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# Political Budget Cycles: A Review of Recent Developments\*\*\*

This paper provides a review of recent developments in the theory and evidence of political budget cycles. Specifically, we discuss three areas where significant progress has been made. First, new theoretical explanations (models) have been proposed where political budget cycles arise as the result of a moral hazard problem between the government and the electorate. Second, more sophisticated empirical methods, in particular, time series methods appropriate for dynamic panel data regressions, have been adopted in cross-country analyses. Last but not least, the focus of recent studies has shifted from industrialized countries to all (including developing) countries, and from the existence of political budget cycles to the magnitude and composition (revenue vs. spending) of these cycles.

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A political budget cycle is a periodic fluctuation in a government's fiscal policies, which is induced by the cyclicity of elections. In this paper, we will review some recent developments in the theory and evidence of these cycles, with an emphasis on the international comparison of these phenomena.

We can highlight three areas in which significant progress has been made in recent years. First, new theoretical explanations (models) have been proposed where political budget cycles arise as the result of a moral

hazard problem between the government and the electorate. Second, more sophisticated empirical methods, in particular, time series methods appropriate for dynamic panel data regressions, have been adopted in cross-country analyses. Last but not least, the focus of recent studies has shifted from industrialized countries to all (including developing) countries, and from the existence of political budget cycles to the magnitude and composition (revenue vs. spending) of these cycles.

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Before we review these developments, let us clarify some restrictions on the scope of this paper. The political budget cycles literature used to be part of a broader literature on political business cycles, which studies the effects of elections (or more generally, politics) on the real economy, such as GDP growth rate and unemployment.<sup>1</sup> The literature, in general, shifted away from studying the real effects of elections to studying the policy makers' instruments partly because of the lack of empirical evidence,<sup>2</sup> and partly because the government does not directly control real economic variables. The theory of political business cycles requires the additional assumption that the government's policy instruments, fiscal or monetary, have an immediate impact on the real economy, which is still a highly contested issue. In this paper, we focus on political budget cycles alone and ignore whether there are real effects. For similar reasons, we will not discuss electoral cycles in monetary policy (interest rate, inflation, etc.). For the sake of brevity, instead of providing further arguments, we refer the reader to Drazen (2000) who wrote in his review paper titled "The Political Business Cycles After 25 years",

"A principal conclusion is that models based on manipulating the economy via monetary policy are unconvincing both theoretically and empirically, while explanations based on fiscal policy conform much better to the data and

form a stronger basis for a convincing theoretical model of electoral effects on economic outcomes."

In addition, there is also a so-called "partisan" approach to political business cycles, which examines the macroeconomic implications of electoral cycles when different political parties have different ideological and economic preferences. For example, left-wing parties are more willing to bear the cost of inflation to fight unemployment than right-wing parties. The partisan model has been partly successful in explaining the macroeconomic fluctuations in OECD countries where a party's social and economic orientation can be relatively easily identified.<sup>3</sup> However, this approach is unlikely to be fruitful for studying electoral policy cycles in developing countries, where the differences in economic and ideological preferences among parties are much harder to pin down, and the distinction frequently does not exhibit the typical Western left-right pattern. In fact, no study on the experience of developing countries used a partisan model. Since our emphasis in this paper is on the cross-country comparison of electoral budget cycles, we will focus on the universal re-election motivation, rather than partisan difference, of political parties.

The rest of the paper is organized as follows. Section 2 provides a summary of the moral hazard model of political budget cycles and its predictions. Section 3 discusses the specification and the dynamic panel data

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1. Nordhaus (1975) provides the first formal model of political business cycles resulting from the opportunistic pre-electoral manipulation of the government.
  2. "[T]here is no significant pre-electoral increase in aggregate economic activity prior to elections in either the U.S. or the OECD countries," Drazen (2000).
  3. The partisan approach to political business cycles started with Hibbs (1977). Alesina (1987) incorporates partisan preferences into a rational expectation model. Interested readers are referred to Alesina et al. (1997) for a comprehensive presentation of both the theoretical results and the empirical tests using U.S. and OECD data.

estimation technique that has been applied in the recent empirical literature. In Section 4, we review the empirical findings of political budget cycles from an international perspective. Finally, Section 5 outlines directions for future research.

### The moral hazard approach to political budget cycles

Before we describe the recent, moral hazard-based models of political budget cycles, let us briefly mention their (adverse selection-type) predecessors. The first such model of political budget cycles is due to Rogoff and Sibert (1988). They assume that each political candidate has a competence level (high or low), which is only known to the politician and not to the electorate. Nevertheless, voters want to elect the more competent politician (either the incumbent or the challenger). They form rational expectations regarding the type of the incumbent based on observable current fiscal policy outcomes. Before the election, the high-type incumbent will attempt to signal his type (and thereby increase his chances of reelection) by engaging in expansionary fiscal policy, which is less “costly” for him than it is for the low type. This leads to a pre-election increase in government deficit when a competent politician is in office (while no signaling takes place when the incumbent’s type is low). In a related model, Rogoff (1990) argues that the incumbent can also signal his competence before an election by shifting government expenditure towards easily observed consumption spending and

away from investment (whose effect can only be observed with a delay).<sup>4</sup> In all these models, signaling is the driving force behind the electoral budget cycle, which results from the temporary asymmetry of information regarding the politicians’ competence level.

These models of electoral budget cycles sparked a new wave of interest in political economy in the 1990s and are attractive for a number of reasons. First of all, they assume rationality and strategic behavior on the part of politicians and the electorate. They focus on the policy instruments (i.e., the manipulation of fiscal policies) that are directly controlled by the government, instead of the effects on the real economy. Finally, and perhaps most importantly, their main prediction (that incumbent governments manipulate fiscal policies prior to elections) is generally supported by empirical tests. However, some of the implications of the signaling models seem to be at odds with both empirical and anecdotal evidence. For example, it is the more competent politician (rather than the less competent one) who distorts the economy in the separating equilibrium of the signaling game, and only competent politicians will be reelected. In addition, since only competent types signal by creating a boom before an election, the testable implications are unclear without additional information on the (unobservable) type of the incumbent.

Recently, a new type of models of electoral budget cycles – based on moral hazard rather than adverse selection – has been proposed by Persson and Tabellini (2000) and Shi and Svensson (2002a).<sup>5</sup> We

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4. Persson and Tabellini (1990) proposed a similar model with asymmetric information as the driving force of electoral policy cycles. In their model, the government uses *monetary* policy to affect the real economy via the Phillips curve.
  5. Lohmann (1998) was the first to use a moral hazard approach in the political business cycle literature. However, she studied pre-electoral *monetary* expansions and their inflationary consequences in a neo-Keynesian macro-model.

will first summarize the intuition of this type of models (all based on Holmstrom's [1982] moral hazard model of career concern), and then discuss the empirical predictions.

According to the moral hazard approach (and exactly as in adverse selection models), each politician has a type (competence level) measuring, for example, their ability to produce public goods without raising taxes (their "productivity").<sup>6</sup> However, in contrast to the adverse selection approach, it is assumed that neither the electorate nor the politician can observe the politician's competence contemporaneously. Given the large set of possible policy issues that a government may face, the assumption simply means that politicians are (*ex ante*) uncertain about how well they will be able to handle future problems, and thus how well they will be able to transform government revenues into public output.

Voters are rational, and want to elect the more competent politician (between the incumbent and the challenger), since that would imply higher post-election public goods production. Their inference is based on the observable macroeconomic performance of the incumbent government, such as the amount of public goods produced, the level of unemployment, etc. The key assumption is that the incumbent government can exert a hidden effort, that is, to use a policy instrument unobservable to the public (or only observable with a delay), which is a substitute for competence. For example, if competence measures how well the politician can convert revenues into public goods, then the hidden effort can be interpreted as the government's short-term excess borrowing. This seems like a reason-

able assumption since the government can, through clever accounting techniques, obstruct voters' ability to assess its borrowing needs. A larger hidden effort (more secret borrowing) will not be concurrently observable to the public.

Elections take place after the incumbent government's hidden effort and competence jointly determine the observable macro variables (the level of public goods, for example). Clearly, the incumbent government would like to increase its performance index by exerting more effort (borrow more), hoping that voters would attribute the boost in public goods provision to its competence. In equilibrium, however, the electorate cannot be fooled: they are also aware of the incumbent's intentions, and hence they can correctly infer competence from the inflated performance indicators. Nevertheless, in the equilibrium of this moral hazard game, we should expect excessive effort on the part of the incumbent politician, and (*ex post*) we should observe excessive borrowing and an increase in the budget deficit before election. Note that here, in contrast to adverse selection models, all types of the incumbent government will incur excessive pre-election budget deficits (independent of their competence level).

The testable predictions of the moral hazard model are that policy outcomes, particularly the government's budget balance, are influenced by the timing of elections. Prior to elections the incumbent engages in pre-electoral policy manipulations to increase his chance of reelection. As a result, a deficit is created. Since all types of the incumbent have the same incentive structure in the moral hazard model, one can

6. Competence could also stand for the politician's ability to deal with exogenous shocks, or other characteristics that voters care about. However, we stay with the first interpretation for the sake of the clarity of the exposition.

test these empirical predictions even when the type of the incumbent government is not observable.

### The dynamic panel-data estimation technique

The most common empirical test of the electoral budget cycles on a panel of countries has the following form (for all  $i = 1, \dots, N$  indexing countries, and  $t = 1, \dots, T$  indexing time),

$$y_{i,t} = \sum_{j=1}^k \gamma_j y_{i,t-j} + \mathcal{X}' w_{i,t} + \beta_e e_{i,t} + \xi_i + \varepsilon_{i,t}. \quad (1)$$

In this equation,  $y_{i,t}$  is the policy outcome,  $w_{i,t}$  a vector of economic variables that may affect  $y_{i,t}$ ,  $e_{i,t}$  an election dummy variable,  $\xi_i$  an unobserved country-specific effect, and  $\varepsilon_{i,t}$  an i.i.d. error term. Equation (1) is a standard dynamic panel data specification because the lagged dependent variables,  $y_{i,t-j}$  for  $j = 1, 2, \dots, k$ , are among the explanatory variables. It is widely known that when the unobserved country-specific effects are different across countries, the simple Ordinary Least Square (OLS) estimator is biased. Therefore, in order to allow for cross-country differences in the time-average of the dependent variable, most empirical studies in the 1990s have employed Fixed-Effects estimators. For example, one can eliminate the country-specific effect by taking a within-transformation of the above regression equation,

$$\Delta y_{i,t} = \sum_{j=1}^k \gamma_j \Delta y_{i,t-j} + \mathcal{X}' \Delta w_{i,t} + \beta_e \Delta e_{i,t} + \Delta \varepsilon_{i,t}, \quad (2)$$

where  $\Delta y_{i,t} = y_{i,t} - y_{i,t-1}$ , and then apply the OLS estimations to this differenced regression equation.

However, the inclusion of lagged dependent variables in equation (1) poses another source of bias with the OLS estimation that cannot be eliminated by a Fixed-Effects regression. For the OLS estimator to be unbiased, each explanatory variable (as a column vector of observations) has to be uncorrelated with the vector of error terms. But in equation (1), this assumption is violated as the vector of a  $j$ -lagged dependent variable,  $\vec{y}_{i,-j}$ , is correlated with the vector  $\vec{\varepsilon}_i$ . For example, for a given country  $i$ , the  $t^{\text{th}}$  element of the vector  $\vec{y}_{i,-1}$ , namely  $y_{i,t-1}$ , and the  $(t-1)^{\text{th}}$  element of the vector  $\vec{\varepsilon}_i$ , namely  $\varepsilon_{i,t-1}$ , are related in equation (1). Similarly, it is easy to see that the Fixed-Effects regression exhibits the same problem: the  $t^{\text{th}}$  element of the vector  $\Delta \vec{y}_{i,-1}$ , namely  $(y_{i,t-1} - y_{i,t-2})$ , is correlated with the  $t^{\text{th}}$  element of the vector  $\Delta \vec{\varepsilon}_i$ , which is  $(\varepsilon_{i,t} - \varepsilon_{i,t-1})$ . The bias of the Fixed Effects (FE) estimator, which is present in all variables, diminishes only as  $T \rightarrow \infty$ , therefore the FE estimator is biased (but consistent).<sup>7</sup>

To avoid these problems, Arellano and Bond (1991) developed a Generalized Method of Moments (GMM) estimator for dynamic panel data regressions. The key idea is to find an instrumental variable for equation (2), which is correlated with  $\Delta y_{i,t-j}$ , but not with  $\Delta \varepsilon_{i,t}$ . Arellano and Bond (1991) note that under the assumption that the error term  $\Delta \varepsilon_{i,t}$  is not serially correlated, values of  $y$  lagged *two or more periods* satisfy these criteria and could be used as instruments for  $\Delta y_{i,t-1}$ . More specifically, the moment condition is as follows:

7. See Nickell (1981) and Kiviet (1995) for the proof of this claim.

$$E[y_{i,t-s} \Delta \varepsilon_{i,t}] = 0 \quad \text{for } s \geq 2, t = 3, \dots, T \quad (3)$$

Although these GMM estimators (based on the first differencing equation) should yield consistent estimates, Arellano and Bover (1995) and Blundell and Bond (1998) show that these estimators have poor precision in finite samples in simulation studies. The intuition for this is simply that when the explanatory variables are persistent over time, lagged levels of these variables are only weakly correlated with the differences of these variables in the differenced regression equation. In order to increase the precision of the estimates, they propose to combine the above differenced regression with the original regression in levels. The instruments for the regression in differences are those described above, while the instruments for the regression in levels are the lagged differences of the dependent variables. Formally, the additional moment condition is the following:

$$E[\Delta y_{i,t-s} (\xi_i + \varepsilon_{i,t})] = 0 \quad \text{for } s \geq 1 \quad (4)$$

Combining the moment conditions for the difference and level equations yields the system GMM estimator.<sup>8</sup> Note that the consistency of the system GMM estimator depends on the validity of the instruments, which can be tested by Sargan-test.

Since the system GMM estimator controls for unobserved country-specific effects as well as potential endogeneity of the explanatory variables, it is a preferred method to the Fixed-Effects estimators. The disadvantage of it, however, is that it reduces the sample sizes due to the reliance on

the “internal instruments”; i.e., lagged explanatory variables. In addition, its small-sample properties are generally unknown.

Though the system GMM estimation technique is still relatively new, it has already been applied in the empirical studies of electoral budget cycles. In the next section, we will report some results obtained using this estimation method, and compare it with the results from Fixed-Effects regressions.

## Empirical findings

Early empirical studies on political budget cycles focused on the experience of the United States. For example, Tufte (1978) documents a number of clear incidents of pre-electoral opportunistic manipulation in fiscal instruments, such as, government transfers. Alesina (1988) performs an OLS regression on a one-country version of equation (1) and finds a significant election year increase in net transfer over GNP for the U.S. over the period of 1961–1985.

Alesina et al. (1992, 1997) extend the analysis to other industrialized countries. They perform cross-section time series regressions using a specification similar to equation (1) on a panel of 13 OECD countries for the period between 1961 and 1993. Their Fixed-Effects estimates imply that, after controlling for other determinants of fiscal balances, government budget deficit is higher by 0.6 percent of GDP in election years.

Although the literature on political budget cycles originated in the context of competitive elections of industrialized countries, recently, a growing literature

8. Similarly concerns arise for the control variables,  $w_{it}$ . One can assume it to be weakly exogenous (or predetermined) and use the following moment conditions:  $E[w_{i,t-s} \Delta \varepsilon_{i,t}] = 0$ , for  $s \geq 2, t = 3, \dots, T$ , in differenced regressions and  $E[\Delta w_{i,t-s} (\xi_i + \varepsilon_{i,t})] = 0$ , for  $s \geq 1$  in level regressions. Election terms are usually assumed to be exogenous.



started to study the experiences of developing countries. In the last three decades, many developing countries have adopted democratic political reforms; a natural question to ask is whether this process brought about side effects such as election-induced policy cycles.<sup>9</sup> Most studies of individual developing countries report OLS estimators and have, in general, found evidence of political budget cycles. For example, Krueger and Turan (1993) show that pre-electoral fiscal manipulation was common in Turkey in the period of 1950–1980. While Remmer (1993) finds mixed results for eight Latin American economies, Gonzalez (2002a) confirms the existence of an electoral cycle in government spending in Mexico over the period of 1958–1997.<sup>10</sup>

Interestingly, cross-country studies, using data from developing countries, not only confirm the existence of politically-driven budget cycles, but also show that the magnitude of these cycles is quite large. For example, Ames (1987) studies 17 Latin American countries and finds that, on average, government expenditure increases by 6.3% in the pre-election year and decrease by 7.6% in the year after the election. Similarly, Kraemer (1997) and Rojas-Suárez et al. (1998) both show evidence that Latin American governments are more inclined to adopt expansionary fiscal policies during electoral periods, which results in a significant deterioration of fiscal stance. Schuknecht (1996) confirms the election-induced fiscal policy cycles for a panel of 35

developing countries; his regression results show that the overall fiscal balance worsens by more than 0.6 percent of GDP in election years. In addition, Schuknecht (2000) investigates which fiscal variable is most affected by the timing of elections, using a sample of 24 countries for the 1973–92 period. He finds that public investment cycles are particularly prominent. All these cross-country studies use Fixed-Effects estimators. Block (2002a) is the first paper that uses GMM regressions in estimating the electoral effects on government policies, for a cross-section of 44 Sub-Saharan African countries. His point estimate suggests that the government fiscal deficit increases by 1.2 percentage point in election years in the sample. In addition, Block (2002b), using a sample of 69 countries, provides evidence that government expenditures shift towards more visible current consumption and away from public investment in competitive (e.g., multi-party) elections.

The above results suggest that electoral budget cycles are not confined to industrialized countries, rather, they appear to be a general pattern in countries where elections are held. Unfortunately, this claim is not investigated to the fullest extent in the studies cited above. In particular, none of the data sets contain all, or a representative sample of, countries with elections. Moreover, the empirical methods vary across studies.

A recent paper by Shi and Svensson (2002a) addresses these issues. Their data set includes all countries – both developed and

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9. Although it should be noted these budget cycles may perform an efficiency enhancing role, by signalling competence and thereby increase the likelihood that competent politicians get re-elected (see Rogoff (1990)).
  10. Several studies focus on the effects of local elections. For example, Khemani (1999) examines the experience of 14 major states of Indian from 1960 to 1994, and finds that election years have a negative effect on some commodity taxes and a positive effect on public investment, though there is no change in local fiscal balances. Pettersson Lidbom (2001) shows that, in Sweden, local government spending is 1.5 percentage point higher and taxes are 0.4 percentage point lower in election years.



developing – that held elections over the period of 1975 to 1995. The data allow them to study whether electoral effects on fiscal policy variables are common across countries, and whether there are differences in the size and composition of political budget cycles between developed and developing countries. Shi and Svensson (2002a) use GMM technique and find that on average, fiscal deficit increases by 1 percentage point of GDP in election years. Moreover, the magnitude of political budget cycles is much greater in developing countries than in developed countries. Regarding the source of electoral budget cycles, they show that political budget cycles are driven both by reduction in taxes and increase in government spending, and the two effects are of similar magnitude.

To summarize, the literature has extended the analysis to include developing countries in searching for the evidence of political budget cycles. This turned out to be a fruitful attempt. The evidence suggests that political budget cycles appear to be a general pattern in all countries with elections and that the size of election-induced budget cycles are greater in developing countries. In the next subsection, we discuss what may explain this difference.

### **Conditional cycle**

Three recent papers offer some insight on why the magnitude of political budget cycles is different across countries. Gonzalez (2002b) incorporates the cost of removing a policymaker from office (the “degree of democracy”) and the probability that voters learn the incumbent’s competence costlessly (“transparency”) in a Rogoff (1990) type signaling model of political budget cycles. Her model predicts that electoral budget cycles emerge only when it is not very costly to remove a politician from office, and that

the size of political budget cycles decrease as the degree of transparency increases. Moreover, she shows that, when there is a positive correlation between the degree of democracy and transparency, political budget cycles arise only at intermediate levels of democracy.

Shi and Svensson (2002a) also introduce two variables to their moral hazard model of political budget cycles to capture the effect of the institutional environment on the size of electoral budget cycles: the politician’s rents of being in office, and the share of informed voters (the percentage of the electorate that can observe the hidden action). They show that 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, therefore, the larger the size of the political budget cycles. Similarly, the lower the share of informed voters, the more voters fail to distinguish pre-electoral manipulations from incumbent competence, and the higher the return from boosting spending prior to the election.

To test these predictions, Gonzalez (2002b) studies the relation between the level of democracy and the magnitude of the political budget cycle in a sample of 43 countries over the period 1950–97. She estimates a vector autoregression system and shows that the political budget cycles are largest in countries with intermediate levels of democracy. Shi and Svensson (2002b) show that the size of political budget cycles are positively correlated with the politicians’ rents and negatively associated with the share of informed voters, as predicted by their model. Importantly, the results continue to hold after controlling for a broad range of other possible explanatory variables, including income levels and an index of political rights.

Persson and Tabellini (2002) presents

evidence that political budget cycles are also affected by electoral rules (majoritarian vs. proportional) and the form of government (presidential vs. parliamentary), based on a data set that includes 60 democratic economies for the period of 1960 to 1998. Their Fixed-Effects estimates suggest that countries with majoritarian elections *cut* fiscal spending during elections while countries with proportional elections raise welfare spending. While they confirm pre-election tax cuts is a common phenomenon with both types of government, they find that post-election spending cuts and tax increases only occur under presidential governments.

### Directions for future research

In this paper, we provided a brief review on the current status of both theoretical and empirical literature on political budget cycles. On the theory front, we discuss the new moral hazard-based approach where political budget cycles arise in equilibrium in all elections, independent of the incumbent party's competence level. On the empirical front, we review the empirical studies based on various samples, including developing countries, in different time periods. In general, these studies confirm the prediction of the moral hazard model. Moreover, it is shown that the sizes of political budget cycles vary dramatically across countries.

An important area for future research concerns how the size and especially the composition (taxes vs. spending) of the electoral policy cycle depend on political and institutional features of the country. Shi and Svensson (2002a), Persson and Tabellini (2002), and Gonzalez (2002b), provided some evidence of what type of political and institutional features matter, but more work along these lines is likely to be fruitful.

Another important question is how the endogeneity of the timing of elections might affect political budget cycles. In most of the empirical work we discussed above, the election schedule is assumed to be fixed or exogenous to fiscal policies. However, 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. Shi and Svensson (2002b) provide the first effort to deal with this issue in a large cross section of countries. They analyze each election in a sample that contain 91 developed and developing countries for a span of 21 years and identify whether or not the election timing is predetermined. This enables them to distinguish between outcomes due to deliberate policy choices and unobserved events that are confounded with both the timing of elections and fiscal policies. Focusing on predetermined elections, they not only confirm that political budget cycles exist in the sample, but provide evidence that the difference in political budget cycles between developing and developed countries is magnified.

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