Elections, Electoral Competitiveness, and Political Budget Cycles in Developing Countries

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Elections, Electoral Competitiveness, and Political Budget Cycles in Developing Countries

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Elections, Electoral Competitiveness, and Political Budget Cycles in Developing Countries

Abstract

This paper presents the first cross-country evidence of electorally-motivated changes in the composition of public expenditures in developing countries. Results show that election-year public expenditures shift towards more visible current consumption and away from public investment goods. To date, empirical tests of political business cycles have ignored compositional effects. Evidence further shows that these effects are contingent on elections being competitive, and that in competitive systems expenditure shares on capital investment are higher in general. This suggests that when incumbents are generally more accountable to voters, an incentive may exist to maximize long-run, despite the accentuated short-run electoral incentive in competitive systems to diverge in favor of more visible forms of public expenditure. (JEL: H5, O11, D72)

I. Introduction

This present paper presents the first evidence of political cycles in the composition of public spending in a cross-section of developing countries. The central measurable issues in both the “opportunistic” branch (Nordhaus (1975), Lindbeck (1976), Tufte (1978)), and the “partisan” branch (Hibbs (1977)) of traditional political business cycle theory, along with rational partisan political business cycle theory (Alesina (1987)), are electoral cycles in inflation and unemployment. In contrast, rational opportunistic models of political budget cycles (Cukierman and Meltzer (1986), Rogoff and Sibert (1988), and Rogoff (1990)) are specifically explanations for electorally-timed distortions of fiscal policy. In particular, the latter theories predict
distortions in the composition of public spending, if not its level. While this theoretical literature, along with political business cycle theory in general, has motivated numerous empirical tests, to date all such tests have relied on macroeconomic aggregates such as total public expenditures, fiscal deficits, money growth and inflation. Virtually none have directly tested political budget cycle theory against changes in the composition of public spending, despite the theories’s specificity in that regard.

Relatively few previous empirical tests of political business cycle theories have concentrated on developing countries. Yet, the implications of such theories may be particularly important for developing countries, where relatively weak systems of political checks and balances are commonly combined with long-term (if sporadic) efforts at economic reform. Political budget cycle theory suggests that such cycles can be welfare enhancing; yet, such potential benefits must be balanced in developing countries against the potential threat that politically-motivated budgetary distortions may pose for on-going economic reform programs. Evidence of political budget cycles in nascent democracies may even imply a conflict between the concurrent challenges of political and economic reform that confront many developing countries. Moreover, all the relevant theories assume the existence of competitive elections. This paper proposes that such an assumption is unjustified in developing countries, and that relaxing that assumption changes the model’s empirical prediction in testable ways.

The paper is organized as follows. Section II motivates the empirical analysis with a brief review of relevant strands of political budget cycle theory. Section III describes the data and empirical strategy, while Section IV presents the results. Section V concludes.
II. Political Budget Cycle Theory and Empirical Tests

While traditional political business cycle literature concentrated on an exploitable Phillips curve, the focus of rational opportunistic models has been specifically on budget cycles. The primary theoretical contributions to this branch of literature are Cukierman and Meltzer (1986), Rogoff and Sibert (1988) Rogoff (1990), and Drazen (2000). Rogoff (1990) is taken here as the central example.

For Rogoff (1990), the central puzzle is not so much that office-seeking (e.g., opportunistic) politicians would engage in pre-election budgetary distortions, but rather why rational voters would allow their expectations about post-election performance to be influenced by such “antics.” His answer lies in a dynamic signaling model in which a political budget cycle arises due to temporary information asymmetries about the incumbent’s competence in administering the production of public goods.

In particular, Rogoff (1990) posits that voters maximize a utility function including the consumption of both private goods \( c \), and public goods \( g \). The public goods production function (e.g., the government budget constraint) takes the form:

\[
(1) \quad g_t + k_{t+1} = \tau_t + \varepsilon_t
\]

where \( g \) is the public consumption good, \( k \) is the public investment good, \( \tau \) is lump-sum taxes, and \( \varepsilon \) is a stochastic “competency” shock (with an MA(1) structure so that competency persists, but only for one period). A critical characteristic of this specification is that public investment decided at time \( t \) only becomes visible and productive at time \( t+1 \). The incentive for the government to engage in political budget cycles arises from a presumed informational asymmetry.
in which voters in the pre-election period observe \( g \) and \( k \), but not \( k_{t+1} \), and hence neither do they know \( e \), until time \( t+1 \) (though it is known contemporaneously by the incumbent). The incumbent’s competence is thus signaled by the ability to provide the greatest quantity of \( g \) for a given \( \tau \).

This production structure, combined with the informational asymmetry regarding competence, can give rise to a separating equilibrium in which the competent incumbent biases pre-election fiscal policy towards easily observed consumption expenditures and away from government investment. The MA(1) structure of the competency shock further implies that politicians’ incentive to create political budget cycles is limited to pre-election periods. Ironically, Rogoff (1990) also demonstrates that the greatest budgetary distortions are promulgated by the most competent politicians, implying that such distortions are welfare enhancing costs of identifying the most competent leaders.

While Rogoff (1990) provides a firm theoretical foundation for the possibility of electorally-timed shifts in the composition of public spending, one might question the applicability of such an equilibrium model to developing countries. Many developing countries are undergoing transitions to democracy, and many voters in developing countries lack the experience of voting that may be required strictly to conform to the model’s suppositions. Yet, the primary question of concern is not the precise applicability of a particular rational expectations model, but rather whether the increasing frequency of elections in developing countries will be associated with increasing budgetary distortion. Indeed, Rogoff’s primary contribution was to provide a rational behavioral model of opportunistic political business cycles. It differs in this important respect from the traditional (adaptive voter expectations) Nordhaus
approach; yet, the rational model’s empirical predictions do not differ radically from the earlier
view. Thus, for empirical purposes, the distinction between rational and myopic voting in an
opportunistic political business cycle model is not critical. Rogoff (1990) is convenient in that it
explicitly describes politically-timed distortions in the allocation of public expenditures (and his
informational asymmetries regarding competence are if anything more plausible in developing
countries); yet, such phenomena are well within Nordhaus’s intuition, as well.

The application of political business cycle theory to developing countries raises an
additional question relating to institutional context. The theories described above take for
granted the existence of particular institutions, such as competitive elections. This assumption is
relatively benign in the developed country setting for which the models were originally
conceived. Yet, the existence of competitive elections may not be assumed in developing
countries. As argued in Block, Ferree, and Singh (2001) and earlier by Schultz (1995), bringing
institutional context into political business cycle theory has direct implications for the model’s
predictions. Rogoff (1990), for instance, attributes the same utility function to office-holders and
voters, with the exception of ego rents which enter exclusively into the utility of incumbents.
Yet, both incumbents and voters share a disutility from economic distortion. Thus, incumbents
are willing to create electorally-timed distortions because they are compensated for the disutility
by ego rents. However, if elections are not competitive, and incumbents face a near zero
probability of losing office, the ego rents are not at risk and the incentive to distort is greatly
diminished. In short, relaxing the model’s assumption of competitive elections leads to a
somewhat more context-sensitive prediction that political budget cycles will be smaller if they
exist at all in non-competitive electoral systems. I test this institutional dimension explicitly by employing a novel measure of electoral competitiveness, described below.

To date, virtually all empirical tests of these theories have neglected the composition of public spending. The earliest empirical tests considered only output growth and unemployment. More recent tests have extended the analysis to include selected aggregates such as fiscal deficits, total public expenditures and government consumption, money growth, and inflation. Moreover, the bulk of empirical tests of political business cycle theory have excluded developing countries from consideration.

Table 1 summarizes previous empirical studies of political business cycles by variables tested, country coverage, and general results. It is clear from this table that political business cycle theory has found its most favorable tests in developing rather than in developed countries. Yet, in no case (with the sole exception of Khemani (2000) which is limited to India) has anyone tested for cycles in the composition of public expenditures, despite its centrality in recent theory. The present paper thus presents the first cross-country test of electoral effects on the composition of public spending in developing countries.

III. Data and Empirical Strategy

The empirical tests presented here draw on a panel of annual data from 69 developing countries (listed in Appendix 1) for the period 1975-1990. Table 2 provides descriptive statistics for the variables included in the analysis. The data include current and capital expenditure shares of total central government expenditures, as well as information on election dates and indices of electoral competitiveness and democracy. Budget share data were originally used in Devarajan,
Swaroop, and Zou (1996) and were obtained from the World Bank’s Public Economics Research Group. Data on election dates and the index of electoral competitiveness are drawn from the World Bank’s recent Database of Political Institutions (version 2), described in Beck, et. al. (2000). The full data set includes 1104 country-year observations, among them 103 presidential elections. The present analysis employs only the 93 of these elections that occurred in presidential systems, which are typically characterized by exogenously fixed election dates. Restricting the analysis to presidential systems thus addresses the potentially biasing effects of endogenously-timed elections (as typically characterize parliamentary systems of government). The use of an instrumental variables estimator (described below) further addresses the potential issue of endogenous elections.

For each budget category tested \((y)\), the specification for estimation takes the form

\[
y_{it} = \beta_0 + \sum_{j=1}^{k} \beta_j y_{i,t-j} + \sum_{j=1990}^{1975} \lambda_j YEARDUMMY_j + \gamma_1 ELE_{i,t} + \gamma_2 ELE_{i,t-1} + \\
\gamma_3 ELE_{i,t-2} + \gamma_4 (ELE_{i,t} \times PCOMPDUM_{i,t}) + \gamma_5 (ELE_{i,t-1} \times PCOMPDUM_{i,t-1}) + \\
\gamma_6 (ELE_{i,t-2} \times PCOMPDUM_{i,t-2}) + \gamma_7 PCOMPDUM_{i,t} + \alpha_i + u_{it}
\]

where

\[
ELE_{it} = \begin{cases} 
1 & \text{if } t \text{ is an election year} \\
0 & \text{otherwise},
\end{cases}
\]
$\alpha_i$ is an unobserved time-invariant country-specific effect, and PCOMPDUM is an indicator of electoral competitiveness. Controlling non-interactively for PCOMPDUM ensures that its contribution to any measured election-year effect does not wrongly include an effect that is present in non-election years as well. The use of this indicator of electoral competitiveness is novel in empirical tests of political business cycles.

PCOMPDUM, is based on a 7-point scale (PCOMP) characterizing the competitiveness of presidential elections in country $i$ at time $t$. This variable, originally derived for Africa in Ferree and Singh (1999) and globalized in Beck, et.al. (2000), is the first effort to quantify electoral competitiveness based on the number of parties competing in the last election. The scale is defined as follows (where the numbers in parentheses indicate the frequency of each category among the 93 presidential elections included in the analysis):

1. no executive (0)
2. unelected executive (24)
3. elected, one candidate (24)
4. one party, multiple candidates (0)
5. multiple parties are legal, but only one won seats (because other parties did not exist, compete or win seats) (0)
6. multiple parties compete and won seats (but one party won 75 percent or more of the seats) (20)
7. the largest party received less than 75 percent of the seats (25)

From this scale, I construct a single dummy variable for competitive elections, such that

$$PCOMPDUM_t = \begin{cases} 1 & \text{if } PCOMP_t \geq 6 \\ 0 & \text{otherwise.} \end{cases}$$
PCOMPDU, unlike more commonly used political indicators such as the Gastil index, is based on objective criteria, and more directly captures the effect of electoral incentives on policymakers.

The dependent variable in each specification is a particular budget share category to be tested for evidence of political cycles. This test is implemented for current consumption and capital investment expenditures as shares of central government expenditures. Political budget cycle theory directly predicts an increase in the former and a decrease in the latter during election years. The appropriate number of lags \( k \) on the dependent variable was determined by the Schwarz Information Criterion.

The presence of lagged dependent variables with panel data complicates estimation. OLS is not necessarily biased by the lagged dependent variable but fails to control for country-specific unobserved effects, while standard fixed effects (within-groups) estimates are biased by construction in the presence of lagged dependent variables.\(^5\) Arellano and Bond (1991) resolve these problems with a generalized methods of moments (GMM) estimator.

Arellano and Bond’s estimation strategy, employed here, is to first-difference the equations to eliminate unobserved country effects, and to fix the resulting inconsistency by applying instrumental variables consisting of appropriately lagged levels of the variables. The set of valid instruments grows incrementally as the year in question approaches T. Arellano and Bond’s (1991) GMM estimator builds on this foundation and fixes the remaining problem of autocorrelated errors in the resulting model.
IV. Results

The results presented in this section strongly confirm the empirical predictions of political budget cycle theory, though with the important caveat that predicted effects are only observed in countries with competitive elections. It is reasonable to expect this to be true, since the incentives for such manipulation are greater when elections are more meaningful and the incumbent has a positive probability of losing office. This caveat is of particular importance in applications of political budget cycle theory to developing countries, where electoral competitiveness is not to be taken for granted.

Current Consumption Expenditure Share

Table 3 presents the results of GMM estimation of equation (2), where the dependent variable is the current expenditure share of total expenditures. Column 1 presents a “baseline” estimation of equation 2 in which I omit lags on the election dummy and its interaction with PCOMPDUM, as well as year effects. The results suggest, as predicted, that the current consumption share of total expenditures in election years is not different from other years in non-competitive electoral systems; while in competitive systems, current expenditure shares increase by nearly 0.7 percentage points during election years. This result is statistically significant at the .05-level. Column 2 augments the baseline specification by including the statistically significant year dummy variables. (The excluded year dummies in this specification are neither singly nor jointly significant.) Including year effects allays concern that the baseline results reflect some idiosyncratic year effect. Indeed, the only difference resulting from the inclusion of year effects
is to slightly decrease the precision with which the competitive election-year effect is measured (which could result from the reduced degrees of freedom in specification 2). The point estimate for the competitive election-year effect remains unchanged.

Column 3 introduces a more flexible dynamic structure, including two lags of the election dummy and its interaction terms. The combination of this dynamic structure and the system of interaction terms permits one to distinguish the time path of election effects in the non-competitive ($\gamma_1$, $\gamma_2$, and $\gamma_3$) and competitive cases ($\gamma_1+\gamma_4$, $\gamma_2+\gamma_5$, $\gamma_3+\gamma_6$). Column 3 reveals that in the non-competitive cases, the lack of election year impact on current consumption spending carries over through the post-election year, and is followed by a decrease in the current consumption spending share two years after the election. Not only is there no election year effect in the non-competitive cases, but there is evidence (significant at the .05-level) that the non-competitive cases experience the opposite change in current consumption spending from that predicted by the general theory.

The competitive election effects depicted in column 3 conform to predictions. The more flexible dynamic structure reveals that the current expenditure share increases by nearly 1.6 percentage points in the election year (significant at the .01-level), is cut roughly in half during the post-election year (though it remains significantly different from other non-election years at the .05-level), and is cut in half again the following year (when the current expenditure share is no longer statistically distinguishable from other non-election years).

Column 4 includes both the more flexible dynamic structure and the significant year effects. The general conclusion remains unchanged from the previous specification, though the estimated election effects are increased. In this case, the current expenditure share even two
years after a competitive election remains marginally significantly different from other non-election years. It is also interesting to note that the electoral competition dummy (PCOMPDUM) is never significantly different from zero, indicating that having a competitive election system does not affect current consumption spending in non-election years (thus heightening confidence in the measured election year effects). The lack of generally higher consumption expenditure shares in the competitive systems further suggests a distinction between the longer-term fiscal discipline that comes from greater accountability to voters, despite the heightened election-year incentive to distort public expenditures in favor of current consumption.

Capital Investment Expenditure Share

Table 4 presents the same series of specifications for the capital investment share of total expenditures. The general finding is the expected counterpart to the findings on current expenditures. The capital investment expenditure share decreases in election years, but only in systems with competitive elections.

Table 4, column 1, deviates slightly from this pattern, in indicating that the capital expenditure share actually increases during election years in non-competitive cases. This result, however, is fragile and disappears in subsequent specifications. Yet, column 1 does present clear evidence that the election year effect on the capital spending share is negative in competitive systems. In the latter cases, the capital expenditure share is statistically significantly different (at the .01-level) from both the non-competitive cases and from zero. The point estimate suggests a reduction relative to non-elections years of nearly 0.7 percentage points in the investment share of total expenditures.8

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Adding statistically significant year dummies to the baseline specification (column 2) wipes out the anomalous election year effect in non-competitive systems, and results in a slightly more negative point estimate for the election-year effect in competitive systems.

The flexible dynamic structure described above is applied to capital spending shares in column 3. As before, this approach identifies separate time paths of election effects on capital spending shares non-competitive versus competitive electoral systems. In non-competitive systems, the time path is statistically flat – the point estimates actually increase in the post election years, but are never significantly different from zero. In contrast, election years in competitive systems are associated with a 1.3 percentage point decrease in the capital expenditure share. This effect declines to a 1.1 percentage point reduction in the post-election year, and is effectively zero two years after an election. As in the results for current expenditures, adding significant year effects along with the flexible dynamic structure merely increases the magnitude of the point estimates for election year effects in competitive systems, while retaining the insignificance of the effects in non-competitive systems. The election-year effect in column 4 indicates a 1.55 percentage point reduction in the capital expenditure share of total expenditure; this effect is less negative in the post-election year (-1.1 percentage points), and becomes a positive effect two years after the election. In that sense, competitively elected incumbents appear to indulge in election year spending distortions as predicted by Rogoff (1990) and consistent with Nordhaus (1975); yet the more flexible dynamic structure further reveals that they also compensate for the prior under-investment two years hence.

In this regard, the estimated effect of competitiveness *per se* is notable. When this dummy variable enters directly into the specification it is generally positive and statistically
significant, indicating that in competitive systems, the capital spending share is on average approximately 1.3 percentage points higher than in non-competitive systems. This may reflect an incentive to maximize long-run growth when incumbents are more accountable to voters.

V. Conclusions

This paper provides the first evidence of systematic distortions in public expenditures in developing countries as a function of elections. This evidence is consistent with models of rational opportunistic political business cycles (such as Rogoff, 1990); though I present these findings as an extension of the empirical literature on opportunistic political business cycles in general, rather than as a test of a particular rational or myopic voting model. I further propose a refined interpretation of models such as Rogoff’s, which simply assume the existence of certain institutional characteristics, such as competitive elections. Relaxing that assumption leads to a prediction that political budget cycles will only be apparent when elections are competitive. Use of a newly available indicator of electoral competitiveness permits explicit testing of this refined prediction.

My results strongly confirm the general theoretical prediction that electoral incentives will lead incumbents to increase more visible forms of public expenditure at the expense of less visible forms. Comparing the election year effects on current versus capital expenditure shares of total expenditures in 69 developing countries, reveals that current expenditure shares increase as much a 2.3 percentage points during election years, while capital expenditure shares decrease by as much as 1.55 percentage points. This is true, however, only when elections are competitive. Countries with non-competitive elections exhibit essentially no election year effects
on the composition of public spending (and may even reduce current spending shares within two years of an election). That is, office-motivated incumbents appear willing to incur the disutility and economic costs associated with distortions in the composition of public spending when their tenure is threatened by competitive elections, but have no incentive to do so when they face a near zero probability of electoral defeat.

There is further evidence that competitive electoral systems create incentives in general (e.g., in non-election years) to allocate larger budget shares to capital investment. This may reflect an incentive to maximize long-run growth when incumbents are generally more accountable to voters, despite the accentuated short-run electoral incentive in competitive systems to diverge in favor of more visible forms of public expenditure.

It is particularly relevant to conduct such a test on developing countries. Given the generally weaker systems of political checks and balances in developing countries, it is reasonable to expect political budget cycles to be more apparent in developing countries. Yet, while Rogoff (1990) suggests that political budget cycles may be welfare enhancing (as the best means of identifying competent leaders), the stakes are also higher in developing countries. During the period of observation, nearly all countries in the sample were undergoing economic reform programs in which fiscal responsibility played a central role. “Preelection budget antics,” in Rogoff’s phrase, may threaten the stability of such reform efforts. Indeed, in some developing regions such as Sub-Saharan Africa, elections are increasingly frequent events as countries struggle simultaneously with the challenges of both political and economic reform. The occurrence of political budget cycles may increase the challenge of integrating political and economic reform in developing countries.
References


51.


Svensson, J. and M. Shi, “Political Business Cycles in Developed and Developing Countries,”


Terrones, M., “Macroeconomic Policy Cycles Under Alternative Structures,” working paper,

University of Wisconsin, Department of Economics (1989).

Appendix 1

List of Countries in the Data Set

Argentina
Bangladesh
Bolivia
Botswana
Brazil
Burkina Faso
Burma (Myanmar)
Cameroon
Chile
Colombia
Congo (former Zaire)
Costa Rica
Dominican Republic
Ecuador
Egypt
El Salvador
Ethiopia
Gambia
Ghana
Greece
Guatemala
Guyana
Honduras
Hungary
India
Indonesia
Jordan
Kenya
S. Korea
Lesotho
Liberia
Malawi
Singapore
Myanmar
Mali
Mauritania
Mauritius
Mexico
Morocco
Nepal

Nicaragua
Nigeria
Oman
Pakistan
Panama
Papua New Guinea
Paraguay
Peru
Philippines
Poland
Portugal
Rwanda
Senegal
Sierra Leone
Somalia
South Africa
Sri Lanka
Sudan
Syria
Tanzania
Thailand
Togo
Trinidad and Tobago
Tunisia
Turkey
Uganda
Uruguay
Venezuela
Zambia
Zimbabwe
Table 1. Previous Empirical Tests of Political Business Cycle Theory

<table>
<thead>
<tr>
<th>Author</th>
<th>Developed Country Samples</th>
<th>Developing Country Samples</th>
</tr>
</thead>
<tbody>
<tr>
<td>Tufte (1978)</td>
<td>transfers</td>
<td>U.S.</td>
</tr>
<tr>
<td>McCallum (1978)</td>
<td>unemployment</td>
<td>U.S.</td>
</tr>
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<td>Beck (1987)</td>
<td>money growth</td>
<td>U.S.</td>
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<td>Lewis-Beck (1988)</td>
<td>output, unemployment</td>
<td>OECD (non-U.S.)</td>
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<td>Keech and Pak (1989)</td>
<td>veterans’ benefits</td>
<td>U.S.</td>
</tr>
<tr>
<td>Grier (1989)</td>
<td>money growth</td>
<td>U.S.</td>
</tr>
<tr>
<td>Alesina, Roubini (1990)</td>
<td>output, unemployment, inflation, M1 growth</td>
<td>U.S. and OECD</td>
</tr>
<tr>
<td>Alesina, Cohen, Roubini (1992)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Alesina, Roubini, Cohen (1997)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sheffrin (1989)</td>
<td>monetary policy</td>
<td>U.S.</td>
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<td>Ellis and Thoma (1995)</td>
<td>current acct., RER, ToT</td>
<td>OECD (14)</td>
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<tr>
<td>Klein (1996)</td>
<td>output</td>
<td>U.S.</td>
</tr>
<tr>
<td>Paldam (1997)</td>
<td>output, unemployment</td>
<td>OECD (non-U.S.)</td>
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<tr>
<td>Faust and Irons (1999)</td>
<td>monetary policy, output, unemployment</td>
<td>U.S.</td>
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<td>Gartner (1999)</td>
<td>inflation</td>
<td>G-7</td>
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<td>Carlsen and Pedersen (1999)</td>
<td>output inflation</td>
<td>OECD</td>
</tr>
<tr>
<td>Carlsen (1999)</td>
<td></td>
<td>U.S.</td>
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<tr>
<td>Ben-Porath (1975)</td>
<td>taxes</td>
<td>Israel</td>
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<tr>
<td>Brender (1999)</td>
<td>fiscal policy</td>
<td>Israel</td>
</tr>
<tr>
<td>Krueger and Turan (1993)</td>
<td>fiscal aggregates</td>
<td>Turkey</td>
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<tr>
<td>Gonzales (1999)</td>
<td>total govt. expenditure</td>
<td>Mexico</td>
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<td>Ames (1987)</td>
<td>total govt. expenditure</td>
<td>Latin America (17)</td>
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<td>Edwards (1993)</td>
<td>inflation</td>
<td>Chile</td>
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<td>Remmer (1993)</td>
<td>inflation, devaluation</td>
<td>Latin America (8)</td>
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<td>Schuknecht (1996, 1999a, 1999b)</td>
<td>output, fiscal deficit</td>
<td>LDCs (35)</td>
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<td>Moyo (1999)</td>
<td>public savings</td>
<td>mixed developed and developing countries</td>
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<td>India</td>
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<td>Shi and Svensson (2000)</td>
<td>Govt. consumption, fiscal surplus, tax revenue</td>
<td>mixed developed and developing countries</td>
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<td>money growth</td>
<td>Cameroon</td>
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<tr>
<td>Block (2000)</td>
<td>fiscal and monetary aggregates</td>
<td>Sub-Saharan Africa</td>
</tr>
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</table>

a. Based, with additions, on the review in Drazen (2000). This list emphasizes U.S. and cross-country studies, and is thus not a comprehensive list of empirical tests (though none of the excluded papers test for compositional effects of public budgets).
Table 2. Descriptive Statistics

<table>
<thead>
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<th>Variable</th>
<th>mean</th>
<th>std. dev.</th>
<th>n</th>
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<tbody>
<tr>
<td>CURETE</td>
<td>79.66</td>
<td>12.06</td>
<td>860</td>
</tr>
<tr>
<td>CAPETE</td>
<td>20.09</td>
<td>11.46</td>
<td>874</td>
</tr>
<tr>
<td>PCOMPDUM</td>
<td>.47</td>
<td>.50</td>
<td>1104</td>
</tr>
<tr>
<td>PELX</td>
<td>.09</td>
<td>.29</td>
<td>1104</td>
</tr>
</tbody>
</table>

(Variable names: CURETE = current expenditures as a share of total expenditures; CAPETE = capital expenditures as a share of total expenditures; PCOMP = dummy variable for competitive elections (defined in text); PELX = dummy variable for election years)
Table 3. GMM Regression Results for Current Expenditures, 1975-1990a

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Current Expenditures as Share of Total Expenditures</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>ELE(_t) ((\gamma_1))(^b)</td>
<td>-.514</td>
</tr>
<tr>
<td></td>
<td>(.415)</td>
</tr>
<tr>
<td>ELE(_t) (-1) ((\gamma_2))</td>
<td>.419</td>
</tr>
<tr>
<td></td>
<td>(.922)</td>
</tr>
<tr>
<td>ELE(_t) (-2) ((\gamma_3))</td>
<td>-1.46**</td>
</tr>
<tr>
<td></td>
<td>(.696)</td>
</tr>
<tr>
<td>ELE(_t) * PCOMPDUM ((\gamma_4))</td>
<td>1.21**</td>
</tr>
<tr>
<td></td>
<td>(.523)</td>
</tr>
<tr>
<td>ELE(_t) (-1) * PCOMPDUM ((\gamma_5))</td>
<td>.409</td>
</tr>
<tr>
<td></td>
<td>(.842)</td>
</tr>
<tr>
<td>ELE(_t) (-2) *PCOMPDUM ((\gamma_6))</td>
<td>1.92**</td>
</tr>
<tr>
<td></td>
<td>(.937)</td>
</tr>
<tr>
<td>PCOMPDUM ((\gamma_7))</td>
<td>-.102</td>
</tr>
<tr>
<td></td>
<td>(.450)</td>
</tr>
</tbody>
</table>

Significant Year Dummies Included: yes

Total Effect of Competitive Elections, time = t, (\(\gamma_1 + \gamma_4\))

<table>
<thead>
<tr>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>.693**</td>
<td>.693*</td>
<td>1.58***</td>
<td>2.32***</td>
</tr>
<tr>
<td>(.317)</td>
<td>(.387)</td>
<td>(.392)</td>
<td>(.622)</td>
</tr>
</tbody>
</table>

Total Effect of Competitive Elections, time = t-1, (\(\gamma_2 + \gamma_3\))

<table>
<thead>
<tr>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>.828**</td>
<td>.469</td>
<td>.942*</td>
<td>.942*</td>
</tr>
<tr>
<td>(.411)</td>
<td>(.392)</td>
<td>(.568)</td>
<td>(.568)</td>
</tr>
</tbody>
</table>

Total Effect of Competitive Elections, time = t-2, (\(\gamma_3 + \gamma_6\))

<table>
<thead>
<tr>
<th>n</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>444</td>
<td>444</td>
<td>444</td>
<td>444</td>
<td>444</td>
</tr>
</tbody>
</table>

Sargon Test (P-value)\(^d\)

<table>
<thead>
<tr>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>.005</td>
<td>.019</td>
<td>.003</td>
<td>.005</td>
</tr>
</tbody>
</table>

LM2 Test (P-value)\(^e\)

<table>
<thead>
<tr>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>.836</td>
<td>.775</td>
<td>.80</td>
<td>.806</td>
</tr>
</tbody>
</table>

*** = significant at .01 level; ** = significant at .05 level; * = significant at .10 level

a. Estimates for the lagged dependent variables are suppressed, but available upon request from the author.
b. Estimate for the election-year effect when PCOMP indicates a non-competitive electoral system.
c. Estimate on the interaction term between the election dummy and the PCOMP dummy, indicating the difference between the election-year effect when the PCOMP dummy indicates a competitive electoral system and when it indicates a non-competitive electoral system.
d. P-value for Sargon test of over-identifying restrictions (null hypothesis of acceptable instruments).
e. P-value for Lagrange multiplier test of second-order serial correlation in the first-differenced residuals.
Table 4. GMM Regression Results for Capital Expenditures, 1975-1990\textsuperscript{a}

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Capital Expenditures as Share of Total Expenditures</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$E_{t-1} (\gamma_1)$\textsuperscript{b}</td>
<td>$E_{t-1} (\gamma_2)$</td>
<td>$E_{t-2} (\gamma_3)$</td>
<td>$E_{t-1} * \text{PCOMP Dum} (\gamma_4)$</td>
<td>$E_{t-2} * \text{PCOMP Dum} (\gamma_5)$</td>
<td>$\text{PCOMP Dum} (\gamma_6)$</td>
</tr>
<tr>
<td>(Y_t)</td>
<td>(Y_t)</td>
<td>(Y_t)</td>
<td>(Y_t)</td>
<td>(Y_t)</td>
<td>(Y_t)</td>
</tr>
<tr>
<td>.594*** (.196)</td>
<td>-.167 (.419)</td>
<td>.491 (.942)</td>
<td>.558 (716)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>.574 (.907)</td>
<td>-.163 (1.10)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1.19 (.790)</td>
<td>.341 (1.34)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>-1.27*** (.268)</td>
<td>-9.54 (.865)</td>
<td>-1.80* (1.05)</td>
<td>-2.11** (.895)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>-1.71* (.969)</td>
<td>-9.60 (1.15)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>-.767 (.628)</td>
<td>.387 (1.30)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>.855*** (.235)</td>
<td>.235 (.330)</td>
<td>1.34** (.598)</td>
<td>1.28** (.644)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Significant Year Dummies Included

<table>
<thead>
<tr>
<th>Total Effect of Competitive Elections, time = t, ((\gamma_1 + \gamma_2))</th>
<th>Total Effect of Competitive Elections, time = t-1, ((\gamma_2 + \gamma_3))</th>
<th>Total Effect of Competitive Elections, time = t-2, ((\gamma_3 + \gamma_4))</th>
</tr>
</thead>
<tbody>
<tr>
<td>-.675*** (.190)</td>
<td>-1.12** (.581)</td>
<td>-1.31*** (.413)</td>
</tr>
<tr>
<td>-1.14** (.465)</td>
<td>-1.12** (.533)</td>
<td></td>
</tr>
<tr>
<td>.398 (.400)</td>
<td>.728*** (.230)</td>
<td></td>
</tr>
</tbody>
</table>

n | 512 | 512 | 512 | 512 |

Sargon Test (P-value)\textsuperscript{d} | .421 | .524 | .486 | .514 |

LM2 Test (P-value)\textsuperscript{e} | .283 | .084 | .277 | .352 |

*** = significant at .01 level; ** = significant at .05 level; * = significant at .10 level

\textsuperscript{a} Estimates for the lagged dependent variables are suppressed, but available upon request from the author.

\textsuperscript{b} Estimate for the election-year effect when PCOMP indicates a non-competitive electoral system.

\textsuperscript{c} Estimate on the interaction term between the election dummy and the PCOMP dummy, indicating the difference between the election-year effect when the PCOMP dummy indicates a competitive electoral system and when it indicates a non-competitive electoral system.

\textsuperscript{d} P-value for Sargon test of over-identifying restrictions (null hypothesis of acceptable instruments).

\textsuperscript{e} P-value for Lagrange multiplier test of second-order serial correlation in the first-differenced residuals.
1. Alesina, Roubini, and Cohen (1997) provide a comprehensive review of all branches of political business cycle theory.

2. The primary distinction between the empirical predictions of the Rogoff and Nordhaus models is that the former points more directly to the manipulation of policy tools while the latter originally described differences in macroeconomic outcomes based on a short-run Phillips curve. Not restricting the present empirical investigation to Rogoff, *per se*, is also important in that I do not directly measure or test competence.

3. Relevant theory (Rogoff, 1990 and Terrones, 1989) suggest that endogenous elections will still create incentives for political business cycles, though they expect such cycles to be dampened in those systems. Nonetheless, one would have some concern econometrically for the potential bias resulting from endogenous elections. They are thus excluded from the present analysis.

4. Beck, et. al. (2000) provide the following qualifying details for this scale: Executives who are either elected directly by the population or are elected by an electoral college that is elected by the people *and* has the sole purpose of electing the executive are scored on this scale. Executives elected by bodies other than these are given the same score that the electing body would be given (in a parallel index for legislatures). Even if the electing body is not the actual “legislature” that is tracked in the legislative index, the competitiveness of that body is used to score the executive. This means that competitively elected prime ministers get 6 or 7. The chief executives of Communist nations (The chairman of the Communist Party) is given a 3, because they are elected by the Party Congress, electing bodies which they do not appoint. Executives elected by small, appointed juntas or by appointed electoral colleges get 2. Rival chief executives in one country, particularly in the setting of armed conflicts, are counted as No executives, and this score a 1. Referenda and votes by “popular acclamation” on unelected executives are scored as 3. If executives unilaterally extend their terms of office, they get a 2 starting in the year they should have held elections. Any executive elected for life, even by the people or an elected assembly, gets a 2.

5. Hsiao (1986) shows that correlation between the lagged dependent variable and the error term in a panel of length T is of the order 1/T.

6. Note that the point estimate for the interaction term ($\gamma_4$) indicates the difference between the non-competitive case ($\gamma_0$) and the competitive case. The total election year effect in the competitive case is the linear combination $\gamma_1 + \gamma_4$.

7. In this specification, the Sargon test rejects the null hypothesis of acceptable instruments. Arellano and Bond (1991) note, however, that this can result from heteroskedastic errors, which are present in this specification.

8. Given the correlation between current and capital expenditure shares of -.85, it is not surprising that the results presented in Table 4 are nearly symmetrically opposite from those presented in Table 3.