely to be result of strategic planning by the incumbents than other elections, are coded as 0 in $ELEPRE$.

associated with a 9 percent worsening in the fiscal balance in the average developed country, predetermined elections in developing countries are associated with a 35 percent increase in fiscal deficit.

It is interesting to note that, in the case of endogenous elections, election years coincide with similarly large increases in fiscal deficit in both developed and developing countries. This implies that the difference in the size of political budget cycles between the two subsamples of countries is the result of different choices of fiscal policies prior to the predetermined elections.

5 Robustness tests

We ran a number of additional robustness tests. First, we used an alternative election indicator that allows the electoral effect on fiscal policies to differ depending on whether the election took place earlier or later in the year. The results using this alternative indicator are similar to those reported above.

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 remained essentially the same. The additional controls had no robust significant relationship with the policy measures considered 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 included the oil price and an international interest rate. Including them did not change our basic findings.

There are a small number of outliers in the fiscal balance data. While there is no theoretical justification for dropping these observations (in fact, based on the time series profile of the respective country they are not necessarily outliers), it would be of considerable concern if our results were completely driven by them. To examine this possibility we dropped all observations with absolute values of government fiscal balance greater than four standard deviations above the mean, a total of 11 observations. Reestimating the model with these observations dropped, however, yields very similar results to those reported above.

Another concern may be that in countries where political competition is restricted and elections can be manipulated, elections may not have the same effect on fiscal policies as in other, more democratic, countries. Hence, we reestimate our regressions after dropping observations for countries with weak political rights; i.e., countries with the lowest score of political rights index as reported by Freedom House (1997). The empirical results remain intact. We also reestimated the baseline model with an additional control, an interaction term of ELE and an indicator of multiparty competition, taken from the Data-

base on Political Institutions (Beck et al., 2001). The new interaction term, and the multiparty competition indicator itself, enter insignificantly, while the coefficient on ELE remains unchanged.

We also augmented the baseline regression with the amount of foreign aid a country receives. Note that only developing countries are affected by this modification. If foreign aid is withheld during election years, this might explain why fiscal deficit increases in election years in developing countries. The data does not support this hypothesis.

Finally, we dropped the countries which only had one election over the sample period. The results remain intact.

To conclude, the results presented in Section 3 appear to be robust to various potential statistical and sample selection problems.

6 Discussion

This paper contributes to the political budget cycles literature in three aspects. First, we provide an empirical analysis of political budget cycles based on a large panel of countries. We find political budget cycles to be a universal phenomenon, a result that generalizes previous empirical findings based on smaller data sets. On average, government deficit as a share of GDP is one percentage point larger in election years. This is a large effect, implying that on average the fiscal deficit increases by 23 percent in election years.

Second, we show that political budget cycles are of much greater magnitude in developing countries than in developed countries. When comparing predetermined election outcomes, the difference in the size of political budget cycles between developing and developed countries is magnified.

Finally, we believe that we have pointed out an important area for future research, namely, the size (and composition) of political budget cycles depends on institutional features of the country. In this paper, we have provided some evidence regarding what institutional features matter, but more work along these lines is likely to be fruitful.

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A Appendix: System GMM estimator

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

$$y_{i,t} = \sum_{j=1}^{k} \gamma_j y_{i,t-j} + \boldsymbol{\chi}' \mathbf{w}_{i,t} + \beta_e ELE_{i,t} + \xi_i + \varepsilon_{i,t}. \tag{A1}$$

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

$$\Delta y_{i,t} = \sum_{j=1}^{k} \gamma_j \Delta y_{i,t-j} + \boldsymbol{\chi}' \Delta \mathbf{w}_{i,t} + \beta_e \Delta ELE_{i,t} + \Delta \varepsilon_{i,t}, \tag{A2}$$

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 $\mathbf{w}_{it}$ is weakly exogenous; that is, $\mathbf{w}_{it}$ is uncorrelated with future realizations of the error term. Thus, the GMM dynamic first-difference estimator uses the following linear moment conditions,

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

$$E\left[\mathbf{w}_{i,t-s} \Delta \varepsilon_{i,t}\right] = 0 \quad \text{for } s \geq 2,\ t = 3, ...T \tag{A4}$$

The election indicator ELE is assumed to be strictly exogenous and we therefore use $\Delta ELE_{i,t}$ 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 (equation 1) are the lagged differences of the dependent variables. Formally, the additional moment conditions are the following:

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

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

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 Sargan 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 equation (A2).

Table 1: Number of Elections Between 1975 - 95, by Country

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

* indicates a country belongs to the developed-country sample.

Table 2: Descriptive Statistics of Key Variables

Variable	Sample	Mean	Std. Dev.	No. obs.	No. countries
Fiscal Balance/GDP	All	-4.37	6.25	1658	85
(BA)	DEV=1	-4.12	4.83	556	27
	DEV=0	-4.49	6.86	1102	58
Gov't Revenue / GDP	All	23.8	11.0	1671	86
(RE)	DEV=1	30.2	10.6	538	26
	DEV=0	20.8	9.8	1133	60
Gov't Expenditures / GDP	All	28.5	12.6	1633	84
(EX)	DEV=1	33.7	12.2	558	27
	DEV=0	25.8	12.0	1075	57
Corruption Index	All	0	2.6	1260	60
(TI)	DEV=1	-2.7	1.4	504	24
	DEV=0	1.8	1.4	756	36
Institutional Index	All	0	12.0	1596	76
(ICRG)	DEV=1	-12.6	7.5	567	27
	DEV=0	6.9	7.5	1029	49

Table 3: Descriptive Statistics on the Size and Composition of Political Budget Cycles

Variable	Full sample	Developing	Developed
APBC (BA)	-0.67**	-0.81**	-0.36**
BA | *ELE*=1	-4.86	-5.06	-4.46
BA | *ELE*=0	-4.37	-4.56	-3.97
APBC (RE)	-0.34***	-0.36**	-0.29***
RE | *ELE*=1	23.7	21.0	29.9
RE | *ELE*=0	23.8	21.1	30.1
APBC (EX)	0.34	0.41	0.20
EX | *ELE*=1	29.0	26.5	34.0
EX | *ELE*=0	28.5	26.2	33.6

*** (**) indicate the difference is statistically significant at 1 (5) percent level.

Table 4. Elections and Government Fiscal Choices, 1975 - 95

Dep. Variable	Fiscal Balance		Revenue		Expenditure	
Regression	(1)	(2)	(3)	(4)	(5)	(6)
Method	FE	GMM	FE	GMM	FE	GMM
ELE	-0.49**	-1.06***	-0.45***	-0.36***	0.04	0.45**
	(.24)	(.21)	(.17)	(.12)	(.25)	(.24)
F-test[b]	1.80		2.58		2.37	
	[.00]		[.00]		[.00]	
Sargan test[c]		7.88		10.4		9.11
		[.64]		[.50]		[.52]
Serial corr.[d]		-0.64		0.59		0.72
		[.52]		[.56]		[.47]
No. countries	85	85	86	86	84	84
No. obs.	1366	1177	1369	1162	1348	1161
Adj. R^2	.69		.96		.91	

Notes: (a) Full regression, $y_{it} = \gamma_1 y_{i,t-1} + \gamma_2 y_{i,t-2} + \gamma_3 y_{i,t-3} + \chi_1 GDP_{i,t} + \chi_2 GROWTH_{i,t} + \beta_e ELE_{i,t} + \xi_i + \varepsilon_{i,t}$. Heteroskedastic-consistent standard errors reported in parentheses. *** (**) [*] denote significance at the 1 (5) [10] percent level. The instruments used in the GMM regressions are lagged levels (two periods and more) of the dependent variable, GDP, and GROWTH for the differenced equation, and lagged difference (one period) for the level equation. (b) F-test is an F test of the null hypothesis that all country-specific effects in the FE-specification are equal. (c) Sargan is a test of the over-identifying restrictions, asymptotically distributed as χ^2 under the null of instrument validity. (d) Serial corr. is a test for second-order serial correlation in the first-difference residuals, asymptotically distributed as N(0,1) under the null of no serial correlation. (e) z-test is a test of the hypothesis that the coefficients on ELE [ELEPRE] in the samples of developing and developed countries are equal in Table 5 [Table 6], distributed as N(0,1), with p-values shown in parentheses.

Table 5. Political Budget Cycles in Developed & Developing Countries, 1975-95, GMM Regressions

Dep. variable	Fiscal Balance		Revenue		Expenditure	
Regression	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Developed	Developing	Developed	Developing	Developed	Developing
ELE	-0.61***	-1.39***	-0.34***	-0.31*	0.09	0.87***
	(.16)	(.34)	(.13)	(.17)	(.22)	(.33)
z-test		2.31		0.14		1.87
		[.01]		[.44]		[.03]
Sargan test	12.7	5.07	13.6	9.82	11.1	11.3
	[.31]	[.89]	[.19]	[.55]	[.44]	[.33]
Serial corr.	-0.02	-0.78	0.48	0.61	-0.75	0.37
	[.99]	[.44]	[.63]	[.56]	[.45]	[.72]
No. countries	27	58	26	60	27	57
No. obs.	406	771	393	769	410	751

Table 6. Explaining Political Budget Cycles, and Robustness Test, 1975 - 95, GMM Regressions

Dep. variable	Fiscal Balance						
Regression	(1)	(2)	(4)	(5)	(6)	(7)	(8)
Sample	Full	Full	Full	Full	Full	Developed	Developing
ELE	-0.90***	-1.13***	-0.54*	-0.78**			
	(0.23)	(0.23)	(0.29)	(0.35)			
ELE*TI	-0.16**		-0.30**				
	(0.09)		(0.15)				
ELE*ICRG		-0.07***		-0.10***			
		(0.02)		(0.03)			
ELE*DEV			-0.82	-0.93			
			(0.59)	(0.77)			
ELEPRE					-1.03***	-0.38**	-1.55***
					(.26)	(.19)	(.39)
ELEEND					-1.12***	-1.06***	-1.13**
					(.35)	(.15)	(.55)
z-test[e]							2.70
							[0.00]
Sargan test	10.9	8.94	10.8	5.68	7.89	13.1	5.13
	[0.54]	[0.84]	[0.55]	[0.77]	[.64]	[.29]	[.88]
Serial corr.	0.50	0.31	0.50	-0.57	-0.63	0.01	-0.77
	[0.62]	[0.76]	[0.62]	[0.57]	[.53]	[.99]	[.44]
No. countries	60	76	60	76	85	27	58
No. obs.	881	1062	881	1062	1177	406	771